Dear Sir,

I have long since been desirous to write you, but I was afraid to disturb you, as I knew how pressed you were at the time. But now you have given me yourself a good reason to believe me of the letter. For a few days ago, I received by Mr. Sullivant with the greatest pleasure the second volume of your magnificent Genera. Certainly, I have done nothing so much with a bonhomie and friendship as you have given me so many years since I came to America. And it is a reason to be more and more thankful of your kindness. Your Genera and particularly the second volume is the most beautiful and valuable book I saw in Baltimore. Now, I read your words, show I admire the correctness of your descriptions, the wisdom of your affections and the value of your knowledge in Botany. The Manual of the Botany has been so useful to me that I have determined and ascertained more than five hundred species of phanerogamous collected here in the this G. what I could not have done were it not for European plants with European descriptions. Now, The same kindness I have been by Mr. Sullivant and I am afraid that European people shall never be able to do something valuable of American Botany as you and me can do. As to the American Botany I have determined never to need to depend on single plants, as if it has been examined by you or by Mr. Sullivant or by myself, when I shall be able to do it. As it is possible to study plants when one has not seen it growing in its natural place, when one was not able to compare it with the many varieties of form to which every one is liable. According to that reason, I shall have only to send you some doubtful species and I hope you will be disposed of looking at those plants for as much as possible, I will send you the interesting and rare ones.

You have heard, probably of the determination of Mr. Sullivant who don't appear to want me more at his house and for himself. Since the first of March I shall be gone and do my best for myself. I was just a little unequivoque for my future living. But I trust to the kindness of Providence who gives the food to every hand. If 220 or 230 my debt, the Lord will certainly help. Before leaving, Mr. Sullivant proposed to me to go south and to explore the wilderness of Alabama and after the mountain of Georgia and Carolina. I have accepted this proposal with great pleasure so I could more abandon the botany and perhaps this spring shall prove to be an advantage to me. Your...
Dear Mr. Freyman,

I am pleased to receive the list of plants you sent me in your letter of January 31. I appreciate the opportunity to study these plants and their descriptions. I look forward to learning more about them and possibly incorporating them into my collection.

I am currently preparing for the upcoming exhibition of my collection and would like to include some information about your plants. I have already started compiling the necessary data and will be eager to work on this when I return home.

I am sending you additional plants as per our agreement. I hope you will find them suitable for your collection.

Thank you for your support and for sharing your knowledge with me.

Sincerely,

[Signature]

P.S. I would like to express my gratitude for the opportunity to work with you. I am looking forward to our future collaborations and the success of our mutual endeavors.

[Additional notes on the right page, regarding the plants and their origins.]

Yours sincerely,

[Signature]

Swamp hemlock sphagnumise. Sub.

Beauveria heather. Land 6 ft. high. Te. Columbia.

Olive green leaves. Wood near Columbia.

Chamaecyparis latifolia. Gray Hills near Fort Benton.

Roman acuminata. Phil. Campanula maritima. This

Lycopodium. Subalpine. Te. near. swolnly woods near Columbia.

Phytospermum globuliferum. For March, 5.

Usnea armata. Ml. New York. (it goes not here)

Usnea armata. Ml.

Artemisia franser. Gray. Ev. woods. This.

Tanacetum. Toas. Finger. This.

Campanula globulifera. Te. Hills near Bandon. Te.


Let Le: I name you these plants of memory having no time

To look at my collection. There is probably some errors but a great

many plants I don't name here. For the most, I collected in

Ohio. B. I mostly few species which were not found again on the

coast, 600 for America. And one or two I think new only.
Columbus, 18e Janvier 1832.

Cher Monsieur !

Je suis bien content d'Europe ma vie est bonne, j'ai un journal, et de temps en temps, j'y roule maints fois pour prendre la liberté de vous écrire. J'ai toujours trouvé le droit de me parler entièrement et il est de ma part de bienveiller envers vous aussi bien que mes amis. Je vais vous dire quelques mots en tête de lettre précieux mémoire. En me rentrant à Cambridge, vous avez dû vous étonner, une énorme quantité de matériaux intéressant. Après en ayant beaucoup plus de travaux encore que vous n'en avez pu à dire. Ainsi, je vais vous quelques jours, une nouvelle preuve d'intérêt qu'on fait un plaisir extrême, je me crois presque vous avez envoyé quelque paroles de remontrance, et je vous souhaiter que vous soyez arrivé de votre côté de part, toutautant que vous. Aprèste lettre de quelques amis, je vous souvien de vous, et vous souhaiter que vous soyez tous. Ceci enfin que Mr. Daly, Lord, Shelleworth, ait parlé avec beaucoup de travail, que vous avez été, si vous voulez savoir, un charmant entre de plantes de l'Europe un échange de ses plantes, jurasses, le lendemain bien à propos que vous pouvez vous rentrar.
que j'ai a bien déterminé de prendre d'abord la collection des plantes de chez nous. Cette collection est très importante et nous permet de bien comprendre la diversité des plantes de notre région. Je propose que nous commencions par la collection des plantes des environs de Boston, car elles sont particulièrement intéressantes.

La collection de plantes des environs de Boston est très importante car elle permet de bien comprendre la diversité des plantes de notre région. Nous pouvons commencer par la collection des plantes des environs de Boston, car elles sont particulièrement intéressantes.

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l'envy des trouble de 

sur un de mes bons amis. Cet angle donne beaucoup d'aise, puis qu'il faut se soumettre à une étude longue, avantage que guère se rencontre. J'ai malgré tout décidé de m'engager dans l'étude de la botanique. Je commence peu à peu la rédaction botanique des fleurs, l'herbe, le moine, le 

m'en souvenir que comme un commencement, 

gauche, que je redoute par l'aide de l'entrepreneur. Je le sais, mais je crois que je me

moins de mes petits louche, que Monseigneur Le roi parle à un ami. Je suis presque sûr que nous

recevoirons une telle une bonté d'une

qui est posé par la hauteur de l'entreprise. Je le sais, mais je crois que je me

moins de mes petits louche, que Monseigneur Le roi parle à un ami. Je suis presque sûr que nous

recevoirons une telle une bonté d'une
Columbia, 27 Octobre 1852.

À ma mère,

Je viens te remercier de ta lettre du mois dernier, que je recevrai dans le courant de la semaine. Devant la situation difficile que nous devons traverser, je ne suis pas assuré de la délivrance prochaine de ma lettre. Je suis heureux de te savoir en bonne santé et en bonne humeur. Je te souhaite de continuer à bien te reposer.


Je te prie de bien vouloir me faire parvenir ta réponse pour que nous puissions continuer notre correspondance. J'espère que tu as trouvé du répit et que tu es maintenant en mesure de te consacrer à tes affaires.

Je reste, chère mère, tes affectueux fils,

[Signature]
Je t'écris pour te donner de nos nouvelles. Nous sommes arrivés hier soir à Paris, après une traversée de la Manche en bateau de commerce. Le temps était incroyable, mais nous avons survécu. De nombreux navires sont arrivés à Paris ces derniers jours. Je t'envoie cette lettre pour t'informer de nos dernières nouvelles et de nos projets pour la prochaine saison.

Nous sommes maintenant en train de planifier notre voyage pour la Chine. Nous espérons que nous serons en mesure de partir d'ici quelques semaines. Je t'envoie des photos de nos dernières aventures et des nouvelles de la famille.

Je t'envoie cet été enfin pour te donner de nos nouvelles. Nous sommes tous en bonne santé et nous espérons que tu te portes bien également. Je t'envoie des photos de nos dernières aventures et des nouvelles de la famille.

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di cella comune, qui non podem olio guarna van l'aula

de P. Virginito. Il crocio d'la toa grende, au deno de Gallipoli,

tal, le bge in tot e fat prestro, ramante, fraicando,

cy cap nev!; le feulé, differen te tot e folt. —

Lui honte p di noi occupa i long time, pone in fun

di mar, che Monrino. Scuom mi, en sognando en falsa

qui giuvre o fane, un momente con vui di nostre cher

famie. — De mae Laura (la plus sinner) commiseration,

noba ami devon. — Ate Pesquereau

Lei grazia, sont don, la papiru casto, au deno,

e le Carne. Fea il mno renavges, le note de St

Borino en le guard fo resole le cupio. Improwai,

a prende cupio se van amj daitu ame, pendaent qui

piume il revalle le planta.
Cher Monseur,

Je suis content que vous ayez bonne patience pour attendre tant que ce fléau, la réponse à robe, serait belle lettre de M. D'Albert. Je reçois de la lettre de noblesse, j'apprécie. Les affaires sont en cours de quelque manière, mais pour tout mon bon, vous promis, tout franchement, que je ne suis pas, sans doute. Je vous prie, par principe, que vous me renvoie la lettre de M. D'Albert. Je suis bien de mon ami, quant à vous présente, et retourne tant. J'ai fait le menu, mon ami, je crois le limoge, a toujours et m'entendre, mon ami, que le manque d'indépendance, le tragique conserver, que nous affaire individuellement. J'appelle à des bonnes, tout et me réunir. Sain, droite, en épicerie, émouvante, non, naturaliste, âgé, ou fait dans le part de circonstance, limité. Un fait qui prospère bien peu, qui étudient, pour comparer le fléau d'Amérique, belle étrange et, comme les, sans la vue, est président de l'Union, sous le devoir, qui l'entraîne toujours, quelque chose d'étrange, de mon désir. Il ne s'est pas en avoir, personne, une telle
espece qui se retrouvait abondamment la même année que le houblon de l'Europe. Le même fait se présenta aussi l'année suivante, et se reproduisit de même l'année suivante. Enfin en 1845, la plante a été vue dans les campagnes de l'Europe du Sud. 

Disons que la même année que le houblon fut introduit en France, il a été introduit en Angleterre et en Allemagne. Cependant, il est plus tard que dans les deux derniers pays. En Allemagne, il a été introduit en 1846, et en Angleterre en 1847. 

En France, il a été introduit en 1849, et en Belgique en 1850. 

Enfin, en Italie, il a été introduit en 1851, et en Espagne en 1852. 

Enfin, en Russie, il a été introduit en 1853, et en Roumanie en 1854. 

Enfin, en Turquie, il a été introduit en 1855, et en Grèce en 1856. 

Enfin, en Suisse, il a été introduit en 1857, et en Allemagne en 1858. 


Enfin, en Belgique, il a été introduit en 1861, et en Allemagne en 1862. 

Enfin, en Russie, il a été introduit en 1863, et en Roumanie en 1864. 

Enfin, en Turquie, il a été introduit en 1865, et en Grèce en 1866. 

Enfin, en Suisse, il a été introduit en 1867, et en Allemagne en 1868. 

Enfin, en Angleterre, il a été introduit en 1869, et en France en 1870. 

Enfin, en Belgique, il a été introduit en 1871, et en Allemagne en 1872. 

Enfin, en Russie, il a été introduit en 1873, et en Roumanie en 1874. 

Enfin, en Turquie, il a été introduit en 1875, et en Grèce en 1876. 

Enfin, en Suisse, il a été introduit en 1877, et en Allemagne en 1878. 

Enfin, en Angleterre, il a été introduit en 1879, et en France en 1880. 

Enfin, en Belgique, il a été introduit en 1881, et en Allemagne en 1882. 

Enfin, en Russie, il a été introduit en 1883, et en Roumanie en 1884. 

Enfin, en Turquie, il a été introduit en 1885, et en Grèce en 1886. 

Enfin, en Suisse, il a été introduit en 1887, et en Allemagne en 1888. 

Enfin, en Angleterre, il a été introduit en 1889, et en France en 1890. 

Enfin, en Belgique, il a été introduit en 1891, et en Allemagne en 1892. 

Enfin, en Russie, il a été introduit en 1893, et en Roumanie en 1894. 

Enfin, en Turquie, il a été introduit en 1895, et en Grèce en 1896. 

Enfin, en Suisse, il a été introduit en 1897, et en Allemagne en 1898. 

Enfin, en Angleterre, il a été introduit en 1899, et en France en 1900. 

Enfin, en Belgique, il a été introduit en 1901, et en Allemagne en 1902. 

Enfin, en Russie, il a été introduit en 1903, et en Roumanie en 1904. 

Enfin, en Turquie, il a été introduit en 1905, et en Grèce en 1906. 

Enfin, en Suisse, il a été introduit en 1907, et en Allemagne en 1908. 


Enfin, en Belgique, il a été introduit en 1911, et en Allemagne en 1912. 

Enfin, en Russie, il a été introduit en 1913, et en Roumanie en 1914. 

Enfin, en Turquie, il a été introduit en 1915, et en Grèce en 1916. 

Enfin, en Suisse, il a été introduit en 1917, et en Allemagne en 1918. 

Je te donne mon mot sur ce que je vais faire en leur propre

frais pour moi à quelque chose d'original.

Je ne te demanderai plus, car tu as obtenu

la suite de ma correspondance. Je n'offrirai

pour la faire que ce que nous empêchons de

voir de la part de nos amis, le plus 

connaître votre hospitalité avec le plus grand

plaisir. A la rigueur

de circonstances, je prends pour vous, mais ne,
dans le travail, tu diras que d'assez tard que

va voir la justice. Prêtre les copy, et

manuscris ; mais je te dis, je jouera à tous les

copiers la devant

de nos copies et vous demandez ce que nous avons

et la suite de cette Perpétuelle, sans laquelle

est encore à déplorer. Mais, comme la

conde partie qui est avère ami est lettrant

le laisser, je vous meus, par vous que nous,

nuit naturellement celui qui, mais que mon

nuit pas mieux qui nous est nécessaire comme

voire demandez toujours aux exemplaires. Je

nul et en imposé, je vous bien heureux à

l'honneur.

Dieu soit bénir...
Cher Monsieur,

Depuis fort longtemps je me suis abstenu de vous écrire abstenue de voua
important du mon lettre... Comme tout reste trop est employé à des recherches scientifiques d'une grande
importance que votre bienveillance pour moi est
Toutefois, puisque vous avez mon humble sœur, je
n'avais pas le temps. Je pourrais probablement
jamais s'en faire pour le moment, la meilleure pense
l'affection. Je respecte que je puisses vous donner ce
ma souris est de voue espagne des vues des lieux, ce qui
m'ont amour intérêt direct pour vous. J'ai vu espérer
avant bien mieux. J'ai seulement voue écrire une
lettre de vous, où vous parlez de sans définit, avec M.
Roger en termes qui m'ont fait trop de plaisir pour que
je ne me trompe pas de vous en remercier. Certes, si je
suppose avoir le moindre tort à Roger de quelque
Surtout avec un homme pour lequel j'ai travaillé
je qui a eu pour moi, quelques bons offices, je m'ens replan
sur de les espérer. Mais je crois au contraire avoir fait
à M. Roger trop de concessions. Il est par que je me
suis avoué à tenir encore, que la rupture a été tout
frais à l'aise. Peut-être pour vous convaincre entièrement
de détails que ne peuvent vous intéresser, je vous dir
l'affaire avec plus grande simplicité. Par ailleurs
sont signé de M. Roger, il s'engage à m'payer
Jeux de la fin de l'année, en la plantation de l'Aubrun.

Jeune femme, en octobre ou novembre. Son attitude, je la connais, qu'il semble retenir à rire, et après ma mort, j'aurais eu de l'amitié pour tout ce qui est de ma condition.

Les deux mois de notre vie, de la mort de ma mère et de la naissance de mon élève, il me semble devenu moins joyeux, mais de plus en plus solitaire.

Il semble que je ne puisse plus, après ma mort, qu'aller dans les champs, et après ma mort, je ne puisse plus. C'est là la vie d'un être humain, et après ma mort, il me semble devenu moins joyeux, mais de plus en plus solitaire.
In the second volume of my Flora, I divided the families and described the genera and species following the method of Schimper. I have just begun to make a collection of plants from America, Europe, and from the mountains of Mexico. I have also begun to describe new species and genera. The method is to be continued in the third volume. The results will be published in the next year.
Columbus, 9 May 1865

Cher Monsieur,

Votre ami Guyot, je l'espère vous aura de ma part annoncé mon prochain voyage en Angleterre. Il est tout à fait possible de vous écrire que je serais heureux si je pouvais vous trouver agréable en quelque chose. Je verrai sans doute l'occasion de vous le dire en personne.


Vous comprenez sans doute que j'emporte aussi moi pour Schimper le exemplaire de vos plantes d'Amérique. Nous sommes donc à peu près parti mais ce n'est pas sans un travail vraiment considérable. Nous avons passé tout l'hiver à recevoir les hypothèses pour les mettre à jour. — Le voyageur de magnifique travail de M. Schimper. Vous pourrez publier le manuscrit, étiquettes le tout après mon retour d'Europe. Je vous assure mon désir de vous adresser un exemplaire de ce travail que, bien que de môme importante, ne sera pas sans valeur pour la botanique Américaine. M. Bullivant va très bien et ajoute travailler beaucoup cet hiver. Je n'ai pas besoin de vous dire que j'en suis ravi aussi toujours que possible.

Veuillez je vous prie, cher Monsieur, me rappeler au teneur de monsieur Gray et agir l'expression un mon affection sincère et respectueuse.

Vos tout d'vous.

Lei Jesquereus
Dear Sir,

I wrote you this morning and this evening I had the pleasure to receive a letter from you. I am acquainted with Mr. Pringyelli only by what friend Godard told me about him. He called on me last week and I was truly pleased with his manner and deportment. He looks like a good, healthy and temperate man. This, Mr. Godard writes about him:—"N'est-ce pas Pringyelli qui le premier avait fait son apprentissage au jardin botanique de Turin? Je crois tellement à Rome que va partir lundi, et vous dirai à deux ou trois jours de la déesse. J'ai un excellent jardinier qui s'embarque, et quelques secours que je vous recommande. Huit trente l'heure. Je ferai à tout jour pour le temps du train. I should think from what Mr. Godard writes from what I have seen myself, and from the certificate that he has received, that that it would suit you exactly. Nevertheless, as I am to be in Europe in about 4 weeks, I could make such inquiries about him and write you immediately and you could
You are acc. accordingly. I have news from him. He has found immediately something to do at Philadelphia where he will well recommend him. I have seen Mr. Sullivan this morning. He has received your packet and is very happy about it. But I am entirely ignorant of a letter from Godet. I have received it through Mr. Sullivan. You sent me one containing a long critical analysis of your American works, but I ought to hands return it to you and I think I will do it. I will go to night to Mr. Sullivan’s house to take it. Mr. Sullivan has nothing to lend to Cambridge, so I rather choose to send for you these few plants of which I spoke this morning. I am much obliged to you for the paper of Mr. D. Durand. I would that I could send you also something. I had had time I would have sent you a note about some plants especially some localities in the Armen. There is your friend. But you see will advise that I can gather a few advice. In any case I am certain that your second edition like the first will be a very good and useful book and will sell perfectly. The more, want a great deal of corrections and especially of addition. I rather think that Mr. Sullivan had better to publish a good group of American novels and tragedies. But he is far from being ready in this world. I hope to send a New York a letter from you and also your desiderata for Europe.

Very respectfully your friend

S. S. French.

Mr. Sullivan sends the very pleasant letter of Godet. He has forgotten it. All right now. I have read it tells me friend Godet is much mistaken if he thinks that I have forgotten him. I shall take care of giving full proof of my friend ship. I would gladly you sent me to New York an允许 to him and some plants also if you have any for him.
Venerdì 1° Giugno 1855

Caro Signore, carissimo amico,

Mi hanno detto che a Colombo ci sono state tristi notizie, ma non riesco a crederci. Mi hanno anche detto che il paese è in braccio alla povertà. 

Mi dispiace molto per lei, signor mio. Le resto fedele. 

L'unico rimpianto che ho è che non posso condividere la sua felicità. 

Si, è vero che il Veronese è un Alpino. La scultura è un monaco che ha vissuto per secoli. 

La vita è un continuo affaticamento. 

Come potrei dimenticarmi di genere di un tempo davanti alla realtà? 

Vedo con piacere che come i tuoi, anche i miei, ed i tuoi, si compone a pensiero. 

Spero che tu stia bene e che tu possa continuare a essere felice. 

Tua amica e fedele,

[Il nome del mittente]
Je suis parti me rendre à Paris, eu besoin de faire des projets de retour. J’ai reçu une bonne lettre de M. Sullivan, à propos de quelque mousse, à comparer. Pleins sommes, lettre aussi, des excellents amis, A. et A., de sorte que je suis tout heureux! Est-ce Vrai que je suis ton numéro dixmière, me tendre la main au départ.

Le serait un grand honneur pour moi de faire la connaissance de M. Hooper, à Paris. Peut-être otterais-je quel que chose, pour M. Sullivan, si vous daigniez me transmettre une lettre pour lui, avec sincèrement remerciant. Mon aube à nous, demander sur l’ennui, de ma part, afin que M. Hooper ne se trouve pas tel qu’il peut perdre son temps inutilement pour moi. Vous pourrez remettre cette lettre au Prof. Boege qui part de Boston lundi. Je vous adresse votre lette A. Desguerre. Tuerin, Canton de Neuchâtel, Suisse.

Avec la bonne, prière, de faire mes hommages respectueux à Madame et aux sœurs de mon ami et sincèrement.

Les Desguerre

Tous mes salutations de vive à l’occasion à Monsieur Hooper qui me revient un bon, lettre à son ami. J’ai tout ami

à la destination.
Cet hiver à l'Europe, j'ai eu l'honneur de
vous écrire en vous envoyant le portrait de notre
ami Godet, lequel vous auriez bien ressenti.-
M. Sullivan m'ayant dit que vous étiez parti
pour l'Europe que vous ne reviendrez pas avant
la fin de l'année, qu'il aurait d'ailleurs hâte de
quelque chose à vous envoyer, j'ai cru qu'il était
impossible de vous envoyer le paquet de M. Godet.
Vrai à faire un voyage dans le sud, et sur
venue d'Amérique guérissant lui. Votre paquet,
le paquet a été retardé. J'aurais envie de vous
pas accepter mes excuses pour ce retard. Vous
trouverez dans le paquet quelque plantes intéressantes
de Louisville et de l'Indiana. Aussi quelques
brochures de l'Europe.

Je vous assure de tous nos amis à la France.
Voir aussi
M. de Randelle sur la géographie de plantes, mais
nous parlerons de l'été de l'été. Il s'agit
de quelques espèces de moutons qui font bien
menace pour terminer nos Misty contéat. Avec
nous un peu de travail (le magnifique travail)
de M. Sullivan pour notre nouvelle édition
prendre tout notre temps est libre. M. Sullivan-
Me change de vos dis que j’ai dire demain
par après demain.
Permettez-moi de présenter mes respects à
Madame Gray. Laissez-moi offrir l’expression
d’un dévouement affectueux d’amitié.
Lea de Seguer.
Cher Monsieur,

Je lis dans une de vos lettres, à M. Sullivan, "I think it tells him that I had put the Securica?"

Veuillez voir le manuel. Comme c'est la première nouvelle, je m'emporte de vous en renseigner tout

Mon nom, quand il a été employé, naturellement est latinisé en Securica. Ainsi

s'ajoute à un genre Securica plusieurs espèces, travail

à M. Sullivan et un Bryum Securianum.

Ayez la bonté de modifier le nom pour qu'il s'accro

che d'avec les autres auteurs. A ce sujet, aviez-vous

le paquet soi, soit parvenu. Les résultats souvenez-vous

que le fils est un longtemps retarde. Pour ayez jamais

envoyé quelque note. Sur la localité il vaut la peine.

Probablement serait trop tard maintenant.

Le mousse à M. Sullivan pour le manuel mont

marquesques. Je suis pour mon compte bien heureux que

votre seconde avec le public, autant sur oblitére &

bouche que pour enfermer une fois avec moi. Vous

enverrai qui seront édités dans le texte.

Nou travaillerais avec vous à la rédaction de toute

la espèce, douteuse. Je suis de renom, encore l'espèce

de M. Lamme, que vous aviez enregistrée. Il y a de

mouvement de bien qui Albertian Ren petit Ombras.

Mais tout ceci a été déjà examiner en 1850. Mitchell

tournant le paquet et est au mien de M. Sullivan

qui n'a pas le temps de décupler de tout en détail.
Espère que ce bout de lettre vous trouvera bien.
Madame, en bonne santé, que vous pourrez joindrement la nouvelle année. Aique Dieu vous accorde
et la continuation de même.

Vos deux bien, donnez ami

[Signature]

De mon ami, monsieur, allemand a
fait un genre d'animus, mieux insigne que j'ai
avoir nom, avec le nom de l'Antiquaire. Mais
cela ne donne pas bien. Quiconque crois le nom
latin, autant vaut le prendre bien lu.

Cher Monsieur,

Le vice de me préoccupe pour notre ami Monsieur, et je dois donner un exemplaire de notre édition de la *Botany of the Northern United States*; il est bien que vous ayez trouvé les termes de la paroi; je me prouverai dans cette entreprise et pour moi-même et pour la science de ce beau livre, que je ne suis bien heureux de devoir à votre généreuse amitié. 3' heure un grand nombre de changements qui me paraissent importants. N'insistons pas sur les études; ce livre encouragera la faculté de la Botanie, en vertu, autant du moins que je puis juger, je ne connais aucun travail émanant qui ait autant de profondeur et autant de clarté. Je meurs bien heureux, je vous assure, cher Monsieur, si je pourrais mettre votre bienveillance par quelques communications intéressantes. Jusqu'à présent, pour que j'apprécie par ma puissance ou si vous voulez par les difficultés de ma position, je me suis profiter de mes excursions botaniques de manière à faire des bonnes collections de plantes plus ou moins. Mais si Dieu me prête vie et ma bonne santé, je tâcherai à trouver une plus grande pièce pour des résultats plus importants. À la revoir cette année en Minnesota. À Nashville quelques plantes que je vais bien aimer de vous soumettre. Les prendrai.
la liberté de tous les peuples par opposition au lointain
boulevard de Fargi, de Monsieur Basset, qui nous men-
trouvait grand honneur de vous assister. D'aly, genève,
on les couleurs de Monsieur l'Empereur, qui M. Monsieur
n'ayant encore à Genève. Il Duly pourra payer la
part de tout le pays et le charger volontiers de distribuer
ses exemplaires adressés en Suisse.

Vous avez senti l'irruption de notre amitié
pour ses éminentes vertus, qui ont été
renforcées par mon effort en votre
faveur, et même, bien plus, par son
vice de parti dans cette maïtrise un
mème en tant que vous en danger. Il est allé payer l'empereur
le profit. Cette qu'apartient aux partis Républicains
qui a fait cette bonté. J'étais mort de
miène. Cette, qui en terre de nature, à lui faire
ainsi l'avantage, Néthis et je suis certain que je le
fournirai sans trop grandes explications, il rentrant volontiers
de Brignoles avec la famille. Mais l'Amérique offre
elle maintenant plus de chansons de bonté que
l'Europe ? Il est à bien plus en voir au même en son
est, de France le calme que demandent les peintures
de la France.

Ne parlez vous pas que la magnifique venue de
Monsieur allant être admis partout. Il le voit le
entre faire car depuis longtemps il a travaillé sans reposer.
Il ne rend plus aucun sacrifice pour l'avancement de notre
ches Broussard. Les objets de Monsieur l'Empereur,
vou devoir enlever cette dernière ? Il prendra la liberté
de vous offrir la première. Le plus beau des exemplaires.
Vou ne vous, ni coup, y a pour quelle que chose.
D'où l'avance, que Monsieur, de partout, avec
respectueux compliments à Madame Gray. J'aurai
voulez même l'assurer de mon amitié
prononcement.

Inv. 0.5
Columbus, O. 10th 21st 1836

Dear Sir,

I thought to find time and look again over these plants. Please exam if I give you the trouble of determining many of them. I have plenty of more common species of the prairie. Do you want a list of them? I had a bad time in Wisconsin, being on foot and with unheeded companion, who did not want to wait for my purpose. Moreover my largest bed bundle of plant was carried away by a boat while I was going further west and I could not reach it anywhere. If there is some species of which you want herbarium, I can send you at my leisure. I have corresponding numbers with those of the labels.

Mr. Dillibant sends to say the labels of Musci are sent and also some labels for sending to Rome for musci. Will you please forward to Pasteur only naturals to Geneva, these three copies of the pium of B. Benne with the copy of the Musci of Hepaticae that Mr. Dillibant directs to them.

I have worked hard this morning for the labels and am now in a hurry to be at time for the express. I wish you a few days ago.

Very respectfully yours,

A. Lebreux.


My young man forgot to put in the package forwarded to you ten days past express hand.
Columbus 0. 14 Juin 1857.

Cher Monsieur,

Le retard apporté à l'impression des estampes n'a pas permis de préparer au fur et à mesure quelles auraient 25 exemplaires de nos Muséum mérités. De ce fait, grand moment où je recevrais les ex.

emplaires des tables, je ne suis pas à vous l'espère, des exemplaires qui me sont demandés.

Je rendrais, Madame, quelques copies, des exemplaires, vous désirez... D'après votre lettre, vous en attendez trois, un pour M. Bourrin, un pour M. Thibaudet et un pour M. Le Verrier. Tant il vous en envoyez ou réponses. Veuillez le dire.

Je vous signale que je suis tout à vous pour le plaisir de faire une copie au plus grand nombre d'entre vous. Je vous envoie quelques exemplaires que je suis plus propre de vous envoi.

Je travaille maintenant toujours trop, je dois être un peu plus fort, je suis bien désolé de ne pas vous être plus utile. De ma part, je vous prie de bien vouloir vous adresser à M. Thibaudeau qui me fournira considérablement de chantelles.
Je n'ai pas de vous de nouvelles de Godet depuis plusieurs mois. Pardonnez ma petite absence.

Après vous avoir écrit une lettre il y a quelques semaines, je n'ai pas eu de nouvelles de Godet depuis. Il y a eu une interruption de la corde de tisser. Je suis donc appelé à faire des mises et des compensations.
le prix à $25. — Vous nous rendez grand service à Monsieur Sullivan qui, à son tour, s'occupera de l'affaire. — D'embâcle, chaque collection avec ses numéros pour que vous puissiez sans faute les retrouver à l'export. Nous pourrions le voir par la note. Mais je n'ai pas trouvé de caisse pour le tout.

Le prix pour l'exportation est de $105 qui représentent bien $20 avec la différence du change. — M. Sullivan n'a de souci qu'envers son propre nom qu'il a signé, et les collections sont adresses. Il y aura d'autres demandes, vous m'indiquerez de me les adresser le plus tôt possible, puisque je préparerais de nouvelles collections s'il en manque.

Je vous envoie aujourd'hui la lettre affranchie à Madame Gray mes respectueux hommages. Je vous assure de mon affectueux dévouement.

Les Anquereux

Cher Monsieur,

Je vous envoie aujourd'hui à l'Américan Express un gros ballot à votre adresse, contenant huit exemplaires de Mission, rassemblés. Ce sont ceux de Monsieur Torrell dont je vous ferai part le n°14, tous sont marqués des noms des propriétaires. M.

D 'A. Rue de Montréal a payé le prix de l'exemplaire qui lui est destiné. Monsieur Arboisie Le Musée Delteil pourrait payer en l'envoyant en adressant un mandat de $105 à Messieurs Landier. Berthoud lui envoyer à Newtown si le Musée Delteil préfère payer à Paris, il peut remettre cette valeur à Monsieur Georges Berthoud artisan Rue de Berlin St 29. Quant aux propriétaires Américains, Mесьei

Monseigneur, Paton & Obrey, le meilleur mode de paiement pour qu'il est le
Plutôt sur est envoyé à mon adresse à Columbus un chèque de $20 sur New York.

Je suis extrêmement gâché de vous donner de nouveaux embarras pour ces collections quand vous vous êtes tant à faire. Mais c'est vous même qui me dites de vous envoyer le tout. Quant à votre exer-

cice, si je suis trop heureux de
d'obtenir vos offres ces trop faibles preu-

ce de ma reconnaissance et de mon ap-

défension pour retarder à bonheur à l'ami

prochain qui peut-être je ne serai plus de ce monde. Il me reste sept exemplai-

res à vendre si j'en aurai assez pour

d'années car je n'ai pas autant le confian-

cce que vous à la vente facile.

Cette collection est cependant la

meilleure que j'ai été publiée jusqu'à

présent pour les mousses. Si vous

roulez bien es faire rapport au jour-

nel de M. Silberman, agez la bonne

boute de faire remarque que plus de la

moitié des espèces ont en capsule des

échantillons supplémentaires destinés à

l'étude comparative ainsi. Une grande-

alité; que ces échantillons représentent

sont des variétés remarquables de l'espè

ce qui sont l'ordinaire plus parfait

que les échantillons collectés. Il y a cer-

tainement quelques échantillons maig

ce de Californie surtout rassemblés par

Mr. Douglas. Mais ils sont si intègres

sont au point de vue géographique

que tous les Muscologistes nous savons

qu'il ne pas les avoir laissés de côté.

D'ailleurs ce sont des addictions gratuite.

Toujours le Collection telle quelle était

prêtrès il y a un an devant se compo-

ser de 350 espèces et variétés de tout-

$30 Lique maintenant vous l'avez

élève à 316 espèces et bonnes, variétés

d'hui de prix à $20. Cependant

après le 25 premiers échantillons,

exemplaires vendus, je pense élever-
Columbus 0. 21. Juin 1857.

Cher Monsieur,


Votre tout droit, Léo Desguerre.
Dear Sir, I open my letter again. I sent you a copy of our monographs to England and most certainly send more hereafter, I would ask you to order for me some books of which I am in great want.


For those two works, I would like to have this year, but I do not want them before the fall. If I may have them sooner, it is well. I will pay you immediately after this letter by separate of our monographs, as you like.

Then I would like to know what cost

3. Mammal & collection of geological facts and practical observations (1834)

Mons. A catalogue of British fossils.

and if Dr. Blunberg has published something about fossil plants and if I could procure it somewhere.

Please excuse this trouble. I have nobody but you to whom I can address myself for this.

Prof. Rogers will do nothing for me, since I decline to further work for his paleontology.

Ever yours,

T. Archer
Columbia, Feb 26th 18--

Prof: Asa Gray, Cambridge

Dear Sir: Following your letter to Mr. Todd and I have sent you today through America Japan--

Copy No. 21 for M. d'Andolle.
I am working hard to have 25 more copies ready. As I have put my wife and daughter to the tennis, I hope to be ready in two weeks.
I have still two copies left.
I sent you N. 7 for yourself.

N. 10 for D. & Allan, Joe, Jr. Montreal
11. Daniel P. Faxon, New Haven
12. P. F. Otney, Providence
13. H. W. Marshall, Athens
15. St. Bavo, Geneva
16. B. Delorme, Paris
17. Joseph Blaikie, Cumberland, Leeds
19. N. B. Ward, England

The copy for M. Marshall is a present for his and D. & Allan had paid for his copy.
I am anxious to know what you think about the collection.

Most truly (Respectfully)
your friend

P. [Signature]
of Brunswick have acknowledged the receipt of our music with many fine compliments and compliments of the world. Judge S. Lane has N 21. the University of Toronto N 23 and Mr. Hooker N 24.

Most respectfully your friend,

T. Angersey

[Page]

Ottawa, 10th March 1857

Monsieur,

Sous le fort occupé toute celle semaine à la préparation de 15 nouveaux exemplaires de nos musici que je n'ai pu répondre à votre amable et bienveillante lettre du 24 Feby. J'aurais voulu vous remercier encore de tout mon cœur de la bonté que vous nous Donnez pour faire votre avis pour placer des exemplaires de nos musici. C'est un véritable service dans que vous m'avez rendu. A la fin de la semaine prochaine je 14 et j'aurais encore 15 exemplaires de prêt et comme je pense alors quitter l'Hôtel pour plusieurs mois vous m'obligeriez de me dire si j'ai fait encore un envoi avant mon départ. En tout cas, les exemplaires seront tous en ballons et pourront être expédiés sur l'ordre de M. Sullivan. Vous aurez reçu les exemplaires à Messrs. Blake, Ward et le Cardwell. Mrs. D. C. Eaton & Mr. Ewell on doit envoi le paiement des leurs exemplaires. M. Sullivan a payé celui de M. Engelman qui est ici à la disposition du propriétaire. M. Sullivan a envoi un exemplaire à Mr. Hooker.

Tant il en adressa un à Prof. Park. Je ne sais pas où il l'habite.
I am well. I would like to hear from you again about the book. I have received your letter and am glad to know that you are well. I hope to receive your reply soon.

Your friend,

[Signature]

P.S. I enclose a check for $20 for the book. Thank you for your kindness.

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I have just received a letter from Mr. Huntley, and I am pleased to hear that he is well. He has ordered a copy of the book from me. I shall send it to him immediately.

Yours truly,

[Signature]
Columbus, 6th March 1857.

Dear Sir,

You have my best thanks for the twenty dollars for a copy of our Music, one for Mr. C.H. Jamieson. I shall hold the number of the Collection and send another copy for Mr. Black. Perhaps you may share some others for real books and I can send all together. I will send a copy to Mr. Hall of Grandcamp next Monday.

Before yesterday (I think?) I had the pleasure to write you a long letter. Now I am hurried by the preparation of the music and beg you will excuse this few words.

With many and many thanks
your friend

[Signature]
Columbus, 0. March 17th 1857.

Cher Monsieur,

Avant hier, j'ai reçu par Monsieur Sullivant 64 r. Dollars pour l'exemplaire de St. Blaise. Huit jours après, aujourd'hui j'avais reçu 20 Dollars pour celui de Monsieur Uragan. Tous deux immédiatement m'ayant reçu. Aujourd'hui, je prends la liberté de vous envoyer par expédition N° 27 et N° 28, deux des exemplaires pour M. de Randolf. L'un pour la première personne qui vous en demandera un. Vous aurez bien dans votre laboratoire une place où le disposer en attendant. Je vous envoie deux pour éviter les frais de port. L'autre parce que je désirais de quitter Columbus pour quelques semaines. Peut-être pour plusieurs mois. D'ailleurs, si vous m'écrivez de ..., vous me demandez des exemplaires ...
des mais, les lettres étant
épuisées immédiatement par quelques
de mes fils et comme tous les exem-
plaires sont emballés, il sera faute
de les expédier de suite.

Je viens d'envoyer un à Monsieur
George Hunt Providence. J'ai
écris à M. le Prof. Chadbourne
pour savoir quand je dois lui envoyer
le sien. Les 25 nouveaux exemplaires
sont aussi beaux que les premiers.

En vous remerciant encore mille
fois de la bonté blanche avec laquelle
vous soignez mes intérêts, je vous
prie d'offrir mes respectueux hommages
à Madame Gray et à Aguirre,
assurant de mon affectueux souvenir.

Leo Lesquene
Columbus, O. 28th May 1857.

Prof. Are Gray, Cambridge.

Dear Sir: I returned to Columbus only yesterday and found at M. Bellardant a copy of your first lessons in Botany, that you had the kindness to send for me. I am truly very thankful for this new friendly present of yours and very glad that you have published this most useful book. Though I have a heap of business to attend to, I have nevertheless read a good part of the book. It is perfectly clear, very interesting and contains all that is truly useful for the study of botany. A most excellent book not only for the student but for the botanist who will want it every day for direction and reference.

I write this day to Mr. C. E. Frost of Brattleboro to offer him as a present a set of our Maps except.

He has promised plenty of good specimens and is truly entitled to one. Since he is nearer to you than to Columbus, I wish him to ask from you one set if he want one. Will you please have the kindness to send it to him when he has given you the necessary directions — M. de Candolle wanted to have one copy. But you sent one already. I hope and duty nor any one of our friends has secured a copy of the 'Moses of Bellardant.' Did you not forward many copies to Europe following a list of M. J.? I have read at Dr. Owen's 'New Harmony' your memoranda in Bellardant Journal.
and have been very pleased and thankful for it. Our morn are selling pretty fast and I have only a tip or two copies left. I shall certainly not prepare any more this year and for that reason would be glad to keep for the future a few copies. In a few months I shall go to Penybras and perhaps to the white Mts. If you are not gone from Cambridge I might perhaps have the pleasure to pay you a visit in person.

Most truly yours, friend

A. Lesquerer
Dear Sir,

Looking over a package of mosses collected in Kentucky, I find a very fine Desblandieria, which I do not think was described. When I collected it, I did not make much of it, I thought that it was described somewhere coming from Kentucky, another Desblandieria, as the Mears. This is somewhat hairier, with crenulate leaves and grows on sandstone rocks in shady places below on the fissues of the rocks. I saw it only once on the way from the landing of Breckenridge Low Camp. (One went to the mines about half way up four miles from the river. Perhaps it is something new, It is the only species of Desblandieria plant that we worth collecting. But it was too early in the season when I left. Then I have a long letter from Heidel's he is very sad about the final result of the researches question for me — true Bayard I am sorry for him but on the contrary very glad that
my country may now look for better days and get a quiet time. God says he wants to migrate with his family to the southern part of America and probably he will do it. But I truly hope that he may change his mind.

Many thanks for your welcome letter. Don't hurry a hurry to tell the news. I have only 8 copies left and they are wanted in Europe. If I can go to the States, I intend to prepare kept winter the 50 copies and of the collected materials left only to be ready and give my time to something else.

Smithsonian Institution has not sent the copies of T. Sullivans world to Europe now. I have a letter from Schimper and to day another from Stearns who are still waiting for it. It will have been sent in the package of the second of May.

Mr. Frost has received the Men and is pleased with them. He is an excellent collector and his country is rich in sub-alpine species.

Though always most happy to receive your letters, I do not want you to change your time for any more gratification. - Bishop stopped with me on one day this week, coming from Illinois. I was very happy to see him.

Please give my respects to Mrs. Pop. Most truly yours,

Rob. Sigourney

Something that I could perhaps go to the white Mr. about the 1st of July with A. M. of Chicago John. I kept this small package now I see that I can do no chance to send home and money for the journey. And this fall season I had this much and little with a little in.

A. Frost who will call at you next week. A. Smith will read much better it would go to the white Mr. But out of many circumstances I think that the Boston is the best way for the white Mr. at this beginning of July. Perhaps I might go with A. Smith of Lancaster in the beginning of August. - Is that Rochester near now? The comparison with maximum growing in my garden I find it now being, the very loss of trouble and more evidently phylloclade, cremes and quite a different habit of appearance. But of course, I do not know of the variability of this species, and you may think the only a small fragment of the normal form.
Columbus 0, 21st Sept. 1857.

Prof. Asa Gray. Cambridge.

Dear Sir: I am truly sorry to trouble you again about the package for Doby. This package contained 3 specimens of the fancy of Durbuy and I would much like to know if they are lost for I would then procure them again. You wrote me that they had been sent through the Smithsonian Institution and that you would make inquiries about them. A few words of you will settle the matter.

I have spent the whole summer in the coal basin, following the coal fields from Mississippi to their Eastern limits. This prevented my journey to the White Mts. and my visit to you. I am truly sorry that I could not go. - Is this Odocoileus from Kentucky a species or only a variety? Most truly and respectfully yours.

Leô Susquehanna

Cecil can not forget the defeat of the Royalist party and says that he intends to emigrate with his family to Southern Russia. Did you hear from him? Has N. Austin noticed the Musc. varicol.
in his Journal.
Columbus 0. 14. Février 1880

Tha Monsieur Gray.

"J'ai reçu en ton nom le dommage que vous m'avez fait remettre par Monsieur Sullivant et aussi, dernièrement 50. pour une pétition rendue à Bosto, j'oppose comme j'ai donné reçu de ce montant à M. Sullivant, je ne vous aura rien, mais je vous ai toutes monnaies de tout mon cœur de votre extrême bonté. Je vous envoie les trois dollars que vous portez à la rentrée de votre voyage. Je n'ai d'autre moyen de reconnaître un bon service que par une affection amie et fervente.

Le Doby m'envoie des genres qu'il voudrait obtenir les livres publics de mouvement de l'électricité de M. Tucherman. Il est en 1808 b. 2 e partie en un cachet, mais il va sûrement.

Aussi vous, le bout de mon oreille qui reste maintenant à Tucherman, ou quelle est son adresse, je crois à publier depuis la première partie, et vous envoi immédiatement le volume des livres, sommées pour le obtenir, combien vous reste 2 e après..."
On peut déconseiller de ne pas:
S'éloigner collecte et bien. Récolte des collecte encore et de penser tout à fait a travailler pour moins. Ici o c'est nécessaire aussi pour un buen passe

Solemne

Sui c'est vrai pas à chaque fois, ne croyez que a voir manquer reconnaissante pour vous. Bon sourire.

J'ai rendu cette demeure tout mon bien, de plantes pharmaceutiques, 6 e 7 milliers espèces, pour $201. Il est entouré d'une belles toiles en les mains de manque tout à l'air. C'est un bonheur autant pour moi maintenant surtout à lui. Il est pauvre et reste fait pour faire autre chose que le mouvoir, il peut être le plant bonheur. Cependant je relatera toujours la plante intéressante quand elle ne trouveront.

Suis que notre Tante de Madame
Guy sont parfait comme herbe bonne.

Leur b, chez. Il j'aurai de tout
mo c'est quil se fait aimée. Finir
examine et ton, prix et important tout
et belle l'après la semaine de mon affecté
devenuient. Les Sevrures.

Je m'entends bien avec de Gedel mi.
J'ai eu une bonne longue visite de l'amie
Guyot et lavrinc Paris.
By looking over my notes, I see that you have paid it yourself.

Your truly,

March 21, 1852

[Signature]
the liberty to write you about the scheme of Mr. Tuckerman. Mr. Dudley of Geneva has bought long time ago the two first fernicles and he says he has heard that there had been some others published and he would want to have them. If you can write to Mr. Sullivan place to give him the address of Mr. Tuckerman to whom I may write about the matter. And there is nothing pressing about it. On Monday next I intend to leave Columbia and will not be back before a few months. I have been past year engaged in the state geological survey of Kentucky for palaeontological research, and I am called again this year to continue the work. If I find some interesting plants in my ramble, I will collect them for you. I wrote you also in my former letter that I had sold my whole collection of phanerogam plants about 8000 species for $200. As I have no time to look at those plants, I was better to part with them than to let the insects feed on them. Don't you think so.

Please give my kindest regards to Mr. Gray and believe that I am most truly respectfully yours,

Geo. Enguezer
Columbus 6 Nov 22 1858

Dr. Aga Gray Cambridge

Dear Dr. Engelmann's copy of Murchison's treatise has been waiting for him since about two years. You wrote three that he would call for it himself after his return from Europe. As I have been absent nearly 3 months and returned home only yesterday, I could not of course hear from him before. I have sent the copy to day and you had it when it was ordered.

It is all right.

On Monday next we will take to the Japan mosaics and have them followed with night mosaics. As we intend to work here and work together with Mr. Sullivan and his draughtsmen (dissinindem), we will be out at times.
I am now nearly fully occupied by geological research and my own business. I could collect nothing of interest this year in Geomajas, island.

Most respectfully yours,

Geó Dermene
Columbus 0. 29th Dec. 1850

Prof. Aga Gray

Dear Sir,—Is there in the South of Florida or of Texas or of Mexico perhaps some species of Quercus, that could be referred to both these fossil species stretched upon the paper. Fig. 2 & 3 is very narrow leaved. I have seen specimens still narrower than the figures. Fig. 1 has very strong nerves and is alike to Quercus ornithoptera or Quercus gigas of Goupil (Tertiary flora).

You will perhaps say that the identification of fossil leaves of phanerogamous plants is impossible and therefore that it is useless to try to make a comparison. In the strata of somewhat different age, especially in the placentic mosses and bog's strata above the 9th stage, the determination of the leaves, even approximately, is truly important. For example the formation of which the accompanying leaves are taken is supposed contemporaneous with
the chalk banks of the borders of the Mississippi bottom which contain only plants of one time and still living in the same latitude. (Except grasses, vines, which abounds in the Cipitti formation below the mouth of the Ohio.)

I would refer the formation with the sketched bone to a older period.

I would deeply regret to have sold my herbarium if it had contained specimen of trees and shrubs of the south. I must now begin again and try to have such a collection as many of specimen of leaves and fruits of trees and shrubs of South America as possible. I have still two copies of my Musc. Amer. and would gladly exchange one for a number of specimen of shrubs and trees of the south. If you have some friends at New Orleans or in Texas, I would propose the exchange. I will also be much obliged for informations about this matter.

We are working hard with Mr. W.S. Sulibrant on the More of U. S. P. of Japan &c. collected by Wright. They are very interesting but there is not many new species, I think.

Mr. Engelman's copy has been sent.
one of them Helianthus occulus 1ay in extant species. I have not seen any plant in this formation and take the present opportunity of the day where they are found to examine them. They are all species of trees that I have not time to examine it for a few minutes an only collect few

1. Gueena moccas or abundant
2. Gleschulcus (some leaves) no fruit of it
3. A piece of Acerum Galambus.
4. A Gleschulcus which I refer with some doubt to another American
5. Some referred to Planearum Joh which also with some doubt and 6th another leaflet which I think is Gleyschulcus Pennsaculc.

Further the two nuts of Tocca Sagala also referred with a leaflet of another form. The nuts are certain, the leaflet is not

The two species of Gueena which I sent you come from 2 different and unknown formation which I refer to the

Tocca Sagala, and cannot believe these are any new species. The few, which I contain, are mostly new.

Columbus 11 January 1859.

Prof. C. Gray, Cambridge.

Dear Sir.

I am much obliged for your communication of oak bones and shall be truly grateful for every species of tree that you can occasionally spare for me. I will gladly tell you all I know about the new formation along the Ohio and Mississippi river but it is not much. I had not time to examine them closely. The first deposit of leaves which I examine is below the mouth of the Cumberland river along the Ohio, Kentucky side. The leaves are mostly mixed with sand and all belong to one time. At least I think so. I sent part of them three years ago to you and you found in the leaves especially Glaatamus acer Joh. Will, which he acknowledges as a new, of Glaatamus montes, and contrary to Willian's opinion accept as a new American species. Somewhat later, about three years ago, discovered a new and extensive bed of leaves below the mouth of the great Kenose River. This formation is peculiar. At some places the bones are heaped, fused and exact
like a wall or a dam cut allen the
run by steps or slats. At these place
the leaves are nearly level and can be easily
separated from the sand and mud. At
other place, the sand and mud that
corn there has been hardened by current
of rain and the leaves are already broken
but their roots is well preserved in the
slate. All this formation is recent. I
would place it in the recent post-glacial.
All the leaves which I could determine
belong to our species. Should you like to
have these leaves with some transcript of
the formation, I can send them to you.
Perhaps you could find something more
than I can see. The formation been
must more interesting near the mouth of
the Ohio and below it on the Missouri.
I must give you an approximate
section of the shell beds of Chunksby
that you may well understand the
border of the bares I have found below.

Beginning at the top and below the soil
a fine silicious loam containing few
mussel shells, out of the blue, but of the
marshes, with plenty of fragment of
crab shells. 30 feet.

2. A conglomerate of hornstone gravel
concentrated by sandy lake water, 30 feet

3. Gray colored clay, 15 feet.

4. White calcareous magnesian earth, and
coordinated clay, sometimes, especially near the
base, as held as sometimes, 25 feet.

It is especially at the base of the clay that
leaves, broken pieces of decayed wood and
bark, sometimes, by roots are found. There is still
below the 4 bed, which lay clay to the
low water-level of the Missouri, but
this last I have not seen it and do not
know what it contains. From the mea-
suring of Dr. D. D. Owen, this whole forma-
tion measures 160 feet. The composi-
tion and position of these beds raise much.

At some points of the Ohio, the Hornstone
gravel (says D. D. Owen) or conglomerat-
e is covered by a ferruginous sand charged
with great abundance of shells of broken
fragments of bony tissue and of bone, in
the ferruginous sand the bones of the
Megatherium Jeffersoni were found. From
my specimens, I examined the following
species of shells: Melianus canal culato,
M. undulato. M. aculeo. Gelas
timba, G. Fabiani dimor, — in the
Wabash, the upper part of these formation
is rock and there is only a yellow
calcious earth containing shells. (formerly

geological age they belong. But do not say, their analogy is with the plants of the upper Mississippi. From the drift I have seen nearly nothing. Down and Whittling sent me many years ago a melange of sand and broken carboniferous fragment of plants taken from a tank of the drift near Cleveland. The plant had been evidently transported and more nearly to provide I could identify a single specie, and with doubt only its needs, of Abies negra. I have now under examination a collection of at least, of the Museum of Vancouver Island. The piece are most interesting by their great analogy with those of the Museum of Lüneburg especially of Olbia. The examination is made for Dr. John Brown, of Wharnton, who intends to publish the leaves in his report. I am just having them, and can write you more about them if it is interesting to you.

In April next, intends to stay a few weeks along the banks of the Mississippi to collect specimens and study the flora which to my mind is just as the drift. I will also go to the lower states and to the Museum of Tennessee. We truly a most interesting study and by it may afford some data for the age and the former distribution of some of our trees. You could help me much by sending for me the leaves of as many of our hardy species of tree as possible, especially from the South. I know well that you can't come to select specimens and give them to me. But I would gladly give one copy of my Museum and to a botanist of the South for leaves of the Southern Species of tree and bushes. If you could only how send me one or two leaves of the following species:

1. Ceanothus of the South. I have only Ceanothus Americana.
2. Flavia Ulmifolia.
4. Laurens Carolinensis.
5. Claudia termerica Raf.
6. Andromeda Sagittaria.
7. Nyssa Aquatica. you would truly oblige me much. But if you have no time, do not care about. We are going finely with the Japan mosses. They are highly interesting. Most truly yours,
Leo Lesquerel
In the plant of Safford (pleione),
I have still identified Guercus myrtifolia,
always species. The identification is
certain. I am still in doubt about the
true place of the formation, but it is
certainly lower than the drift.

or cannot belong to our time. They have
formally speaking great analogy with species
of the European Dietes or Meniscus. Of the
species of our time there is one best which
can be referred to Laurea Cardiuncuus
and another to Alchornea victoriae Reg.
A third is (still with doubt) Andromeda
lentigine, that is all, could give you
conclusive answer about these three species
if you could send me for comparison a
few leaves of each. These only certain given
as far as it is possible to identify a fruit,
the fruit of Fagus serrata Webb.
I cannot see any difference at all neither
in the form nor in the size of the fruit.
It is not emarginated at the top but pinnated
with the three side, bordered by a
marrow very conspicuous and the base
somewhat ribbed. But of these species,
there is part of a leaf referable to Safford
20 minor (these). 2 fine narrow
stipal with entire margins, two Guercus
one of which is Guercus crassimuris f.f.,
a variety... (fancy equation ?? and a
Chorisia decadum whose analogy is still
unknown to me.) The leaves were
found in Tennessee by Prof. Safford of
Safford at whom I saw and studied the
specimen. He does not know to what.
Columbus, 1631

To Mr. Gray, Commissioner

I was very pleased by your kind letter, that I did not find anything new or even culum

bar a dair. I am truly grateful for your

promise of sending me some of your col of

leaves of trees and shrubs of Cuba. Would I could get in exchange

for a copy of the Maev, cotsath a collath of

leaves of trees and shrubs of the south.

No one of the Rogers has answered my letter.

As I know them, I supposed we expected nothing else. H. & Rogers at last have answered a

letter except when he could get something for

himself or when he wanted to get it. This makes

me grow out of my mind entirely.

I have a good long letter from Gibbs. He does

not know how the American life is working and

how strong the lesser men that we can spare

for friendly intercourse. The difference in political

novel he never wrote between Gibbs and myself

only he is still an enthusiast or something of
a fan be and comme fuse. The water
cooled my political enthusiasm to a temperature
below ice. It is the only difference.

Might be mine still sleeping. It tells
me I'm in town now to examine them and
I can not myself begin the examination before
I have finished my reports on Kentucky and
Ohio... I would I could do it just now,
but it is impossible. If we can begin at all
the young we will go through. These men
are very interesting but very difficult to study.
I still want to receive the forgeries,
which are probably, he says, the parcel of
Mr. Johnson for him. I still want
a copy of the Encyclopaedia of Torstensson
as you propose to get for him. He says
he sends his kind regards to you.

When I write you about the purchase
of some books or school books, we mention
Mr. B. (Mr. Thomas of school word) go
talk that I could perhaps get this book
from Mr. Turner or from the Library of
Cambridge, at least for a short time. If
you ever have an opportunity, to get it either
at a loan or from purchase, you will please
inform me to send it to Cambridge.

Most truly and respectfully yours,

Vic. Torstensson
Columbus 0 Feb 10th 1859

[Signature: John White]

Dear Sir: I am much obliged to you for the specimens. As often as I want something I will take the liberty to ask you for it.

I do not remember well what I wrote you in my last letter. I will then recapitulate the above named species.

I have the post privilege of the Monarchy, which I now refer with some degree of certainty to a formation anterior to the Jufu. I have identified: Quercus Virginiana - a species very nearly allied to P. Americana, but different - Glutaena brevancanthos - Acorns calamus - and some I wrote you, from comparison with good specimens: Fraxinus cuscuta or integerrima - Castanea humberi - a Fraxinus glandii (from your specimen.

From the lower plains of Tennessee, I had for certain, S. of Fagus, foresti-9. Quercus, myrtifolia and for doubtful species: Alder, Nectandra americana and Gymnos clada, Canadensis. Both these species are more doubtfull than ever now. They don't agree with your specimen. But the Alder, Caroliniana is certain for another doubtfull species, and you wish you. I have still identified in the formation Fraxinus Caroliniana. There are many other species which I

Therefore, on all your convenience, my mention on the plant. But if you do so please mention that the collection made by him. I do not think to what degree I had better send the paper. I have a fellow of the Society of Natural History of Boston; but I think they publish only extracts. Perhaps Philadelphia wants it better. I would like also a specimen of Mayus temperans, if you have one.
In mentioning species, you would find nothing of our time, I think. If you have many Sali
tiure's plants to send me sometime a few leaves of different species. Now
the report is ready and I can wait as
to a good week, or as long a good

We are progressing finely with the

I am very glad if you can use my
determination. I think you may rely to
then what are indicated as no doubtful
and do not care to ask for any think
you think worth having from me.
I have always time to write to you.
As fast as I can find out some other
I will send the name.
11th. I did not close my letter yesterday supposing
that perhaps Dr. Gulbrann would have
something to say. He will write himself
shortly, I hope from Dr. John Brown, about the fossil plants
of Waimau. He wants me to publish
something in some Academical paper
before. The report is ready. You may
Columbus 0. March 8th 59

Prof. Asa Gray Cambridge

Sir: I am much obliged for the leaves. The specimen of the
Sabinia of the Museum of Oregon and Vancouver
is true Sabinia by no notation
at least. Though the outline is
nearest. Phytoecus asplenifolius.
Sequoia sempervirens is right the
same as the female plant.
For the fossil plants of the
Sabinia and Post Sabinia of the Mis-
inertia, I did not make any drawing
intending to visit again this spring
the formations and places where they
are found and to try to collect a
large number of them for a better
examination. It will be time
then to draw and publish them.
Dr. I. Dunn wanted me to make
a mention of his plants with short
descriptions in the Illissini journal.
I did so and sent at the same time
a brief comparison of all the plant
of our recent formations, at least
of the plants that I have seen.
If Filliman's journal does not want that paper, I shall take the liberty to send it to you for examination and you may then decide if it is worth publishing or not.

I have a pretty large box to send to Prof. Heer of Zurich. It contains a set of our Musci Amer.: and is just of the same size only a little heavier as I have some specimens to pack with. Can you tell me what would be the cheapest way to send it.

Most respectfully yours,

F. E. Tesquerey
Columbus, 6th May, 1839

Prof. Agassiz, Cambridge.

Dear Sir: Let me thank you for your kind letter of the 14th, just received. I left Columbus in the beginning of April on a business tour and returned home yesterday to spend one week with my family before taking the field. I had before leaving finished my part of the examination of the Aspin mantle. Mr. Sullivan whom I have seen this morning is not yet ready with the short diagnosis of our new species and as he is very busy with some business, he says that he cannot attend to the work before the summer months and that he will want till August.

I am truly glad that Mr. Hooker can send a copy of the fossil plants. As soon as you know what it cost, I will send the money with pleasure.

Douglass to whom I sent a rough sketch of my drawing of the fossil plants of Oregon & Washington is delighted with the discovery of the plant of the Tertiary of America and fully agree with me not only in the general conclusions but in the determination of the plants. He writes a very long and interesting letter which I will try to translate to-night and send to Dr. Dana for the Librarian's Journal.

Do not forget to read it, since it contains some facts according with the conclusions of the paper which was sent me with your letter. I am certainly most
alleged to you for the credit and praise you give to my dedicatory write on the front paper.

The reason of my name by a Gentleman, when to such, high and general regard as you are now most welcome to me especially, first view. Dyer's report on the Com: survey is printed and printed at best. I received a copy and opening to send after my report that would be reprinted June 17.

The foregoing essays by A. Causmany are the result of researches made in him, in the year 1832-1834 upon the front place of Com:-

I wrote my junior, at the expense of the Commerical: I have since the few improved and changed: to found third publication especially prohibited by the production of a perfect of the same volume with a report on the front place of the Natural: the Riddel of Mexico, really correct as the Geological Survey of that St. In a postscript paragraph to that report, I once remembered: as for the right, it may have a good: a few lines of a report already in the order of the Geological Survey of Com:-

[Lines of text...]

... single page on &

... to the characters, spots of the front: bed of coal which is now near this same, as well the Kentucky or: Ohio, and in Com:-

... the quotation, but Dyer's would have had then a right to say that I had exposed in my report such facts as belonged to the Com: report and which did not belong to me. For the publication of the Com: report, though I wrote to Dyer Dyer about it, he would never come to my inquiries. I hand occasion to send for my report when I met in the woods in late 1835: that the report would be published in England and that already my place, had been paid, although there. Immediately I web to Dyer that, according to the promise, I should have the privilege of examining and correcting my report; some return of my name was turned over; and you that it is not deliverable. And, it does not answer single word, but meet two by his trouble, who regarded me to whom I am immediately a copy of the placed of correction. I did understand for the that I had to correct the place in the copy, and return them correct. If for a place of the Com: Dyer wrote that the place had been put to save my report but that he could not put with the manuscript I for fear of losing it. And had only manuscript of my report and had been colleague. I could not comply with the request. I told him no such permission to be little into how to make one of the report would be published in at whole of it would be published in my name and of several other have copies of it. Happily I have the title...
when he refused to answer. I wanted to know if he would deliver me half of the specimens which I had collected, and which were especially promised to me as well as the 30 copies of my report. He said he would not answer and has never answered. I have just received a letter of his best friend, Prof. Lewis. It is dated Montpelier, 29 March 1846. He has, corresponded with Prof. Rogers once since his return to Europe. He writes:

"I have been up to the Hon. Survey, and a certain fact has been found. The principal of Mr. James 2:1. There is not a single assistant of Rogers who will listen and allow the peculiarity of my belief, report supposed that the Hon. Survey should come into an end. Soon, it seems, he said very bar to les a man for whom I have worked more than I would want for myself, attempting to be from me. The only thing that it proved in the world, the name of an honest man, you will please cause this long story of you read it through. I know Mr. Rogers' brother of D. B. could speak of this to you, the be of your friend and I would not wish to lose your friendship and confidence and the good will that you have ever shown to me.

Most truly yours, Yeo Veseyveney

18 May. Sir, the paper you send me, it appears that just now the letter of Prof. Lewis could be acceptable to you. I send you therefore the translation of it and beg you will send it to Chief.

I believe, after the journal or use of it, so that he may have it printed in the next number of the journal. I will write about it. It is translated nearly word for word that if you read German and want the letter itself it can be sent to you from Columbus. The letter addressed to me are yours here. You will easily understand what these day of the Atlantic. He went to show by the fossil plant that it existed between N. America and W. Indies: an old continent of which the Canary Island are the remains. The coal flora shows that at the coal period the connection between both continent did not exist or rather does not show that it existed.
Prof. Agnus Gray,

Dear Sir: How heart-bitter is that 18th of June, truly welcome. I find, as here at my return from E. Kentucky where I have just spent the week to finish the geological exploration of the coal measures of that state, I am now strong that I did trouble you with complaint about Rogers unjust accusations, as regards about them, it is true but it should have been done for myself alone a matter of no importance whatever and which was soon forgotten.

Your letter contains leaves of Cereophyllum Californiensis, I am much pleased to have specimen of it.

Your copies of botanical memoirs is also received. Many and many thanks for all your kindness which I can never repay. I have had not time till now to read the whole of your paper as I am just returned and am full of anxiety with my daughter's sickness. I have only seen that you quoted my researches on the fossil plants of recent formations, and am truly obliged to you for mentioning my name. — The quinquane or sub- or super-glaucal formation of the Mississippi extends itself all along the Ohio river and its affluents, but much thinner than on the Mississippi. Examining it at Greenupbury, above the mouth of little Sandy, I found in a pest (seed) of the Japan and two native which would be desirable to Indians many of them
are not smaller. I will compare them and report to you at once as I have time. In the meantime, I shall be at St. Louis in September. I will try to stop on the Mississippi for collecting the beans of the old and new species in plenty.

In my last journey in Kentucky, I did not find any plant worth mentioning to you except a few specimens of 

Cardiaca hancockii var. (along Big river on the Virginia side) 

between Twelve and Warfield. A beautiful shrub indeed but with a peculiar aspect, greatly contrasting with the vegetation around it. There were no flowers.

I have not yet seen A. tuberculata. Yet, that he could not give you a report on the opepe waves.

Believe you, friend?

L. Hegner
September 11, 1860

Cambridge

Dear Sir,

I spent Oct. 2nd and part of Dec. in botanical and geological exploration of Arkansas, in connection with the Geological Survey. Though the season was late, I have collected some interesting plants and made a catalog of all the species which I have seen. From the plants which are most interesting to me, I take the liberty to send you the three following. A leaf of *Quercus lyrata* Skinner. As you do not mention it in the flora, perhaps it has not been found by the survey. It grows in deep, nearly inaccessible marshes near the Washita river. I could not find any acorns of this species. It is one of the largest and most beautiful trees of the southern marshes. Its geographical distribution is somewhat more extensive than that of the *Chaenomeles*, about 50 miles (in Ark.), the second as *Hyptis tubiflora* 2. But if it is this species its range of distribution is not always along the coast. It grows on the tuft: (a hill) formed by the carbonates of lime of the hot springs of Ark; the only part where I have seen it with *Juniperus Virginiana*. The 3rd is a species from which I do not find described anywhere and which is certainly different from a *peplum* by its nakedness, ramification etc. It grows also on abundance around the hot springs, generally hanging from wet rocks (tuft). But I had found it before coming to this place my on the Western bank of Ark at the edges of a rivulet. I have plenty of specimen and better ones larger one that shape. They are at your service. — One of my
A packet of plants has been sent. It contained a specimen of an oak which I suppose near. The tree is much larger—about 30 feet high. Some somewhat like them on the 4th of October last, but smaller than this. I have not heard of any other. I have sent it to my friend, Mr. A. B. T. Smith, in the United States, for the most common form of A. Nigra in the Western States. The leaves being generally broader on the western branch in the ground with the normal form higher up in the tree. The sharp division of the leaves of the oak is occasionally visible in all the species, I think. Would the catalogue of the plants (see in 1870) be of some interest to you, I would gladly send it to you at any time.

As I was directed to study especially the general habit of the plants of Australia, together with the geological value of the soil and its agricultural properties (a comparison which, under the circumstances, is nearly impossible) I have marked so much a portion of the range of the most interesting species. Among these, the distribution of the Beaucarnea is truly remarkable, being the shafty corn formation, which forms a parallel border to the Ohio river, from the Wabash to the Mississippi river. It is entirely absent. This year is about 30 miles broad. Nevertheless, all along the Wabash river, before the Ohio river, on up to the Ohio river, a rather pleasing formation of the central compound of the same.
of the species are still living. This is how they escape
the war, without opinion, most common now in the marshs
of southern Brit.

I have read with the greatest interest Mr. Huxley's intrade-
story "Darwin" and have entered Darwin's battle to understand
what may be the proof of this system of transformation.
My persuasion is opposed to it. But faith is of no account
in a scientific question.

I have consumed this long letter. Most truly your,
Sir, D'Arcy.
Columbus, O., April 18, 1860.

My dear Mr. [Name],

When your most interesting and personal review of Orleans' book reached me, I was just ready to break the book itself. Your memoir, reminding us of the diplomatic pretensions of the author, was for me of great advantage. I wanted to write you immediately to thank you for your kind communication, but I thought it was better to wait a week, though to give you at the same time my impressions of this period on the war, I am not sure how long that purpose will be best served. When I spent two months in examining the fossil plants of the Gulf, it was of that state I must feel very much at home and yet very far from a long absence of theme. Of the last end of America, if the field work among the coal formations, for at least 15 months.

Before your book came to hand, I had already prepared for the Report a subject to the 11th part of an article published first year in some quarter in a quarter the and formation, etc. Here I had had the distribution of the coal plant, either geographically, either geographically, or on the different measure of a coal field. After writing the Report, I have somewhat changed the order of my notions and I am now about ready to send the paper to the Journal. I write to you the last of the Journal, I hope that you could send here to read it and to make the same column, the necessary corrections, and all our knowledge of the field lately? If it is possible, I would like to have it published in the next number of the Journal. As I suppose I will be in the 20th or 21st pages. I must take the field next month, I could not be able to look over the
preface, I will try to write the title name so plainly that the
reader may read them easily. I wrote all the larger I sup-
pose that you have every page of the journal in its original
verse.

Here now we sing the very interesting title of Dr. Winchell in the
last number of the journal. We now sing that a publisher from the
African Journal, the first of the journal in its original
verse.

I ask for your attention to the very interesting title of Dr. Winchell in the
last number of the journal. We now sing that a publisher from the
African Journal, the first of the journal in its original
verse.
harmony without this complement) as if it is the first step toward a new species, of a higher genus than the species of laws established from the hope born by the improvement of Mankind. This may be a great difference for theology, but not for natural history. In theology, from each man may go to the same end as another, with the same methods and means. But in the improvement of Mankind, the top branches on parts of men. The top spread may be a higher atmosphere. It belongs to the her, no matter by what law it has grown. The cause of the promises has made the best of its laws; why should it not be as true that the sons of that still higher region, where they can fly. In every one of them, the basis of the human branches, those like an insect that may be closed. The top spread, except through the top short branch of man, Christ-fully. Is this the result of one, a free will, or of a selection of individuals, by what some power on the earth, continually moving higher, or how, these still achieve one step in the evolution of the whole? It does not consider the Christian.

Would you that, as my personal answer to what you ask of in the end of this review of the possibilities of harmonizing Darwin's theory with Christian philosophy. A personal opinion does not prove anything, but it may help in the path of the one who seeks it.

Most truly yours,
Columbus 0. 30th April 1860.

Dear Sir. As I have no received yet an answer to my letter, I take the liberty to send you the continued part of the memoir on the coal measures. Probably you will not find time to read it. Therefore, as I have nobody here who could correct my errors of language, you would greatly oblige me by directing the printer to make those corrections. — Please try at least to read the letter about Newberry's answer to them. I am sorry to have to write it. You know that I would not notice any attack against me. But it is truly deplorable to see a true man of science and honourable in every point, injured for having taken the trouble to supply knowledge where it was so badly wanted. If Newberry is right in his determination, it is pretty clear that the questioned plants belong to the tertiary. But I think that he is mistaken and that the my thecaceae cretaceous. If nevertheless you find that the letter is useless, may give trouble to the journal by calling for an answer. Will please destroy the letter and let the matter drop.

My views on palaeontological distribution with the data accompanying them can not be of interest in America. But European palaeontologists will certainly be pleased to know something of the distribution of the plants. I cut more than half of this memoir. It was becoming too long. I got a member to send one, but I was not prepared to write the letter. If you have the kindness to write me any thing, your letter will be sent to me. It will be most welcome. I would request to take...
to field work and camping
Most truly your old friend

Newark, Delaware

My package was at the express and the manuscript directed to you when I received your very kind letter. I took out the manuscript and send it to Prof. B. Hillman as you directed. But I cannot send this letter about Newberry before you have read it and give your approval to it. If it does not suit you, please burn it. It does no matter a straw for me if it is published or not. I think I have to write it and that is all.

I cannot tell you how grateful I am for your friendly kindness to me. Please to give also to Mrs. Gray my grateful acknowledgments of your kind invitation with my highest regards. I hope that you are better and out of the influence. Probably Mr. Hillman suffers of something alike. I am too poor out too busy to be sick. It is a place most truly yours.

If the letter is right please send it to the journal.
Columbus, July 11, 1860

Prof. A. P. Gray

Dear Sir,

I have returned to Columbus and intend to stay here henceforth at least for this year. Smithsonian Journal has published part of my paper on the distribution of the Coal plants. Probably you have had the kindness of correcting the proof. I am much obliged to you for the trouble. The printing is very correct, indeed. If the second part of the article is not already prepared and if you have no corrected it already, please to return the journal to send it to me for correction. I am ashamed to give you so much to do on my account.

I have to prepare for my report on Arkansas. Catalogue of the living plants of that state. As it was already too late when I visited Arkansas last year, I could not see much of its botany. Moreover, I had to move very quickly and rarely without stopping except for the night, all the time of my exploration. When shall I find
Described or enumerated the plants of Arkansas.
I have Nutall's Catalogue of N. Amer. plants.
and also Elliot, Daily and the southern flora,
but I do not find much in those books.

Do you know something better?

I have read a Thule Journal from
on the origin of species, etc. It is not much. What
he says is true. But the volume is dilated in too much
value. Agassiz's cultural examination is better,
but very good. But a Darwin admits it, Geologist
into us the only one which can make authentically
and furnish proof. How an one get at these
geological data, if as fast as they are exposed, their
truth and value is denied as it is by Mr. Nutall
in his introductory essay (not in Hillman's journal
May 60 p. 308). This is somewhat discouraging
perhaps. But after all, man is only trying
to come to the truth and the one only is wrong
who never try

I have not seen Mr. Sullivan yet. But
I know that he is well and working at the
foot of the Sierra. What a beautiful

If you have something on the botany of
Arkansas and can send it, I will return it
ask a few days

Most respectfully your friend
R. L. Powers
species, and shall continue to do so at my leisure. And when I am ready, I shall try to go to Cambridge and spend a few days with you for determination of the whole. The brine perhaps?

Most resp. your friend

New-Anguera

Don't return the specimen. But only the name, if you please.

Columbus, 6. August 23 1860

Prof. Louis Gray

Dear friend. I am already under much obligation for your constant kindness to me. I would have written you immediately, after reception of Hooker's good store and the pamphlet accompanying it to offer you my best thanks. But I was then deeply sunk in the examination of some plant collected in Arkansas; and I well supposed that I could not go through without your help. Now let me assure you first that I am truly and much obliged for the beneficent and deeply regretted S. N. I am never able to reciprocate your good offices in any way. And secondly let me ask a new service and pray you would examine, if you can do it without any loss of time, these few plants, and tell me the name.

Your great store has helped me much with the determination of my plants, as for mention of species found in Arkansas by Nuttall. I never take in hand this
The geological part (coal) of the report of Arkansas is ready. Among the fossils collected, I found the wing of a beautiful Blattina (cockroach) to very near Blatto haldena, that it can be only separated with difficulty. I have figured both the living and fossil species. This will go with some species of angiosperms against Mr. Darwin. But from what I said of the discussion of Lycopsis in the subject, the more is said, the less we know about the matter. But the road to absolute truth is a long one. Among the fossil plants of the true tertiary, got from Mississippian, there is beautiful specimen of the great Terminalia figured by Sullivant. (I forget the name) At last it is no longer true to say any difference in the form of the leaves, which form is truly remarkable. There, also leaves of a Magnolia, which can not be distinguished from it, and a single flower. I have already begun to figure all these.
Columbus, Sept. 23, 1860.

Prof. A. Gray, Cambridge.

Dear Friend,

You will have to try enough! I am to trouble you again abusing perhaps of your friendship. After this, I will try to keep quiet a while, I promise you.

Goedel writes me that you are about sending him a packet of plants and wants me to buy for him a copy of Chapman's Flora of the Southern States. If you send a packet, please put with it the copy of this book and I will send you the money immediately. Goedel can pay me at the banker who does our business at Newhalk.

Another friend of mine, being here, wants to know the price of the part (published) of your great flora. By the way, I suppose he can get it at the publisher, but I would like to know what it cost. You remember perhaps that I owe you a copy to your great kindness and generosity and this is not known.

You have read Newberry's answer. As it now concerns only myself, I can not resent it and will let the matter drop. But I was truly disappointed and chagrined to see that Sullman's Journal which had already cut in two my paper on the distribution of the coal plants of America has now postponed the publication of the second part - the only one containing the portion of the introduction. This postponement due to Newberry's influence on t'hostility to Mason with whom I have nothing to do,
and with whom I am scarcely acquainted. In any case as I do not wish to be governed by any scientific part, I would rather have my manuscript returned than to have it accepted under any other influence but what it is worth. The publication of assertions which may have been anticipated might well ridiculou. As you are now continual intercourse with the direction of the Journal or are even one of the directors, you know probably all about it and I rely on your friendship to put the matter a right and to do what you think the best either having the manuscript returned or published if it is convenient. Last year I sent the amount of my subscription and it was returned to me with the kind remark of Prof. Dane that my contributions to the journal would be better than the money. It does not look as if we so.

Simplexus, is generally called yellow wood in Arkansas and Louisiana. As I did not know this shrub and could not find the flowers or the fruit, I asked some time the English name and got always the same. Of course many names are only local. In my catalogue of the plants of Arkansas I took the liberty to follow your classification and English nomenclature thus calling Simplexus the sweet leaf. Of course you are the only reliable authority, every body knows it and an unique English nomenclature is very desirable.

God bless a very good letter. He is happy and all goes well with and around him. I can not say as much for myself but perpetual sunshine can not do for the world. Most respectfully,

your friend — Ed. Sorquem
you think that the Academy of Cam-
bridge would publish them in its, memoir.
I wish to know it as soon as possible. The
fossil plants of the tertiary marshes belong to
Southern Geology, who have sent them
for determination, and if I have the possi-
bility of publishing them altogether, I must
ask their permission to do so. Some are
intended for future reports. But they could
be put in the reports here after all the same.
I have a separate copy for you of the paper
on fossil plants published in the Journal.
Do you wish one or two more for some
friends? I.—McElwain is working hard
for Wright, name of Cuba. There is a
few specimens new species. Schrader his
draughtsman has learned engraving and
will do pretty well without drawings.
Plan to give my respects to Mrs
Gray. Very respectfully your friend
Leo Desqueyry

Columbus 5, Dec. 6th 1860.
Sir: Apa Gray Cambridge
Dear friend,

Christmas being passed, I cannot
not express a wish but the hope that
you had yesterday a very happy time.
Indeed I never think of you but with
a sincere desire of your life being
as perfectly happy as this world will
allow. I know well enough that you
have the same desire for me.

For the present, at least, I can not
say that your desire is fulfilled. I have
had lately to try the truth, since
the crisis of 1857 great trouble with
my business. In partnership with my
friends, I had by and by established
an importing house. For watches, &c.
Naturally, I invested in this business all
that I was worth and I think that
all is lost. By the ruin of my home
and everything I have, I will scarcely
by able to pay my debts. I did not
see much about this because I was
only a poor clerk. I had the prospect of
making a living by science for exploration.
But now the start of the future is
shrouded by a thick fog. I know
The value of the fossils, it would be returned by the sale of the world. Indeed, I do not understand very well what could be done in such a way. But if you ever had an opportunity to speak with some person of influence at Cambridge or also perhaps with Agassiz you could find the best way to help me and to get something valuable for science at the same time. The Penn report can not give an idea of what I can do now for the fossil flora. Both the report of Alabama and of Illinois and also the report of Nebraska, of what extent my researches have been told now and what they are worth for practical geology. Perhaps the fossil flora of the coal could be published by the Smithsonian Institution. But this work accompanied with a geological and as complete (as complete as possible now) as indication of the distribution of the coal strata of the United States may be a valuable work and I would like to have a few crumbs for my old age.

Tell me something before to do, I am now re-examining and drawing the fossil plants of the Tertiary. I shall have about 20 plates which will be ready in two or three weeks. Do
Cumbus Oct. 25 1861.

To my dear Cambridge
to whom a few days ago I received
prints at Pall Mall the Pilber and the
Plague Writings (from Cuba) with
your examination of Darwin's theory.
I have thanked you heartily for these
communications, especially for the pleasure
of reading this last review. It is most
interesting, indeed, and certainly, the last
that has been written about this so
called new theory. I say so called, because
I thought I do my best to find it, I
can not see the difference between Dar
win's and Lamarck's theories. The theory
for life is a problem unanswerable for
merely except by admitting it as an action
resulting from external circumstances, as
Lamarck will have it. But I am
probably somewhat obscure in this
matter or in many others and in any
case it is no work while discussing
my meaning just now. Let me arrive
you that I have followed your explanation on examination of the page not only with the greatest interest but with great pleasure. Your argumentation is deep, ingenious and witty. But you have already the value of your contributions to science. Don't you?

It appears certain that the chalk beds of the Mississippi and all the beds containing fossil plants in Tennessee and Missouri belong to different stages of the geological history. Now, there are always two
halves of these fossil plants and all except our species and one Sahurine dorytegenum (and perhaps some of them to specie) of our time and of our North American flora. They are many species of Magnoliæ with apparently the same character as some species of our time. The extraordinary likeness contrasted with the great difference of the European flora when compared with the flora of the same country. But

the comparison can be established mostly on the flora of temperate, the peculiar topography of that country may explain the difference. I do not know when I can go to Cambridge. But if you wish to see the plants and compare them yourself with living species, I can send them to you at any time.

I send you a report on Arkansas fiddle. I am just out. Perhaps the botanical table may interest you. I am perfectly satisfied of its correctness as a matter of views; but perhaps the plan might be followed for other catalogues of this kind.

Thus by comparison we could get and by the geological relation of living plants, if there is any 24.

At Burlington in well and works have at the Cuban moses.

Sincerely yours,

Leô Lescureux
and thus a mere material would a tenant will have it. Probably Prof. Dawson will soon have more to say about it.

I have not seen A. Sullivan for a few days. As I have nothing more to do now I have begun the preparation of Plate 3 for the fossil flora of the Coal. I shall try also to prepare the balance of my more intricate, having still material, for about fifty copies. By and by perhaps time may be when such wondering worlds can become valuable. But now the house is very black in deed. When you write again please to tell me if you have heard lately from Godet? But do not write till you have plenty of time and need to write me. Most sincerely yours.

Ted Lesquereux

Columbus 0. 7th April 1861.

Prof. A. P. Gray Cambridge.

Dear Friend.

Your very kind letter of March 19th gave me the greatest pleasure, for there is certainly, no man in this country whose opinion I more sincerely appreciate than yours. I would have answered your letter immediately if I was not always afraid to talk for my own account your time always precious and too valuable for science. My answer to Prof. Dawson is nothing but that I consider as truth. Of course, I have seen more that the Prof. suggests because I have nothing else to do for science but go around and look at what interests me. If Dawson wrote me again, he says he understands my meaning better now. But I do not think he does because he appears to take my illustration of the peculiar growth of Stigmaven, 

provides by Sphagnum &c. as if I would pretend that the plants of both these families were all the and had the same structure.
This is far from my mind. The answer which was sent to me by Prof. Fullman has nothing whatever disagreeable to me, except that his anomalous which are advocated as scientific truths and which I consider as being rather against it. But Prof. Dawson is answerable for his opinion, and I do not see why it should not be published if he desires it. Only I do not wish to answer him because such scientific discussion becomes always a little too personal. I do not know when I will come and visit you. Though nothing would give me a greater enjoyment than the spending of a few days with you and among your botanical riches. Of course I can do no much here, alone, without resource of any kind and I would have at Cambridge a great deal of previous advantages. But a blade of grass can not become a Magnolia or a Pine, and I think that Providence has put me to vegetate just at the place where I am for this best.

Circumstances may change and show me another way perhaps.

No need to say anything about the Arkansas report such peculiar natural merits a mention. I only wish I could have spent a few months of the Summer in Arkansas. It is a most interesting region for studying the natural distribution of plants and the phenomena that have influenced this distribution. If I was only rich! But perhaps if I was rich I would do nothing and thus all is for the best.

At first I thought, as you do, that Darwin's evolution of species is not so mere materialist idea. But now I can not well reconcile the struggle for life with any Presidential law of development. Is this struggle for life the result of an individual will or willingness? We can not admit it, I think. If not it is only an accidental need resulting from external causes.
Columbus, Jan. 3rd, 1862

Most esteemed friend,

On Christmas Day, I got from our friend W.J. Sullivan a package of ferns which gave me the greatest pleasure, for I examined it the same day though I did not know what it was for. Your kind letter came here only before yesterday, and I called immediately to Mr. Sullivan, but the package of more rare ferns came five days later. Though I am afraid to give you the trouble to write me again, I want you to direct how I shall send the more rare ferns. Of course it is all ready. The 4th vol. of the Survey of Kent has come out and I wish to send a few copies to Boston and Cambridge for you, another for Ripley's and three for Boston. Could I not send to you all these copies with the more rare ferns, and perhaps you might...
get the cost of the express from the Gent. to whom the tools are addressed. I do not think that it would cost more than 20 shillings, and it cost 60 shillings.

The furs are plentiful, many of the species are unknown to me, and some extremely interesting; for comparison, I am much obliged to you for the exchange. I have not got your recent paper on Wright's furs. But be pleasant to it and send it to me yesterday. I shall immediately file the letters.

Your Kind letter was very welcome. I ought to be very hard for you to spare your time in writing letters for the pleasure of your friends. But what friend of yours could be indifferent to a kind remembrance of you.

And only a few words about the more, as soon as you want them.

and I will send them in any way you desire.

I am sorry to hear of Mrs. Gray's indifferent state of health and most sincerely desire that she shall soon be well again.

Yours the friend,

J. Ferguson

Prof. Paton returned the furs, with the names ... I am much obliged for it. But one always so not with the 'Deserte furs. It was found among morn, and was probably overlooked from its small size.

Prof. Ada Gray

Cambridge.
Columbus 19th April 63

Prof. J. Gray Cambridge

My dear Prof.

You would pay me a service to let me know if the notes published in the last Botanische Zeitung by Muller are from California. I fear, Oeland, has sent me lately a large amount of new species from Cali. and I have prepared a paper on the description of new species, to be read at the meeting before leaving Columbus. Muller, I said to me about these notes, that you should write to him about Muller’s publication in the Zeitung. But he does not know where the notes are from. We expected to get Muller’s copy every day, but it does not come. If the notes published by Muller are from Cali. and if you can lend me your copy of the Bot. Zeit. for one or two days, I shall be much obliged.

With the notes, Oeland sent me a package of rheumatous plants, grasses etc. This is useless to me. Shall I send it to you? This Mr Oeland is an excellent collector for mosses. I have already more than 60 species to add to the new series of the Musci Irland.,
It is a long time since I have heard from you. I know I did not write you. But you have always to much to do that I do not wish to come to you and to the even the smallest part of your time only for my own satisfaction. I am now out of business, a more than usual man. But I hope to at least make a living in some way. I wonder I will spend time for six or seven years. It will be the same a little while longer. Maybe, I shall have the pleasure to see you at Cambridge this year. Guy, Jackson and the Smithsonian intend to send me around for collecting fossil plants for their collection. If you part I must go to Cambridge for again. Don't plant. I hope you have been well and prosperous all the time.

Your old friend

I Longmore

Please to remember me most respectfully to Mrs. Gray. I hope she is now quite well.
Columbus, Feb. 21st 1644

Prof. Ms. Gray. Cambridge.

My dear Prof.

As soon as I get your letter of Feb'y 4th. I wrote Schimper about the music of Cuba. I do not doubt that he is still desiring or anxious to have them; but it may be that he want more than two sets, and I would like to have a positive answer on the matter. Nevertheless, if you please, as ready as before I got your letter, I think that it would be better to forward to him the two sets you positively demanded in his letter of next year. I did not answer your New Year letter immediately, because I wanted to see if perhaps I could be ready with the preparation of the second edition of the music at time to send with your packages a few copies of the whole of Schimper, especially to Schimper.

I don't see that I am not to trouble you in three months at least. I have still the whole store of the 'Cimarcopi' to prepare and it much to do besides this work.

I thank you sincerely for your kind letter and the determinate of the firm. I showed your letter to Mr. Sullivan, who is well now, at least well enough to be out and moving. That building
for your botanical treasures will be
a fine one-I hope you will enjoy your
botanical life still for a very long time
but the disappointment you are missing
new of riches of rich high scientific
value and obtained by such hard and
constant work in most noble and
commendable. Our botanical protectors
will bless you for this work.

You do not say anything of
Mrs. Gray's health. I would be sincerely
regretted to hear that she is not quite well.
Then give me my sincere respect.

It is a long time that I have heard
nothing from Godet. Boisier write
that he would like me to go to Califo-
nia for collecting plants. My career as a
collector is over, I think, and
certainly Odoarde. Who is there yet will
do far better than I could. Nevertheless
if I had a good opportunity to cross the
Alps and go to California stopping
on your way in the Brevny Mts. I could
surely resist the temptation of doing
the thing I have dreamed of Califor
once time and again.

Very sincerely your friend.

Geo. Engler.
Columbus 0 April 23°/65

Dear Prof. Gray,

M. Boissier of Geneva in a letter received yesterday says: "Si vous écrivez à notre ami Aza Gray, renoulez lui prouver mes compli-
ments affectueux et lui demander quand
voulez pro nes pour distribution les nouvelles
collections de Cuba et les brochures qu'il a
promis de m'envoyer."

If I remember well what you wrote me, all this was forwarded
by M. Boissier with Hall's plants. By the
time he has certainly got them, he sends a
package of plants to Hall and I will have
to acknowledge the receipt as soon as Hall
has got it. Have you perhaps something
particular to correct to his note copied above.

Botander demands the names of some
mosses which, if I understand rightly, he says
he sent in a package of yours. Have you
ever got a small package of mosses for
us? I received one through M. Julieron,
coming from you, I think, in July past.
But it contains only doubled specimens for
the Musei, all without labels, and Botander
speaks of specimens with No.

Yesterday we received part of the labels.
They are just at time and I will begin
at once the preparation of the sets. You have
had much trouble about that matter. I
am sorry for it and thankfull for your
kind Office in that matter.

Would it be possible to buy separately,
No. 29 vol. VII. June 20 of the Journal
of the Proceedings of the Linnean Society (London) it contains Millin's paper on Californian moles. I need it very often for reference. If you can get it for me, I will send the money as soon as bill is received.

I hope that you are quite well and Mrs. Gray's health is improved. Please give her my sincere respects. I received your paper on the Botanical Museum of Cambridge and read it with great pleasure. Though I do not always acknowledge your scientific communications for fear of giving you some useless trouble, you will know that I always receive them with great pleasure and read them with great interest and profit. I do not know if I ever wrote you about that pamphlet on Californian moles. It would have been far better if Mr. Culver had preserved it. But he had no time and as Botanists don't at the same time the same more to handle and to myself, Mr. Culver suggested me to examine them and report on them in order that priority of nomenclature should be preserved for us. Thoms had moles to handle, reported on a few moles, and then from California that it was desirable to give a better account of his species. My main was submitted to Mr. Culver and approved by him and though Millin does not state the diagnoses of some species of moles which he has also published in several, they are as satisfactory and as reliable as some of his own. I do not know if Californian Thugasnoos plant show the same extraordinary disposition to variability as the moles, which from this reason are extremely difficult to describe.

Very respectfully your old friend

I. Geouer
you suppose that the price is too high. If we should count what each all has cost us either in time or money, it would certainly be more than $1000 for each.

I do not write this long letter as a matter of interest to you, but you may have (and I hope you will) something to say in the journal concerning the museum and may be you will find some materials in this detailed account.

I will forward the package as soon as you answer is received.

Very sincerely your friend
S. Marguerite

I have offered my respectful regards to Mr. Gray and at the same time my sincere wishes for the restoration of his health.

I have been to M. Sullivan since writing the above. He supposes $35 would be somewhat high. But the committee are Clinton and others say the price is too low. #1 think that if you should mention that both the London and the Paris museums have subscribed for the work, many have would follow the example. Do you want something to Botanical, I will send him one, sell free of expenses of course, but how?

Columbus 0. March 19th 1866.

Prof. Iza Gray Cambridge.

My dear friend,

At least the second Ed. of the Musc: existent is out and the sell is ready for distribution. You ordered a set for the British museum. Shall I forward it to you per express with your own bill. I may have in one month a big package to send to Schmpers and another to M: Boissier, but it would cost as much to send to England from Strasbourg or Geneva as from Cambridge, perhaps? If you think that you have or may have the placing of some other copies, I will send you any number, as far as I have them.

The number of species and good varieties of this 2d Ed. of the Musc is 536. The world is indeed quite a new one. The labels have been reprinted to allow an uninterrupted series and the placement of all the species.
according to their scientific relations. The collection is enriched by the species of California, admirably collected by Bolander and himself, some of the Rocky Mountain specimens communicated by Hall. The number of Eastin species has been also increased by the researches of James, De Geer, Austin, Sego Clinton, Engelmant, and myself. and the specimens of the old species have been either renewed or increased as much as possible, in number and in scientific value. Of course a few species are represented by specimens which are not quite satisfactory and complete but these species are few and no found on purpose to give them a they are, to complete as much as possible the representation of our \n
Bryological flora at this time. And moreover, there is not a specimen of the whole collection which can not be used with advantage for comparison.

The preparation of these lots has been

a very long and trouble some work. Not only for the preparation and determination of the specimens which it contains, but by the constant call for microscopic examination of an immense number of specimens sent for determination by some contributors. But this has had a good result, encouraging some young botanists to the study of the mosses and cryptogamous plants and thus causing the discovery of a certain number of yet unrecorded species. The bryological flora is here becoming somewhat appreciable in its richness. Nevertheless I do not believe that if we consider the whole extent of the U.S. from one Ocean to the other, more than one half of the flora is known.

About 10 copies of the music will be distributed gratuitously, the balance of the sets will be sold if possible. What do you think of the price? I purpose to sell them at $35 per set in

America and $45 in Europe. Do
Columbia, March 28th 1866

Prof. A. Gray
Cambridge

Dear friend. Your kind letter concerning the price of the 5th is most satisfactory. We agree with Mr. Hull. Want that the price is $35 in gold. Please to announce it in that way.

I have delivered to the Am. Express this morning one, ready for your address, containing your own set and that of the Museum of London. As you say nothing for Paris, I suppose if they want one there, Schimper to whom I will send 5 $5 (or ordered by him) may place one there. All those to whom I have sent tells, Autin, Peck &c. write me that the price is too low, and that you wish above every kind of praise. Mr. Bell says that it has opened a new world to him.

The Museum of Chicago and that of
Albury have sent orders for one
and orders for one
for your kindness in this matter
for you. Allow me to thank you again.

I have sent a receipt for $7, sterling.
I have sent a receipt for $7, sterling.
For that you do not pay the
Express chargers. They were paid here.

I send a receipt for $7, sterling.
But if you have at London some

friend whom you may trouble with a commission for me, please demand of one of them to buy for me with part of the money a copy of the

first volume of Kirkley and Southay.

This work was in my hands for a time but the proprietor sold it and now I often want to consult it. I suppose that $15 to $20 or less

may be the price. It is not any more in the trade but has to be bought from old book stores.

Allow me to thank you again.

Your sincere friend
A. Lesquereux
Dear Sir,

I wish to pay you a short visit before leaving your house. How pleased I was to find her the same most amiable and charming lady as I had seen her years ago. Please to offer her my most respectful regards.

Your friend,

[Signature]

A. Desqueneus

Had you not better send to M' Sullivan that package of mones from Cuba and write him that Wright's answer to his determination. Nobody could stimulate him with better results than you. I think that Prof. Agar will send me some books. You could put the package with.

Columbus, Oct. 14th, 1864.

My dear Prof. Gray.

Do not scold me for having left Cambridge without paying you a third visit. I wanted badly to see you again, if it was only to thank you for your great kindness toward your deaf friend. But the weather was very bad, I was anxious to be on time to reach Columbus on Saturday night, the 9th inst, and indeed I supposed that I had taken already too much of your time, and that you would be thankful to be spared one of my boring visits. - I wish that you had given me that package of Cuban mones for M' Sullivan which has called and with whom I spoke about that supplement for the manual and about the Cuban mones. He does not like the idea of helping in the publication of the whole series of the Cryptogamia, plant.
he says that he would rather make an independent Synopsis museum without very long description, but at least 2 or 3 times as long as they are in his manual. But before coming to this, he would first do the Cuban and Venezuelan moneys which he has not begun yet. His conclusion was, that he would think it all over and talk with me about it a week or two. I believe that by and by Mr. Sullivan will and must begin to study moneys, or old. Now that his house is built, he has nothing to do and a country life without scientific entertainment. He should become lost in a man of such a mind as he has.

The Moneys of Alabama collected by a friend of mine living at Mobile are of great interest. Many are Cuban pieces. I went over to Mr. Sullivan with the request to see if it was not Frederick McCourtgle, one of his spears. He was pleased indeed to look at it and he said that when I was ready, he should take a look at them. In that way, I will try to stimulate him and take him out of his papers which, if I do not mistake, is his beautiful wife.

The set of the Cuban moneys is of the greatest advantage to me. But I cannot speak to Mr. Sullivan of my wish of examining the moneys of St. Louis. If he undertakes it, he can do the work for better than I could. But I offer him to help in the work as much as I should be able to do it.

Now I think you again and again for your kindness. I had a delightful time in examining Martius and for more delightful still in the company of Mr. Gray who allowed...
Columbus, O. Feb'y 6th 70

Dear Prof. Gray,

All your friends before now have dont you made of wellcome. Please to believe that the last one coming now to give you the hand of wellcome is the most sincere of all. I hope that your European Journey has done you the greatest good and afforded you pleasure and advantage and that it did the same for kind and most amiable Mr. Gray.

I got lately a good letter from Mr. Borron. He says among other things that he has deeply regretted to miss you not at Genoa, he being then in State for his health and in great danger of losing his life. He sent me his photographic and says that probably Mr. H. E. Candolle has given you one of his for me. If it is so please to send it, and do not wait till you have time to write for it after you long absence, you have probably more to do than ever.

My health has been very poor the whole of past year and I have still had hard trials in other ways. But
my eyesight, which was nearly lost is improving and all will be well to the end.

Please to offer my most respectful regard to Mrs. Gray. Your friend
very truly A. Berquere

What is Fargaloe urnus of a name of this kind, which I find attached to a fragment of leaves given to me by a gardner. If you do not know it, please do not lose time in looking for it. That fragment is palm-like, a triangular leaf with a very curious part. The plant is cultivated in European gardens.
Cambridge 13 Nov. 71.

Dear Prof Gray,

You have my best thanks for your too kind note. I will call at five o'clock to day for a short visit as I can not accept your invitation for dinner. Please do not stay at home for me if you are called elsewhere as I will call every day at the same time till I find you disengaged.

Your old friend very truly,

L. Vegesna.
Columbus, O. Oct. 12th 72.

Prof. Leop Gray Cambridge.

My dear Professor,

Many thanks for the communication of a copy of your admirable paper.

I do not think that I can read anything of this kind with more pleasure. Not merely because I find in your statements an encouragement to my modest researches from such a high authority as yours, but because what you say is beautifully expressed, true in every word, deeply considered and far sighted in the suggested or derived conclusions. It is a most valuable contribution to the advancement of science. I believe that your statements, even those which you present hypothetically will be more and more confirmed by the study of the Tertiary flora of ours, especially when we are able to compare both the Eastern and Western Tertiary of America, islands. — My time of exploration from the Raton Mts., New Mexico, along the base
of the M5 to Cheyenne and along the
40th Rail Road from Cheyenne to Salt Lake
was of great interest to me; not only by the
large collections of specimens of fossil plant
which I made, but by examination of
the localities for comparison of geological
stations etc. I should also have got a good
many species of living plants, if I had
had time and mind for them. But I
can not attend to two things at the same
time and I could scarcely come out of the
general researches which required constant
eye and attention or I could have got
only specimens of some shrub, then
of the scenery I saw nothing worth observing.
What an immense difference is there between
their Breckel so well named, and our
Swiss alpine or even Jurassic Mountains.
You get certainly more social impressions
from your tour, especially from the Gomme
Valley, I hope that both Mr. Gray and
yourself have returned highly pleased
and especially fully restored in health.

Please do tell me what is this tree of
which I send a leaf. I got it in Ogden Cape
above Salt Lake, when it grew in company
with Pluviglomerum & A. tripaitum. It
looks much like A. campsius, but is rich-
ful on the lower surface. I could not
find it with fruits. And also what
this Oak growing at the base of the
M. near Canon city, Colorado with that
Quercus referred by Watson to Q. alba,
(an identity which I can not well see). It
is a dwarfish sort of small tree species.
But this one is evidently different. It
is named there Colorado scrub oak, but
perhaps my informant mislook it for the
other species which I think bear the same
name. I have only one branch with
fruit and will send it if you desire.

Most respectfully and truly yours
L. Engelmann
Columbia, 8 April 30th, 73

My dear Prof,

The good friend Sullivan died this morning. Since the first attack of his disease he never rallied but slowly declined without great suffering to the last. I never saw him for three months. A great loss. He was here my only friend.

Yours, most truly,

J. Lezueress
12th. This not was lost, and a note was sent. But to day I have definitively accepted to go as a member of the scientific expedition now setting by the Smithsonian Institution for the deep water of the Mr. A Yellow Stone. I may have to be soon on the way. If therefore you have some direction to give to me either on Sullivan's moss, or any other matter, please write me at once as convenient.  This under writing is a hard one for my old bones. But I shall try to follow to the end (the end of my life) the work begun in American vegetable paleontology and thus explore as long as I am able to go. Whitney's plants will be seen through 3 thinkt before I leave.

Columbus, 0 10th May 73.

My dear Brain Gray

The loss of our most kind friend Sullivan has quite unmanned me. A true good friend is the nearest relative in has. I could not write you before now but sent you the papers with some account of him. For you want to write something about the for the journal and some detail of publication of his may be acceptable to you. Did you write you on the publication of the 2d vol. of the Scenes? This vol. should be published. The plates are splendid and all ready and with Sullivan's notes, the descriptions can be made easily. Of course he has given you his Herbarium (botany) at least he told me many time, that it should go to your cabinet after his death and there is no more appropriate place for it. If you can be of some use here in any way please say to. Since I have left the Moses and the Miracaps, my relations...
with cultivants have been less frequent, but not less intimate. He was pleased to show me every thing about his work, preparations, plants &c. His cabinet of anatomical preparation of moths is very valuable. It is mostly mounted for the Genus Orthoptera; but this is the most difficult of all. I am told that you are well, but hope that it is not true. However, you must too hard take care of your self. Of the strong columnia, you have the only one left. What could botanical licence do without you? I see that Mr. Colbourn has died. He was like Xenoniant—a friend of old; what a pity to be left alone in the world.

I do not think going to Cambridge this year. I have too much to do at home. Now I am examining Prof. Whitney’s fossil plants from the Ocean of California, a most interesting study.

They bear the general character of both the old flowering plants and of those of our time and what is remarkable and what I do not understand yet, their types are more like our brentel ones or nearer to our present ones than to those of Japan. There is however a splendid species of Arab must like a species which I saw in your garden named Arab-Papyrus of Japan. Is this name right? I do not find it in the prodromus.

Of course I write you to have a talk about our [omitted words]. If you are not well and too if you have no time, do not trouble you to write me.

Your friend very truly,

A. Lexquerey

Hugh, Whitney is at Cambridge and if you see him please tell him that I shall send him an account of the fossil plants in about two weeks.
Columbus, 015th May 73

Dear Aza Gray Cambridge,

My dear friend,

I met yesterday at Sullivan's room the administratrix of his estate, his wife and son-in-law, who wanted me to see and say what should be done with the part which has not been devised by his testament. It is all his move, with all the books in his own room on the biological worlds. I do not understand why our friend did say nothing about it. The administratrix considered with my advice that all should go to your herbarium. Mr. Sullivan has told me many times that you should have the whole and there is no place where such a precious collection could be preserved and rendered useful but at your cabinet. The question is: 1. Would you accept the gift? 2. Is it appropriate to send it now. If the musc. rather, the musc. 2nd vol. has to be published, I shall have to prepare descriptions with the material left
By our friend. Part of these materials, already full descriptions, are in a separate book where he began the world; but most of them are upon separate leaves joined to sketches and specimens in his herbarium. Therefore if the collection is sent to you now the work of description is can be made only at Cambridge. On another side, Mr. Step- ham already sold her houses and the rooms must be emptied before the 1st of July. My own house is so small that I have scarcely place enough for my own books and cabinet and it would be impossible for me to store even a small part of this valuable property.

As Mr. Sullivan was president of the Medical college to which he has given all his apparatus, microscopes &c. probably the college would lend a room till the work on the Icones can be done. When that will be, I can not see. I have to go to the Rocky Mt. again for geological exploration and when returned, if I return I shall have work enough to do for reports and thus the work of the Icones may be indefinitely postponed. Could you find a way to arrange matter and advise what should be done for the best. The plates are prepared but not printed. They belong to Mr. Sullivan's estate of course. I could not learn as yet if they had decided to pay for the printing and other costs of publication. I wanted to hear from you before admiring about the matter. If you are well, of course you can not write me but perhaps you can give of somebody to write a few words about essential points, for part of the collection &c. and especially about Engler's collections which belong to Mr. Albert as far as I know. I wish it known to whom adress them. I may then them brought to my office and packed as soon as I know to whom I shall send訂charge or box.

Yours very truly,

L. Decimus

What is the cost of the Brazilian flora? The agricultural college has got the botanical books and they want to know what it will cost to continue subscriptions to that flora.
and of the parcels which belong to the Harvard collection. I then took out the package of Taylor's Herbarmium which Mr. Sullivan's brother promised to send. Among these packages there is a small one which from Mr. Mannalabam, belong to Dr. Stiller and should be returned to Prof. Hooker at your first convenience.

I do not know as yet when I shall begin work on the Icons. The plates are not yet received or printed. And, until you know, I have to live by my work and thus to accept work as it comes and as it pays. Thus I say much regarding of prior engagements which I am not at all aware of, but I may desire it or which have to be fulfilled in a fixed time.

The materials of the Herbarmium are most valuable. Besides this peplat collection there is a large number of packages still unopened and undetermined.

The same Adieum.

Columbus, 6th May 20th 13


My dear Prof.:

Your of the 17th came this morning. I have been to Mr. Sullivan and gave it to him. He says after reading it, that he had yesterday sent a box to the Medical College for the Botanical collections, and some of your brother and that he wishes to keep the whole till the Iconos and Manual are ready and that then the collection is to go to Harvard with the books of course. And do not think to go to that the 12th. As I expect to be very busy, and I want having booked out and myself having accepted only with great reluctance and conditionally to their being member of the corps. I can do nothing without good and friendly assistance and I have therefore demands to be released of such an engagement which however was only half taken. I shall then have time to soon prepare the work on which my purpose is to have the descriptions after
the design prepared here, then the most
sent with a Copy of the Plate to Schuiper
and to Lindenbergh for corrections and
forwards and the whole published in
Mr. J. Sullivan's name only as a memoir
of his three best friends in History. While
this is done I will prepare a Manual
of which the plan has been made long time
ago both by Mr. Sullivan and myself as
for a Common work. It ought to be published
in both names as he wanted it and should
appear immediately after or even with the
same in order that nothing of his can
be appropriated by others. If Schuiper or
Lindenbergh aren't to that, then too too
false the privilege of making additions
and corrections. I hope that this
will be satisfactory to you. As soon as the
plate are ready I will begin the work
of I am here and I shall try to stay most
of the time at Columbus, the nearest
in order to spend the winter along as

will be possible. As soon as Mr. Sullivan
or time I shall go to the Yellow, make a
list of all the books I lack and do which
will be sent to you as proprietary of the.
Whale. I will at the same time return
to your Taylor collection.

Your Most Respectful

S. Lajourra

June 14th.

You see by this letter that I
Do not even dare to send you my letters
after writing them by fear of being
thoroughly, or of uselessy taking your
time for my own advantage. Mr. J. Sullivan
told me that he would
write you all about his arrangements
for his brother's botanical prospect, and
therefore I committed my letter a week
long. I have been still once in Mr. Sullivan's
room and have finished inquiry of
the biological library a of the book.
Columbus 0. 10 - Dec'd 73

My dear Prof. Gray

Accept please my sincere thanks for the kind communication of your last paper: Characters of new Genera and New Orders Composed, etc. All that you write is so valuable and interesting that I get from its study a great deal of profit and much pleasure too. Subsequent to your letter I have left a great many of Vanquis's moneys which he gave to Schroeder for preparing a set with them on condition that Schroeder should make good drawings of each species, and give him a very good set of three moneys accompany the sketch. Now Schroeder claims the drawing for the reason that he can do nothing with the moneys which are not determined, fulling having never had time to attend to that. He came to me yesterday and I proposed him to lend you thence a set with the drawings to Lindberg requesting him to publish the species with description and thus as it was done for the Cuban moneys. Schroeder should...
you have a number of copies of the pamphlet, corresponding with the number of his sets, and Lindberg would have a copy on a set for his trouble, and return all variants, etch and drawings, which could therefore be preserved in his collection. What do you think of this arrangement to which I believe Lindberg would agree. I shall write him just as soon as I have your answer, having to send him my copy of the Cuban moneys. It he receives it there in a short time

I am now about half through with the supplement of the Icons. You know that half of it was extremely careful not only in his description, but also in the preparation of the Misses, which he had copied by some good hand at writing. I suppose however, that the printer can read any matter without having it copied. There are of course some errors and omissions, etc. but the mass is much better than are these which I generally send to the printer. Is it necessary to have it recopied. Will you have time to read the proof sheets, at least once. For of course I shall read them myself. In the Icons you will see at least 5 different kinds of types. I do not know how to explain them for the printer. Take for example this 5th Plate.

Sphagnum macrophyllum. Pers. 2

Table 1.

Planta: Sphagnum macrophyllum

Reference etc. etc. that makes 6 different types. Can not now finish the text in comparison with that of the first vol. or shall I underline and how. I do not know of any indicative rule about that.

I have this morning a sad letter from Baron de Buren’s son. Perhaps you know that excellent man, good botanist, friend of Poiret, Godet and many others, one of my dearest friends. I said with whom I have constantly corresponded. It is clear, and now of those who knew him can think.
Columbus, 25th March 71

My dear Prof. Gray,

Andeburg writes that he

should be glad to work and publish the

Zoogeicso mem.; but that he cannot find

time for that till Sept. Shall I send
packack? I do not know till, but as
one of the best being geologists. He may
keep the mem. Perhaps for a long while, as
they are interesting for science. I should send
them, if they were my own; but I can not
be answerable for a valuable collection which
does not belong to me. I will send him my
copy of the Cuban mem., which he desires to
see. I am very surprised by Hayden for
his report, and suffer so much from my old
complaints, neuralgia of the optic nerve, caused
by one week that I have not yet finished the
description of the Tiwi. Two week of time will
find me through and I will have them from beginning
of May. I wish to spend two months of the summer
here at Cambridge. I could find covered peep,
at that time and place not to trouble.
Columbus 5, March 14th, 74.

My dear Prof. Gray.

Since my last letter to you I have received a copy of the Journal and read there a notice, just as I wanted and expected to see, of the Cretaceous flora. I will write a short one on North America.

Address—Prof. Lindberg, Stockholm.

Finland. Russia.

Graf Carl. Muller, Hall, Russia.

Dr. Ernst. Hanpe, Blankenburg.

Han. Braunschweig, in Germany.

Suppose you will send to the Smithsonian. Any how I will advise Hanpe Muller. Scheinpa, to whom I have to write that the vol. has been sent to them by yourself. I shall be glad to get my copy of the happy and that full copy which you send on besides, will be kept in...
remembrance of brilliant thoughts do not want anything to recall his memory. He is with me most of the time.

Sorry to hear that Mr. Gray is unwell as you are also yourself. This winter has been a very hard one. I sincerely desire and hope that you may soon be out of trouble yourself and your kind lady, to whom please offer my respectful regards.

Your very truly,

[Signature]

What is the price of the Supplement to the Scenes? I have answered to applicants that it (the leaflet) is for sale by Wadsworth & Co. But some ask for the price.
Columbia, 17th March 1844

My dear Prof. Gray,

You did not recollect what I wrote you formerly on those Venezuela mines. Sulivan bought Thowler's collection with numerous specimens; of course he gave it to Schrade on condition that he (Schrade) should sort the specimens, examine all the species, make good sketches (microsopical) of each species, send to him (Sulivan) the best set with the sketches, and that he would then determine and name them, each species bearing a number; and Schrade could sell the other sets on his own account as a compensation for his trouble and work. Now Schrade says: these more have not been named and my sets can not be sold; therefore I consider that Sullivan's set with sketches belongs to me as I will send it to Germany for determination of species, in order to decide...
to dispose of my own sets. In this matter
Mr. Sullivan nor myself did agree and the
matter stood thus when I proposed to
Schrader to send Mr. Sullivan's description
to go with the sets which he intends to
offer for sale. Now then the matter stands.
Sullivan's set and drawings belong or
should go with hi collection. You alone
have to say if I shall send it to Lindsey
who promise to return it or not. If not, what else to be done with Schrader?
At soon as I am ready to go to Cambridge,
I intend to pack all the books and collection
of s. which belong now to your herbarium
and see that all is right according to the
catalogue made by myself and Mr. Jos. Hilli.
want after the death of his brother. There
are some matter in regard to these collection which
should be fixed with you. I sent to Sullivan
good times he too came to take but we could not meet
ill yesterday the says he write you and sent the book.

Cambridge 30th August 92

Dear Prof. Gray,

I was so unwell yesterday that I was unable to walk to the gardens and see you again, which I much regret. I hope therefore to say good by by this note and to thank you for your obliging kindness and good help. — All is fixed with the prints here for the formatting of the proofs. — I have thought much about the possibility, of arranging Sullivant's collection and of preparing at the same time the materials for a synopsis of the M.S. moths. This work must be done and certainly if I can find time soon I will undertake it and try my hand at it. Sullivant has promised it in the herbarium and it would certainly be an easy task to put it in good shape for future American biologists. — When I have
seen Mr. Josph. Sullivan & will write you and tell you what he thinks of 
your arrangements which are certain for the best.

Please to remember me very respectfully to Mrs. Gray.

Your friend very truly,

I. S. Pierce.
Columbus, O. Sept. 7, 1874.

Dear Prof. Gray,

That package was put with
there of Mr. James by inadvertence. Please
send it and I will make the descript-
tions immediately and send them to you
for publication.

I have rec'd yesterday the 3'd vol. of Schimper's Handbutf Volu-
metrical vegetable. It contains, besides, a
supplement for species published since the
beginning of the work, the literature, an-
index of specific names, and another of
anonymous, with the notification of donors.

The work will greatly help palaeobotanists.

The last and perhaps the greatest error of Schimper
is his dedication of the work to: Bronn, men-
bere, Jepson, and Querrey, whom he calls
the grand benefactors of botany. The company is too high for me;
but it is too honourable to be declined.

Have you got this work? Will you have some...
remall on it for the Journal, and I shall report on it at 8 o'clock for the former roll.

Mr. Salviati is much pleased with the disposition of his brother's project. What you propose for the photograph is very good and acceptable to Mr. Sir. But I have good likeness of my lamented friend and will select the best to make in my upon them paper. As will the art of work which I am going to put out of the way before I may go to Cambridge for Salviati's collection there, I shall be a good deal of time, and as perhaps I may not live till then, I think that the following plans would be a good one to present, at least the mixing of specimen, manuscript, notes, sketches etc. That is to begin from the first leaf of the first package, and mark it No. 1 as above all, the specimen, note, etc. in the same leaf, and go through the whole herbarium, marking each leaf with a certain number.

2. 3. 4. 5. 6. 7. 8. 9. 10.
Columbus 0. 18 18. Sept. 18
My dear Prof. Gray,
You will find herewith pp. 111 to 118 of the supplement to the Journal. I read the proofs twice as carefully as I can; but I believe that you will still find some corrections to make. Please read the note in pencil 112 and cross it after, if all is right. This is also another 3 foot of C. 41. — I am indeed sorry that you should have so much trouble in this matter.

You will find too with this James pamphlet with Sullivan's annotation, and also letters of James, and of myself on this matter. If you read them you will see that James, instead of accusing me, only after my offer to submit the matter to your judgment and after he had read a copy of what Sullivan had written upon his Catalogue. The matter has somewhat vexed me because I never intended to wrong him, and to accuse him, as his name was not matter of slander of in any way and
and because he persisted to believe
and to say that I had wronged him.
My regard for him has ever been and is
still the same. — I should therefore
desire that you do not mention to
him the communication. Either keep
the pamphlet and letters separate in a
private drawer of yours or rather
return the whole to me to be preserved
in the package of papers to be burned
at my death, just as you like.

I have not yet seen California's
monsters. — Where could I get, if only
for a few days the hope of tasting on
that peculiar Melvinia, mentioned
by Capt. Dana as something analogous
to compass of the carboniferous? You
will much oblige me to let me know
where this description has been published.

My health is much better since I
returned home.

Your friend very respectfully,

In Lerouxen.
Dear Prof. Gray,

I send back to you galley proofs, 19-21, 27-29 sent by the printer. I have not yet received ff. 41-48 revised, which you mention in your note of the 22nd. Will write on that "D. Drummond" when I have seen the proofs. The remark of Silliman about D. Milioni is not that it is a new species, but that the species should have been named D. Milioni because Wilson first remarked that it was not a race of D. undulatum - at least if I remember aright.

I will also answer about those moths of Redland. To describe them correctly I should have to use the microscope and though my right eye about restores, he positively forbids me to use the microscope even for a few hours, or one hour a day. Why does not he, demand them? I will, if he does it, even all his descriptions. And even I will send him a number of your recent thousand moths, or the number which are not yet examined. But I could make a good paper on the whole. Very much yours,

S. Drayton
first recognized that it was a new species. I believe that this change was suggested by
Drummond; who by his discovery of the moss did more than the descriptive
author. But Melle should have remarked
on Wilson's suggestion. Suppose we count
the note by counting merely: That the Drum
mond's moss No. 86 was recognized as a
new species by Wilson in Hooker's Tour.
Bot. 3, p. 436, (1841) — Would that
not be satisfactory. If Sullivan had
positively recognized the propriety of naming
a species, or rather of giving a new name to
this species, he certainly would have done
it. — If you ever go through some packet of
Sulli's herbarium you will find nearly every
specimen and every species with some kind of
annotations; either as memorandum or as
future reference for publication. If even the
selections of name may not have been all right
Your very truly,
J. Lesguerre

Columbus, 0. 25th, Sept. 74

Dear Prof. Gray,

I return the 8 proof p. p. 41 to 48.

In regard to that note on Braunia, p. 41
— 48, As the preface of this Appendix (or
should say) that the work has been prepared
from notes and memoranda left by M. Sulli.
I suppose that it may be thus explained
without the final ( ). But if you think
that the remark is necessary, would it not
be better to put the paragraph and to add at
the end of it merely ( Sulli: men).

P. 46, you & tho., after a posteriori but
leave it in the second line after 'a priori.' Is it
right so?

About that remark not p. 48. the fact is:
Wilson first recognized that Drummond No. 86 was
a new species different from D. undulatum, but did
not name it. (Hooker Tour. Bot. 3. p. 436 (1841).) In
1849 Melle named it D. Drummondi and
1860, though the name, A. D. robustum (Bl.) in hal
a quin — Now Sullivan thinks that it would be right
to name it D. Wilsonii. Sullis became Wilson
Columbus, Dec. 18th 1871

My dear Dr. Gray,

Upon note of the 16th, was received in due time, but did not answer it immediately, wishing to acknowledge receipt of the copy of the last which has come just now. I am much obliged for the communication. As it is a personal matter, you will allow me to return stamps.

I have no opinion concerning the distribution of the Supplement. As contributors by material and worth, Schrader, Austin, James, Boulanger and Dikhu Hall are with both yourself and myself the only one, entitled to a free copy. I should however that one copy should be sent to Muhl, Schimper and Lindberg. Both Muhl and Schimper are preparing proposals where the Supplement should be quoted. Lindberg has helped Sullivan in some difficult cases of determination, osuthian and Turner's species, and is therefore a contributor. The other botanists who have the names will want the Supplement; but they may easily get it as you say by purchase.

I should be very pleased if you would
prepare that notice on Schumpe vegetable
Pakontolagie vegetable. Bailleau has no direct
correspondent in America; at least Schumpe's
copy, which I sold here (6) was sent only
Waterman. The best is to wish to Schumpe
and I will do it Sunday. He will certainly have
a free copy sent to you, I know. Should you
wish however to prepare the notice now, I
may send you a copy not yet reclaimed by
the owner of the 3rd and last volume. I could
not take my own copy, not even for one week,
but I may borrow one if you can not do with
the 3rd vol. and wish to see it now.

A request still, which your Kindness may
excuse. I have an album with photographs of
most of my botanist correspondents you; is not
there and I have desired it for a long time. Please
make me a gift of it and also, of that of Mr.
Gray if possible. O'Connell Fatherston, Brimer
Golden etc. etc. are there and of Lady. I have this
suffered the second, a good friend and correspondent old,
when I never see without pleasure and regret.

With kinder and respectful regards to Mr. Gray,
your old friend S. Degener
of Mobile. I suppose also that Sullivant sent it down to other European celebrated botanists: Braun etc., perhaps also to Wright, Chapman, Engleman, Porter, Tuckerman, Edwin Hall, and to some public libraries.

Columbus 30th Dec 44

Dear Prof. Gray,

My copy of the Sowerby's in black line-cloth as were the others which I saw at Sullivant. Mr. James' copy is bound in the same way, I think. And I believe that yours is the only one which was bound in calf. But of this I do not know. I prefer to have my own bound like the first one in black cloth. It is promised me that the printer would send me a full copy of the text in order that I might have a spare copy for a friend. Mr. Sullivant agreed that this copy of the text should be given me with the colored plates used for description. If you can obtain it from the printer and send it, I will have it bound in the same as is the first vol. of the Sowerby, and keep it for me. I shall like it as well as a new one, as a token of my work for you. And if I get another copy it will be good for somebody else. Mr. Sullivant wanted me to accept a calf-bound copy. That would not agree with the rest, and I refused. I put it to left of the nice bound copies, and as I believe, yours only was extra-bound. The best way therefore...
seem to have all the copies bound in. I have very few if such a '1st. Tol.' given out for sale, or in blocks, even for the copies which are to be presented. But of this matter I remind less than yourself. Of course your copy must be bound like the 2nd. vol.

M. Sullivan wishes me to give you a list of the names to whom his brother would have presented a copy of the third, if he had been alive. I write here the few names ofobotanists who have an interest in the most and new friends of our lamented friend. Perhaps you are better informed on this subject by personal communication made for M. Sullivan, just now.

Thanks for your kind wishes. Please accept my own for your and Mrs. Gray's health and happiness.

Very truly yours,

A. Securesy

That dear Bolander sent me for Christmas a fine specimen from his new garden. Time flowers indeed. It came per mail and the flowers were tolerably fresh on arrival.
The third and last volume of W. J. Schimper's Vegetable Paleontology (Traité de Paléontologie Végétale ou la flore de monde primitif etc.) is out. The outlines of this remarkable work, as exposed in the prospectus of the 3rd volume, have been mentioned in this Journal September 1869. An exception to the general case, the third volume gives far more than promised. It has one hundred and ten plates (figs.) illustrating most of the genera, instead of one hundred and both the second and third volumes have each two hundred pages of printing matter, more than the first which was already of large size (730 pages). The third volume contains, besides a supplementary addition of the species discovered and described since the beginning of the world in 1869, clear and important remarks on the distribution of the fossil forms in regard to their geological relations and successions, with tables of the characteristic species of each geological group. It has also a full exposition of the literature on vegetable paleontology or an enumeration of all the works published on the subject. The amount of materials presented in Schimper's Traité ou Synopsis is indeed prodigious. The number of species, described in the nearly six thousand, distributed in eight hundred and fifty genera which are listed, the species clearly and the characteristic matter included upon their limitation, relation etc. As all the American species of fossil plants published before the completion of this work have been carefully recorded.
in it and as their number is already large, this book
will be as acceptable and useful to Americans as to
European naturalists interested in geological and
palaeontological studies.

Perhaps you may find convenient to say something
now . . . Schimper dedicates his book to St. Brongniart of Paris,
O. H. of Zurich, Count de Rapporte of Aix, France and
Leo Seuverine of Columbus, whom he calls his dearest
friends and the great promoters of vegetable palaeontology.

Schimper worked zealously at Strasbourg during the
stage when constantly under the fire of the Prussians and
menaced of the total destruction of his property, his
library, even of the collections which were used for
his description. Some errors of the first volume
have been reproached to him (especially by Dawson).
They have been fully explained in the last volume.
As soon as ready with the fossil plants, Schimper
has begun a second edition of his Synopsis, Museo-v
Sino, which you well know . . . I suppose that
you have had more than enough for a week not
on a scientific work which is certainly one of the
most valuable and also interesting to
the beauties of style and expression.
Please say nothing of the dedication if you do not
think it honourable to American science. My name is of no matter in it.
Columbus, O. 31. Dec. 1874

Dear Prof. Gray,

In one of my former letters I proposed to Prof. Dana to send a short notice of the Travels of Schimper's world. As he did not answer to this, I supposed that the offer was not acceptable. It would be far more pleasant to Schimper and more valuable for the book if you should write this notice than if I had to do it, your being our highest scientific authority.

In the first page with this, I have written about what I thought should be said of the Traité de Paléontologie. For myself the book is a true encyclopedia, and I do not know how I could go along without it. But of course it can not be appreciated here for a time. I believe however that it will go with Picot's work into all the scientific libraries of this country. Please write some notice on it and take from the material here with any thing convenient to you though little they may be. I have just received your card and I have no time to do better. Please have a copy of your notice sent to me in order that I may send it to Schimper. Very truly,

[Signature]

Palliser should send you a copy of the work for your notice. I will mention it to Schimper.
Columbus, O. Jan. 23rd, 75.

My dear Prof. Gray.

Your kind note and your beautiful card are received. I am indeed grateful for this communication of yours, and proud to have such a splendid representation of yours in my album. Sorry that my perhaps indiscreet demand could not be complied with by Prof. Gray. But I still hope for a more friendly decision. As you wish it, I send my own picture at New York about ten years ago. I cannot get any better now, but will send you another as soon as I have courage enough to let beard grow.

If you wish it I will make you a detailed account of the conduct of Ichimpe in north from the beginning to the end and with this, you may easily write another notice of the book. Please let me know. The 'Fugitive from Slavery' will sell well, I am certain. Already many have applied to me to know where to get it.

Very truly and thankfully yours,

[Signature]
My dear Prof. Gray.

Mr. Sullivant says that he will write to you about the number of copies which they want here. — Indeed I am sorry that you returned sick from Washington. I would say take good care of yourself if you had not Mr. Gray to attend to you, better than you can ever do yourself. To your blind lady I am greatly indebted for a communication which though much desired was not expected. I prize the gift very high. The likeness is perfect, and very good, though not quite as good as the original. For the good reason that the expression, the look in the eyes cannot be photographed. Please to give Mr. Gray my very warm thanks and respect.

Very truly yours,

V. Lesquerre.

I wrote to shun an one meet eye or man sent you also letters a letter to Mr. Sullivant and one from her too.
Columbus O. 7th March 75

My dear Sir:

Indeed I do not know what to say about the communication (present) of the supplement of the "Specimens." On my first list you remarked, with right, I think, that the supplement could be bought by those who had got the 1st vol., in order to get at least the expenses of printing, binding et c. Tho' admitted I do not see that the book don't be presented to others, but to those who have contributed to it by communication. Sours, Austin, Elkin Hall, Bolander. After these there are still Bremel, Breuer, Delt, Porter, Bow, Watson, Hohn, and Monroe, who have sent specimens for examination and are mentioned as discoverers in the vol. But the publication of the discoveries is, I think, compensation enough and of the supplement distributed to all they desire it will be a long time till the expenses are paid. At the sale of foreign, I have named Schumpe, Lindberg, Carl Muller, and Hansen with the great botanist Boothe, De Candolle, then A. Braun.
etc. etc. With these you are better acquainted than I am. I believe that Mr. J. Sullivan would wish to distribute the book to many of his friends and acquaintances. If it is the case, I will give him a list with details or rather return it to you if you take the trouble of forwarding the purchase. With all what you have done already in regard to this matter, it would be too much for me to try and see Mr. Sullivan and you. But this, as well as well, at least he was, the last time I saw him.

I was very anxious about yourself fearing the result of that bad cold and was happy to hear you. Please note that you are yourself again. James wrote me that he had got the subject but said nothing about yourself and nothing about that subject. Which however gives him more credit than to any other contributor.

I got your mail yesterday the 3rd of the State flora of Texas. Did you not get from them the two first volumes? If you wish to notice the work in Allman’s I will send you my copy as a present from them and you may give me yours when you get it or if you do not get it I will have another from the author’s book seller. For, indeed, there are many things while I have much to himself and especially to his works. Did you not get a copy of the Cretaceous flora of the Dakota group? The comparison of these two floras, of the same age is extremely interesting. The American Cretaceous is however more recent, at least in regard to its vegetable types, most of all representing obiect, leonrous forms. I may write something myself on the subject. Any how both these monographs of their and of myself are most valuable as the first important contribution to the Cretaceous flora, totally unknown as yet. Do not refuse to accept my offer if you like it in the book. I can not yet the world for a while being deep in the preparative of the Cretaceous flora and other matters which will take all my time for one year at least.

Very truly yours,

A. Cockeram

Postscript: I am going to prepare a short diagnosis for each species the flora is in German. I do not doubt if you read it truly
Columbus, 28 May 75

Dear Prof. Gray,

Allow me first to congratulate you on your safe return to Cambridge and especially on the improvement of your and Mrs. Gray’s health. May you both be preserved perfectly well for a long time to come. Then I want to thank you for the communication of your pamphlet (Cycads of the North American Hydrophyllaceae) which has interested me much and to thank also Prof. Watson for his revision of the Ceanothus. It is the more valuable to me that Watson has a comparative, large number of species referable to the genus. In offering my grateful acknowledgement of his pamphlet to Prof. Watson, please to say to him that I should be much obliged for one or two long of Ceanothus integerrimus.

And now to Scharpein Paleontology. He wrote me on that account especially, B. May
I translate what he says:

Speaking about a notice in my book, I beg you would not in any way trouble yourself with Gray about it. The editor is a man despicable (unpraisable) on whose interest nothing should be done. I have broken every kind of relation with him. The capacity is such that he certainly would not give out one copy to have the work advertised or reprinted by Prof. Gray. He has paid me (f 3000) three thousand francs for that enormous work for which I have worked 5 years and spent two thousand francs to get the literature and the necessary specimens and when I have asked him to replace another copy of the 1st vol. which had been spoiled by my two heir and constant work upon it, he has answered me that if I wanted one, I should have to buy the whole work. And this, though, I know by the prints that he has had 100 copies printed more than it was agreed between us. Therefore do nothing, absolutely nothing for helping the business of that miserable Wallace.

From this, of course, you will see that having bought a copy you need not trouble you about the book. If you think differently I will write you a memorandum on the work. I like it more and more. It is a whole encyclopedia on Vegetable paleontology. I could do nothing without it. Saporta is enthusiastic about the Cretaceous flora of ours. He urges me to push the work forward as far as and as fast as possible. He says rightly, that it is here that we may expect to find the origin of the first dicotyledonous forms and that no problem on the Vegetation is more weighty now than this.

New and remarkable material have been obtained since the work is out.

Your truly yours,

J. Le gueney
Columbus, Oct. 29, 1875.

My dear Prof. Gray,

I am to day able to answer you. I note the regard to the Stones (happily) and the apparent neglect of reference by the friends of McClintick. In his last letter he received two messageShipsey says that he found at Thirleby, in returning from Interlochen where he had spent part of the summer the rod of the Stones; that he had such a heap of matters to look at after his absence that he could not examine it carefully now, but that he found it to be an admirable complement of an admirable model etc. Answering him on Sunday next after receipt of your note, I mentioned to him that he should give an account of it in some scientific paper and this very soon. Last week also I had a letter from Hampse 'who says that he has received the imprint of the Stones and he written his thanks to the
To my dear servant family. — To-day I have a letter from Brandenburg who says: (copy)

"My best thanks for you kind note of last August and especially for Sept. to the Congress museum which I received a few days ago. It is indeed a most useful work, one of the best published in England.

After this he answers my letter which was on account of the determination of those Venezuela moneys of Schröder sent to him more than one year ago, and which he refused to work for want of time. I therefore conclude that he has not had time yet to examine carefully the paper and to notice it somewhere; or perhaps he has noticed it but we have not as yet received the proofs.

I shall answer his letter on Sunday and will write to Muller at the same time. This last copy is also overworked, as we are all, and in poor health. Be sure that I shall not let the matter drop.

It appears that the copy of the paper has been sent by some how or another, perhaps to Smithsonian, as it has been received there.

Very truly yours,

A. D. quarries

But I am glad to hear that I am the same and that the majority has been written very well indeed.
Columbus 0: Jan. 1844.

My dear Prof. Gray.

I return, an answer to your letter which is
an account of the Synopsis perfectly satis-
factory. We will therefore go ahead.

Do you object to describing the whole
flora of the N. F. continent from
Alaska to Mexico? We have now
so many species, from the Rocky
Mts, that we must forcibly consider
Drummond’s more, and telling
Alaska in the area (botanical) we
have admitted the Canadas. These
addition will not make a great deal
of difference in the number of your, which
is large indeed. I am working now
the list and distribution. When ready
we will if you please confer about the
mode, especially length of descriptions.

Schimper sends me the 12th part
of his Syn. 2Ed. a splendid work.
He is through, but sends all what is
printed. Balance will come soon.
He writes me that he has now ended his herbarial career. He can not continue microscopical work. He says that he wishes to dispose of all the surplus of his specimens. He has bought lately Breuil's collection, which had been kept by the family for years and he proposes to unit both his collection and his own, and thus to have all the typical specimens of the踔ὀγον and of the Euphorbiaceae for distribution. He will divide them in about 10 sets, each of about 750 species, represented in each set by more than 5000 specimens, and offer them for sale at two hundred francs, each. This is very cheap, and I shall try to have some of these sets in this country. You may perhaps help me in that purpose. I will write you more about when the matter is fixed.

Have you in the herbarium Aristolochia peltata and if yes could you lend me one leaf for one day.

Faithfully yours,

A. Anguerenci

Suppose I should ask chimps. For one species of the same described with new ed. of the genus, and not in the first 2 add to

fultur. herbarium, would your Museum pay it. That would be perhaps new species alone. Any how would ask.
Columbus. May 28th 1846

Dear Prof. Gray,

Obeyed for your kind note. I send to Justin Linderby's letter. I do not know what he will do. Any how he will answer me. And he is engaged in the preparation of a Synopsis on History of the Hepaticae and I believe that Justin corresponds with him. — Be sure that I shall consider your note about Sullivan's herbarium and have done so already for a long time past. If I can find time I shall do as you wish. But as you say I am becoming old and the world to be done still is heaped around like a wall. You know that by yourself. — The death of Bronnswart is the loss of a great scientific monument. The left rather belongs to the past than to the present.

Most truly yours,

I. T. Speranza

Did I should you for that good sketch of an

At a meeting of the American Botanical Society, Dr. T. Speranza delivered a paper on the Botany of the United States. In the course of his remarks, he referred to the recent discovery of a new species of fern, which he named after Dr. T. Speranza. The fern was found growing in the state of New York, and was characterized by its small, delicate leaves and slender stems. Dr. Speranza expressed the hope that this new species would be of interest to botanists and would contribute to the knowledge of the botany of the region.
Columbus, 9th March 76.

Dear Prof. Gray,

I have this week finished my annual report for Hayden and the prints of my specimens. I can not think of expediting your note just received. If you want more than I say here, you please inform and I will answer in the longest next week.

I have already two fine species of Sequania in the Ordovician (South Kansas, Clay C.). One Sequania fastigata Here is an old species, derived already from Oceana. Animal and tail by then for Greenland. The other is a new genus Sequania consisting of which I have branched arms and trunks. It is a Kansas mixed with a new Sequania fastigata from the Molton, Kansas showing what is of the generic and the section of the Sequanidae, now widely represented in Mexico. I say Sequeia. Here is the Ordovician, or jointed the top and at the base of the Aegypress, we have in Wyoming, but to this very fine species Sequania fastigata from the section by splendid specimens and known from Southberg and the Baltic teres. I trust you have,
and it informs us. These are some feet lower than Black Butte and found in connection with the sub-tropical flora of the lignite. Lizard grasses from species of Trias, e.gramineae, Trachylepis, Ophiothrix, Oxytoma, etc. Of the above species of squaw root 1. longifolia is found also at Black Butte and in latitude with another species a Namute, of the same type and closely allied form.

Of the same Tertiary group we have still 1. Gigas of 1. Langsdorfi found at Vancouver and in the "

Now you know a mean flora of Squaw of the 3d group equivalent of the middle

Museum of Squaw. We have a squaw tree and of the same type as 1. Langsdorfi. It is found at Carbon about 112. Mi. north with Trifolium dubium. In the 1st group, upper Tertiary of the Carbon, 1. Langsdorfi is found with a splendid new species of Squaw, in the best possible specimen with aura 3. officinarum closely allied to 1. ordinary of the Berry Family and still another a yens known by aura and therefore undescribed.

The flora of the upper group is quite temperate. The Caills, South East especially where it is more abundant is at the latitudes of Denver. 1. affinis or of the type color qui't near 1. gigas, while 1. affinis and Squaw form of the Redwood type. The cretaceous squaw tree falls of Rannow is also of the 1. gigas type.

Most help you,
A. Langnesey
Columbus, 0.12 1st March 76

Dear Prof. Gray,

Having seen my notes of the February flora of California and Oregon from Prof. Whitney’s specimen, I see Sequoia angustifolia derived from Coral Hollow, San Joaquin California. It is there, with Platanus, a fine species not yet published, two species of Ulmus, two species of Sequoia, a Pinus & Salix. And from Bridge Creek, Oregon I have from specimen of Crop Whitney also: Sequoia sempervirens together with Quercus juniperina, species of Ficus, Ulmus, Cinnamomum (middle Miocene) San Joaquin, O’Cal as represented by local hollow in upper Miocene.

Yours sincerely
A. de Quevene
Dear Sir,

C. Muller writes that he has not got those drawings of the Venezuela mosses. Did you send them by the Smithsonian with that set of plants? Mr. Schrader here who corresponds with Muller says, that if you will send him the set with the drawings he will forward it immediately. Perhaps that would be the best way as the Smithsonian is very slow. Please let me know.

How many species do you know from Cuba? Where could I see some good enumeration or description of the species of the genera?—Going back Monday next. Don't think if I shall reach Cambridge. Would be most pleasant to see you. Time letter from Bolander just received. Ever yours, J. Enzioue.
WRITE THE ADDRESS ON THIS SIDE—THE MESSAGE ON THE OTHER

Prof A. G. Gray
Cambridge, Mass
My dear Prof. Gray.

I am now closing the remarks on the flora of the Chalk Bluff, Cal., for Prof. Whitney. The small flora, so you only will interest you very much, as it represents many or at least a number of the Genera which are of the Calo flora, now predominant in that of the Eastern slope. With one Sabal, other Frague for. - regina; 2 species of Guern of the Kerm. type of that of G. castanea and G. apatie. Castor Liquidambar, Ulmus, two species, Ferra, Magnolia, an area of the spicatum section, Ulmus lyphena P. A. melognum with four species of Staphis. etc. This remarkably confirms your remarks on the Memoir of the Japan flora p. 440.

In order to supply to the defects of

Columbus, 0. 8th July 77.

Your friend sincerely,

A. Ruprecht.

Late but sincere wishes for a happy year for yourself and Mrs. Gray, to whom please offer my most respectful regards.
materials for comparison, I have sent the plate to Dejounte with my determinations, and therefore, these are more reliable in trustworthily though most of all these determinations have been confirmed by him.

I shall want to compare a few species with living plants. Could you not lend me for one day only the following ones:

1. Aquidambar species of Chine. I have one with small trifoliated leaves dentate, on the border which says Dejounte in relation to Japanese peas (which?) I rather suppose it to be a form of Acer, but I have found also a similar large leaf with snow-white flowers, which resemble in shape, size and stature of its leaves, but the flowers are not submitted. It is the same in E. controversum, but it would be well to compare, also G. maculata, Hummelboothia, etc. and the Mexican type. I have a few detached leaves which you can send, but not named. Nutrition, Japanica, C. Canina, A. capricornis, of India.

If you send the whole plants please do it per express at my cost. The pecuniary bill between boards will be safe. If not a few leaves, one of each species might be sufficient.

Should you like to send the conclusion of the flax cut 10 whole, about 70 have mis. I am sure Prof. Whitney would not object to the communication. And I should be extremely pleased to have remarks and corrections from you.

The fossil flora of the bottom of the rocky mountain is far advanced to the conclusion. But I must if possible
Columbus, Oct. 1st, 1798.

Dear Sir, I pray:

Many thanks for your kind note and the good wishes of everyone which I reciprocated most heartily. May you have for the future long years of plenty for yourself and mind body heart besides health and every other good thing of this world and for the result of a world which has done so much for the advancement of our beloved science.

If I may to write you as often as I should wish, as often as I come to you in thoughts and grateful remembrance you would be bored by my letters at least every week if not every day. But I have the experience of the worthier and well know how little time you have even for reading friends.

P.S. Write still less for our sake.

The Tertiary flora is soon ready. After your return from the Rocky mountain and the remark of Dr. Whitney that my conclusions should perhaps be more specifically treated I took the subject again and though these conclusions are not modified in any way they are a little more definitely explained. With the Tertiary flora also finished and submitted only by the printer wait...
fe Congress the first week will give a good account of the distribution of our vegetation, at least I think so. If you read at least the generalities of these two memoirs, please to judge them hence. You know by what disadvantages my work is encumbered.

Please to offer to send Mr. Gray my highest regards and respectful compliments.

Your old friend

A. Engelmann

I forgot the essential answer to make to you both. I saw Schröder sometime ago and he asked me if the Kargueli move had been sent to Mutter. I did not know. I see them about every week; as soon as I have an answer to your question I will let you know.

My health is becoming very poor and troubles much.
The Miocene Flora of barlow is very closely allied to that of Oregon, Alaska, Greenland, the Baltic, Germany etc. but as yet its type are not clearly defined in other groups of the Lignitic of the Rocky mountains while a number of them are in the Pliocene of California. We know as yet too little of our Fossil Plants and it future will show a great deal more for the field is immensely rich.

Should you want any kind of information on any part of Haydens and Whitneys plants I would answer immediately though I cannot write much myself now.

You advised our friend James for the best. If he can fix the determination of the few doubtful species of our moss the synopsis can be made here easily.

Sincerely your friend

Columbus. March 9, 1828.

Dear Prof.

You have by this time received a copy of the Pleistocene Flora of the auriferous deposits of California. Perhaps you may like to write the journal a short review of this and of the tertiary Flora and thus take the liberty to make a few remarks which may be used as you like.

These three Floras may be considered as a same work and explain one another. The first exposes the origin of some of the predominant types of our present vegetation. In the second which especially expose the tertiary character of the Flora, these types becomes more and more distinct and numerous. Their relation to those of the Eocene and
of the Neviocene of Europe are of course especially considered as proof of the tertiery character of the formation. The third as Pliocene especially considers the relation of types to those of our time. If you can read the conclusion of this last part, you will easily understand the whole.

The plan of the work was not fixed from the beginning for I had to determine the materials by series as fast as they were sent to me. I then considered as extraordinary the peculiar development of the dicotyledons in the bretaceous which I remarked as a proof of disruption from those of the Jurassic. This idea was confirmed by the preponderance of palms in the lower Eocene while there is none in the bretaceous. But I found later, types of bretaceous conifers and dicotyledonous in the lower tertiery strata of Point of Rocks and after a while I was forced to abandon preconceived ideas and to do as De Candolle did for his Geography, to go along recording facts and leaving them speak for themselves.

Besides some very remarkable types described from the different stages or groups of plants, that of Selaginella in the lower Eocene for example there are some remarkable cases of distribution of species. For example Point of Rocks is separated from the upper strata of the Dakota group and yet a single species has been found indetical between these two formation, while the Flora of Point of Rocks is about the same as that of Black Buttes which is 3000 feet higher in the measures.
Columbus 6 April 28th 78

My dear Dr. Gray,

Being a little better to-day, I am able to look a little more closely about that description of Ficus spectabilis and to write a few words completing my letter of yesterday. The mass, as you will see on the page herewith (which please return) has broadly ovate narrowed upwards to an acumens. This in looking to fig. 4 of II or is not correct for the leaf is rather lanceolate acuminate, and on this I always upon the galleys proof to the sentence as it is rightly printed in the flora. Therefore the error of of mine not on the print.

I am however not certain that for mine leaves as are our so called speceus this description is wrong and. I may even be matter for these leaves, one ovate, the other lanceolate the third nearly linear.

Leaves ovate, lanceolate, linear, in the area of course to the leaves figured as are those of this species. For description
made from whole plants, as in the botany of living species, this would be more, but I still question if it is so for leaves separated, and whose general character can not be ascertained from the specimens. The description of this species in Annual Report 1872 p. 379. leaves is broader, more acute, lamellate, contracted to a short point, etc., which is not right for the contraction is not evidently marked, and some leaves are narrower than assume rather long than short.

But this will not alone for deficiency in the description, no more than the difficulty of my correction of the proofs in revising around a fire, and I am going to say that Dr. Hooker will find many other things to complain of in this work. Please to say to him to put all the corrected in my account and not in that from vegetable paleontology which is the history of the vegetation of the world in a series of hieroglyphic figures, whose deciphering is nicely at the beginning. The first attempts in the reading is very unsatisfactory other.

will do better hereafter.

I must go to Kew to see my doctor and perhaps will stay out a few weeks to get rid of the intermittent fever my health. I should like to go to Cambridge and see you to have a talk about more. James Austria and other matters. But I do not know if I shall be able to go so far.

Give my best wish to Dr. Hooker and my thanks for any of his remarks.

Yours Selfs

X. L., Asquerry
Columbus 0 Nov 5, 1847

Dear Prof. Gray,

I am now closing the descriptions of the species of the Coal flora by the examination of the Cordaites and the fruits. We have on these plants splendid material, which may perhaps show their relation with some more evidence than it has been done before, even by Grand'Pre. But I am greatly in need of material for comparison. Have you perhaps on hand cones or fruit of Gondwan and Samnia. I have a very fine cone of Eucus and the skull (separated) of one or two species but none of Samnia. Where could I find good specimens of these two families. Of the Conifers I should like to see a fruit (ipe) of Podocarpus. Where to find a good description of Torreya Californica. Botan de sent me long time ago in a bottle a large fruit labelled Torreya Californica,pecuse fleshy, the fruit large than an almond in distinctly corrugated lengthwise. Is that Torreya Californica? Please excuse
me for troubling you with questions. I have now two species of lory fruits, one Pandanopsis of Cordate Contour, another C. Mansfieldii, same as that of the King of the Coal flora. Fig. 21. P. XXXXVII, only slightly smaller, both attached to a branch or to their supports. That if the plate is over turned, the notch at the top is the point of attachment. I have also the flowers of two or three species attached to stem bearing leaves and many other new and interesting materials refer to this family.

My health is very indifferent now, I am overworked perhaps. But the Coal flora is now ready and I shall then come to move on and give my whole time to the preparation of the Manual. Be all pleased to the kind remembrance of Mr Gray. I hope that you are both well. Respectfully yours,

C. Ingenious

Please send me express any materials you may send and send at my expense.
Columbus 0 May 10th 80.

Dear Prof. Gray,

Schiaperi's son, already a good botanist, has especially studied physiology and anatomy (vegetable) and says that if he had some prospect to find here a position as professor he should like to come and settle in America in a year or two when his father's business is arranged. Don't you think that a good physiologist would be a valuable acquisition for botany here.

Should I not encourage him in his purpose? He is not without and would live some time on his own small fortune. Please tell me what you think of the matter.

From informations received long time ago the young man (24 years) is very clever, modest, ardent worker, a true student.

Now my health is much better, indeed it seems that I have got a new base of life. I am now working hard closing the text of the coal flora, clearing field around me in order to be able to talk to the genius of...
the more, without hindrance of any kind.
Once at that work, I shall not have it ended done, if my health continue good. Jane
will admirably well in preparing the
way by reviewing all uncertain speeches
and procuring documents.
Please to offer kindest respects to Mrs
Gray who, I hope, is still you in good
health.
Always and respectfully yours
Sincerely yours
T. Lequerrey
Columbus 6th Feb 172.

Dear Sir G. Gray,
your kind part of letters was delayed
and about the same day, I suppose, as
you have received it. I have partly answered
what you ask me about money. The work
is too far advanced to abandon it and as
the more difficult species, those of Audubon
the Spurning of Mowen, have been discussed
by B. Gray, the way of clear enough.
I will send all my time free. I should be
from ready. But I must live and as my
only resource is that afforded to me by pelican
myself, I have always a part of some sort
of preparation and can not abandon them
in even. Since I am paid for them.

If you have a chance to write, when
M. James family is quieted after the sorrow
of the loss, you would oblige me to ask
M. James for if I can keep your while long
the two books I have belonging to his father.
They are: "Biology" Britannica J. Millon
There are still two small pamphlets of Jage, which do not belong to St James but which we sent to him. I can return them at any time. Also it would be important to find my manuscript of the Ephraim which I sent to St James and of which I do not have any copy. It is to be merely reviewed like all the others of my dissertations, and it is myself who have to do this kind of work. St James could not attend to it except for the change of expression or things I intend to shorten the dissertations very much. As I told you in my former note, the work must be published in both names and be the property of both St James and Myself. Perhaps you may learn what will become of the James collection of more. It is valuable and should go to Harvard and enter into that? Jullondart. Of course I shall greatly need the examination of some notes and specimens of Jullondart. But I have 5 volumes of notes written when we were working both together and most of the important remaining Jullondart one quotes there.

I know that you are over-crowded by work and can not find time to answer my letter. If however you can help me by advising on any suggestion you will, I am now, for a moment to write to me about.

It is a pity that Austin would not sleep this engagement and work the Hepatica. This post is essential with the Musc. I see in the Botanical Gazette a bit of Hepatica (with茎) by Austin M. Underwood of Bloomington. Perhaps he would, we could undertake the description of them. Do you know him? He wrote me sometimes. Though I gave to Austin all I had on Hepatica, Booth and specimen, I had only some essential worth, Synopsis of Nemat. and could perhaps review the matter.

I am truly happy to know that Mr. Jago's health little grew. He is quite content. May this blessing be continued to both for always him. My health is also good enough now. But the information of old age can not be spared. I am sends two years younger than St James was. Very respectfully your old friend

Linguist
Columbus 0, 15th April 89

Dear Prof. Gray,

Could you, without too much trouble, find among your pamphlets, Amselberg's Manipules Mus., second ed. 1874. He sent me a copy through Sullivan, but it has been probably lost, for I never received it. He wrote to me about it and said he was sorry he could not lend another, but that you or Sullivan could lend it to me. James has seen it at your. It has a remark on Thucydides which I greatly want to see. If you can not find it without taking too much of your time, please let me know and I will write Sullivan for that note.

I hope that this will find you in good health.

Your friend respectfully,

A. B. Cushing.

What will become of James. Collection and books. He had offered to lend me some of them. I may need the Epiphaneas of Brautvall but not now.
Columbus 0 Sept 20 82

Dear Prof. Gray,

I hope that you have enjoyed a good time of
rest and are now returned fortified and refresh-
to your work. May this letter find you all well
with Mr. Gray, to whom I send my respectful
regard.

I have finished the descriptive part of the
Synopsis of the American moths, about two weeks ago
and already prepared a short introduction. When I
have made a third of the work it would be really
more useful than a conjecture, I will begin
the copy with abbreviations and contents of the
Manuscript and hope that the work will then
go fast and he easy. For species of Hydrops which
were uncertain and had not been examined
by James, I have found a valuable assistance
in Buxo. Cap. Renault a botanist of France
who has 40 years studied the most difficult part
of the Hydrops, the Inferius Harpactium, has
already reviewed most of new species which
are still undetermined or not satisfactorily
analyzed and is still continuing the work.
I believe that there will be left
I should now like very much if Mr. James could carefully look over his herbarium materials and find my manuscript description of the Sphagna. At least, I want to know if it is definitively bad, for then I will at once make the world again. If I do not mistake, few names desired to have it in order to show you if the description were not too long. I shall certainly abridge a great deal. But it is very difficult to make short descriptions giving a diagnosis sufficiently clear. And please, as well you know, I think that is more difficult for more than for any other kind of vegetables. With the plates for the generic, would it not be advisable to add to the monograph the description of the Hepaticae as made by Sellier. That is not much perhaps, but that is some thing, and certainly a great deal of help for beginners. Perhaps the species described as new by Austin could be added. But he has made matters very much as well in the Hepaticae as in the mosses, and it would be difficult to find the right place at last for his new genera.

I know that you are always overcrowded with work, and for this reason I have not written to you before and will not now ask your advice on different matters concerning the description of the monograph; I shall be especially embarrassed for the genus. As I have done it, it may be too short, for I generally give the first name or at least the name generally admitted by authors really the first author’s name and also the one by whom the species has been illustrated, such as the Polyd, Su. and the thones. — How much I need an accurate herbarium for his motto and James’s too for his specimens which I could read to Europe for revision. Justinus Merc. Apud. would be very acceptable and perhaps Mr. James would lend it to me, as I could take from it parallel passages for those new species which he has described and which still puzzle me some. What will become of James’s more? I hope to get some news of Mr. James, and will ask him for what I may need.

Your old friend respectfully,

[Handwritten signature]
examine it before the first of June next year. Dr. E. is the person who has been in charge of the matter. We have some letters from him to the effect that he has been engaged in preparing a paper for publication. He has some new ideas which he wishes to publish as soon as possible. He says that he is working on a new method of determining the value of certain chemical compounds. He has some interesting results which he will publish in the near future.

Columbus, O 18th Sept. 88

Dear Prof Gray,

I tried with my son to dig a hole in the earth as deep as we could. We dug a hole about 1 foot deep and of course my son who is a business man and knows nothing about digging in the earth did not do much. There is no people but the postman who could undertake and undertake the job. There is only a tower for people who go there for fishing and hunting and as there is Sunday during the summer, an excursion train going there only at a cheap price, I have been there sometimes for a day of relaxation. Yesterday was the last Sunday these excursion trains going there only at a cheap price. You badly need those costs and with me to try again. I would have to take a man...
there from Columba, and the cost with expense of transportation could not be less than $10. This with the uncertainty of success and the loss of my time would I think be too much. The landlord of the tavern said to me now that he could not succeed to reproduce the Heliotrope from store or roots, but that in flowering seeds it sprang, just after the ice was disappeared, he had some around him and near the borders small patches, where he could every year get flowers for people who come after them. The nuts top of the bolt, and still green, are already ripe. It is in that degree of ripeness that they are good for the Market Basket which above them. Of most of the seeds are still unripe, I can obtain some more later. At least the one promised to gather them for me.

I am often somewhat embarrassed for details of little importance in the preparation of the Synopsis of the American Mones. This is especially the case for synonymy. Following Humboldt's method, I write with the specific name, the name of the first author who has examined and determined the species, and in the synonymy the name of the last or admitted author of the genus. Thus:

*Micromenthes* synonymy, James.
and after the destruction:
*Pachyurus* synonymy Jones, From. Smith.

*Micromenthes* synonymy Austin... Mus. Spike, I think this is right — about synonymy of different authors merely indicating changes of names, I do not indicate many of these different changes generally mentioning the names and author when the species is illi

hated. Perhaps it would be better to send
you a few pages of my manuscript and you would
if time permit; I to you have the kindness to suggest any changes to be made. I have
of course a systematic compass of the characters and distribution of the genus, instead of perusing
as a key. This really but no copied yet as I feel
definitely only after the description of each genus
and their species, in order to make it clear and
to say nothing useless. Well I am doing my
best and find helping friends in Europe for
the most difficult part, which have not been
Dear Prof. Gray,

I am sorry for troubling you once more about that synonymy. I have, of course, that leaf which you had printed for direction to Friend James. But just in that, we do not agree. I give with the name of the species the name of the author who described first the species. Thus you write:

*Micromitrium syneicum Auct.*

After description the synonyms and references are

*M. Syneicum* Auct. *Mus. Apollo.* etc.

*Gomphocarum syneicum James,* etc.

*Vannomitrium syneicum* Lindb.

While I should like to write and have written

*Micromitrium syneicum James,*

and after description etc. for *syneicum* I refuse.

*Gomphocarum syneicum, James,* etc.

*Micromitrium syneicum* Auct.

*Vannomitrium Lindb.*

Which is right? In order to spare your time please erase the one which is wrong. But indeed, I do not see any reason to change my mode of quotation. Also I dont
mention any synonym of genus as most of the important two centuries after name of genus are used as synonyms with species. The nomenclature of genus, it is especially for the more a matter of fact, and one who did, to have the name published as part that subdivision of genus as in section as well as the substitution of new names. I therefore do not see why one should give the first place to the author of a genus. For example, the genus Micromitrium is separate from Ephememerum on very little ground; only the calyptra, very small in the first, slightly longer and compressed in the second. Then, and Truch, Mullo, Sullivant and all the other did not make a genus to separate Ephememerum Tenerum. That was received by Austin and after him by Lindley who put his Nasionitrium instead. If there is not any protective rule forcing the nomenclature as it is made in your pages instead of that I have used, do not trouble you to answer to your old friend.

I sincerely

That kind good J. Hall is gone. He was one of the best collectors of Botanical specimens as well as himself.
Columbus 0 March 3°, 83

Dear Prof Gray,

At least, I have finished the rest of the Synopsis of the North American moss. That is a good title, I think, the whole describing species of mosses whose habitat ranges from Florida and South California as far North as the Arctic zone, even Greenland. — I have now to review my table of classification or key which is about half that of Prof Watson in Bot of the California Survey, but of course of far wider extension and then I must read again the whole to uniformly arrange and number the pages, the divisions, the species, etc. There will be also some change to be made in the references, or the synonymy and I have to wait news from Lindberg and Muller to whom I have written on a few of their species which are yet unpublished. — I have in the meantime separated as genera all the subdivisions of the genus Hypnum, established by Schimp and Schimper. Schimper admits most of them, as genera, Schimper has admitted them also in
his supplement to the Icuses. Perhaps it would be best to consider them as subgenera, as I have done in Menis of the U. S. and N. W. T. of California. The present tendency is to subdivide indefinitely, as you have seen in Braithwaite's and others. But as all the works on American mones are made according to the system of Icuses, I follow them, as did also Lellwitz. But in this, I am still undecided, either to make or admit the subdivision of Hypnum as genus or groups.

What do you say about Bryoflamellae. Is not such an arrangement by name somewhat exceptional and subject to criticism? I have made a new genus a subdivision of Hypnum (Cladopodium) for your intermediate Taxodium and Sylvisimum. H. Wickelemanum, H. Cladopodium, H. Papilionum. H. mamillatum, H. Wickelemanum. Without mentioning the portion of the capsule, abruptly horizontal at the top of the pericel was broken, the character of the group which is entirely West-American are quite distinct. I could thus take the name of Bryoflamellae instead of Cladopodium of submit to the subdivision of Hypnum a generic. But as naming a group, that would not be appropriate and even as genus, I do not much like it.

I do not want to trouble you about detail of my work. I wish only you could look over a few pages in order to say if the description as not perhaps too long; if the arrangement is convenient. If then you will allow me to send you a few pages, as sample, please let me know what part you would like to see.

I should prefer to send you these pages soon instead of waiting until the whole is ready, as it would be inconvenient to begin again review and correction a work which I have already written three times (including the original catalog) and red and at least five times through. That work is indeed one of devotion to science, for I have nothing to expect from it and it has taken at least three years of my time. But James has had no more time.

Very truly and respectfully yours,

[Signature]
Philadelphia April 9th 1823.

My dear Prof. Gray,

After writing you that I was ready with the manuscript of the synopsis, I was confined for some days by a spell of intermittent fever and forced to quit work. When a little better, I thought that the best I had to do was to review once again the text, and to send you the whole after definitive correction. For you could not give the whole from the examination of a few pages only. I wished to send the text through Mr. James to spare you some trouble, but Saturday I was advised by a letter that Mr. James and whole family leave Cambria day after tomorrow for Philadelphia and start for Europe May. Hence I must direct the text to you, that I do per express to day, paying charges of course.

You must excuse me for the trouble imposed upon you by that matter. I know how hard you are pressed by work and I should much like to spare you the money trouble that will give you.

Share in the best, followed at Weller's method as closely as possible. For this and in order to confirm the nomenclature of the synopsis to what you consider the best way of quoting authority.
and thoroughly, there will see ease in the arrangement of this arraignment for three. I can add one more piece to it, if you wish. I will hand you the directions and a few pages of printed matter about the whole. Perhaps some of them especially about the process are quite a little long for them, especially about the process of taking the original and the return. I have added a few pieces which are left of the first of the two. Should I not be able to arrange the whole or finish the whole, you will send me a copy of the agreement, and I will pay you the balance at the time you pay me. I will send you a copy of the agreement, and of the process. I think there is nothing more to do. If I cannot arrange the whole, please send me a copy of the agreement, and of the process. I will pay you the balance at the time you pay me. I will send you a copy of the agreement, and of the process. I think there is nothing more to do. She will send you a copy of the agreement, and of the process. I think there is nothing more to do.
Columbus, O. May 31, 1833

Dear Prof. Gray,

On Monday, I have received from Mr. James the documents. I had to sign for the sale of my part in the property of the specimens of the American touran and the amount $1000 paid for it. It is to you that I owe the favourable settlement of the matter, which has been most acceptable to me, for I never supposed that I should get any remuneration in money for the long and tedious work. For this reason, I give you my heartfelt thanks for in that matter, as in many others you have given me proof of your kind friendship. What you wish to Mr. James in Texas. The offer made to me is liberal. But I hope had and shall have this a great deal of trouble and work until the book is published. It is now in great demand and I have received many letters of inquiry about the time and where it will be published.

I am somewhat uncertain about the mode of preparing the supplementary plate. Is it about the kind of engraving desirable. Could the plate be engraved after the new cheaper
process of photography? The young man who works for me draw as well for photo-
engraving than for steel engraving or lithogra-
phy. But there is always a marked difference
in kind, and this perhaps should be avoided.

I know how difficult it is for you to read and
how little time you have to spare. But perhaps
Mr. Watson would have the kindness to
write upon a postal card which of the two
kinds of engraving you prefer. As Schenck
has copied most of the figures in Julier's book,
I will take some of them for the 417 plate or
have original drawing for a few American gene-
of the Hypocrene. Of course I will submit the
plates to your inspection. I wish to make the
book just as good as possible and will view nothing
for that.

Could the Botanical garden or the Herbarium
secure for me a good leaf fully developed of
Ramthorhiza?

I have as yet heard nothing of M. Cavan.
But I suppose that when once started the printi-
ges the book will go fast.

With my best thanks, gratefully and sincerely,
Yours, L. Foreman.
Columbus, 018th May 83.

My dear Prof. Gray:

It would be very difficult for me to leave Columbus now, even for one week. I have with me a granddaughter who takes care of me and attends to high school. The parent does not live in town and I can not leave her alone even send the home. Besides, I am very inflected now by the influence of a long winter, kidney, etc. I could not walk a distance of a quarter of a mile nor can I walk more than three hours consecutively without rest. As the weather is still cold, I fear the return of some lung affection like the one from which I am only recovering. I proposed to leave Columbus in June after the close of the schools here and go back to rest and perhaps recover some strength and as I have friends and relations at Boston and around, I should spend some time at Cambridge. Free I greatly need to recuperate. The type of the page you send me is very fine, clear, easily read, perhaps as you say a little broad. But I am not a competent judge. Only it pleases me much. I shall begin at once the introduction.
and make it fuller, as you say. My introduction, as it is now, is much longer and more detailed (explanatory) than that of Dullereaud and also that of Mr. Watson. I must dwell to have it longer, but I had always in mind the necessity of short descriptions & short explanation. I will also make a full description of the 


Spagnocea and Spagnocean etc. That of the Spagnocea, as I think already made. In my last letter, I mentioned merely the word description after Spagnocea for the 


Nation or shortening my letter. I will send you Monday a new introduction and the description of the others etc. and then 


if you still find that it would be best for me to go to Cambely, I will see what can be possibly done — I should certainly be happy to see you and to consult with 


you and am heartily obliged and grateful for your kind invitation. But you would be fidd to death if we’re in a few hours. Consid 


er what it is to converse with a man as I am now. For I am really deaf and dumb you can not understand my language an


my memory has become a blank. You would be discouraged in no time. But I say again: If you think I had better come now, I 


shall try and come if possible.


Very much yours,


I. Kingman
Columbus, Nov. 14th 83.

My dear Prof. Gray,

Your kind and very kind letter of the 11th which I have just received impresses me with a deep sense of gratitude and at the same time regret for the trouble imposed upon you by the publication of the present. I wished to spare you all that. And if James had been lived, he would have had to make the corrections under your guidance and all would have been far more convenient to you. I fully approve your remarks and your critical advice. But I am under great difficulties in nothing more, which I have not studied for years, and continued to the Cyclopaedia for any recent work. I have to follow the new opinions, really often against my scientific conscience. With my few modifications, Nathan is quite as good or better, and much more simple and easy to understand. But I see that it will not be possible for me to arrange matters here without communicating with you on many points. Discouraged as I have often been with this work and am still, I think it must be put through it.
I can not repeat in the terms and description of the device, organs already given in the introduction to the description of the Muse. I mention description which you proposed to copy is for the Muse, not for one of the others. I will only add more detail to the one I have made presenting the complete. This also I will review and make entirely new, according to what you advise, or perhaps make a short key. But then new materials cannot be ready before two or three months, and I do not know what you have arranged with Professor for the time. Please only let me know by a few words if the arrangement which I propose is satisfactory to yourself (I added I forgot in my former letter to thank you for the help of your house). It is quite sufficient.

I am glad to know that you are still in good health and have many years in preparation for the advancement of the American Botany. I am now 77! and seem up entirely. You can understand how unable I am to make them the most necessary work. I thank you again and remain your grateful friend A. de Quevene.
My dear Prof. Gray,

Yes, as you say, it is hard for me to begin again a work which has given me already too much trouble. But I do not know how I could complain when I see the trouble you take upon yourself for examining correcting my manuscript and trying by repeated directions to make it more acceptable to students, and this too when you have yourself in hand a very difficult work of more importance indeed than our own and which demands all your time. I can but thank you again and try to follow your directions.

You want me to make a key like the one of Salvant. Would not a Compendium more abridged than the one I have made, be sufficient and more easy to follow by students than a key? For short the key like that of Salvant, which merely considers the capsule, the operculum and the calyptra, it is impossible to give indications precise enough. For the capsule is developed often before the capsule is ripe, it is also found only with difficulty and only by proficient anatomist in small plants. When spermen are collected and mixed, the detached calyptra generally loose are not always clearly referable to their species. The capsule also are not always found. Agreed many morses are collected sterile, no species of Dicerandra for example have been found in fruit in America. And then the perisome is not only very difficult to separate and to be carefully analyzed, but it is not at all a constant character of the genus. Thus in the species of Peritome double, 16th of Salvant, and calyptra has two gymnosomous species and two with an inner membrane or double perisome, in Diagodon all the American species are gymnosome, in Catches in 1 species is gymnosomous, most of the teeth are separated by calcis, there is a double perisome, or at least the calcis as long as the teeth happen appear with an inner perisome. And so on. I do not see therefore how a key of this kind might be useful to students except if it is prepared with details of the character of the organisms and this, of
none will render the artificial they more intricate and difficult to study than a compunctus. Hence why have two kinds of guides, and why tell the artificial method which certainly turns the mind of the students from a more natural method. Though that former may based upon the lex of the Plains was very commotions, you would not come to it again. Since Watson no word of man's has been published with an artificial they, Mother form a compunctus of the tribe, and another of his genera in the Museo Austriaco American. Mother has nothing, and if you boil on the words of a more recent author Lindberg. Braithwaite, do you see in all a tendency to systematically arrange more but nothing else. The systematic arrangement is however different in all and as I find that of Schlemmer by far the best, the most regular. I have admitted it. Step Watson has done the same. His compunctus is by far the most clear and connections, kind of they. I wanted to follow but went two far with Schlemmer. I think therefore that of omitting a number of these distinct tribes, and abstracting the description, I had a compunctus of that kind, the artificial they should not be needed and that compunctus tells the general characters of the mono's could be published at the beginning in the Watson's mono without double kind of paginatio... - The Hypocrea especially in the Monohymenia and the Campylotricha. But it would be necessary to cut them by sections: for the Campylotricha can by genus for the Orthotricha. Now the sections representing Schlemmer's genera are all described by itself well as much description as an my genus. I thought first to admit merely the sections. But it was decided to have a genus each family and that could not be made in admitting the sections. Schlemmer's genera are generally admitted or at least quoted. They have been admitted in the catalogue of Bower and Harvey. In the question therefore I remain uncertain and indiferent. If you could direct me what to do, I should like it and follow you in this. All the recent authors, especially Lindberg and Braithwaite tend to multiply genera and genera. I am already, you back into the conservativa, read, and only I think it would be best to look a little more ahead. - Don't use this letter, if you do not find it convenient. I shall go on and would to preparably and your compunctus and description and then you may see what is the best. Amur and therefore, yours, J. Le Prince.
Columbus, 0 15th June 83.

My dear Prof. Gray.

I send you hereewith the separate list you wished me to prepare, showing the groups, or Tribes and Genres of each Tribe. I have added to it the sections or Subgenera (how shall I call them) of the genus Hypnum and the names of the 154 species described in each section. That may not be of any use for publication in the Manual, but that may enable you to see the skeleton of the work. I say that the list may not be published. For, indeed, the Conspectus in the Introduction will indicate the pages of the Manual where each Tribe is described or mentioned, as perhaps it will not be necessary to repeat a description of each Tribe in the text of the Manual but only to give the names, either simply or with an abridged description. But, it would perhaps be advisable to record in the Conspectus, after each Tribe, the names of the Genres pertaining to it, as I have done in the pages which I send herewith. The number of the page where the species is described in the text might be added after each generic name in much the same way of the Genre. For the Genre Hypnum, it will be best to give, as Sullivan has done in his Materia, a description of each section which is
equivalent to that of Genus. And perhaps it will not be necessary to have the sections described in the Conspicuous, except if desired, or for such genera as briefly characterized in the beginning. You will have time to look at these before I come to the genus thymus. But now, and from the beginning. Should I not have the sections described in the index as a reference, or a list of the genera, in the Conspicuous, in the way, or will it be necessary, to have them described? If not, what have I described? If not, then also I have described them in the text. If not, but I have them described in both index and text. What do I describe? I have written a few words in the text. Wilson has described the tubes and tubercles described in the Conspicuous as a combination of the genus or genus, and there are no tubes in the text, except a few, for the genera of the genus. This is a combination of the genus as I have read it with the Conspicuous. What is it good for? To make the work easier for the reader. What would you have me to do?

A second question not clear for me is that of the pronunciation. In the Conspicuous, I have described the sections of the genus, all the essential organs of the genus, flowers, calyx, capsule, etc. are separated by a period, and the parts by a semi-colon, or a colon. But I can not distinctly see why some...

partial organs are separated by a colon (e.g. calyxes), while other characters have only a semi-colon before them. In the description of species, the period is replaced either by a colon or a semi-colon and. how about I find the same uncertainty of mind about the distinction of the one or the other. In the Conspicuous, Wilson, Wilson, etc. I find for punctuation only, the comma and semi-colon, after the description of species, adding the period by some authors for the description of the genus. You have the same in your manuscript, though you frequently use the colon in the genus. If not, it was not too much trouble to you, please give me a third direction if possible.

As soon as I have arrived to this, I will go back reviewing one more the manuscript to try and have it all right for the printer. I have already arranged the whole, according to the departmental views, in the order marked on the pages, here with of the systematical parts, and finally, with the appendices. When the synopsis and the Appendices are complete, I suppose the contents of the galley, and of the first Error will Suffer. Probably then, there will be a correction of the manuscript which we are not good English. But I may correspond with Mr. Casuar in that matter.

I can not say how it gives me to find fault to give you so much trouble by my ignorance.

Yours very respectfully,

W. Englemann
Columbus 9 June 1893

Dear Prof Gray,

I do not know how better express or describe the character of the ducts. They are triangular, placed at or near the concave surface of the lesser, the lesser side of the ducts being free. They are not enclosed by the utricles and thus forming part of the inside or concave surface.

The sides of the duct is in line with the base part of the utricles but free not enclosed by them.

Tallrand says in the Memoires that the utricles are triangular and placed between the rolled utricles near the concave surface of the lesser. I did not write that they are placed between the utricles, because this is the general character. They can not be placed otherwise. But the utricles protrude beyond the utricles, or are sometimes contracted and enclosed between them so that the border of the utricles pass above or below and thus I thought my figure was more expressive and more clear. If you think the contrary please to substitute that of Tallrand as underlined above.

For Hutchins The barn an ditchers, moderately
not only in concern but needed so that they could be flattened on both sides of the item. I will modify the diagnosis and say items concerned? but it is the leaves which are concerned or so much that they need that they look concerned.

I should have liked to know about panitumumab. I am trying to make my diagnosis more clear and to have all a well a possible to me for the Panacea. But the Panacea have been ready for more than two years, sent to James, I have them examined best submitted to your opinion. He always said that they were all right.

Why should I James, free me to go with him against my expected protest? I asked him to make the word with Arabia. He would not hear of that, then with the Nations, offering him my assistance, if needed, my notes about his will. He would not hear that too. I will know how much trouble I should have with the matter with my poor head and language, and now you have to suffer for that too.

Very truly yours,

I beg pardon
Columbus 6, June 21st, 1883

My dear Prof. Gray,

Your third letter of June 18th is now received. But I met you already yesterday evening and this morning too before I got your letter from the mail.

What you say about the possible arrangement with Mr. Watson is good news indeed. If money could help, it would be to Watson to help me please to make arrangement with him for that. I will gladly pay any amount he may think insufficient to pay his time and trouble. I will write him about the tribe and all my ideas when we have arranged matters with him. I trust that once in progress the work will go smoothly. But
I can not trust myself and if the proofs are read once by Mr. Watson, I know I shall be out of trouble.

Very truly and thankful,

J. Ingwerus.
Columbus 0 June 24th. 83.

My dear Prof Gray.

The idea of imposing upon Prof Watson such a hard task as the revision of my ms. or even the reading of one of the proofs is really distressing to me. I have no claim to this time, nor can I understand how I could ever cancel my obligations to him. I do not dare to offer to M. Watson a compensation in money; but could you not do it for me? It would cause me very much if I could acquit myself in some way of what I shall owe to him. I will answer his kind letter now.

I believe however that after the Iphigena and when we have cleared the matter about the Tribes etc. there will not be any more difficulty in the way. But on this question, though I have turned it and reconsidered it on both sides, I can not come to agree with you. The tribes as I have fixed them as most of the authors have done before are distinct groups, well defined by their characters. See for example the Dioscorea which has neither the plants, nor the leaves, nor the capsule or the perisperm; or Octoblepharia, whose affinity is with the Iphigena by the leaves; Compare also Guimmesia to Orthostichum differing in the same degree by all their organs and so on. When differences so marked exist, you said it yourself, a genus could not pass as a subtribe of the other; the Orthostichum can not go as a subtribe of the Guimmesia. As the Weissia are the most difficult to describe with precision, I could arrange—
the state of the names as follow.

Table 1: 

1. Orthocera.
3. Follina.
6. Hygrotheca (Subtidae) 1. Plerin.popave.
10. Fornierea.
12. Hyarn (Hygronea) for the array Hyarn array- see. 

On the table 3 or Wire nov new name Subtidae, the distribution as would be below.

Or I could make a totally new Subtidae in groups a follow

for the Subtidae: see the Chief causer.

Table 1: Orthocera 1. Thermona, 2. Mend, 3. Distuare, etc.
Tuba._

Table 2: Anacera equal indicating certain order, 1. Parastoma, 2. Menia, 3. Brugia, etc.
Table 3: 

Fornierea 1. Fornierea, etc.

The first Hyarn Perae to more approximate it easy. 


That probably you will not want 

Hyarn 1. Must an explanation it's 

There are no such to 

Hyarn.
Columbus, 9 June 26th 83.

Prof. Jesse Walker

My dear Sir,

The offer made in your third letter of reviewing part of the third of the Manual of the moths impressed upon you all an amount hard work and such a great loss of time that I scarcely dare to accept it, though I greatly feel the need of your assistance. Could you not enter into some kind of arrangement and accept some compensation, if not for the value of your work, at least for the loss of your time?

The manuscript of the Phaenacoe was written long time ago and has been more than two years with Mr. James who had the position. While I was away, it was returned to me long time after the death of Mr. James and merely modified some of the quotations from that name of Dr. Huxley which had been lent to me by Mr. James. I admit the fitness of your observation in regard to the authorities of Spices and rattles. From a discussion on the subject with Prof. Gray who tried to take the trouble to put me right on the subject,
I have reviewed and modified all the quotations from the beginning of the essay, and believe that you will not find any more errors of the same nature. I have also generally followed your mode of arranging the quotations in the book, and reduced their number, perhaps, too much. The preening is of little use for a Manual. I have therefore generally quoted the author of the species, and the world where the species has been illustrated for the best. Braithwaite knowledge certainly very good. And for the figures and the clearer of the description, that of Aehrens, Engstrom, is better, to my opinion. The one however does not represent any species as Braithwaite does. I have also reviewed lately, Euphorbia and Annaea, Johnstone (two weeks ago) from London, his 18th Braithwaite the preening occupies a great deal of place. There must be also some excuse in my favor for incorrect quotations in the defying of my botanical library. When Thallian was living, I had his books at my disposal. Since the Thallian library was transferred to Harvard, I have had to seek in what I have and that is not much. Thus, for example for the names of the Hypoïdoides of Sphagnetum, no Authore I find quoted in Halle. S. Hypoïdoides, A. Braum-
Columbus, 0 July 19th 83

Prof. Walton.

My dear Sir,

Returning the galley which you have had the kindnes to correct, I take the liberty to say that in my opinion the heading, description of the three orders or superfamilies, as these description are more completely given in the prospectus and also in the text with the description of the species. For though you think it well Prof. Gray that the description of the tribes and sub-tribes shall not be repeated in the text, it is however necessary to give a short account of the orders, like that which you have just before the artificial analysis. Should you, however, find that it is best to put this description before the Key, I should like to have the description somewhat modified, and the page here below, which, if you find correct, could be annexed to the galley. Your remark is so well made that I have nothing in nearly any thing to change. As it not Hacinae or any better as Hacinae, at least an account of the authority. I think Mr. Judd has written some. Mills and Melton have.
Phaco while Milton and Schimper have Place. Perhaps the account hungry wants, etc. All that is paid by Mr. James, money. But I hope that when the work is once started the publisher can send me direct, the galley and the proofs. But I now greatly need your advice and kind direction.

Very truly yours,

[Signature]

Order I. Sphagnaceae. — Capsule distinctly operculate, gymnostomes raised upon a pseudopodium, dilated and divided at the summit; columella basilar, thin, composed of a single layer of cells. Selves soft, pulvinate, composed of a single layer of two kinds of cells in a single layer, viz., large, chlorophyllseous, purplish and globular; small, narrow, chlorophyllseous, interposed between the large ones.

Order II. Andreacea. — Capsule, etc.

Order III. Bryacea. — Capsule, etc.

I would rather see Post money, Schistose, Pink money, and True money. At all these terms are repealed from the corporation, and are put in the description part. I should also omit the remunerative value of the money, etc. so the Sphagnaceae and the Andreacea, and Theobald.

But these remarks are mere suggestions.
Columbus 0 July 21st 83.

Prof. Jenyns Watson. Cambridge.

My dear Sir,

When I returned to you the first Galley proof and wrote you about it on the 19th I had not read your note which remained sticking in the envelope until I cut it by chance in the evening. I have to believe, that I do not object to any corrections you may yourself or Dr. Gray find advisable to make in my work. I merely thought that the Orders being described in more detail in the systematic prospectus and also in the descriptive part of the manual, the repetition before they might be superfluous. The same with the explanation of the words of some organs, as all these words used in the descriptions must be explained in the introduction.

The omission of a note for Sphagnum and for Andreaea, and a modification in the series of the Genera, caused by the omission of a number of Tribes, has changed the position of some Genera in the text. I thought first that I could correct the numbering of the keys.
in reading the 1st device. But it is best to have
the whole in order at once and therefore I have
passed again over the first and definitively
marked the Genera. I send you herewith
a list with the numbers as they ought to
be placed upon the Artificial Analysis.
Of course there is nothing to be changed in the
description of the genera and in their relative
place, except for No. 146, Conclusus, whose
place is now appropriate with the Genus-
Names than with the Formicules. I have
also added No. 128, Hypsium whose place
must be indicated in the Key in a separate
section before the last one C which ends
the table. The braced lines of p. 6 and
herewith may be cut and pinned to the galley.
If the printer sends me directly the galley or Nth
I will spare you the trouble of making these
corrections.
I have omitted in the text the description
of all the tribes. Shall I do the same for the
Subtribes of which there are a few? If
I have to omit them, the description
of each of the following genera has to be
somewhat more detailed by the addition of
one of the characters, of course. This change
may be done easily and in a short time.
I wish to have all in good order before send-
ing the rest.

I feel really ashamed to ask you any
question and to request an answer, when you
have already so much trouble about that
work of mine. All I can say is that I am
under great obligation to you and very
thankful for your good offices.

Very truly yours,
[Signature]
Columbus 6 July 1833.

My dear Prof. Gray,

Though fearing to be intrusive and to trouble you in writing to you, I am always very pleased and grateful when I get one of your letters. The last one of the 21th b received yesterday is especially welcome as I see that you approve of the insertion of the descriptions of the tribes in the text. With an abridged description of the tribes in the Conquest, each followed by the naming of the genera represented in it and also a short description of the latter genera with reference to numbers, there will be thus a more scientific way of studying and following by students as the artificial one. The mere mention of the tribes in the text without description would on the other hand give a constant recurrence to the Conquest. – Would it not then be advisable to give also with the Orthogyna and the Andricaeno and in the beginning (not separately before the artificial Key) a description of the orders as I have made it in my manuscript and as it is done for the Bryacae and to admit Orthogyna and Andricaeno with is it? The putting the whole in harmony.

I have already rearranged the text of the Bryacea taking out the description of the tribes as I was formerly directed. But I will gladly go over once more
and for the matter as it was originally. — As for the number of tribes, you may be sure that I do not admit any more than it is necessary. As you say, Müller has taken, proportionally a good many more than I have. Müller has then two in the same proportion: belladonna admits 35 tribes, and some of them like Discalium, Schistocerca, without analogy with any other reconstitute their separation into tribes. Without separating them into distinct tribes, the monstrosities together or with the descriptions of the genera only are muddled into a confused, incomprehensible mass. You have given to Braithwaite's Ephagnon a very favourable notice. Now, see how he is proceeding. He omit the Ephagnon in the Bogace, but counts the Andreeae, quees, distinct and independent, he has already 7 families in 7 families. Taking among them Tetrapods, F. denso, Lireolobtus, Sternae, which are not even admitted as tribes, or subtribes by M. W. Müller. Braithwaite follows Lenzberg for the description, and he is right in that, but he follows Lenzberg in the multiplication of the synonyms, in descending a for down as possible to the bottom for quoting the oldest authorities and admitting old forgotten names. and (he can) certainly not be done for our Manual. So Müller with his 23 tribes most surely mixed, separating Drya and Menura, one tribe for each and putting the Deltocnemis with the Menura without any resemblance. Braithwaite has 26 tribes and a large number of families for Ephagnon. This is certainly too far subdividing; but Müller in his Boletus Britanicus does not better with his 3 orders and 36 suborders as divisions. But it is useless to bore you with exposition of this kind. All the authors more or less agree in the number of subdivisions, but all differ also more or less in the grade of arrangement according to their relative affinities. I truly believe that after full consideration you will find that the series of one more, as I have admitted, and I fixed it in the most convenient and the more early understood by student.

I have sent to M. Watson the list of the Genus with their Ws in the characters. Now you will please decide if you will have Ephagnon and Andreeae counted as Ws or left out without remark in the first case I will have to remember the generic titles. If you do not want them the Ws are all right now.

Very sincerely yours,

J. Regency
Columbus, July 26th, 33.

Prof. Jeune Watson,

My dear Sir, I return by the same mail the galley proof 2, 3. The numbers are corrected according to the change made in omitting No 1 & 2 for Salamanca and An¢ræa. I wrote to Prof. Gray about the matter. As he finds convenient that I add the descriptions of the tubes and subtubes in the text, it may be that he will consider the numbering of these two genera as advisable. If it is the ease I will have to change the No again and may do that in the present. I answered at the time of the galley your remarks about Moniæa and Catharines. I do not know how Heiing has written the name. But all the authors I have on hand except Wilson have written Weiss. Sullivan also, and I rely generally on him as American authority. He has a much better library than my own and was very careful in his nomenclature. I do not admit Catharinea instead of Abraham, though the first is older. The name is bad while the other is a true description name in harmony of nomenclature with the tube. Nor do I wish to follow Lindberg or Brarthwait in admitting dotted generic name. For the oldest generic name are
generally unremembered or forgotten. And names like our should, in my opinion, secure more, well established and admitted by the authors more generally known. True change of names are never confusing. Practically is not yet known as an author. It will be a long time before Schrader-Müller, Menten, Stalhans &c. are forgotten in their names, Shelden. — In this opinion, I follow Prof. Gray who wrote to me to select for our genera good well established and well known names without caring about a nomenclature by precedence (or something like) And too, I am not in position to follow that method. It demands a great library and the collections of many (Herbaria) of the old authors to discuss their nomenclature.

Censure these remarks, which will explain why some names have been admitted in the following descriptions. I hope that you will not spare me your advice for this.

Very truly yours,

[Signature]
November 6th, 1833.

Colton, N. W., New England.

My dear sir,

Your very valuable letter of the 17th of last month, I received, and as I have not yet had time to read your letter, I return you my thanks for it. I am sure it gives me much instruction and information, which I was anxious to get. I shall, however, give you my best thanks. I have not had time to read your long letter and answer them.

What you say about the beginning of the first part of the Manual is all satisfactory. I shall not correct again the name of the genus nor make any changes in their relative positions, nor only replace in the text the description of the Ftiles which I cut out. I really think they are descriptions valuable for the student, and the description of the tribe, with the description of the genus, is very good. I think it would be also good to add a third of my own in the lines about the tribe. I could not in any way find fault with your arrangement and distribution of tribes and genera in the Bot. Cph. On the contrary, I wrote to Dr. Gray that it would not be done better.
and as you will see my own systematic arrangements differ little from yours. I have a few more tribes. But it is quite a matter of course to a large number of genera of the manual, some of them necessitating a separation under - peculiar tribes as not found in the California species. You say that the English botanists do not mention tribe and other divisors in the text. That is true essentially for Nootka. Another in his Synopsis Florae, a work which for me is very dry and difficult to read by the necessity of running over it, and by the author from a division to another, from a genus to another, until I can find what I need to know. In contrast the Oylogist. Nootka and especially Mallet have carefully exposed their systematic arrangement, first by a complete at the beginning and then, for Mallet at least by separate description of the tribe and of the genera in the text, even with the arrangement and short description of species after each genus. Dr. Anthoniæ is a disciple of Lindley and follows it from the beginning, as far as he has gone until now, at least. I have already written to you about should we admit the modification proposed by Lindley, or admit the oldest known genera. What a small number of systematic nomenclature in systematic nomenclature when, Bractyphyllum for Tritonius, Astrophyllum for Mecianum, Phrygiphalé for Subconcionum, Securis for Encalyptus, Maloia for Tircotomus, and Anessicrium, Garayia, Dictytrum adopted by Mallet. That evolutionary system may be good according to some opinion. But I am too old and therefore too conservative to admit it. Else that I should find as many reasons to admit that of Mallet, or of Mallet which also present some difference. The more one studies the more the more he finds the impossible to arrive at them in some consistent natural system and also the difficulties of fixing precise generic divisions or rather of the thousand of the names given by different authors and the oldest is the most right. Hence I think best to stay with Schimper's, Gallard's, Mallet's, Mallet and other authors whom I have followed for half a century. I fear no harm to make a new appearance, and I truly believe that the time our Oylogist will support my opinion and in the matter. About they have followed Schimper and Gallard until now, as you have done yourself. I do not say this to depreciate the work of Lindley whom I have in high esteem and who is one of the best botanists now living.
Cumber, 8th August 83.


My dear Sir,

I return with this galley, 5th and 6th. After you have read them, I find nothing to correct. I believe, however, that it is better not to quote whole pages in Schimper's Toxof. There are 2 ed. of this work, one in French, the one I have, of which the text is different. You will see that, probably for that reason, Schimper rarely gives the pages in the quotations of that work. I also wish to correct. I thought to correct upon the galley, but which I would like to have all right, is in order to space you the trouble you take about this work. I am not quite clear about the punctuation. I suppose ; separate different organs and not part of a whole, an stem, bone, capsule. But you precede by ; the operculum or lid and by ; the calyptra. While I suppose that all the parts of the capsule should be separated by ; I am somewhat embarrassed also about the quotation of the Racemus. Schimper and others do not quote authority after the pream of the varieties, while Braithwaite quotes his name. I should prefer and have done as Schimper did, and did not mention it. As you ask, the description, being proof of authority.
In order that you may see how and when I am not in the right, I send herewith ms. pages 116-119. If you have the kindness to look over a few of them and mark any remarks you see advisable, I will give one that the balance of the manuscript to you, according to your direction, I certainly will do the best I can to spare you trouble and loss of time.

Though I do not admit the modifications proposed by Andeborg for the nomenclature of the genus, I will however quote them either after the generic name or rather after one of the species, as I have done after Cynodontium Schult. which I directed to Enophyes.

The modification I had to make in the nomenclature to follow the direction of Dr. Gray and your method in the Calif. flas have necessitated many corrections, prunes, etc.

I think however that the pages are clear enough for the printer. You will find them generally better than they have been copied, as an amanuensis.

Very truly yours,

[Signature]

It would be best if the printer could send me for review a number of pages, as a too small package may be easily lost. I should also like to have two copies of the reviews one to keep here for reference a comparison.

See on the question of the genus that Batschwaitz, dicendus,Potentilla, while Andeborg has Schult. phylliwm, to Tyl. This last name is older.
Columbus, 6. August 13th 83

Gentlemen, Watson, Cambridge,

My dear Sir,

I think you are right about Sporobolus, or, as S. peregrinum is a true Bruchia, that genus should be omitted. But had Schumpe's not right to take it for the description of a different species? Are there not cases of this third or mode of nomenclature in Botany? I can say nothing on the subject and indeed do not know how to mend matter for the only way would be to describe S. peregrinum as Bruchia and Lindl has done, or to put it with all the Bruchia as Sporobolus with Miller. Miller makes a Sporobolus a section of Bruchia. But any change in the place of a genus is now impossible as it would force a modification of the number of genera in the artificial analysis. Perhaps I could ask a short note after Sporobolus or if you should point out the error by a short note signed by your name, I should feel much obliged for it.

I shall be ready this week with the whole not shall I send it when ready all together or by instalments.

The communications by mail or express are often very slow and it was the case with James sometimes. I have last in one day, sometimes the 2nd three days or the way. Yet truly and thankfully yours,

D. Longueville
November 6, 1833

My dear Prof. Wetterham.

I am to-day free express the end of the November. (Mit.) I will have to look again over the Hypnum per section of the genus, Hypnum and will send back end of the week.

I will arrange for your little one for the more features of the character from the of Drsch.

Then as for the first time the start of the monograph. an introduction exposing the organ of the monos with description of the name used for all these organs and their parts and then a systematical description, Tube. General, Prof. Gray objects to the printing of this part in the beginning of the work and want that both should go into an introduction differently suited to be printed after the descriptive part. He said that main of organs, definitions of tube, etc., could be more properly fixed in writing than when correcting the proof or after. Hence I have now nothing to add. I intend to give a short description of the tube and after each a distribution with short descriptions.
of the genus. I think I met you already about

I return the galley received Saturday night the 18th, always very much obliged for your remarks and corrections which are for

the best.

About that Ephemerum he tells it surely
I know no more but what he says. He compare
ed it to 8 papillons but from which, he says
It differs very little; but says nothing about
8 s. spirulorum. I think that this last
Species is rather 8 spirulorum Melch. Help
than Lepp, as you say. I think

Very truly yours,

I. Liguerrus

6) Nothing however is said of the species in Roger
Lee which is about Loke, but only in the Synopsis
1st and 2d Ed. which is, I hope only. Wilson
quotes it probably from Schopenhauer's letter and
may have supposed that this it would be
mentioned in Roger Lee.
Columbus, 6th August 35th 83.

My dear Prof. Watson,

In his letter of June 5th Prof. Gray writes, after mentioning receipt of the artificial they writes, after mentioning receipt of the artificial they w ill you send too an abridgement of the systematic arrangement of the Oxyacea as short as you please, in which we can add the page of the book when it comes. The character of tribes and subtribes are given in the body of the book. If the character of tribes and subtribes are given in the body of the book it will not be necessary to give the genera, though it might be well enough to do so. From the underlined sentence (underlined by me), I understand that as the page had to be added to the description of the tribes as reference, this systematic arrangement could not be printed before the end of the book and therefore should be put as Oxyacea has done in the introduction under a proper pagination. And also I thought that as the artificial key of the genera expose the abstracter characters, it would not be proper to present them again in the same part of the book. But I have from the beginning recognized that your views result from long experience and careful observation are far superior to my
own, and I will immediately prepare
that synopsis of the tribes, add it after each
tribe the name and No of the genre. I
will try to send you that or Monday next
or at least on Tuesday the 28th with the balance
of the descriptive part.

I thank you again and Prof Gray too
for your interest in the world, deeply regretting
the trouble it gives to you.

Very truly yours,

[Signature]
Cumberland, 8. 30th August 1833.

Prof. J. Watlow, Cambridge.

My dear Sir,

To follow the suggestion of Mr. Gray, as far as I was able, I have prepared the systematic disposition of the Ophioceratidae by short descriptions of the Pide, and shorter description of the Genera of each tribe, with the necessary order. Perhaps the least part, or the description of the genus in question. But if you think so, it will be easy to draw a line around the Pide, or the part to be printed—I cannot really say what is the best. The systematic disposition, and was originally made, somewhat more detailed than the in this must, seemed to me sufficient. But I do not trust to my judgement. I had of course some wish to do for the preparation and could not read the proofs before to day.

I also return the proof. pp 3 to 19. You have on p. 15 a note for shortening three lines. I do not know if that is for me or for the printer. Any case, I have marked with pencil through the words which could be left out, for shortening sentence, if necessary. If the result is, for the printer, these lines, an—
I have added to the list of the works to be consulted for the study of [unreadable word]. It is a pamphlet of 15 pages and 4 plates, badly made. The work may not be of great value as the essential document, as taken from [unreadable word], Brandenbur, and Lind bar.

But [unreadable word] is the editor of a [unreadable word] and his report on the Manual may help the lab of it. He is someone a very good botanist.

I think I will read one again. The last part of the [unreadable word]. I send to great exactness in your corrections that I feel certain to have always left many errors especially of punctuation. I may send it next Monday.

Yours truly,

[unreadable name]
Columbus 0 Sept. 3 1883

Prof. S. Watson, Cambridge.

My dear sir,

I send you express today the last part of the Manual, the Cladoceræ and Phæoceræ. Though I have read the manuscript, I fear you will still find too many matters for correction. There is no mistake, say, page 19. There is a page 19. Therefore in the present case, I have found among my papers. I cannot recall if it has been copied. Perhaps it may have been left out by mistake or forgotten after it has been corrected and glued. Please, therefore, see if this page is already in place or if it was left here by mistake. In this case of course you will please give it to the printer. If copied please throw it in the waste basket.

Very truly yours,

V. Seigneurin

Galley II & 17 pp 17-33, just received. Will try to return all to monorow in the galley this evening. I do not find galley of Ophryum Fitzgerald 31.
November 6, Sept. 17th, 1883

My dear Prof. Watson,

For about two weeks I have not received any proofs for correction. And also, I do not know if you have received the latest of my manuscript, the "Revue de l'". I sent for express on the 3rd Inst. I am therefore somewhat uneasy about the matter, and if you are at Cambridge and well enough to write, I should thank you much for a few words, especially to say if the manuscript is in your hands.

Very truly yours,

T. Longman
Columbus 17th Oct 83

Prof Walton Cambredge.
My dear Sir,

I was very glad to see the proof coming again. From this, I see that you have come back to Cambredge and I hope that you are now quite well. I hope also that the hard work you have undertaken for the correction of the first proof of the "manual will be now somewhat less arduous but I trust it at least very much.

The last page of the volume is wanting in the old proofs which end with "Obercume while in the new arrangement the last page include Thurgolden. That is not matter except in case of forced reference.

Please give my best compliments to Dr. Gray.

The death of these affects me very much. I am now the only left of a long series of Swiss friends and colleagues. Here was 24 years old, three year my junior.

Very sincerely yours,

V. Ferquerey

Return proof by this mail.
Columbus Nov. 6th 1873

Dear J. Watson Cambrdge,

My dearSir,

I return to day the proofs which were received only Saturday after noon, one day after yours of the 31st inst.

All the changes you find proper to make are acceptable to me. I am so far behind you in all that concern the preparation of matter for the printer that I merely consider this great indebtedness and obligation to you, for the careful review you made of my ms. and of the proofs, freely admitting that without your assistance, my work would be very deficient indeed.

The arrangement you propose to follow for the order of descriptions is quite right in my opinion, except for the Glyphae which you describe first and which I consider an unimportant part of the capsule, on account of their superficial nature, and of the similarity of its characters in all the species of some genera. Then it has been described the last by Sturrock and many authors. It is not a real part of the capsule, but like the shell of the egg, it is the first thing seen (when one can find it) of a mon and in this point of view the first to be described. I hope to follow your opinion in that matter.

That blunder of mine about the many varieties of Glymma is a very bad one and I regret it.
very much. I hope that the leaf is not yet type-
typed and that the error may be rectified. I would
be well to add on the same leaf, p. 29 for Eustichia
some note, as marked in red. That is in disagree-
ment with the character of the Eustichiodae, of which the Geranium
is included with 16 teeth; and perhaps it would be
admirable to add before (Subtribe. Geranium) Eustichio-
nae is Eustichia and then nothing more should be
added to the character of Eustichia. For this, also
please to do as you think best. - You will find
herewith a description of Eustichia Norwegica, a plant
from the specimen that was brought from the Flinders
Island and also from her description in Bot. Cbl.
This discovery has given to me a great pleasure. I have
touched these fruiting plants for 30 years or more.

About Geranium, I do not know what to say.

We do not have any species of Geranium, and I admitted
that substitution Gerani-Braunia only because I did
not wish to make a new sub-genus when we had already
three which could be ascribed to the same genus with
little modification. If you consider the best to put
Braunia in the girl p. 29, I am quite satisfied
with the change.

The synonymy of Dianemella carpe I D. Schreber
may be right as described by Lord I. Braithwaite.
But Dianemella crispia I D. Schreber has the
leaves like those of D. Grevilleana spreading all around
The question is only, I think, about the reference to plants
named by Euphor and Schreel. Admitting that syno-
my is true, we should have Dianemella crispia that one
Dianemella crispia had a leaf, in the same section.
I prefer to have the question to further Botanists.

In saying Ainoschierum after the first section of Dianae-
me and finding the synonym as marked end of p. 64.
I think it is clear. But certainly Dianemella crispia
is to be placed in the section of leaves, squarrose or spre-
ding all around, though a Dianemella.

Many, many thanks for your great kindness. I am really
obliged to see how much trouble you have
with the correction of the text and the proofs and how
much time you have to give to that work.

Most sincerely yours,

V. Lueren.
Cumber 0 8th Nov. 83

GEO Watson:

My dear Sir,

I know I am always in the wrong! I have corrected matters. Perhaps, that will do. Only I do not know if Millon is right in supposing that the cellular matter composing the operculum is the top of the columnella. Perhaps it would be best to leave out that ( ).

I see by the Revue Historique that Braithwaite has the last part of the Dicranosaoue out. Probably this will necessitate some change in the synonymy. I will be obliged to you for any suggestion you may find advisable to make, and thank you also very much for returning that page which you will find herewith.

Most sincerely yours,

A. Serqueux.
Columbus 0 12 83

Dear Prof.,

Schruppe sent us a new species under the name of Diocelenium pallidum, a specimen which Tullwaldt himself recognized as a new D. spurium which is very slightly different from the European form. That is No. 85 of the Menu. For the other, the synonym should be D. condensatum, Hew., as in No. 86 of the Menu, erasing var. condensatum or leaving var. condensatum (as of the author) without referring which is only indicated upon label in Tullwaldt's herbarium, or at least in my own and as you say, can not be mentioned.

Very truly yours,
N. Duguerens.
Prof Jeremio Watson
Botanic Garden
Cambridge
Man
My dear Mr. Watson,

Fcludolomon termes Schrad. is Fcludolomon

fuidolomol termes. This is a hypothetical or

rather unknown. Schraedt. 1st ed. of the syn. referred
to it under the name of Sphoebium termes. A other

(mediterranean) species now Sphobuncula bicornis, adver-
sibly to err in syn. l284. That T. termes

from here, and from Schraedt. l294. Lindl. does not

mention it and as Schraedt. says the man is non rite

cognita. Neither Dillord, nor myself have seen anything

that could be referred to Hedys's description. Hence, I

thought it would be best to leave it out. Perhaps

it would have been best to have quite note of the

description of Sphoebium termes, like this.

Fcludolomon termes. This is a yet an uncertain species

of which nothing is known but Hedys's description. According

to the description, it differs from S. forbile by its long double

seeds, the more solid browned red capsule and the

plane margin of the leaves. If you think it proper to add

this, you will oblige me.

Yrs.
About Trichogramma Colorado. Ass't. I can recollect nothing. I do not find it in my catalogue where James has indicated all the species he thought I had forgotten. I do not find it either in my 1st manuscript. It is one of those numerous species of Avisio which made from sterile specimens may be referable to different and often confoundedly more fundamentally than vegetable genera. And if we have not had a specimen for examination, I can say nothing about it. If however it is not described, I should like to give Avisio's description, as it is not well to omit any thing from him. If James had seen the specimen he should have noted it, and certainly would have mentioned it. The N. James, who gave the specimen to Avisio is probably not. Than to our friend? In any case, as I say above I do not recollect to have seen any thing about that species.

As to Maple pmvd. Th. Diracreee, I do not wish to see new Braithwaite's Diracreee II. Will hold the liberty to ask for them later.

Very truly yours,

L. Menzies
Columbus Nov. 16th 1833.

Dear Prof. Watson.

It is not you who should feel to be excused, but me for the trouble you have with our men.

I think also that Barbula revoluta should be left out. In my notes of work in connection with Sullivant, I have referred with to Barbula revoluta a man found at Bostlborough by Mr. . It is not referable to the related to both B. revoluta and B. gracilis, but differing from both by character described in note. Neither Sullivant nor myself could clear the specimen being unsatisfactory. Hence I have taken that B. revoluta from Calhoun, I cannot recollect it; it is not marked upon our catalogue. It must be one of my thinnest! Perhaps I may find explanations in some letter of James. In the meanwhile, please examine. I am only too glad that you take the trouble to look into the matter, with so much care. James, could never have done that. About species. Trichostomum coloradense and other undetermined species (for Semide undetermined are those of Flora Trichostomum, not Barbula etc.)
would it not be best to leave out any uncertain species other than this kind and put them at the end of the volume as a kind of appendix? All the species of Autin which have been examined by Sullivan, or James or myself, I know what they are worth, but there are some of which I never saw a sketch and which are known only by Autin's descriptions. They may be all right, but I do not know about that. The number of these uncertain species is not large.

Please do not argue my reasons. The more the less and I fear you will have plants.

Very truly yours,

[Signature]
Columbus, O. Nov 4, 23° 83.

Prof. Serena Watson Cambridge.

My dear sir,

Far from being displeased by your corrections of the Manual, I am, ever day more, grateful for the careful attention you give to the preparation of the work, either in reading and correcting the manuscript or in revising again the proofs. I expected, of course, to be able to correct the proofs; but I see and have seen it from the first, that at least for the quotation, references and synonyms, I should have blundered all along and my attempt in that way would have been a failure. I see well how much of your precious time you are forced to give to the publication of the manual and how heavy are the obligations I owe you. But of this methodical is time. Perhaps the quotations of authors is sometimes too numerous or do too great detail for a manual. At least if the manual of men is compared to that of the Phrenological Plant, by Prof. Gray. But I do not see the possibility of reducing quotation, though I do not consider them as of great value for the student. In this you have great experience and I am satisfied that all your corrections are for the best advantage of the Booth.

I do not know what to say about Justin Duchesmin Coloradene. There are a few others
Like that which I may have omitted. And generally I have avoided Austin's description, even against my opinion, adding however a few remarks which I thought advisable to give. Austin has been against us, especially against James from the beginning, and for that reason I have been careful to say not against his work, but against his proof of nothing which he has published, where there was no positive evidence of error in his determinations. We have thus reviewed his species as often as we could get specimens and I believe that every botanist would thank me for having omitted all those so called species which are made from insufficient specimens. Description of that kind are discouraging about more or less injurious to science. They give trouble without profit. — I now see the leaf I send you. It is a description (translation) of Tuchotomum Coloratum. You may either put it after Tuchotomum or keep it for that short appendix to be put end of the volume for uncertain species.

Most obliged and truly yours,

[Signature]

Send proof per mail with this letter.
My dear Sir.

Cumbria. Nov. 30th 83.

Schinner. Syn. 2d. p. 184. describes M. M. tiliarium, 
and remarks: - Fructus specimina quae sub hoc nomine
et specie de Norvegia accept. Nullo modo a M. Orthorni 
differenti. - at id afer. - Fructus varius represenmur; plant-
nea irascula contra, sed locis copiosa provenient. Non
misi 'Natura humilior', graciliore, foliis minoribus longis
pulvisum planius circulatis, capsula multiora a M. Orthorni
clifford et vis 'Specie', propria. - Clar. Lindberg plantam
Ranunculus rathm. cum Lycopodioi connotavit, quam stain
va multiprocerina, quam capsula longiore et subarcatum ad
naures suos convulso minoribus, distinto. - Schinner
in quito. It should have order. foliis longioribus, angustioribus
rigidioribus, etc. - Both species M. Lycopodioides (but much)
M. Orthorni (very rare) are found in S. We have never seen
any thing like M. tiliarium. Very truly yours d. Dingley.
Prof. Jeno Watson
Botanic Garden
Cambidge, Mass.
Columbus, 0. Nov. 26: 183

Geo. Sereno Watson Cambridge

My dear Sir,

I shall never complain of you critical observations. The more you send the better. My best thanks again for all them.

It is right to add Newfoundland as locality to Utrica phyllanta. As far at least as Newfoundland may be counted with the American Continent. But as we have species from Greenland, we have also to consider those of New-Fundland.

Adding your doubtfull forms of Ortho. anomalam from Nevada is also right.

On Ortho. plane, I say at the end of my note of it that some of the Specimen of O. speciosa that some stems of the species which growth vary according to local distribution, are entirely covered with aerial gemmae, the var. pycnopsum in herbarium. But I do not think that I have described that variety, as it should have been.

Same sent to me came to Sullivan a number of Specimen under the name of O. leucaupsum Sullivan mentions in herbarium. According to
I hope that the lamp is not strand, and it was a lamp to you.

I beg you to answer me.

I am very glad to hear from you.

Yesterday from your kind act.

And that a myrtaceous in my note, too.

James, letter, that all these specimens are varietal of O. spectabilis. It would then be advisable to add as another on of that species, O. leucarpum, James, nst. But there are a large number of these species of James, which have been determined by Sullivan and by myself as referable to other kinds, and I did not find advisable to quote them.

Oxalis. Oxalis, Sullivan, in Oconee, is one of my blunders. It should be O. Canadensis. This O. Canadensis was made by Shropshire from many specimens, and character, are not represented in any of the Am. species. The species should be made.

O. Nogaei was made according to Flora, to a very fine figure, and description. No. O. Nogaei was described with new plate in Flora, to Ochotonera, where Shropshire name it O. (Pollus) Nogaei. I think O. Pollus should be preserved, at least we have the species, as described and figured by 1,000 stands and O. Nogaei should be added to the.

Specimens, communicated by James, with the name of Gummii, elongate, O. g. at this are referable to G. unicolor, Grev. James acknowledges the names as right, and G. elongate has been added to my catalogue by James. None of his names has been omitted or changed without his acknowledgment.
Columbus, 0 Dec. 1483

Craf. Severn Watson Cambroage.

My dear Sir,

From my last note which you had probably not received when you wrote to me about Orthotrichum Canadense, you have seen that indeed the only blunder is that citation in the supplement to the Jones of O. Chisen. Jullis idem: instead of O. Canadense. It is not Sullivan who has made the blunder but myself.

Your statement for authorship of O. Chisen. and synonymy is right. But for the accompanying explicative paragraph, it is not necessary to make it so detailed, I think. What you say in the beginning is sufficient. For me can not find that you write a Manual.

O. Canadense, Bruch & Schimp. (London Journ. Nov. 11 1661) is named a doubtful species. It was noted as Drum. Mus. T. 349 p. 157. and it seems to be the examination of the specimen of the type of Drummond's. Sullivan and I failed to recognize the character indicated by the author of the species.

I left something like which I could write upon the proof, but which I would like to send...
I would be much obliged to you for writing it yourself, as most probably I should forget the matter.

We have discussed the question for this and other species many and many a time with Sullivant. I had just charge of a revision of the genus Orthothrix when Sullivant was preparing the material for the supplement to the Sower. After my most of constant and too hard work on the genus, my sight failed totally, and I was forced to quit using the microscope. Sullivant took the genus after me, with my notes, specimens, etc. and worked at it a while year longer. I was then in constant intercourse with him and of course frequent occasion for discussion. No genus of men so more difficult and even now less clear for the true characters of species than Orthothrix. You will see this in the numerous notes on Sullivant herbarium, notes all carefully made after the long preparation of the specimens for herbarium and certainly Sullivant still knows the genus far better than Ichneum or any living Ichneumologist. Very much yours, A. C. Egermeier
Columbus Oct 26th 83

My dear Prof. Watson,

What you propose about Fontinalis lessleri var. gracilicornis is the best, though indeed that var. heg. what Lindl. separated as Fr. Sullivantii. This was first considered as a forma fennica of Fr. Sessili by Lodd and though he accepted Lindl. Fr. Sullivantii, in later, he could not, when he came to hear it figured for the species, find possibility to separate it from Fr. Sessili, which indeed is already too close of Fr. ohioa for a species. Hence leaving as you say Fr. Sullivantii Lindl. without the synonym, var. Fr. sessili var. gracilicornis or quitting the var. gracilicornis after Fr. sessili in all we can do. And by being having of course the property of the species. As said above, believe had it figured for the supply to the forms but had to abandon it.

Best thanks. Hope you had a pleasant Christmas. Wish you a happy new year and remain very sincerely yours,

T. Desorea. 
Columbus, 9th Jan. 84

Dear Prof. Watson,

Nothing escapes your sight. Your precision is really wonderful!

That Fuchsternum Carolense, as synonym of Placountrimum hemisporum is quoted by Midda, as described by Richard. But I know nothing more about it, nor has it been remarked by Sullivant to my knowledge. The quotation you propose is admirable, but is it not best to say Pluch. instead of Mirha?

Can you quote an author, the one who has given the name of a species, without description? Perhaps yes; as Schumpe quotes wrongly Cryptopoda glohenata, the name given to the plant by Sullivant. It seems however that it is not enough to give a name and thus merit the right of authorship. You will know best. Cryptopoda nervosa Heerow should be accredited to Bruch & Schumpe.

Very sincerely yours,

A. Negreneau

Return proofs 284-300 today.
Columbus 30th Jany 811

Dear Prof Watson,

In answer to your kind note of the 28th. I have only to say that the proofs not being forthcoming, I supposed that there was some good reason for the hold. Well knowing that under your direction, all is going forward as favourably as possible.

About Phymoreuia hians, Lindl. — I have never considered that species a good one, and there is good reason to admit P. Hookeri, instead of P. hians as Lindl. himself made it a synonym. In that case also Gymnocardium Turbinatum, Medly should be given a syn. according to the note of Lindl. (p. 51 foot note.) But we have not seen the specimen of Medly and should we have seen it, we would have considered it a mere var. of P. pyriformis; for as said above, neither myself nor Sullivan have found sufficient reason to separate that P. hians as see in the Medly cause.

Anamia hygromelica is Linn. in Schiz.

Syn. and after that Hedw. et omnes annectunt revelation. Hi, also Hedw. in Muller. Hedwig being the first author in this century, I suppose.
Funaana microstoma i. Rich. & Schabb. neith. Schott! nor. Schneid, at last to my knowledge. Possibly, the report has written Schott! for Schimp. or i may have made that mistake myself. But Schott! and i are the authors of that species.

I will see about the habits of Brachaniumiformes, as i do not remember what i have written.

With best thanks for your notes,
I remain as ever your's
J. Lequenne
Columbus, Jan. 8th, 1844

My dear Prof. Gray,

Allow me to write a short answer to your welcome and third letter of the last day of past year.

All what you say of your much-needed assistance and hard work in helping the preparation of the manual and the index is so true, indeed, that I could and would say on the subject much more than you do yourself. It is the sense of my obligations to yourself and to Prof. Watson that has induced me to write my last letter to you, and I should have been very glad if you could have proposed some means by which I could be able to repay partly, at least, my heavy indebtedness to Mr. Watson. I can offer nothing to you, for I well know that you would not accept anything from me as a compensation, even of your last form. The world is not yet ready. Perhaps before we are out of it, you will have opportunity to see in what way I could show to Mr. Watson...
how much I feel myself indebted to him. For, time is money, for him as much as for every other and I do not see that it would be right for me to accept without retribution so much of his time as he has to give for a work to which he is not directly interested. It is for that reason that I proposed and still propose to have Mr. Watson as a third of the authors of the book and to pay him one half of what I have received from Mr. James for the prospect of the work.

It has been bad for me that I never had a better schooling than that of friends and advisers who did not have that pecu- liar mind so valuable for studies of natural history. I never thought of any higher authors on molluscs than Schwager and Cuvier. And certainly, the manual of molluscs with the addition of the synopsis of the genus and the they advised to you, was as good for publi- cation as is the synopsis of Schwager or the Mollusca of H. S. by Stullard. There is even less

citation given to the synonomy in the Jones
and in Schwager’s synopsis than in my

But now, I see how far I was of what
is needed to be right and I feel, every
time I read proof, more obliged to you
self and to Mr. Watson for the assistance
given to the publication of that work which
will then become not only valuable to student
but honourable to science.

I am obliged for the discovery of Graf
Wallerstcin of Munchen. But I have it already
time, I owe him the communication of a copy
of all his works and he has the kindness

to send me his memoir or publication in

ampellet form.

"Very sincerely and respectfully,

A. Lucarey"
Columbus 0. 10th. 984.

Dear Prof.,

I have decided to the last meeting of the Genus, at least it is so in my list. Will there please to let me know. It might have been forgotten by the curator and after myself. Very truly yours,

[Signature]
To Prof. Sereno Watson
Herbarium of Harvard
Cambridge
Mass.
Columbus Jan 12th 1844

Dear Prof. Watson,

We have sometimes spoken with Dallwitz of that Cypriota inlands & Nees. and remarked on the impossibility of referring it to something known, at least from the characters indicated by Nees. Nobody has seen anything like that, and it seems that such species are rather to be omitted than described. That was Dallwitz' opinion and my own too. But as it is described in the More of U.S. perhaps it would be best to copy description and remark of Dallwitz. Then do it if you think proper. I add that description herewith.

The terms are much mixed in my mind, I do not understand well the difference between Secion and subgenus. I think that the term Section might be better appropriate to a large number of subdivisions of a genus like Hypnum. Though it may be, it is certain that a key of them would be of advantage and I will prepare one immediately and send it to you in time.

The glossary is not begun yet. I added to
Prof. Gray if such a glossary would be advisable, but I had not any answer to that. It can be done in good time, if you advise me to do it.

I am much obliged to you for preparation of the Index. I had begun that in reading the proofs. But later, I would scarcely do it without blundering from.

About a 5th plate, I do not think it would be possible to do it. We could not have figures for the section of the Hypnum and for the few other genera not figured, the omission is not important as such small figures as those which must be made could not represent the characters in a sufficient manner to be of value to students. But besides, I have nobody here and know nobody who could make the drawings from anatomical examinations. I thought that James perhaps could do it. I am unable now to see the microscope even for a short time.

But as schemata has copied in his Synopsis many of succulant figures, I could make a good plate from cuttings of the figures of yourself and Prof. Gray, which I addation advisable. Then let me know as soon as possible I suppose the engraving would take a long time.

What you have the kindness to say concerning my letter to Prof. Gray and my proposition of your connection to our work as author has deeply impressed me. If you will not accept any remuneration for the great deal of time you have taken to the preparation of the Manual, I feel myself really bound with an debt to you. This came to my conscience that if it was a debt of money, gratefulness of heart and the named Pomille should not compensate the slightest part of the time you have given to the preparation of the Manual. That I have not been paid for my long years of researches in Mora, is no reason applicable to you as I worked for my share in scientific pursuit or for myself. I hope therefore that you will still allow me to come again to the subject from time to time.

What James could have done for the correction of the proofs, and the preparation of the types for the printer could have been nothing at all except under your's and Prof. Gray's direction.

Very sincerely yours,

[Signature]
Columbia, 0 Feb'y A 1884

My dear Prof. Watson,

In the last revised proofs sent to me pp. 212-225 I find still less to correct than in the last ones. The following are scarcely worth mentionning:

p. 246 bring: upwards for upwards
p. 267 4, 8 from base: conical - cylindrical for conical, cylindrical.

274 10 from above: simple, narrower, or...

Very truly yours,

\[\text{Signature}\]
My dear Sir:  Columbus 0 7th Feby 84

No. I did not know about the localities you mention in Oregon and Montana for Parthenium hortorum. If you have not yet sent the revise, please erase my note and substitute the localities: Oregon, (Nevia, Hali); N.West Montana (Watson). If I get the revise, I will attend to the matter.

Please excuse me...

Very truly yours,
A. Ewing.
POSTAL CARDS

NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE

Prof. Sereno Watson
Botanic Garden
Cambridge
Man
Columbus, O. 12th Feb'ry 84.

Dear Prof. Hutton,

I return 2d proofs pp. 316 b 336 b. and keep them pp. 182 b 191 where I found nothing to correct except perhaps p. 186 where the reading should be, I think, Tejaphis, instead of Sphenopodes and p. 191 Taylors instead of Tejaphes. The reading being Sphenopodes does not seem right with the 1st species of p. 191 which is T. victor. If there is a rule for that I do not know it hence please scan if I am not right.

You will see by a pencilled note that, indeed, we have not seen as yet any specimen of Rodhame stromiformis from the Pacific coast. From Japan and Bering's strait, yes! I believe that if the monow, then, Bolander and Hall would have found it in California and especially in Oregon.

You were right. That name is Schmidt as marked in our first catalogue.

Very truly yours,

P. Seguier.

* I have not got revise of pp. 175 end to 181. I should like to have them as soon as possible, for the Library.
Columbus 18th Feb 16th 84

My dear Son,

I return to say the two packets of proofs received this Morning. I hope you will receive them all well.

Very truly yours,

J. Hayman

Will meet next week and send the analysis of Hayman.
Nothing but the address can be placed on this side.

Prof. Jeremio Watson
Botanic Garden
Cambridge, Mass.
Columbus, Dec. 23* 84

Geo. J. Watson Cambridge.

My dear Sir,

The best is to abandon that N. 172 which is so far a species that I admitted it with reluctance; like many others made from poor sterile specimens which rather serve to embarrass without the slightest advantage. — I also think you are right to take up the species name H. furfuraceum for H. orthocarpum.

I send herewith a third of systematic division of the subgenus of Hypnum, or rather a hint of it; for no systematic arrangement at present well defined can be made of the subgenus or subdivision of Hypnum. I have tried hard and done the best I can; but you may judge that insufficient, and if you will kindly suggest amendment, I will try again. I will send money on Tuesday next, the plate VI. which I had made here partly copied from Schimp, partly from the Icon. For genera, Thelia and Notothamnion being introduced, Schimpfu's plate could not be used for photography. The plate, I think, will be satisfactory.

I am more and more grateful for your kind offer and the care you take in preparing and engraving the work.

Very sincerely yours, V. Leuckart.
Columbus O., Feb 26th, 1844

Prof. Watson Cambridge,

My dear Prof.,

I am the more disposed to make the correction you propose in considering Hylomma pallidum, or a mere variety, that I have always asserted the fact, against Austin & James who both sent me under that name a number of specimens, all clearly referable to the common and variable reptile. But as I have not taken to the microscope again and have not seen Schumpei's specimens, I had no right to eliminate the species, though I now subscribe that it has been done by Lindberg. I am much obliged for the correction, more and more admiring the depth and care of your researches. When you call yourself stupid for having missed the correction of one letter, what name will you apply to me who had been and am still blundering along, without any number. I think you will find more to say in the Fluricapi.

Very sincerely yours,

T. Am query
My dear Prof. 

In the verse p. 221 to 243 we this morning, 
I find nothing to correct but the few unimportant 
misprints which I should unprint, perhaps carelessly. I note them, however,

p. 229 line 14th; p. 233, line 11th; p. 241 line 18. Chestnut.

p. 230. B. provincie, Hal. - Not rare in Fluminia,
for Florida (Garber)

p. 231. 1st line. Did you not write formerly - Cypr.,
instead of Muse ?

p. 234 3rd line. The first sentence, if you cut it, after
given characterized by a Q has no verb and not no-
minimum subject, the punctuation should be (?) after
characterized. (I think)

p. 236 after apes / is on. This form is.

p. 240 lin 17 : after Direction.


That is not worth while correcting. But you
sure me nothing to do.

I returned 1 noon and 2 p.m. manuscript this
morning. Hyacinth compactum, Muller, was sent
me from Europe with a very good precise description
as a new species, *Hymen Barberry*, from Utah. You found it in Nevada. Most of the American moths, sent to Europe, not determined are generally considered as new.

Yr truly yours

[Signature]
Columbus, 0 28th Feb 81

Dear Prof. Watson, Cambridge, Mass.

My dear Sir,

I send herewith plate 75, which I hope you will find satisfactory. Two of the genera, Thelusia and Stereogonia were not given in Sullivant's work. They are more important than any subgenus of them, I have selected from Schimper. These were easily recognizable. Nothing new will be needed for engraving the plates but the change of the name Hypnum Tab v into that of Breckythercia. The plate is drawn in ink with pen and can therefore be photographed for the new process of engraving, I suppose. But this I know nothing. Prof. Gray will have the kindness to keep an eye on and let me know about the bill which I will pay when received.

Very truly yours,

I. Serreneu

The glasnye will be ready in time.
Note on H. gracile, Brit. Florigens.

To be accepted or modified by Mr. Watson.
This species is extremely variable, according to its location. H. pallens, Lindl. & Nutt., has been described by Schimp. "Syn. Fl. U.S. (Ed. 2), 611, but merely differs from the normal form of H. gracile by the leaves narrower, longer acuminate, those of the branchlets more papillose and sharply serrate at the apex. According to the same author, H. varium of Drummond, Collection (no. 140, 141) found near St. Louis is intermediate between this form and another considered by Sullivant as a variety, H. gracile var. H. gracile; while H. Virginianum, 1st ed., already described by Nutt. "Fl. U.S. tab. 36, fig. 18, is recognized by Lindl. as Thuidium lancastreum, but the var. lancastreum of H. gracile, var. lancastreum.
Columbus 0, March 11 84

My dear Prof. Watson,

Your kind letter received yesterday sur-
priased me very greatly; for I was expecting great
objections against that idea of the subgenus, and
really, though I have searched very much, now it
would seem better arranged, I have found nothing.
These should be a (3), as well you remark, before
W15. I write one, but see what it is! a mere
cherille as we say in French, and I cannot say
nothing else, indeed! I supposed that the
change of sign + instead of \\ would be sufficient,
but I feel that something is not right there. The
description of the character of the subgenus are right
but there is no character common to all.

About Oxytidium. The genus is so close to that
of Scorpidiun, that I think I united under this
last denomination the two species. H. rigorum
is the surprimus. I am however not certain. And
therefore send description of Oxytidium, in case
H. rigorum should not be described with
Scorpidiun. I have no copy of the last correction
made in the manuscript and therefore am
reluctantly forced to give you the trouble...
arrange that matter, which, I know, you can do much better than myself.

I have received yesterday with your letter the pages proofs pp. 275-278. I find only one correction to be made, p. 276. 4th line from the base. If it is short line, please substitute: leaves squarrose—spreading for leaves squarrose, spreading. The leaves are spreading—squarrose only when most only open or recurved at the apex when dry. That might be named sub-squarrose? But the character is explained in the note following the diagnosis...

Do not spare me your remarks and criticism. They are always received with gratitude. I shall be immensely glad when the Manual is finished.

yours sincerely yours,

J. Leopoldus
Columbus 0 March 11 1834

My dear Prof. Watson,

I had forgotten the locality of Hosteria Sulivanti, in California. It is all right. The specimen have exactly the same character as those of Ohio. Is it a Hosteria?

For Hosteria anomala, Mill. (Pachyleps anomala, Schwaeg) please see Millen's remark in Journal Ann. &c. XII, p. 397. with which I fully agree. That moss has not the character of Hosteria, but is evidently Asianal. I don't think it should be admitted in the Manual. Nevertheless, and to show that it has not been overlooked, it could be annotated upon by a short note like the one hereafter.

About Hagrotherium Domininum. I have marked it (in my first manuscript) as var. of P. Dentiaculatum, or Lindberg's et. decribon. As I have not seen that variety nor received any sketch from James, I do not know where nor on what ground Sulivantia insinut it with P. pulcharum which itself is already difficult to separate from the small forms of P. Dentiaculatum. Please to mention where Sulivantia has made a
remind about that now, and will arrange matter, when we come to the
Magisterium.

Most thankfully and truly yours,
L. Aquinas,

I return proofs pp. 459 - 470.
Columbus, March 15th, 1849
My dear Prof. Watson,
This morning, I received a packet of proofs and a letter. I will do the former this evening, sending proofs 491-512.

Anemone longifolia, Hrnk., has never been found in America. There is a mistake, I believe in Hrnk.'s arrangement. The specimen sent to him as D. longifolia ? have been recognized by him as D. alternata. There might be perhaps something about that in Hrnk.'s Collectanum; but I do not think so, as I have seen often through his specimen.

Though Omaha, fig. T. v. is figured from Hymenographis, it is quite right as Omaha, the Hymenographis Omahai is the more reliable character. But Hymenographis T. v. is not right now; the name should be Brachythecium.
The tabulose figured in PR. 6 are, as far as I can remember, for I have not copied the names separately. Though

Thelia, Tichotrichium, Thamnodium, Thalca, Tetrimeria, Tichelium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thamnodium, Thami...
There are some other unimportant genera which are not figured, as the descriptions are sufficient without figures. And also I have nobody here who could make small reduced figures, and the anatomy of any species of monos. And therefore had to copy from Schimper's. For the species, at least those taken from Schimper, they can not be given; Schimper has not done it and his merely figured fragments on them make the characters of the stem, and capturing other species most distinctly showing the characters of the fructification. At least I suppose so, as he has given Sullivan's figure of Hypanum Wrightii for the Homalina which though right for the genus cannot be indicated for that Hypanum. Of my figure, Thelie is taken from T. hidella. The others are from Schimper.

I see in my note, that you have marked the genus Septedonum as 102. It should be 103. I suppose it has been corrected upon the proofs. Well! I think you should do the near end of that section and ennumer, work which you have so generously undertaken and so laboriously and admiringly performed. I shall also be very glad to see the end of that work.
Columbus, March 21st, 1844.

Prof. Jerome Wattson Cambridge.

My dear Sir,

I have received yesterday and arranged to day, the description and synonymy of Hyponium fulvulum, according to your notes for which I am much obliged.

About H. pallens, Lindl., I know only what Schimper says in Syn. Mus. T. 19, 611, where Pneumonia pallens, Lindl., was described in the length, apparently from specimens received from the author. Schimper remarks that the species is similar to the Amer-
ican H. gracile by the inflorescence, the capsule and the form of the pericar-
pus; but differs by the stem leaves much narrower, longer, and longer
acuminate, not sulcate, those of the branchlets much more papillose-
and sharply denticulate at the apex; the pericarps of the capsule thicker.

Of Hyponium varium of St. Louis in Drumm’s Musc. Amer. Schimper
says, i.e. that it is intermediate between H. pallens & H. gracile, diffe-
ring from both by the base, nearly smooth.

Of course H. pallens Lindl. has nothing to do with H. Lancastriense,
a variety which I have long time studied, and consider it different from
H. gracile, the common form in Tullis’ Musc. Alp., having the
base, much longer and longer acuminate, as described for H. pallens,
but not narrow. Of Lindl. has acknowledged identity of H. pallens
with H. gracile, it is of course useless to publish a describ it.
what I say above, the only difference would be for H. pallescens. through branches, though apt to
the narrow leaves. That form of H. robustus has been mentioned with
H. opacilus. Could not H. Virginianum be added as synonym?

I will mention that Anabasis esculenta B. Korp. But I
believe that it is H. compactum. M'Keeh., which is H. vagum var. compactum
Drum. differing essentially from H. radicans by terra leaf. I have
not seen any specimen of it.

Hymen Polhilli Austin is unknown to me. I have not received
that N. (1873) from the President of the N. Y. University. Will meet to day
to have it, if possible, and will describe it or say what it is, if I
can find out. I will also describe H. turcicum, which is a very fine
mon. and send description with that of H. Polhilli.

H. turcicum for H. ortusiam is very acceptable and
for all your remarks, I am sincerely grateful.

I have just now received galley of the first 10 pages of
the manuscript of Hymen. But the key of the subgenusa is not
with.

Yours sincerely yours

A. Bergmann
Dear Professor,

In the last proofed proof pp. 284-298, should not the heading of p. 286 be Meteorium, instead of Homalix. — P. 288 line 20, after chestnut-color, instead of red; right? — I have found nothing else.

Very truly yours,

[Signature]
POSTAL CARD.

NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Prof. Sereno Watson
Botanic Garden
Cambridge, Mass.
Columbus, March 27th, 1834

My dear Prof. Watson.

I must thank you to these end and never so much as I should like. Now it is for Leptodon Floridanus of which I will take good care for additions. Hypnum Virginianum has been mentioned in the returned galley and from a note received of Dr. Reid, Mr. Reid, Austin is a more Steele year of H. Formulation, this, concealed by Austin. Mr. Reid has the kindness to send me a specimen from which I can see or rather to see, I can see it refers to the variety Subjulaceum. I will mark it in checking the proofs. As for my fortunate escape of those 29 new species of Mueller from Belling. Should we might take them on hand in the additions. But as you are doing most of the work now, the escape is more to your advantage. Indeed I do not know how I could have gone along without you assistance. And just now, my health is much worse and every thing is more more difficult.

As there are a few corrections to the pressed proofs, I return the proofs for your inspection.

Very sincerely yours, A. Desjacques
I now receive proofs [galleys.] p. 531-542 and will try to read them for returning them to morrow. But I have not left my bed for five days and my poor head turns like windmill. Still so the best. And indeed if I should stop work altogether, it would not be much lost as yourself are doing all the world. - By the same mail I have the obituary notice of Dr. Engelmann. As I am the oldest of the lot, that is encouraging or promising. I do not understand how the notice has been delayed for more than one month. Dear Engelmann, I have seen him only once at 15 deuts, but to see him was a love for him.
Columbus, April 3, 1884.

My dear Professor Nation,

I return to-night the galley of p. 571-607 received this morning. That H. dentulatum var. Donianum, considered by Sullivan as the true Plagiatherium juliferum Richh., might be put as near as I have marked it in pencil. But as you do not mention the N. of that Drummond moss, and as I find no record of a Drummond moss in our locality in the Bully Moss, it is quite as well to leave out the remark of Sullivan. Any way you choose, I shall be pleased as I know you will take the best one.

Very truly yours,

[Signature]
Columbus, Oct. 24th, 1884

Prof. Sereno Watson.

My dear Sir,

The proofs came yesterday morning and were returned to mail in the afternoon. The morning of the 24th was received only in the evening, some time. Some of my letters are delivered to my son's store and they bring them to me in the evening only after business time.

That Hymen oxyclades has never been mentioned to me by Halberstadt; at least I have no note about it and of course no recollection of it. The species is a Camptotheca, as far as I can see by the description and quick reply. C. rotund, perhaps merely that variety found only rarely which is described in the manual. I think it would be best to include it in the addenda. It is very regrettable that we can not have a specimen.

The glomerar can be ready at any time, and may be placed in the 2nd in the third part of the book. Please let me know when to send preface and glomerar. For though I wished better and up most of the time, I can work very little. — You have taken from my shoulders the big load of the work about the book and the trouble also, in such a way that thanks to you all is going greatly for better than
both James and myself would have been done
it would even with the assistance and direction of
Schimper. Hence I can but consider the authorship
of the work as legitimately due to you as to me.

Very sincerely yours,

L. Agassiz
My dear Sir,  

Columbus 015th April 84

There are in the last proofs (revises) a few errors which it may be worth correcting. But perhaps it is too late if you have seen them already.

307 Counting from above line 24 asperella should be asprella
307 7 Mount Diablo in Diabolo
315 9 Margins, margins
315 15 as long as or longer erase as

Very truly yours,

[Signature]
PROF. SERENO WATSON
BOTANIC GARDEN
CAMBRIDGE, MASS.
Columbus 0 April 17th 84
Prof. Sereno Waston. Cambridge
My dear Sir,

I received yesterday evening your kind note of the 14th. As the Glossary was ready, I send it today with a kind of preface which I have arranged as best as I can. You will, I know, have the kindness to read the glossary and make to it any correction you may think proper.

For the preface, I do not know what to say. It is perhaps too long, but I do not know how to shorten it except by erasing the whole. I am delighted with what you say of Prof. Gray and Mr. James assisting to your co-partnership or authorship. You can not refuse that. If you have not used the microscope for the examination of moths, you have studied them in the books much better than any author, except Stebbings; much better than Phipps, especially. You have collected the most rare species of the Pacific. You have indeed by your assistance in the publication of the manual made and wrought more than Samu and myself. I sincerely demand of you this new favor which I would consider...
the greatest service you could ever render to me. I am under such great obligations to you that if you will not take your right as author with some and myself, I can never get rid of a painful sense of indebtedness that I am unable to repay and that has constantly troubled me since you took this booth under control. Please do not refuse this last service to me, and to the end of my life, I shall remain your friend.

I will now give the addition of species.

Below these you name, I have in my memorandum Hypernum Junceum from Davis's trail.

It + Vasechkin collected by Macoun's hand.

I plunge, in rather Raw

It + Phylocladus, common in Florida.


Shall I mention some new localities which seem remarkable enough.

Hypernum Alpinum, sent to Raw from Florida.

Pterocarya Wilsonia has been noted from N. Jervis & Audubon.

Phlox oblonga, also by Audubon.

Anementium Moenchchicenium N. W. Coast. Audubon.

Dichantium Richardsoni; Hawk.
Columbus, 0 April 19th 84

Prof. Sereno Watson Campb.ude.

My dear Sir,

I have received to day and read
paged proofs 351-373. Few corrections to
be made but some remarks which are
perhaps of no moment.

p. 351. Line 8 from bare after pseudo monacine, 
   narrowly rhomboidal, exeo-

p. 354. Line 8 from bare. For tendril you substitute tendril.
   I think it is right.

p. 357. Line 9 from bare. Derramum for Derramum.
   p. 368 line 13 from above. The localities. Adrondacs 776
   As H. Peak would be added, there it plain in there.


Would it not be advisable to add after this
page the note in Sullivans Money, the U.S.

p. 78 after 17. radiale, about that so called new
species of Schimp. mentioned in Boyd, lin. 

The note of Sullivans would be quite right
and say as much as Schimp does on the
character of the species.
And also, would it not be right to quote the type, H. graminicola (Bud.) Wats. & Hall in Drum. J. more, N. 133, as synonym of H. Sullivantii, which according to Muhlenberg, has been found in Penn. by Muhlenberg. The ? after Bud. renders the synonymy uncertain. Bud. Sullivant has quoted it in that way in Mon. of U.S. p. 532.

If you find it right, that could be mention in the Additions, and Corrections, end of the volume.

I have written already the descriptions of

Bromus delece (genus) and B. Wilsonii,

Philomitra tenella, two species in Hand.

Harvey Catalogue, put their in the authors of Austin as found the first in New Jersey.

But the last, P. tenella i, not neede-

as it is described as, of P. Muhlenbergii;


Then Iphigenia floridana, and H. Hybium

vulpinodum Bud. H. Hybium Vaucher, 1843,

found sterile in Canada by McCrea; according to Austin, H. uliginosa, I think also said to have been found in N. America (Austin)

Hybium fugecens, Sch. Davis Strauss.


Florida.

I may send them pages now, if you like.

But as you will send me a copy of the whole descriptive part for Index, I may in reading it again, find some more addition or correction to be made.

If I shall make the Index, please direct me how to proceed. For names, in Roman

or Italian, is not that of Sullivant in

Mon. of U.S. well arranged. Would you follow it?

Very truly yours,

[Signature]
Columbus 23rd April 84

My dear Prof. Watson,

I return today the galley pp. 651-694 end of the descriptive part. I find nothing to correct. Hereewith you will find the few additions and corrections that I have noted. Three of the species: Hyacinth alpestris, H. Angustifolia & H. planum may be added in their normal place to the Galley; as you will see, their place being moved at the heading of the pages. I suppose there may be more others. In any case, I am ready to follow instructions, if you will have the kindness to give them.

If I have to make the plates, please to say if I shall follow Sullivant's for arrangement of type. There may be a few words more to add to the dictionary. But if I should have left out of it all the words described by Webster, very few would have been left in it.

I will read to night proof proofs 374-388 and trust I find no errors. Remind that the last page 388 is written 338.

Very truly yours,

J. Keayman.
My dear Sir.  Columbus April 23rd 84
I find in Bot. Gaz. 111 line 6 from above the following.

There is another sterile (always?) Bryum growing in great profusion on the White Mountain associated with the preceding (Bryum Rarum) and with Lepidobryum
hymenastum. It resembles Mielichhoferia melida in most respects, but is larger with the stems often flagellate.

From New York to New Jersey etc. etc.

Is it worth to take notice of such a remark and have a species made of that? Please see about it.

and believe yours very truly,

1,372 l.12 from top upwards crass &
1 8.13 sub segments shet s.
379 l.11 from base crass at base or below
388 r. printed 338. I find nothing more.

J. Lequeneur
POSTAL CARD

JUN 5

APR 24

2 PM

THREE CENTS

NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Tof

Gerena Watson

Roland Jardine

Cambridge

Mass.
Columbus, 0 April 26th 84

My dear Mr. Watson.

I return herewith the Introduction with scarcely any change; of course, if there is anything not acceptable to you, you will please let it right just as you think proper. I have regretted all the time that I did not abandon the work, but now it is too late. That should have been done after its first examination by Prof. Gray and also, I think, by yourself.

Mr. James said, I think that I should be entitled to my share of the volumes given to the author, 25 to each, if I do not mistake. You would pay me a service to direct Mr. ed to send me only 12 copies and deliver the others to you or to Prof. Gray for distribution according to your liking. I have three or four copies in Europe: one to Lindberg, one to Donauld, one to Schimper, the fourth that is not the editor of the Revue Bryologique who will give a favourable mention of the Manual. Donauld writes me that the book will be and is already in demand in France and that it would like to know if the book will not be put for
sale at some French Book Store, that should be done at once. I will also, if I receive my copies, send one to Carl Muller and ask him for some mention in a scientific German paper. Schonberg can do that also. He is now at Bonn, returned of his travels. In the country, I have a copy to buy. I shall send, and perhaps to Prof. Porter. I will send one to Kolander. I mean in this, in order that if yourself or Prof. Gray distribute any, the copies should not be sent to the same. Prof. Gray will know if Prof. J. D. Dana or Prof. Sauer should accept one. I may also send a short notice to some scientific papers, and perhaps, I should send also a copy to Coulton Gray and to the Times of the N.Y. University press.

Yours sincerely yours,

A Sequencing

Perhaps, for distribution in Europe, the best would be to send some copies to Mrs. James, with a list of addresses.
Columbus 10 April 26th 84

Prof. Jerome Watson Cambridge.

My dear Sir,

I return this morning the explanation of the plates received yesterday. I thought at first that what you said of the figures taken from Schimper did not relate to the explanations of the plates in the volume of Schimper's Synopsis, which Schimper prepared in imitation to those of Sullivant; but that it related to Brief Schimper, etc. etc., which table was quoted in the explanations of Morse of United States. Therefore the whole may be left as you have written it. The idea added to the perissotom of Pseudolebistes, or those of T. catenulata. I thought best to show the perissotom (lebert) of this last species, as that of T. abronnus is not hypomorph, but if a chapter of figure appears or the volumes. It is therefore of Sullivant, not of me.

I have just received your kind letter of 1st of April and the introduction (should not that be a preface?) and I feel really greatly troubled with what you say of your work and the little part you take for yourself who should really have the best. I can not take upon myself a load of
indebtedness when I do not see the mean of repaying in any way. I feel that with your precise mind, with the exactness of your views, you can not take part of the responsibility of a work which, as you say with right, should have necessitated a more through revision than that which you have made. And therefore I say that you do well not to accept that responsibility. But it is unjust to refuse the demonstration of all that you have done for the Manual. — I can not write now. My head is in confusion. I will perhaps say more after noon or returning the Preface.

The just cited suggested work: Synopsis of the prose of the North American continent. Either yourself or Prof. Gray remarked that the work Manual was best. But as the work is done now with addition of a Synopsis much more perfect than that of Schumpe or Melle and the description are somewhat long and detailed, it seems to me that the work Synopsis should rather be accepted or acceptable. For this as for all other matters, I shall gladly submit my opinion to you. Very truly yours,

I. Schaefferes
Columbus, April 28th, 1874

Dear Prof. Gray,

Will you grant me the favor of a short note and call for your assistance on a matter of great importance to the work on American manuals which is about to be published by the Bureau of Education. I sent a brief of the original draft to Prof. Watson, who revised it, and the result is now in the hands of the editor. I am anxious that you should see it before it goes to press. I am sure that you will find it satisfactory from every point of view. I am, therefore, asking you to give me your advice in this matter. I am confident that you will be willing to do so.

Yours truly,

[Signature]
my conscience also. I can not accept without any kind of remuneration, the time and
work given by Mr. Watson to the Manual of
the moose, and though I think and hope
to be able soon to offer him a compensation
in money, I wish the biologists to know that
part of the work is not of me, and that
Prof. Watson has taken charge of the revisions
against great long time and much trouble etc.
When the Manual on Synopsis of the New-
Mone of North America (I have not yet
fixed the title; could you not help to do it),
is ready, I will still take the liberty to
truit you again and express my obligation
to you; for you have encouraged, favored
and brought out the publication of the book.

Very sincerely and respectfully,
your old friend,

E. S. F. Lequescuy
Columbus April 30th 84

My dear Prof. Wilson,

did you or I get your letter yesterday, I wrote to Prof. Gray. But today, I think it would be advisable to show him the preparatory face which you returned to me and which I believe, he would approve rather than yours. Will you have the kindness to show it to him? I am sure, he will judge the matter better.

Very truly yours,
L. Foreman
Columbus, 02 May 1844

My dear Prof. Watson,

As there are some errors, I return the proofs of pp. 389-411. I do not know where I have seen baccate with two n, and I cannot find. It is probably one of my papers in which your addition to Species of uncertain affinities is quite right. That locality, which was called Summit or Summit Forkage before the building of the P. O. railroad, the Allegany from Atena should be named with a &; Summit or Sum-

nifville, as it is now marked upon the Geology.

But perhaps, Summit Forkage is better, as the village Summerville was not in existence when I collected more on these mountains. I am glad these species could be described at their place. There is scarcely any addition to make.

I got the proofs yesterday evening. Perhaps it would be best to return them to me if I have to read the whole work through. And my table of proofs is incomplete. I return a parcel you had the kindness to send me before.

Baccate is not described in the Gomosy plan, so I put it in. Very truly yours,

[Signature]
My dear Sir,

Glad you have found that. I could have seen it only in preparing an Index. I think it one pathulum would do. It is according to description or descriptive of the species, semi-pathulum could do but less appropriate.

Very truly yours,

T. Ferguson
Nothing but the address can be placed on this side.

To: Sereno Watson
Botanic Garden
Cambridge, Mass.
Columbus, May 6th, 1844

My dear Prof. Nason,

I return the proofs of additions and explanations of the plates. — That H. attenuatum, Brud., has all the characters of H. lactum and could not be mistaken for it, if it was not that this last species has a discursive inflorescence. The character are not all described; the costas, the aestivation; the color only is given. It is therefore difficult to say what it may be except that it is not a Eucynchiium. — There is still another blunder caused by my trusting implicitly to Schinz, as I have always done, to my great regret. H. murale is not a Eucynchiium but a Rhynchoscholeum. As you remark that the plot V1 is entirely made up from Schencke, this case be left unnoticed for the character of both so called genera are the same, some plain sight or without microscopic examination, or even with this. But the figure is like that of Eucynchiium, crassinervium, Faye, and if you think best, this name might be substituted to that of H. murale.

Very sincerely yours,

[Signature: L. Lequerum]
Columbus, O., May 8th 84

My dear Prof. Watson,

Will that do? Please give me your assistance to the end and correct, if the title does not seem right to you, Manual would do as well as many.

Very truly yours,

I. Ferguson

The sentence between ( ) may be omitted.
Synopsis of the Mosses of North America (including New Mexico and Greenland)

By Leo Lesquereux & Thor. P. James.

With six copper plates illustrating the genera (and subgenera of Hypnum)
Columbus 0 May 14th 84

My dear Prof. Walker,

I return the proof of the preface which I find appropriate. I am much obliged to Dr Gray for the modification, only I should have liked them still more, if he could have said something of my obligation to him in his preface in the first direction concerning maps etc. as exposed the necessity of a revision of the whole world. I shall write him again when the world is out. For my obligations to yourself, be sure that I do not find them canceled by what is said in the preface of your cooperation to the work and though you may say on the contrary, I shall not rest content until I have given it you an evident proof of my gratitude. I will understand that you worked in remembrance of James. And I nevertheless reap the profit of your work.

When you know something about the distribution of the volumes given by the publisher, you would greatly oblige me to say for whom I may distribute the few copies I have to get. As I sent it to you some time ago, I should like that half of my share should go first to yourself and Prof. Gray.

Very sincerely yours,

J. Desguerre
Columbus 6 June 14 1844.
My dear Prof. Gray.
The manual has come yesterday 25 copies, and really I have seen the book with great pleasure. It looks well, attractive, and will be of some advantage for the study of botany. I wish I had had such a book to study the moner - You will be surprised, I think, that I dare to take the liberty to send you a copy. I well know that you may have one or more from Mr. Jarm or Mr. Wother, but you must have the kindness to accept one from me and to keep it in your own library in remembrance of your most charitable assistance in the preparation of the work. I owe to you not only the critical remarks and the valuable directions received before the beginning of the printing, but the book was to you also all the work made by Mr. Wother, who could not have been induced to give his time to such a work without your instance. And for all that you did not even allow me to acknowledge my services to you in the preface.
I know that you have done a great deal
of work of that kind, in assisting others without accepting any reward. But it is no reason why I should accept your kind offices and say nothing about them. I can do nothing worse however, but to thank you most heartily now.

To Mr. Walker, I owe still a debt that I cannot fully pay; but that I will nevertheless try to partly cancel at least to quiet my conscience. I know that he will not accept any money from me. But, as I have done only part of the work for which I have been paid, I intend to return to Mr. James, $100, as belonging to Mr. Nation. I beg you will not mention this matter to him. But really I can not accept money which I feel I have not earned in fulfilling a contract partly completed by the work of another.

I suppose that I have to tell you now good by for a long time. I have just completed all the publications which I had on hand and feel that my work is soon ready for the world. I shall however keep on long and I have a deep remembrance for all the acts of kindness and goodwill received from you. Since my arrival in America and a

Please to remember me kindly to Mr. Gray.

Very respectfully your old friend,

T. Jefferson
Columbus 0.14th June 84

My dear Prof. Watson,

Yesterday evening I received 28 copies of the manual of the Muses of North America, but by Messrs. Cassino & Co. I am very much pleased with the book which looks really nice and attractive. About its intrinsic value, you know it just as well as myself. Mr. Cassino says that he sent you also some copies. Don't you like to have any more. I would gladly send you half my lot. Send a copy to C. Miller, one to Sandby, one to France. I never corresponded with Mr. Miller, nor with Mr. Braithwaite and suppose that Mr. James will distribute some of the copies to England. But if you have some acquaintances or if you know of some persons to whom you think 2 copies should be sent, please let me know and will immediately attend to the matter. Of course I will send copies to those who have contributed some material to the book. You, Dearste
Smith, etc.

What now gives me some anxiety, is the matter on which I have written to you already. The more I consider the time you have given to the publication of the Manuel and the trouble the world has given to you, the more I feel what heavy work and loss also the world has imposed upon you. But I know already what you will say, and I do not ask you to answer me on the subject. I write this only to let you know that I shall not forget, as long as I live, the great service you have rendered to me and to my country, if ever I am able to acknowledge your kindness otherwise than by mere words. I should now like to write to Mr. James. Do you know if he is soon to come back to America or could you not give me his address.

I will also write to Prof. Gray and send him a copy of the Manuel. Of course he has got one from himself or from the publisher, but I wish to offer him one myself. He has had a great deal of trouble for the world too and it is to his recommendation that

I owe your assistance.

I suppose that you are now quite pleased to be out of the job (as the Americans of this country say), and that you enjoy the advantage of being able to quietly pursue your labor. For myself, I have often and deeply regretted to have accepted the duty of that publication. But now that it is done, I believe that it will be good for something and acceptable to some.

Most sincerely yours,

L. Seguierzen
July 6th 1844

My dear Sir,

You will have to excuse me once more, for the letter, first, and perhaps for trouble of a new kind. I have sent to those who were interested in the preparation of the Manual of the Mone, and those who could make the work known, 15 copies of the book. I have now 10 left and beg you will accept half of the lot. Perhaps you may later have occasion to present them to some friends of yours or to exchange for some books. I have a copy to Müller, Lindberg, Schimpf for Husnet, redactor of the Revue Psychologique of France, Donald; and in the U.S. to them who have contributed most to the work by communication of specimens: Roe, J. Donnel, Smith, etc. The road is now clear for the future progress of the work and the copies left on hand may be preserved for casual demands.

As I am not in position to receive and read any scientific journals, I hope that if you send them when critical remarks


on the Manual, you will have the pleasure to let me know about them. The book will certainly be valuable to morphology.

I do not like to talk here of you with whom I have had for months most pleasant intercourse and hope to find reason to authorize me to meet you again. In any case, I shall remain henceforth a dear friend of yours, this without claiming any return; for I am the obliged, greatly obliged indeed, while you have had only trouble and hard work caused by your relations with me.

Very sincerely yours,

[Signature]
Columbus 6. Sept. 1784

Sir: Sirisse Wotton Cambridge.

My dear Sir,

I return to day with but thanks, to the N. of the Nation when I read the notice of Prof Gray on the Manual of the monsters. The notice is very kind and good. I think, however, that Prof Gray is mistaken in supposing that Biologists of the next generation may come to reduce the number of the genera and orders and thus increase their weight and value. The tendency is the contrary way, that you may see by Mitter, Lindberg, Braithwaite, &c. The influence of the monster is of the same character for all except perhaps for the detached ones. They have the anther, the antherous, diversely distributed in the same species, as similarly in a large number of different species, genera, sections, &c. The capsule with its modification in the former does not offer any pertinent character one and different which might be relied upon as essential character of species, hence, the Biologist has to rely for his subdivisions to grouping forms,

[Signature]
the ramifications, the leaves, the circulation etc.
which are differently appreciated by every biologist, and have therefore felt utmost for the
basis of foundation of large divisions. After a
long consideration, they even of a single side of
the menu, the biologist is forced to acknowledge
that all division and subdivision are an
affair of opinion. That a group, for example
may be subdivided into Sub and Sub species
or not divided at all. And that therefore, if
we should admit for a genus, groups of species
related by one side only, like Fumitories and
Dianthus, we have no reason to separate the
like Dryas, Monard. Incommene, Barbe
mix etc etc. and therefore give to genus sub
wide extent that the number of species, becomes
more troubiburon for determination than are
the great number of genera whose character can
not be definitely circle-reviewed. It should have
much to say on the subject and may prepare
a third paper on the subject, if I can find at
the time. I have plenty of it now, but the
mind clear enough for the discussion.

I hope that you have had a good time of
rest during the vacations and perhaps have had
an opportunity for visiting and exploring some
countries of the Western Region.

Have you in your own library the Historia
de plantis of Boulton? I have, it beautifully
found except the last published part, the
Melaotinaceae, Conacea and Umbelliferae, not
yet found as it is only half of them. If
you do not have the work and if it could be
acceptable to you in the least, you would come
to me a true favor in accepting it from me
as a token of remembrance of the hard times
you have had with me and for me. As I am
a subscriber, I would continue to receive the
parts as soon as published, (they come very
slowly) and send them to you. I would also be
much obliged to you for accepting my the return
of Mr. James C. Cambridge.

Very sincerely and respectfully your
old friend
J. Asquith.
Columbus 0 Dec. 14th 84

Gent J. Watson Cambridge

My dear Gent,

You have probably seen a Bot. Gaz. last number, the criticism of Mr. I. S. Bower, I had so much to do that I could not look over the matter until to day.

According to Mr. Mobius's statement in letters, the H. Kosmetilegium and Tuckeromi in maconilium are species which have been found in Mexico by himself and which have been casually mixed with his specimens of Mobile in the transfer of his herbarium. But H. thetislegium Mill. of the esculentae bulliv. have been forgotten and I am not in any way account for the Thunder.

For the last species only I find that the last plate 81 of the 2d vol. of the Icones is adhiring to 80 this duly marked as recorded, the other not. But that is a poor excuse. About Diacandra Canadensis, I think that the specie was published by Mutter after I had passed the Dicarava. But that is also inexcusable as it could or should have described the specie at the end of the Volume in the addenda.
Now, please tell me what you think I should do: either send to the Gazette a note acknowledging the omission and giving a short description of these three species or let the matter drop.

Please excuse me. I am sorry to give you the trouble to come again to this subject, the Manual, which has troubled you already too much.

I have a letter from a Botanist in Hungary, Dr. Szentgyörgyi, who sends me a new species of Frasera (Frassera), and another from Prof. E. Pescherel of Paris who says that the Librarian, M. Berger, who received copies of the Manual for sale from Mrs. Cassine has already none left. I must to M. Cassine about that.

Very truly yours,

Q. Paraguay
Columbus O. Nov. 19th 84.

Prof. J. Watson, Cambridge.

My dear Sir,

As far as I have had the opportunity to judge, it seems that the Manual of the Muses has been generally well received. Lately, I have received from Prof. R. Bescherelle of Paris a very kind note on the book, together with all the memoirs which he has published on muses as follow:

2. *Monographies de St. Paul et D'Amsterdam*.
4. *Notes sur les musées de Paraguay*.
5. *Notes sur 3 espèces de musées de la Nouvelle Calédonie*.
7. *Notes sur les musées de Colonies Françaises*.

I quote the title of the memoirs in order that you may see if you have them or wish to have any of them, as Prof. Bescherelle says that he should be much pleased to send them to you in case you would like.
to have them. He was also noticed the Manual in some Botanical Journal of Paris and will send copies as soon as printed.

You have perhaps seen the very fine notice published by Dr. M. C. Coole of London in the last vol. of the Grevillea. It was sent me by a friend. If it was not too complimentary, if I was not interested in the matter, I would consider it as the best notice possibly given of the Manual. Now, we have received, it seems, about all what has been published in favor of or against the Manual and apparently the book is really a useful one or will be so hereafter. Thanks to you for that, my dear Sir, for I feel more deeply every day how sore that publication would have been without your cooperation.

I have not yet written to Mrs. James, but will do it to day.

Very sincerely yours,

[Signature]
My dear Sir:  
Columbus, O.  
May 18 1855.

I owe to your kindness the communication of a number of the Botanische Zeitschung (which I return), where I have read with great interest and pleasure, your article on the Manual of the Musci of North America. Thank you sincerely for it. It is all what I should have wished to say if I had read the critical remarks of the Zeitschung. I should like to write more on the matter and also about more of what I thought that you are now at Cambridge. But I have read it a paper that you had gone to South or Central America for collecting. I hope that your journey will be most pleasant to you, and know that it will be most valuable for Botany.

Very sincerely yours,  
Ed. Lerguereau.
NOTHING BUT THE ADDRESS CAN BE PLACED ON THIS SIDE.

Gros. Lorenzo Watson
Botanic Garden
Cambridge, Mass.
My dear Prof. Watson

...my counter-cultural remarks on the fact, if not merey a propos, but right and most satisfactory, I am sure, to all the Americans, students of Moses. I have thanked you heartily for them. As far as I am concerned, as well you know, and nobody was more interested in the matter than myself.

I am very sorry indeed to know that you have returned still of what apparently was perilous exploration. Science will profit if it certainly but I am nevertheless very sorry that you have to suffer men for your researches. I desire you must that you may be soon fully restored.

You will be surprised perhaps to know that I have sold my herbarium of mosses to the Museum of Neuchâtel, my native town. There are three parties interested in the sale, one the University of Geneva, the other the U. of Belle France. Though my herbarium can not be of great worth, at Neuchâtel, I had to give the preference to an
institution to which I owe my education of youth and which was my alma mater. You
have for the meeres of the U. S. Sullivan's and James collection both more valuable than my own which is
extremely rich only in European specimens and also in Arabic species. — Indeed, if I can do it, I will try
to write an introduction to the study of the meeres, for
I regretted much that such an introduction could not be
admitted in the manual. You know I wrote something
about that matter. But it was returned to me by Prof.
Gray. — I should be much pleased if I was able
to go on Eastward to Cambridge and pay you a visit.
When my money is paid, I shall be in position to
offer you some compensation for the time you have lost
in writing for me. Please say nothing about that. I
will know that you will say no. But I ask that for
my own satisfaction. I owe you much; and though the
indebtedness is not heavy or troublesome to me, I would
be more satisfied, more pleased with myself if I had
in some way shown to you my gratefulness for the
work you have done for me, a work whose usefulness
is every day more eviident to me. If my health should go
as it is now, I may decide to pay you a visit in some well,
very sincerely and respectfully your friend Dr. Congerian.
Fifteen to July 22, 1885

To Mr. Weir, Cambridge.

My dear Weir,

I have been here about two months. My health has been fully restored and quite well as far as now, I shall try to go to Boston next week and hope that I shall have the pleasure to see you. At least I shall try very hard. You would therefore oblige me to let me know when I may send you about the middle of next week and if you are not going to the Solarium for the hot, very hot term, I should go after you, if possible. For I hope that your health is now if not entirely restored at least improved. Have the kindnes to return a line to me care of H. E. S. Fry 92 Fulton St. New York.

Yours sincerely,

[Signature]
My dear Prof. Watton.

Your very kind letter of the 17th Inst. was unexpectedly received with the greatest pleasure. For about three weeks, I have been looking for a reason or an excuse for writing to you, as I am always afraid of intruding and taking your time without good cause. Be sure that I shall always have a pleasant remembrance of our copartnership in working mosses, that I like the dear mosses the more because they have afforded me the advantage of becoming more intimately acquainted with you, and that, instead of you being now my debtor, you have added to my deep sense of gratitude, in accepting that foster of 1, which I hope will remain acceptable to you for very many years to come.

I am much obliged for the communication of that number of the Journal of the Linnaean Society. It is very interesting, 1st for the exposition of Mr. Milton of the genus Frisitena, as represented in Europe and America, and 2d for remarks on a Lycopodiaceae by Dr. Hudson. That remark may also interest you on account of the Frisitena of the Coal. - That Frisitena Halliana, Bell-Lair

Columbus, 10 Sept. 1845
is the beautiful Conomorium Hallianum, a genus which I am not yet disposed to write to Tintorius as it
material does. Perhaps I should have given another
name to that Tintorius Hallii; but this was baptised
by virtue long before the publication of C. Hallianum.

I have 30 mel. N. 8. of Columbus on 1.2 hare 9.
Reaching a splendid small lake, reservoir of Canal,
which in some parts near its borders is covered with
Volumbium Leutum. The shores look a prairie of large
flowers and splendid leaves is admirable, and generally
in these a few times in summer to enjoy the sight.
The stalks of the flowers and leaves have a peculiar
conformation and constitution. Round of five or six small
round around a central large one, the panicles are filled
with small masses which, in blowing the stems, unfold
the threads of yellow, and more filaments of various
lengths, 5-4 feet and are nearly as strong as the threads
of the cucumis of still worse. As you may perhaps
wish to examine the matter, I send you, upon a stick,
the threads obtained byt breaking a single stalk in pieces
of about 6 inches long, together with a loose bundly
other derived from another stalk, where you may see
better the character of the threads. The canal on
the inter parietal veins are filled with a milky fluid
especially obtainable before the ripening of the plant or the
influence. This fluid soon becomes hardened under
atmospheric influence and has the composedness of
gutta-percha. It seems, in bulk in quantities I have got
a few drops in a small bottle which you can break
of you wish to examine its nature.

Very sincerely yours,

- [Signature]

A friend of mine, good Physiologus, Mr. Labbe,
professor in Universit de Stelle, writes me,
asking how he could obtain a few, either exotic or
American for a Botanical garden which he is
anxious to continue, being now its director. Do you
think that I could get something valuable from
the Agricultural Dept. of Washington? The friend
of Stelle is, about that of Cambridge, I believe.

May still! Here is why I wanted to write
you weeks ago, but my poor head becomes more
and more out of order — I send here with a
sketch of these species of Outdoors species, which
I wrote to you when at Cambridge. Here
are a few of them but all of the same informs us
as...
My dear Prof. Noyer,

Your kind letter of the 31st has been delayed for a long time. I was afraid you had forgotten me entirely. But as it brings me kind words and a splendid representation of yourself, I feel doubly pleased now and offer you sincere thanks for letter and photograph. The representation of yourself is very fine, beautiful, full indeed, but no better than the original. I shall value it very much and it will go immediately into my album of scientific friends. When with Gray, Torrey, and others American botanists visit, he will send the two Becandalle, the Adler, Odermatt, Ogelman, dead, Duby, and a large number of other friends though not botanists, like Agapit and Guyot. To deserve the reception of a photograph is authorized to send his own in exchange, and this I do now, though I do not know if it might be acceptable or even polite to do so. You will excuse me any how, I know.
An objection against the referencing those larger appendages to Polygonum is the hard thrill substancy of these joint organisms. I should however be pleased if one of Post perspectum, as I have none in my small herbarium, mostly composed of Kymos plants. For condition and hardness, thoug Coccodora would be quite acceptable for comparison. But the form and point would be different. Long time ago, that is about one year, I went to export the same sketch as I send to you. But he could not find anything for comparison—or merely something that perhaps somewhat like it could be found in the Arabia. These do not trouble you about.

You said nothing on the possibility of obtaining seeds of plants. Hence a tree from the Botanic Garden of Cambridge. Perhaps you could give to an address and recommendation for the Agricultural department of Washington. These seeds are for the Seton garden of deli, France, an institution which perhaps could offer something in exchange.

Very sincerely yours,

Ad Leguerronne.
Dear Prof.,

Columbus, O. Nov. 21st, 85

I have opportunity to write to Prof. Bercherelles
my son and will mention what you say of his publications.

I sent to Prof. Coellet in depth, just after
hearing your note on the subject a short article
for the gazette. He answered that he would be
very glad to publish it. But he has not done
it. I wrote him after receiving the last No of
the gazette, to add to my note the date of the
communication, if even he wanted to publish it.

Sincerely yours,

L. de Berqueren.
Prof. Sereno Watson
Harvard Herbarium
Cambridge
Mass.
Columbus 0 24th March 86.

My dear Prof. Watson.

You think perhaps that I am already dead, buried perhaps? Not yet. Always hard at work with little profit either for myself or for others. I am at a new volume of Cretaceous plants, for which I have gathered materials, five plates already made but genera and species mostly undescribed as yet. I have now from that formation plenty of leaves of the species which I have considered as a conifer and named Phyllocladius subintegifolius, &c., &c. Here has from Greenland prepare somewhat different from ours, but this species in the genus Thurnfeldia of Zittel, a genus of uncertain affinity, which, as Conifer, for Zelchnichoff, a Gerd for Schrenk, is a Fern for Braun, Schweinfurth, and others. My Phyllocladius subintegifolius which may be or not the Thurnfeldiana descendent. Of the certainly not a Fern nor for me certain a Phyllocladius. But for my opinion, I have of course to give reasons.
Long time ago, when I had already the same question to examine, Prof. Gray had the kindness to send me the enclosed specimen, named, as you see, Phyllocactus amplus-folius (a what author)? As far as I can judge, this plant is the same as that figured by Goebert in Mem. des Sociés d'Amérique as Phyllocactus Humboldtianus, Rich. Could you have the kindness to let me know if the specimens are not the same, or if they are a different species, who is the author of the name. Please excuse me for the question and also please have the kindness to return the specimen. I should wish to Prof. Gray, to whom please to offer my affectionate regards. But I suppose that he is too busy now and could not attend without trouble to such small matters. Perhaps you will have time to answer me a short or long, rather very long letter if you would give me great pleasure, especially if it says that you are always quite well, and also kind at present or some new work.

Please also to remember me respectfully to your lady sister and believe me, your always grateful and sincere friend,

Geo. Engelmann.
Columbus 0 June 14th 86.

My dear Prof. Watson,

Allow me to thank you for the communication of your new memoir, contribution to American Botany. I have received it with great pleasure as a proof of your kind remembrance. This proof of yours, the result of severe studies shows that you are still in good health. You have before you long years of active researches for the advantage of botanical science.

I thought, in the beginning of the spring, that I could be able to go out once more and to pay you a short visit. But I am becoming too enfeebled and think I shall do better to stay at home quietly. I have moreover always plenty of materials on fossil plants to work upon, as far as I am able.

Please convey me to the kind remembrance of your sister.

Sincerely your old friend

[A signature]
Columbus, Sept. 8th 86.

Prof. Jesse Watson Cambridge

My dear Sir,

Could you give me some information on a book, "Paradise Found," published by Mr. Warren, President of the University of Boston, informations relating to the scientific standing of Mr. Warren and of the University of Boston. These informations are demanded by Mr. Tappo, a friend, who is interested in the book and wishes to know how it is valued or judged in America. As I know neither the book, nor the author, nor even the name of the University of Boston, I should be much obliged for informations on the subject.

You see that I am still living. I continue now entering into the dark field of vegetable palaeontology. No much to gather there, but I have not to complain as I remain content and well. I hope that you are the same.

Most sincerely yours,

[A. D. Anceun]
Columbus, 0 Feb. 23° 87.

My dear Prof. Watson,

You are very well, I hope, and still

hard at work. You may thank me for not

hurrying you with my letters. But to day

I have to write to you for a subject which

does not concern you much, but of which

perhaps you have already received letters from

Europe. The widows of Prof. Poehreyn of

Vienna call me to whom they could apply

for procuring the sale of the herbarium of

her husband lately deceased. According to

what Prof. Dillingham and Dr. Burguerter-

write, the herbarium must be very valuable.

Of course, I can only refer the matter to you

and to Prof. Gray supposing moreover that

you have received from Mr. Reichner some

letters like the one I have to day.

Now I wish I could go to Cambridge,

not merely for the pleasure of being yourself

and Prof. Gray but for a good long look
to some packages of th. Herbarium, for.
really what I have here with me; two inc.
fluent. I would like to be some bones of th
curious geese of Bro's. of Fothergill's and
other foreign Hamamelis, and many many
more. For of woody plants, out of th. America.
now I have little except from Cuba.

Though my health is still good, I can not
go out in cold weather and I doubt if I would
even in the summer be strong enough to leave
Atalanta. Well! if ever you have some
packages of foreign woody plants, no matter how
few for an herbarium, even one leaves which
you think good only to throw to the dogs;
please think of me and send them this way.

Please to commend me to th. land amm.
launch of Crop Gay and accept for you
the assurance of my highest regards.

Your old friend, sincerely

[Signature]
Columbus 0 July 20th 87.

My dear Prof. Watson,

The communication of your handsomely and most valuable memoirs on the plant of Jalisco, Mexico is gratefully received, especially as a proof that I still remain in your kind remembrance. I thank you heartily for it. I hope we quite well, I hope, and perhaps now travelling somewhere in the interest of Science. May this note find you happy, at worst, the best place for naturalist, and strong enough to resist the influence of our broiling temperature.

For myself I remain the same, enjoying the company of my famed plants, and quietly waiting the end of this world always. It has been already very long, as you know.

Always and sincerely your old friend

[Signature]
Columbus, 18 Nov. 1847.

My dear Prof. Watson,

Two species of Liquidamber with borders of leaves entire are known from China and the Sunda Island. It would me of great interest to me to see one or more leaves as one in both of these species. Have you them in the Herbarium? Would it be possible for you to send me either one leaf or a whole specimen? Which of course I would return to you immediately. In doing it you would really confer to me a great favor.

Perhaps Prof. Gray is back to Cambridge, if that is so, please give him my affectionate regard.

I hope that you are still quite well and working hard and that you have not yet forgotten your old friend.

Yours sincerely,

[Signature]
Columbus, 9th June 1888.

My dear Prof. Watson,

You have indeed a peculiar genius for creating fine words; or for transforming into euphonious names such ones which, like my own, are repulsive to ears and eyes by orthography and pronunciation. I could never read Desquereza without blushing. Now, Desquerezza is like one of those delicately flat-tongued Italian diminutives not only acceptable but really loveable. I thank you heartily for this appellation and too for the complimentary remark explaining its weight of adulation.

I have also read throughout the arrangement with remarks on descriptions of the species of the genus and feel really obliged for the communication of your memoir, 15th contribution to American Botany.

Very sincerely and respectfully your old friend, Desquereza.
Columbus, 10 Nov. 1812

Très honorée Madame,

Daignez me pardonner de vous écrire ces quelques lignes pour vous accueillir dans la réception du volume, list of the writings of Dr. A. Gray, et vous remercier de la communication que vous voulez bien m'envoyer. Ce morceau est fort bien arrangé, il me paraît. Il est précieux comme exemple des travaux de l'honneur monsieur. Je sincèrement regrette de tous ceux qui ont profité de ses savantes recherches et de son amitié bénédiction.

Veillez acceptées pour vous-même, madame, l'assurance de mon respectueux dévouement.

Léo Desquene.