My Dear Sir,
your & Mr Carey's letter is receit for the remarks & corrections I thank you.
As to Carey Willdenov's 's specimen I sent to you from Dr Short & Engelmann which came into the error.
But as soon as I saw the description of C Steudelii
I corrected my label informing Dr Engelmann of the
error sometime ago. The carep which you call Aristata Santher Mr Carey points out I will soon
over haul my specimen & carefully examine & report
the result. Dewey called it C trichocarpa, but it does
not look like a specimen from N.Y. I have
collected it in three places here & they may some of them
differ from each other. One form I supposed was C. crenata
at first sight without examination. C. grossin at bottoms
of ponds. I have 2 think C. vulpina collected with
C. sparganioides. Mr Carey says Mr Holton was in N.C.
Mr Holton had his specimen of one I think it is suspect
Mr Sullivant's Dr Engelmann had specimen of me
naired with others C. sparganioides-Helianthus trachiculiceps? I think can be distinguished as the leaves are not delile. Is it not a var. of this
I had a specimen of Astrolagus distortus, but didn't think
my Astrolepis pedunculata that I have had but little time
for examining my plants. I have now more leisure &
I can tell more about them, I could tell but little about
Carae till lately. Hechera bipada, aren't you
think maybe synonyms? Your Truly

[Signature]

18 Mead
Doctor A. Gray
Cambridge
Massachusetts
Allegedata Hancock to John April 12, 1849

My Dear Sir,

I have just received the plants fourteen you sent me, and have all before, but they are beautiful specimens. I will be valuable for some use in my book on

Name of

Delandtia Botanica Vol. XII, XI, VIII, IX

I should like to see the tenth Vol. also. The genera of Plants will have Vol. II been published yet? The published account of the Vol. I plant 10 has just arrived. I appear to be very interesting. I received a short time ago, a Memoir of Mr. Baker. I am much obliged to you for all these things. I still want some more white N. plants to complete my herbarium. I was in some hopes I would have got some few in return for the parcel I sent you. But perhaps you may find some more yet. I will add a list of such as I still want.

St. Campanoidea — Alpine

Oxypile nemorosa

Cordia plumidifolia

Sect. Cattleya

Sedum jufferinum

Gentianae

Petunias

Flax

Trichostemum nigritum

— Flax

Calamintha paniculata

Eriostemon — M. Montezuma

Gentianae

Oxypile nemorosa

Herbarium

Caracol beimie

Carey Sauvetram

Carocarum

Chalybaea

Cassia

Calamintha paniculata

Calachanterea

Cortus

Cortus

Cortus

Dipalma

Dipalma

Dipalma

Eriostemon — M. Montezuma

Flax

Herbarium

— Flax

Herbarium

Herbarium

Herbarium

Herbarium

Herbarium

Herbarium

Herbarium

Now the 4th part of Vol. 2 of the N. Am. Flora been published? I have no specimen of about 150 species mentioned in the part sent me, and some plants of these described in the Notes.

Yours very truly

J. B. Mead
Augusta, Hancock Co., Ill., Dec. 11, 1858

My Dear Sir,

After a long time, I now renew my correspondence with you. I have for some time past been reviewing and arranging my Herbarium. Our mutual friend Mr. J. T. Holton calculates some things during the past season. While he was here I told him I would like to send you more plants but had, when he returned East, it would be inconvenient for him to take a package for you at that time. I have many duplicates of some plants such as Carex richardsonii Rchb. Sometimes quadrangularis. At this time I did not find till the next season after you wrote me. I met with several Trees about Tolstoi from Augusta. I do not often meet with a plant which I have not before seen.

Mr. J. T. Holton told me he had a volume of Desfontaine Prodromus beyond the one I have. The last year's from you in "Paradecima Forti" I notice posterior to his new. I have another beyond the same section of Kotschyiaceae. I would like to possess the continuation of this work. I have the 2d edition of your Manual with the additions. I have several plants described in it of which I have not seen.

Dr. C. W. Short is the only Botanist who sends me regular exchanges of plants. I received a parcel of European plants from him about two weeks ago. He sent me a package containing some of the most interesting from Mississippi. I can get no exchanges from any other Botanists I know of.

Have you published the 3d vol. of your Vill. Genera? or anything beyond the Composition in your 1st Foreign work? I would like to obtain, if possible, some more European Cereae. Which I have not by exchange otherwise but have not seen a set with Carex Houghton. Mr. Engelmann promised me some more plants on his return from Europe but I have not heard of his return yet. Please write me a short statement of your Botanical Work being published by the Smithsonian Institution.

Please write me a short statement of your Botanical Work being published by the Smithsonian Institution.

Yours very truly,

S. B. Mead
Oviedo, Fla. Nov. 17, 88

Prof. Sereno Watson,
Cambridge, Mass.

Dear Sir:

I write to ask in regard to a small species of Sabal common in "scrub" land around Stetson and elsewhere in this part of Florida. It differs from the description of S. Adansoni in that the spadix is reclinate and much shorter than the leaves, usually resting on the ground to 12 to 18 inches long.

Although I have lived in this part of Fla. 7 years I have always been particularly interested in palms, I have never seen the true S. Adansoni (type) growing wild. Mr. H. F. Hart tells me that it occurs in shady hammock instead of open "scrub," it is rare.

If this form with short spadix is,
not familiar to you I would be
pleased to send you a long specimen
with roots, as I am about to get
a number to put in my Palm garden
here. Has this form a specific name?

Another thing, I notice a
part of the plants of Serenoa serrulata
here are very conspicuous, glaucous
at last surfaces of the leaves and
am told that in the Lake Worth
Country this peculiarity is so general
as to give a distinct tone to the
landscape. Is this considered a
variety, or merely an individual
peculiarity of the plants that possess it?

I am trying to get together all
the species of palms that will endure
the climate here and have so far
about 170 varieties, of which probably
half will consent to more or less
prolonged existence. and I find great
confusion evident by comparing the
seeds sent to me by different dealers
under the same name and doubtless
have many times received the same
thing under different names. If a
checklist, if such a thing exists, were
a great help, are there any
such lists, either general or of
particular regions, that I could
obtain, either here or abroad?

If I can serve you in any
way by collecting or observation I
shall be glad to do so, in the meantime
my apology for troubling you with
these inquiries is that you are
the authority, and there is no one
else whom I can ask.

Very truly yours,

Theodore L. Mead

Oviedo

Summary of questions asked above:
1. Is the form described a var. of S. adansonii?
2. Is the glaucous form of Serenoa serrulata a
   recognized variety?
3. Are there any available checklists of palms?
Bloxom, Accomac co., Virginia, March 8, 1891

Sereno Watson, Sir:


While botanizing among the ferns here last summer, I found this species quite common here in most upland woods & enclosed some specimens to A. Commons, Wilmington, Del. Who informed me that it was also common in his vicinity, but doubted whether it occurred in lower Delaware. I suggested to him that he inform you of this matter; but as he did not let me know whether he would do so or not, I have taken this opportunity to write direct to you, as I wish to help make the Manual as complete, or correct as possible. The term, "smell sweet scented in drying," is very appropriate as applied in 6th edition of Manual, to this species, while the term, "pleasantly scented" in the fifth edition, was misleading, as the species is nearly or quite scentless in a fresh or green state.

*Lepturus* is growing abundantly on "Borders of brackish marshes" & on brackish marshes on the bayside of the county. Perhaps you may enclose some specimens in the mouth of a bottle.
Feliz cumpleaños.

Hasta en su última

 (...continuaría la escritura...)
June 25th, 1888

Deer Gray

My dear Gray,

I am not at present well engaged to write to you, but I am about to return to California from the north, where I have been with my friend Mr. & Mrs. Mooms.

It is very hard to decide the proper name for the new species of Oenothera found in my garden, and it is a very difficult matter to fix the characters for them. I have to say that the flower of one species has the flower of a certain S. Billarderei, which I had supposed to be "a variety of Calceolaria," but now it is in bloom in an intermediate state between the European and American forms of Calceolaria, so that it is hard to say where the characters of the other end. Horticulturally speaking, the flower of a flower of S. Billardei is its chief distinction.

In a cultivated state, the mark that characterized the variety became fixed and characteristic of a plant, and a many "New" forms known to the best varieties reproduce themselves exactly, upon proper seed. I suppose the rule will be that where kinds resemble each other, their chief characteristics they should be set down, and then only.

Yours truly,

[Signature]
But then what would become of Quercus? Accident, it is a natural preference which we cannot identify distinctly in the bark of the latter being lighter in color. In the former, how is it in the former? And yet it is to essentially different, as the ability to reach a malignant disease which follows the other - Helene - all over the world.

It will be very convenient for me to collect specimens of oak, for you this year. I have to collect some interesting bark here, and I shall have them for you with great pleasure. I received last year a few because I wish I wish to the former. I have seen the bark here exactly like the Macaroon. I think the year old bark is better. I can be more readily distinguished, and they may be also collected in the same kind of bark, and found in the same place as the Bruce. I shall be glad to make use of a collection of the Australian plants and seeds. The very brunch pleased by a set of the Australian seeds and have received. Independently of this I should be glad to make use of a collection of the Australian seeds for Mr. Mullet. I can promise you from a hundred to two hundred different species for this purpose.

It is quite convenient for me to collect a few seeds or specimens of Quercus virginiana. I have a great desire to have native trees, and if the plants of the whole world, a beneficent
to obtain as complete collections under cultivation as I can get. So that I try to have parties to collect for me whenever possible. I have very much yet to add to my small beginning and should be glad to receive at any time a few of any seeds that may come into your hands for distribution.

With great respect,

Your very truly,

Thomass Beecher

P.S. Morris informs me you are examining specimens of Japanese plants. I have a catalogue from Japantown, Co. Kämpferson. The leaves are much more alternated than our species, but in other respects much resembles it. In case you should have specimens of it accord to me to enquire whether the flowers were very different from ours?
The Germantown Nurseries,
Chew Street, below Gorgas,
Germantown, Philadelphia.
June 7, 1873

Dear Dr. Gray,

I trust you will find me for saying that I have never had any ambition to be titled a "Professor." I have never had the slightest notion of being a man of credit, nor have I ever written a line in my life in praise of myself, nor to seek for praise. I have never had any ambition to be considered even a scientific man, as the phrase goes.

My parents could not educate me. They had twelve children to bring up on twenty dollars a week. They had eight when I was twelve years old—a darling little boy. Children at one birth, and then had two children at another, and then had but one child a year. A neighbor paid 40
Chilling a year to entitle him to a vote to send me to a
charity-school. I went to
work for 1 shilling a week
at 14, twelve hours a day
and have been to hard work
every day since that I was not
sick. It seemed a ridiculous
thing for me to ape the "Professor"
or in any way to ask for honors
with which education were not
justly claimed. The only ambici-
ous thing I have had in life is to
make hard work honorable
and to show that even men's
real labor may be intelligent.
When I wrote my little book
of mandarin trees
after of 14 - it was four years
to present time. I could
find no publisher in Boston
or New Philadelphia. The
quis was always "Who are
you?" diffident at least agreed
to let his name on in the
book, if I would bear the expense.
I had only $300 in the world. I
could not stand the defense of
The Germantown Nurseries,
Chew Street, below Gorgas,
Germantown, Philadelphia.

To large a work. In cut the
down prices constantly to its present
dis. As it was my own
venture I answered the
question who is Mr. Murray Medland
by simply adding "Gardener".

When D. R. Knox wanted
Rearden's advice, he was
shrewd enough for a high sounding
sage or Latin title. I would
wish it in my other condition
Reed single or "Gardener"
paper. The long handle of
"graduated" on some title page, I was finally over-
ruled to permit. It was not my wish. It was contrary
to my desire.

In the "native flower"
prospectus there was a long
list of titles when submitted
prize to me. I struck the
most obvious one. When
it was finally submitted
But to me it struck out the heart of these two men. But no attention was paid to this. I feel that perhaps the publishers, like any early experience, want to meet the public impression. But only little men can write intelligent books and I regretfully give way to them as a matter in which I am not business. Our number being why did you not bring "Vice President of the Academy" thing, because I did not bring even the men. The publishers had not found it out, and I went with attentions for it.

Now I shall join finally not be at all friend as you cannot afford to keep as well of my work as anyone else does, I think
The Germantown Nurseries,
Chew Street, below Gorgas,
Germantown, Philadelphia.

Meehan

My home never paid me
tow in public of praise.
If any thing I have ever
done, but I have been
conscious about knowing no credit or
praise for my works. I might
have given some thought to
this. But I have been
too conscious of my own
deficiencies, too anxious to
learn, to thinkague for Re-
Corruuting that have often
enabled me to improve
littler by littler, to let that fact
remain long on my mind.
I must be candid. I have to say
That you showed favor to
my work in Nature flowers,
never once entered my
mind, for my publisher
likes who have been induced
by the pressure of partial
friends to put the worke
me, and not by any
own seeking, I shall be done
of you hear too hard on them.
May have visited a great
deal, I have no reason to
love nor any thing to
Sav. I have all the money
I want, and all the
hour I want, but it is
difficult different with
them. So please be
as light as you can with
them but do with me just
as you like, only leave
them. I wish somebody
had written for me now
I had been found for me
latter. In this I have
only done, what I have al
ever tried to do — to
be as useful as I knew
how in the situation felt
seemed to warrant me.

Very truly yours,

Thomas Melham
any way we read this year. It is so little trouble, while collecting seeds for my back, I wish to get a few more than I want, and hope if you have any, or want of any kind that way, no delicacy will prevent you from offering me the pleasure of presenting them to you.

Very truly,

Mrs. Mechanus

Germantown
June 21, '39

Dr. Asa Gray
Highfield, Dix

By this mail I send you a pot containing a flower of a beautiful Cactaceous plant from Mexico, which is of which recently described. I think one may prove a new genus. It has a very anomalous manner of growth. The root is tuberous, but the right taken about 1 1/8 inch long each, and the facts...
The Dachia tuberosa, from the Collar of the plant. Its roots are woody, looking like a stunted pine, about as thick as a pipe stem. The succulent branches come out at the top. There is in a German collection an "Opuntia tuberosa," which I have not seen, but the spiral stamens and the contracted tube is different from Opuntia. Though the succulent stems are round and have the spine, arranged as in Cereus, the flowers are permanent, and open and shut in the same as in Spinus. I have been very fond of studying Cactaceae, and have now living Specimens of over 20 Species. I am pleased to say that I received a great many of Dr. Mueller seeds. Many are now blooming. I shall be pleased if I can show you...
Printing and Lithography
were almost on my heels
before the chapters were
complete. I have to let
you know that you may
understand how there
was some "Maste" in
this part of the work which
was known always from
beginning.

Now, secondly - I remember
well, that as I was writing
that chapter, pen in hand,
A occurred to me that
an "Explanation" in each
definition of Before from
some familiar dictionary,
would be very much to
the point, and your call
lighting up the table waiting
a chance to read carefully
for review. I took it up.

The Germantown Nurseries,
Chew Street, below Gorgas,
Germantown, Philadelphia.
Feb. 18, 1850

Dear Dr. Gray,
I did not go into details
as to "how I fell into the blunder
of erectile" your error, because
the many in which people get into
these troubles, are generally
"so wonderful" to tell. But as
I have been interested in literature
particularly, I will let you
in near as I remember, though
it is rather a long story.

First, I must say that
When our first series was
being revised, I put myself to
warding others who were
working in the same field,
and succeed I have
many foes because I did
and must not interfere with eating work. At the conclusion of my first series, I had all prepared for the second (M.S.S.) when the publishers, without giving the "annuaries" complained from subscribers, the duty of Jesus was clearly engaging the success of the world," and "must have more, now it is happened. But though I seldom allow any personal considerations to influence me, I could not help remembering that while I was always liberal with my works to other friends, they rarely showed any consideration for me. I have noticed, kindly I think, in the Garden's Monthly and elsewhere, Latins, Scandinavians, Russians, Williamsons, Grays, and others, really, but all purchased with my own money, except in one

American instance—Gray's "American Botany." And in addition to handing over my own works freely to many of them; and generally, except in your case not even an acknowledgment of the reception of the work, certainly never publish an advertisement of the literary notices made of them at my own cost. I think therefore I may be pardoned if I let personal considerations decide that I might for once, even with more foresight, make an instance. But I had no foresight, and I had to wait till the
The Germantown Nurseries,
Chew Street, below Gorgas,
Germantown, Philadelphia.

And burned in the closing and knew what I have judged and I well remember that I was struck by it and that at first I decided came at once in my mind and that I remembered that it did break first in the central line and I thought I remembered that well all elements did break first in that way and that when the verbal culture separated it was only by dying after death and for the moment I thought this was when I am instructed in the word normally and,

My sincere thanks

Due to Breckham
without reflecting any
true, I believe I trust
it is well that I can
remember to tell you "Now"-
Neen thing occur, but I
did you have I am
most justified in this
perhaps "if" as you say
I had looked to the reference
to the lady of "if" "if"
bad mention more of "if"
"if" something else, I think and have
"if" and done it!

I will not ask you
how you made the
blunder in the first
instance, but I have
no doubt there are
some "if" which made
the answer natural
enough - as you would
not with your usual
care, have done it.

You say "if" are
"if" for calling attention
"if". I believe you
are, I am not, I am,
but I have not done
much of it with friends
like, except where I
find it comes in the way
of some of my own work.
Where contradictions occur
it becomes necessary for
me to clear them, or
the people will think
Dear Dr. Gray,

If there is any error about the figure mentioned, it is not as you unequivocally assume the result of "recklessness".

I had to let Mr. Eager make some drawings under the eyes of friends — as I supposed — in Cambridge while I was in Europe 35 years ago. This was given him to do with others. It was just in with my friend Roth, but I rejected it because "I did not think it was ever found in the United States." I wrote to Mr. Eager to inquire if the authorities of Cambridge had not made some...
Mistake. He wrote some
Amstel influence so that
he has followed any in his
thing and that the reported
that the land had been
actually collected within
the United States Boundary.
This was two years ago, and the
Chapter written at that time
and the matter passed from
my mind.
If it had been more
recent and since I have
learned to mistrust
the accuracy of almost
everything, I might have
misunderstood, more particu-
larly so, by exactly who the citizens
were who were consulted,
but for the I was "green
men and am suffering for
it now. - Ezra T. Meckan.
Feb. 24 1886

Dear Mr. Gray,

I have to thank you for your kind and Cordial Letter of Feb. 24. No one is more anxious to have my work as near perfect as possible, and I always value all the help you can give, and have to deeply appreciate. I don't expect it to be perfect. I expect to make blunders—some that I cannot correct. I must say that I sometimes have neglected the changes you sometimes make and I am un undeserved. I must confess that your efforts to help me have been of immense service to me, and I am quite grateful.
The necessity of extreme caution, and in a great measure have I derived this increased caution from the letters you have written to me.

I do not believe the story, that MacNeil has been arrested in Cork. I think you sometimes needlessly sharp; sometimes that you expect more from me than I intended from others engaged in my work; but I am always ready to meet you for ever help. If ever thought merely extended and not absolutely I have to make use of my own resources for office, and keep for more.
The Germantown Nurseries,
Chew Street, below Gorgas,
Germantown, Philadelphia.
Dec. 14th, 1881

Dear Sir Gray,

I can not tell you how much pleasure it gave me to see your hand writing me.

More.

I have replied to Miss Bowen — but under threat not to buy in her solicitation. I had at the same time a card from her. She has 1st. S. 20-
cents, 2. 25, and is looking for the 30th. The M. S. F. and drawings are ready.

3d. series was to appear in January 1882, but the publisher died suddenly.
last September, and his affairs proved so confusing that he administrator has been unable to settle them yet.

I have been trying to

waiting for some one to

buy the copyright from him,

and for the last two weeks

have been looking for a

new publisher, but without success. I have found one

who is willing to "try" another

series but only at the price of 2 plates a month, till another series is complete,

but I have not come to

accept any back than

perspective. In fact two

series have been unsuccessful, and it seems the time for "Experiments" is past, and

I should like it continued now as a regular

periodical monthly serial

remotely as in the

Botanical Magazine.

But I find the great

trouble is to find a

publisher without the

three years experience I have had, to see it

as seen who have

had it.

Very sincerely yours,

Thomas H. Meehan
Cars leave Ninth and Green Streets, Philadelphia, every hour.
Horse Cars connect with the cars in Germantown, direct to Upper Church Street.
Horse Cars along Eighth Street, direct to Upper Church Street.
Chestnut Hill Cars to Gorgas Lane Station, is ten minutes' walk from the Nursery.

Germantown Nurseries,

THOMAS MEEHAN, Proprietor.
Office, Upper Church & Chew Sts., Germantown,
NEAR PHILADELPHIA.

March 12, 1881

Dear Mr. Watson,

I send you by mail some and flowers suitable to the season and the locality of the long distance to which I have kept them for a neighbor who insists that several years ago a lady friend of his found it wild in Florida, though another says the lady took it from a mill in the northwest. It came here from a traveler from Florida at any rate. I have made no attempt at cultivating it as I thought your investigations into the order would make the matter more certain.

It grows only about 2 feet high.

Sincerely yours,

THOMAS MEEHAN
Germantown Nurseries,

THOMAS MEEHAN, Proprietor.

Office, Upper Church & Chew Sts., Germantown,
NEAR PHILADELPHIA.

Julv 1, 1857

Dear Mr. Watson,

A correspondent from Woburn, Mass., sent me three years ago a tiny root
of a plant he found near
there. He had never found
but this one himself, but
had been informed that it
was often found on Maine
borough Island. It has only
flowered once, and proven
to be a variety (Scarcely that)
y. d. Martagon. I thought you
might like to know it is found
in this continent.

I flowered the root of
thalamus grandiflora from
Rose Mountain last year.
You do not place it in the
pendulous section (in the
Canadense) but I find it
Difference in this respect, except that the leaves (periwinkle leaves and all) are shorter and broader. It is difficult to find any difference that can be described. Of course the color is brighter than most of the Canadian ones. It has in flower June 10th. The Canadian one from New Jersey grew beside it from April June 20th, and I have some pine trees, which will have their first flower tomorrow. I have been watching and taking notes of the growth motions of doing this business and find it very pleasant study.

Sincerely yours,

Mrs. Melkm.
Nov. 6 1885

Dear Mr. Watson,

I thank you very much for your suggestions about the Anthriscus. In editor has continually before him the picture of the "devil" knocking for more "copys", and is apt to write hastily and therefore erroneously. I always try not to forget this liability to error, but sometimes get caught for all. And in this case I was sure that Michael had distinguished a Var. Camboda - Anthriscus Atamasco Var. Camboda, and was annoyed after getting your letter, to find mistakes of the kind, either in My or anybody else's! The A. Camboda itself! I had such in my mind, and thought the one in English culture (it is not in America) was this light colored A. Atamasco.
early summer.

Aside from this I think the "Rosa" ought to be some distinctive place for the two forms that a flower can reach. The Rosa may, as the flowers white with a faint expression does not begin to reach the beautiful dark shade which Blackrose was in mind when he wrote 'Purpur' and 'Rosa mull.' I am written. There is surely a wide gap between this and Emerson's "white," and Turner's "rose color." I have never had the two forms growing side by side, so as to get the exact comparative differences but my recollection is that the red in purple atropurpureus is quite as much difference from the white as the 'Pure White' of the 'Treatise' has from the white. Indeed all these seem to me to come so closely together that it is not easy to separate them.
In regard to the Polaronium Question, there is no attempt by florists to separate them botanically, they simply contain the names in use a quarter of a century ago, because one good common name has been introduced to supply lack which they once knew as "Essenium"—"Zonal Polaronium" is the term that obtained the greatest popularity, but this was not near wide enough, and now all the forms used for building purposes, which have been derived in any way from the old "Artemisia Grassium," are
called *Sceinopsis*, *Melargonia* being infertile
and very unequal-petalled class.

Glad to believe you
have got wholly over
your melancholy
Lincely yours

Clemens Mcllvan
July 4, 1887

I am glad that I can send you the hasty notes. I had failed twice and agreed to see when and how the pistol elongated. I was satisfied that it occurred at night. I cut specimens and placed them in a tumbler of water, and the work was done therefore by "midnight lamp." Speak the whole evening in studying the development. Since getting your letter, I have gone over the papers still in the tumbler, and think you must be right about there and being a pair of annexed author about the Chupine. Yet the attendant phenomena are very curious.

As the pistol moves from the opening papers, a derived version of the after
I give an interior exactly of
the above so another batch
back 38, and the lake near which
which appears to be the lake
hercules called it tulare.
a pair of very small compasses,
placed under this mass uplift
it off exactly as a cap leaving
the clear water sufficiently exposed.

Of course it was very assumed
that these being authors' the
plain 
or the entirely
confirmed with the style.

I am thus getting your letter
I feel that the pistol in its
surround has carried with it
the whole mass of wisdom
from the author, wisdom
there and so completely
as to make the pre-

one another (been barren)
and so nicely on to preserve
the outline of another in the


The cholera bug is ever so complete in 30 minutes. It returned to me after lunch. I have never recouped my strength in the day time, and it was this failure that led me to cut the trip short.

Yours most truly,

Thomas Meehan
July 22, 1887

Dear Mr. Nelson,

Last year a correspondent in Alabama, with a botanist in his own estimation, but knowing plants, sent me a dried up stem and leaves of a hibiscus, which he said was growing only in limited locations and different from the wild cases that he had seen. I tried not make it out, and sent the rough piece to Dr. Gray, who was also uncertain and advised me to try to get a root and grow, I managed to get a few roots, but it will make two flowers. Thank to send you one. How do you prefer it dried or fresh. Refers if the two will be open to borrow. It appears to be a seed straw helix.
Dear Pen

In all possible

You will never

The sooner the better.

Flower

Flowers

The dear beautiful

About the garden's edges.
Dear Mr. Watson,

I must say in sum, you must better see with my observations.

Did you know that two moreDelphic slavers enclosed the whole fruit in Circumfluentus and that the four slavers of the Cattle Erthe are wholly silent?

You have now seen the little black plant on the left hand edge of each line of the Cattle. Did it ever occur to you how analogous it is with the Clandestine chest (from French “chest”) “file” at the base of each power?

It is all a “missing link” between Nucleans and Conjugate?

I regret that the present...
Sent (via a separate newspaper) are the last of the letters -

"You want me with "First" First General to learn the math."

Eliot Johnson

William Melanson
Dear Mr. Watson,

Your correspondence must be delayed
through my own illness, but I hate to make
one more in the bondage. But Echinocactus
papryracanthus? will not wait. I wish to
ask you more about it.

I have an European correspondent who
is willing to pay well for your Cacti, to
let him know of it. F. papryracanthus,
the finest one at length, but with a broad
dish and Bccna as all likely to be while
as described by Spegaiiana. I thought
best to defer sending to Europe till it
bloomed. It is now in bloom, and I
send you the rough propagator. I have
made it.

If you recognize it all right,
you must send for the plant.
and if you mark it for science, of course
you shall keep it, for science before
each any day.

I also enclose Memoir of a
plant raised from seed sent me
from China. Is it Asparagus asiaticus?
I hardly think quite true, but I do not
know who is else.

The publisher of The Gardening Monthly
asked me some years ago. The family (widow
and young ladies) would have continued it
if I wished, but I could not come to
manage it as the father did, and I
feared what is a valuable property
now might be wrecked in the future.
An offer of $5000 cash coming along, I
advised them to take it. It threw
me out of "bread and butter" and
the last I had come to loved, but as
my flowers and terra cotta is now
going on, and will probably end
my days in my life time again,
I might perhaps to work after 30 years
Heady work on one thing.

We hear every few days through
Eustisfield of things. I am, indeed
that all is so utterly hopeless.

Very truly yours,

Matthew Meekins
Long ago Dr. Gray wrote to me that he was under an agreement with Professor Parker to write to us that agricultural or horticultural journal and the American Agriculturist. But there was a misprint in the agreement or he would not send them. This was because I did not use his name to a short piece suppressing he would not object. I could give you references to a good number of paragraphs, but I doubt...
Whether it would be
fair under the above
understanding, to use
these at his

However should you
think differently I will
with pleasure go over
and mark them up.

Have a large amount
of private correspondence
relating to Physiological
and Neurological Matters

Chiefly both from China
and Dr. Engelmann,
but these are hardly ever
of value till a hundred
years,

So, when they treat
Moor Light on contemporary
events in which the world
did not know there was any
interest till long after.

Thank you for the
last page of the reply which
you kindly sent to me. It
had a special interest to me
since my having a few years
ago visited one of our botanists
between the Chalons for
Cephalopteris briareoides
and ours. It shall be agreed
for its identity with Buckleij
A-biophyllum. Finally satisfied
that my position was correct
I write (I am leaving from
memory) 'I think this support
I shall be honored to read
the revision of Villeneuve.
The synopsis from "but
of " to the "Mellisappren
cy " of " Var. cancellata!"
Naturally I was struck by
his coming so near to
reaching it.
Mary How Jones

[Signature]

I think he could print a notice
by way of earnest to Botany, is it
The reference to Beale's Flora;
but no Whatever life seems
to have violent that.

I speak freely the last of the
Matters. It would have been
a task, but yet satisfying count
have to have told this frank notice
to intelllect.
My dear Dr. Watson,

I spent the whole afternoon trying to find a specimen of my collection, but without success. I have been looking for Anacampseros, but without finding a solitary specimen of any kind.

When I started in life, I determined, someday, to have a first-class herbarium. Through life I have been daily collecting specimens to make one with all my Alphabética bundles for commence in putting away. The natural order, general and species, were all in alphabetical order.

Conservantecce thenceforward, be in the set, the bar had at work on the two past years, and where it has gone to I cannot imagine.
The fact, however, remaining that I am unable now to confirm the exact date locating things. I have no doubt whatever of its existence as reported, because I carefully compared it, at the time, with specimens in such a lab.

As I have said it might have been an introductory form to their Colorado, if it was on the line of the Kansas Pacific, and not a Clayton of one of the unfertile towns, yet high and desolate, but living up along there. It was on the collection made on the Rocky Mountain excursion after the St. Louis meeting of the Linnaean Association that I found it.

However, we can all be mistaken, and if there is likely to be any
doubt about it, omit all reference to it, or if we're writing at all, just 'bout the agreement.

Thanks for your examination of the Landsmaid. I now feel satisfied that it cannot be or himself as your material must be so much fuller than ours for examination.

Very truly yours,

Thomas Meehan
COMMON COUNCIL  
PHILADELPHIA.

Gen. Council, Apr. 28, 1871

Dear Sir: Webster:

Mr. Samuel Strickler is from Albany, N.Y. Texas, Walker
married to day. Mr. Hedfield and I decided it was over in Texas.
and for want of
books, would not decide where to
place it. It was voted unani-

mously to "send it to Watson".

Unkiap over your monograph

to-night in Kep. I funny it may

be. K. S. Hill.

It was also voted to send

you the enclosed "Kononia" as

I say, but which Mr. Hedfield doubts

because of the heavy alternate

leaves, and apparent want

of insinuate. The alternate leaves
certainly are stronger, but the tendency is tripleness and general character of romance, I much think would place it there.

This is a scrap my wife has just brought from Santa Cruz on the Pacific Coast. Oddford doubts whether the attends leaves can possibly belong to the flower if the flower be an Abromia, but Mr. M. seems positive of they do. There are no such leaves in any of our Yearbooks.

Oliviney yours

Warren W. Mehan
Germantown, Phila. Aug 25, 1891

Dear Mr. Davenport,

Our young men have placed the Redbarks in on the list of subscribers, and are very thankful for it. It is rather a heavy undertaking for them, and they are as yet uncertain how far they may go in free distribution. I have always felt that the good which Cambridge does for botany entitles her to every help free. I hope we shall reach the necessary stage when we may do so. Our prospects are good; indeed I am sure I mixed well with many scattered subscribers before sending the work.

I am sorry I mislaid the kodak. I do not know what became of it.

Very sincerely yours,

Thompson Meehan
Dr. Serene Watson,

Cambridge,

Mass.,

Dear Dr. Watson:—

Our Academy expedition, which accompanied Dr. Peary to Whale Sound, has brought back a small collection of plants from McCormick Bay and Discob. We have enough to make several sets, and in the interest of geological botany, we presume you would like to have one set, which it would be a great pleasure to us to send you. We have some four items, which we cannot positively identify as to the proper species. I enclose you them in this envelope. Would you kindly decide for us what they ought to be. You may return them, as we shall have better specimens for you in the set which we propose to set aside.

Very truly yours,

[Signature]
Germantown Nurseries,

THOMAS MEEHAN, Proprietor.

Office, Upper Church & Chew Sts., Germantown,
NEAR PHILADELPHIA.

Dear Mr. Watson,

I said last year that Mastin!,

had told me he had waited

that person from Canada

by Macoun, and not yet

received.

I have only just had

the chance to see for myself how it

is, and you are right in

what you said. The Canadianis

is.

May 18, 1875

Valley of Beaver River B.C.

Mr. 1232.

This scarcely brings it into

"Canada" as I understand it. Perhaps

Kendall had not come broad

idea of "Canada."

Trusting, Mr. Meehan.
Dear Sir,

at my return from a journey to the southern parts of Switzerland, a few days ago, I found your letters which you had left at my house with our friend Duffuor note. It is impossible to tell you how much I did and ever shall regret that your visit to Bale should happen during my absence, thus depriving me of the pleasure of making your personal acquaintance of which I was long ago most desirous, and to which I have looked forward with so much anxiety since last spring when a letter from Kinott announced me your approaching visit. Is there no prospect of your returning to Switzerland before leaving Europe? Having not heard of your having been at Geneva, nor at Bon where you would no doubt have visited Mr. Smith, the most zealous Botanist, I conjecture that you have not been in the interior of our country, and therefore I still entertain some hopes that you will one day return. So then, allow me to beg as a great favour that you will again take your way through Bale and inform me by a few lines of the day or week when you think to arrive here.
Having long ago wished to get connected with some botanist in the United States, nothing could be more welcome to me than your kind proposal of forming a botanical correspondence with you, and you may depend upon my exerting all in my power to satisfy your desires. For I keen to be fortunate as to see you here, I would certainly not have let you depart without a good parcel of such plants as my duplicates afford, chiefly from Switzerland, Savoy, and France; but they are still at your service, and on care you cannot come to fetch them yourself, you would do me really a favor by indicating whether and when I may have the pleasure of sending them to you. I hope you will have the opportunity of accepting my offer, as I myself would be very happy to receive plants from your country in exchange, and, indeed, as soon as I shall have a favorable answer from you, I shall send you, with my plants, a long list of my desiderata of North American plants, most of the more common species of the United States I possess already, particularly from New York, Pennsylvania, and Ohio, but in the plants peculiar to the southern and more western countries, as also from Canada. I am very poor. At all events, whatever plants you would send me, whether new to me or not, would be most valuable to me as authentic specimens of one of the celebrated authors of the newest, and undoubtedly the best, Flora of North America.

And now, after having hoped with so much freedom, if not indiscretion, my wishes, let me entreat you to acquaint me as frankly with yours. With the hope that these lines may find you still at Berolin and in good health, and that they may be the beginning of a long correspondence, equally agreeable and useful to us both,

I am, dear Sir, with perfect esteem,

Yours most truly,

J. H. Meissner, M.D.
Prof. of Botany at the University of Rostok,
Switzerland.
My dear Sir,

I ought to begin these lines with a thousand apologies for my long and almost complete silence towards you, but finding that I really have no sufficient excuse for it; I shall rather acknowledge myself guilty and with sincere repentance ask your pardon. You would however be too much wrong if you thought that I had neither thought of you nor of the deep obligation in which your kind & most valuable communication of plants & books, which I have received in due time, as well as your letter of March 5th, has placed me, for, indeed, since your first letter I have always been thinking of procuring you the visidinata, of which I contained a long list, and have employed many of my leisure hours or which after you have been a short in procuring my herbarium for that purpose. But the greater part of your visidinata being of the rarest descriptions, especially those related to Germany, I have either as specimen at all myself or at least so duplicated, I was obliged to apply to other sources for procuring them, and this necessarily caused a long delay. Now at last I have the pleasure of announcing you that two weeks ago I have directed by the "Nouage", to Mr. Loraine & Co. to be forwarded to you by the earliest packet, a case containing the following objects:

1. A thick parcel of plants for you.
2. Two smaller No. of Ograzon Plants (from Mr. Shuttleworth).
3. A number of brochures on different subjects of Natural History (together with a few prints of the Lithographs of L.H. vi Zey, &c. &c. &c.) partly for yourself, & partly for the Library of the New York, to which I would request you to present them in my name.
4. A panel of plants addressed to D. Monk, Louisville, Kentucky.
5. A parcel of seeds for the same.
I hope it will not occasion you too much trouble to forward the letter parcel to Mr. Boots, and that you may easily receive his part of the package for freight only but in future, if you prefer, I shall direct the parcel to be sent to Philadelphia. The chezours of the sheet are all for you and each of your friends or may take an interest in them. I have also added two copies of my 'Trees of Europe.' Oh! which you may give to some of your friends or to your public institutions or scientific societies. My parcel of plants contains a good number of you deciduata, with an addition of other trees and shrubs, European plants, and a collection of the plants from America, Macaronesia & other parts of the world, which I care to get. These plants, though frequent in that season, will, I hope, be agreeable to you. I am sending some hortensias from those parts, and they will be quoted in 5 shillings. Page of America, which is now in the hands of the author, having my observation in his hands for comparing Tussilago-Species with those he has collected in the same country. Tussilago-plants are frequently quite wrongly named, but they can all be corrected by Girardin. Mr. J. H. Smith, a very distinguished horticulturist, proprietor of a beautiful hortensia colony, has expressed me to send you, along with my own parcel, a collection of European plants, some of which I have added for you yourself and some of European genera found by me in the United States, where I have them. These species, common of the Mediterranean shores, have not yet been found here before.

But now let me express you my sincere and warmest thanks. The highly interesting communication I had the pleasure of receiving from you two years ago. The plants were recently sent to my collector from the more western and southern states, particularly Florida, as also the Japanese gardenia, from which I have received excellent parts of North American plantspar already in full. I am led highly obliged to you, and beg youclass to offer to lend me the communications. I take the liberty of suggesting you to send them as soon after their publication or through my book seller at Paris, to make your gifts of the first volume for the supplement to your own newspaper, which I have not yet finished, just now, and I am very anxious to get acquainted with the following ones. I have the pleasure of giving you a good deal of trouble, especially the great hortensias, which the plants of which I find almost inedible, such as they are, in this country.

I have heard with great interest that you have visited the highest mountains of North Carolina, and I discovered a number of new & interesting species. I shall immediately do what I can to duplicate them, and your assist would be highly grateful. I understand the chief habitations of the plant, in which I have not yet been able to get any wild specimens. I have given which I am particularly advantageous to get me. J. H. Smith, a very distinguished horticulturist, proprietor of a beautiful hortensia colony, has expressed me to send you, along with my own parcel, a collection of European plants, some of which I have added for you yourself and some of European genera found by me in the United States, where I have them. These species, common of the Mediterranean shores, have not yet been found here before.

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My dear Sir,

I was very sorry to see by your kind letter of Feb. 1st, which I received eight days ago, that the box I had the pleasure to forward for you to New York last summer has not yet reached its destination, and I have lost no time in taking such steps as may, I hope, lead to its recovery. I caused the commissioners who sent it from here to M. Loroux & Co., Havre, to write to that place, and I have just received the answer of these gentlemen by which they state that they had only received my box and shipped it off to New York by the "Ulica" on the 13th Oct. last. This vessel having since returned to Havre, there is no doubt of our box being really arrived at New York, but it seems that either by some error in the direction or by negligence of the commissioners at New York it has remained undelivered. Let us hope that by applying through some of your friends to the office where the effects of the Ulica were deposited at its arrival you will be able to recover it, or at least find out whether it may have gone. Were it lost, I should the more deplore it as part of its contents, especially the Turkish plants, would be almost irreparable. At all events I trust by you...
to favor me as soon as you can with a few lines of information about the result of your inquiries, which I still hope will not remain altogether fruitless. I give Mr. Lomax & Co. at Norwich the direction to Wily & Rutnain Book-sellers, New York, whom you must remit me as your commission-ners there, and have lately requested them to write to them on purpose, to know whether they have received the box and what they have done with it. Anxious as I am that my parcel should at last come to your hands, I expect your answer, with great impatience, and should at present unseasonable, I am to do my best to repair the loss as soon and as well as possible.

It is not long ago that I have read an account of your appointment as Professor of Botany at the Harvard University, for which I congratulate you with all my heart, thinking that in this situation you will be the more favorably circumstances for your scientific pursuits, and being fully convinced that your influence as teacher of our favourite science will powerfully contribute to its spreading more and more, through your pupils, all over your vast country, and its being usefully applied by many a rising talent. I am par-

suchly glad to hear that you have the advantage of possessing a Botanic Garden, and it would give me great pleasure to exchange with you seeds, as I do every year with about 25 of the chief Gardens of Germany, Russia, Italy, France & Holland, so that I am able to supply you with a great variety of plants of all countries, and especially of the most species which every year appear in the catalogues.

Many thanks for your kind offer of the continuations of your excellent book, of which I have not yet seen more than the first volume, and small parcels will be sent me here as soon as possible, and I am to thank you in return for the kindness you have shown to my friend Sketchworth, and with whom I have say you are also in connection.

Believe me, dear sir, with sincere esteem,

Yours very truly,

C. B. Masson.
My health is at present tolerable. I have spent three weeks at the Lake in the valley of the Simme, Canton Berno, for the sake of rest & good air, but the place is not elevated enough (about 3,000 only over the level of the Sea) to afford true invigorating mountain air, and even then we suffered greatly from the uncommon heat in July. The scenery is very fine, but little known to strangers; and the surrounding mountains & glaciers, reaching the height of 11,000 feet, are good tertanizing grounds, but my short breath & general weakness did not allow me to visit the higher & more promising spots, so I returned with only an indifferent harvest. Having finished the Conclusions for the Fear Persecutions (they were even published lately) I have now work under hands for the present, but occupy myself merely with what I may call quiet & private studies in my lodgings, and therefore I am still very glad & thankful of if I receive any acception to it.

But now goodbye! I wish you will have a good passage to America, and long enjoy the circumstances of your late voyage & travel in the Old World; in good health & happiness. God be with you!

Ever yours with sincere esteem & regard

C. V. Mantener Professor

P.S. Sept 3rd. 1869.

My dear Sir,

It seems to be written in Heaven that we shall never meet in this life! All my wishes & hopes of at least seeing you during your late visit to Switzerland were vain, and I have not hope now that if you return once more I shall have better luck for them very likely I shall be no longer among the living. Four or eight years ago, you have been here when I was absent, and this time I let all I could to avoid seeing you again. Taking it as a matter of course that at all events you would go to Geneva, I especially wrote to one friend, B. Jodlolle, to let him know, for your sake, where I should be to be found at the supposed time of your stay in Switzerland, and he was kind enough to inform me of your movements, telling me, the last time, that you had left Geneva already for Interlaken & Lemeon, but without mentioning till when I ought find you at one or the other of these places, and adding that you intended returning to England in August for the British Association at Exeter. When I received this information [on Aug 3rd] it was too late to travel to Geneva or even to Exeter, and I came to Bern a few days afterwards. I still had some hope to find you perhaps at our friends Schonberger's, but in vain, and I learnt from him that you were then already in England. I cannot tell you how much I am vexed & grieved, the wish to hope of seeing you up of thanking you rise once for all the kindness you have always shown me, being so long been — and shall still be to my last foremost — one of the dearest of my heart.
Presuming that you are probably still in England, I direct this letter to the care of J. Hooker, Esq., whose power it will no doubt be forwarded to you, wherever you may be, even should you already have sailed for America. I send you also a copy of my memoir on the life-writings of Martius, written on request of the Russian Academy. I take the liberty to add another copy, with the request to present it in my name to the Boston Academy of Sciences & Arts.

I am still expecting the result of my attempts to enter into connexions with some American collectors from whom I might receive the rare plants of the U. S., especially from the more Western parts, and from the Sandwich islands: according to your recommendation, I have written, in Jemvant, to Mr. Horace Mann, with a rather great sort of desiderata, but I have not yet received an answer, perhaps I have, on account of his health. I have not written to D. E. C. Barry, Davenport, Iowa, thinking it almost superfluous since you have sent him my letter, but I think now I had better do it, as I expect I shall receive a letter from him in a short time. From Deland I had a letter this spring, in which he promises to send me shortly a parcel, together with one for Mr. De Candolle, which, however, I have not yet received. I am extremely glad to hear from Mr. De Candolle, that you intend to continue your excellent "Flora of Florida," though on a somewhat altered plan, more similar to your "Manual," which I am sorry to say I have not yet seen, nor more than Chapman's Floras of the Southern U. S. We cannot yet have such works, except on special order, but I hope I may procure them from England. I am much happy to hear also that your trip to Egypt has proved perfectly successful & agreeable, and that Mrs. Grey has so well recovered her health. I hope & wish with all my heart that it will be lasting for ever.

You will be surprised to hear that the Herbarium of Martius will not be purchased by the Russian Government; Dr. Lübbe just writes me that Prof. Lübbe & Regeli, who had been charged by the Minister to report a advice on the subject, had at last done it, but in such an indifferent & almost haphazard manner that the Minister will not even bring the matter before the Chamber of Dignities. Is not it a pity to have the very same, as the public Herbarium of Mursin ("Karel Acad. Monac.") is not at all a rich one, and greatly inferior to the other museums (for Botany, Anatomy, Palaeontology, Mineralogy, etc.) of that city, or the only valuable just being the Brazilian plants collected by Martius & a few others, the rest of which is not even a small collection; while by the acquisition of Martius' collections, it would have been brought to a more uniform & universal completion, and raised to equal rank with the other public collections of Mursin, and even almost with the Royal & Imperial Herbarium of Berlin, Vienna & Wiesbaden. It will be rather difficult to find a purchaser, especially in Germany, where Governments have at present no money except for soldiers & guns, and where the new school of Microscopists — the interest for systematic Botany & collections has sunk to a very low ebb indeed, I hope that some time here or by in Germany will. Have a knowledge of species — much less of the vegetable kingdom in general! And I should agree with an Architect on an Artista. I have a description of Westminster Abbey for instance, and he gave me the most minute account of every single stone & all the other building materials that have been employed in the grand Edifice, but with more — much that give me a real idea of image of the whole building & made me associated with the laws & beauties & peculiarities of its architectural style. Why, just as the case with our modern Cyprians & teachers of Art in Germany. With a descriptive history for the time (History, i.e. the knowledge of plants & the distinction of species), they confine themselves to the mere ornamental studies and do not even know the name of the commonest plant that grows at their house door!
To Prof. A. Gray.

Basel, Feb. 17th, 1872.

My dear Sir,

I have just received with unutterable pleasure your kind letter, and I hope you will excuse me that, instead of replying to you in extenso, I send you my enclosed answer to Dr. Torrey with a note to Mr. Cooke, both which I desire you to read before forwarding them to New York, the exact address of these friends being unknown to me. Thus you may see what I think and feel concerning Mr. Cooke's intentions and liberal offers which he makes to me in such a noble, delicate, and sympa-thizing manner, as what I have written to Dr. Torrey, I would exactly have said to you too, and so one letter may perfectly serve for two. I need not, therefore, repeat here that I gladly accept all offers and proposals made to me by Mr. Cooke and myself, nor how happy I am that Mr. C. will kindly allow me to retain still for some time certain parts of my heeb, a proposal which greatly soothes and softens the painful separation from such an old friend and daily companion. I look forward on this final and total separation about as a tender father would look on his beloved daughter going to be married far away, when all his feelings will concentrate themselves in the wish and prayer that she be united in happiness to a worthy, noble-minded and loving husband, — but Mr. Cooke's intentions, particularly that of placing my heeb. in the Columbia College, New York, leave me no further wish as to the future lot of my dear child!
I believe I have already told you in a former letter that I shall go on collecting till my last days, therefore, if you can engage any of your friends to send me plants, for inst. from the Western States, California, Sandwich Islands etc. I would receive them with great pleasure & still incorporate them in my Herb.

I hope & trust you will receive as quickly as possible Mr. De Candolle's report on my Herb. & I need not say that I am always at your service for any information concerning it you may wish to receive.

You say nothing of Mr. Gray's own health, so let me hope that I may take it as a good sign. My own health, I am happy & thankful to say, is much improved since several months, so that I am able again to occupy myself the whole day and dare look with better confidence on the future.

Poor Lenormand's death (Dec 11, 1871) was a hard blow to me, he was in every respect a most excellent, worthy man, & the most agreeable correspondent that one could wish.

With kindest regards to Mr. Gray believe me my dear Sir, ever your truly attached & obliged friend

C. F. Meissner.
Basel, Switzerland
Feb 11, 1873.

My dear sir,

It is so very long since we have exchanged our last letters that I could not wonder nor complain, if you had meanwhile given me up as a correspondent! Indeed it was in July 1872 that I received your & Dr. Torrey's last letter, and at the beginning of Sept. that I sent you my answer. You had then both written to me just before you were on the point of starting together for your great journey to California, Oregon & Mount Rainier, and you kindly promised to give me some a count of it after your return, which would have interested me in the highest degree, but which I have not yet received. I hope at least however that your journey has been happily accomplished, and that you are now since returned home, well satisfied with your journey. In my last I have already expressed to you my warmest thanks for the kindness with which you have so successfully promoted the sale of my book to Mr. Crooke, New York, and now I hoped to be able to inform you of the safe arrival of part of it in New York, of which however I am surprised & sorry to say I am myself still ignorant. At present I can only tell you that about two thirds of my whole stock, consisting of eight very large strong boxes, carefully packed in a basket accompanied with all the necessary papers procured by our American Consul, Mr. Ernie, have been shipped at Rotterdam in an American Steam packet which must have arrived at New York long ago. Now you may imagine how anxious I wish to be made sure of its safe arrival, and I should be most thankful to you for procuring me any information concerning it. Though the season & weather
were not the most favourable at the time, yet we have no particular fear of the loss of our vessel & cargo, at least neither our commissioner nor the Consul who have written both to Rotterdam & to New York for information, have heard nothing of our vessel being among the number of the losses at sea that took place about that time... We still hope therefore to hear soon of the safe arrival at New York, and also of the payment of the rest of the payment of the whole (5000 Doll.) which according to Dr Torrey's last letter had already been put at his disposal, although Mr Cook did not press me at all to accelerate the transmission of the Herb. On the contrary he has dealt with me from the beginning in the most noble & generous manner, expressing quite spontaneously his wish that I would keep the Herb with me as long as I might wish to think to make use of it. I have therefore never ceased to collect & to enrich it, to improve its arrangement, it being my pride & duty to do so.

In the precarious state of my health which obliges me to pass a great deal of my time in bed, I can of course do but very little and am almost out of connection with the botanical world & unable to give you scientific news. I must cut this letter rather abruptly. I have hope however to get much better & stronger in the good season & still to be favoured still with your & Dr Torrey's letters. Meanwhile believe me, dear Sir, ever yours faithfully,

J. F. 

P. S., the packing up & forwarding of the Herb has been done by the great kindness of Dr stroth, a very distinguished botanist of Basel, and he has also written on the subject to the Columbia College, New York.
I s.
A few lines of
Yan. Aton. (Ferd. Bong.
my Meliborg) are in
the top of the box
sent to me in mid 76.
I sent that the box is a small 3 to 46th handkerchief but the last 9670.
2 2 7 7 0.

I r.

my 7 7 0.

It not be referred
To the E. Hylunx I read in the
earlier I am an interesting news
the morning for a further
search. I was fortunate and
find two correct clues of
twofimples. They were mostly
suiln green and had the edges
the yellowish east of makeup ex-
cept in a few instances; I
had not known that they are
like a worn one awaiting a
new wish for to deepen the
color. I see find no sufficient
small wood plants of the sound,
although I must have examined
End 20 or 30 specimens A. I knew of my up to me sufficiently small. It seems to be growing from the main subterranean stem of another (putify the error?). At any rate I was compelled to erect what seemed the connection, but as I immediately located it close in its nature can't at a moment and I think it may "be". Most of these plants if C. Hylax were used
and growing along with a trunk of about 18" or thereabouts, but this again was unclear as it seemed a subterranean stem had been pushed out from the main stem. This is certainly not the case in the large one. C. Hylax, but I surmised there was a case with the same C. Hylax (S. Lapp. 2). However I do not know a matter having examined! I send a bit of you think of help from Lapp. 89 with the same word C. Hylax also the name of this is Lapp. As I am sure 4 S. Lappone. I have not yet seen more on the large C. Hylax. This is not abundantly C. Hylax 1 5
minister. No time Cane-hope means how there about the Sack in some 20 miles NW. A saw adorned. (Aston). Hulke

When this note is led by you in will learn this book may sent 4 sheets at some time.
Charleston
S.C. Dec 8th 17

To Mr. T. Gray,

My Dear Sir,

Some years back I went to see Mr. C. G., and I sent him some of the fruit of Ceylon. I sent him a few, leaving some hopes of meeting with several plants in a new spot. I seemed to be in the young. I am sure that you will find enclosed in a

Yours truly

J. H. M.
little bag certainly are, as they are last year's seed & were found in among the vines. These are certainly some, some of them having stopped at the inner or leafy germination, some fruit is also sent.

I have at home three as stout young plants of the same in no way mutilated & in perfect condition. They are too bulky to be sent through the mail but they might be very easily sent by a friend. Should you care to have them they may be sent you in that way.

Very truly yours,

A.S. [Signature]

I trust my health is now quite restored. After reading and answering your note last at Hardeeville I never heard from you again, a reply having come from [C] that you were in an agreeable state of health and had benefited very much.

My respect to

Cornwell of Albany.
We must hear the moral truth of God's Word, who went to heaven as better. All these things are under us, and if you are told of us, it is a concealed movement for political effect of that government. Whether help is to law and order, or means our liberty from Consecration! Think of me my 25, 25, the 7th, as a term managing Election places between a Northern Centrist coffee & a Negro middle Calm. I was thinking them so as to give a fair count for Ware Hamilton! Laugh a week as my reason to you, let! I can this a "bremen" thus, effect on my part for the State. — God help the humblest efforts of all!
The letters, like your friend's last, are so much in my mind — all of us here got so well until...

...to face letter from in the neighborhood of your letter, that
to remain in Ashantee! My family
and yours...

Country Swarms with us & friends, while here
all over towns when
were really to be of
we are named
that we shall be so
much in mind. Meanwhile
in this
last (?) effort a
rule of the other's, it appears...
10 Panel speaks last week with little satisfaction. A letter I still think of lying on a table.

* * *

A course I saw another leaves on plants—aphid-hall or winter—only the brown pods.

that I had been two species of smooth beans, one with shiny pods, another smooth or wrinkled, but that I had not seen in 15 last for a long time. That was a little too hastily inaccurate, but evidently I had in my mind a single instance, and when I found at "Pritchard's" Planting in 1867-66, and which I believe was smooth or wrinkled. But that was a long time ago, and I now do not feel entirely about it! I hope yet to find that same plant, and find also smooth fruit. E. had a right to ask as to the inconsistent statements.

Bluffton
So. Ca.
10 Dec. 1876.

Mr. Strong
My Dear Sir,

My Gonzonobus card came last night.

Since returning here in '67 I remember to have examined only three different vines of Gonzonobus—viz, one at Bluffton, one at Sandy Run, and one on the Creek (from all three of which I have collected prickly fruit, and from the one
just two yards only.

I still remain flowering

The large press

Tastent you came from

Sandy Run, and as I

got no flowers thru this

year or the last – I

conclude that those you

refer to and describe

came from that spot.

From time to time

I have seen plants

in the wood side, but

I do not remember

that I ever examined

them. Next year I

hope to clear up

the mystery, and

it seems not unlikely

that the flowers

with "cylindrical whitish

pubescent inside – smooth

outside" – will be found

on vines bearing prickly

fruit at Sandy Run !!

Please keep this

note for next summer

when I send you

(Very truly)

[Signature]

P.S. L. Engelmann

has written me like

wise, and says that

in '74 I wrote him
just sure I might belong to the
sp' bearing smooth & not be striaed.
My seeds & collections were all
scattered by the storm occupying
my house, & I cant but think
that these vines (found my in my
old lot) came from the ant beds
seed-what I had expected to
germinate under the foilage
Can of Mr. Barrow in the
flying rain of storms! And as
I think I was right after all
in saying that I had seen
the smooth or sturdy foliage!

Yours very truly,

[Signature]

R. H. Barlow

Last week I sent you a packet containing
the leaves of S. Vines, which I found days
before a letter containing a letter of my
soil in a small packet the same effect as in 74 with rests of
the large ant, by the way in the husk of
leaf, but a slight. The result was just as
before. The ants were common, & after watching
them a long time I left them tussing to pull
the musk into their holes, but in no single
instance was there any sign of unusual or
stumpy or irruption. No ants are especially
fond of the same honey, & are away the very first
touching the without relation to have been the
quickly similar if the secretions were insects
before, but not one shows it! The other day
I watched several insects feeding up & down the
track, but no sign was produced on them. I say &

Petals of this S. smooth flowers are huddled
at base with 13 greenish yellow tips. 135 mm
I send you also 6 of 8
hirsute, just collected, with the hirsute of yellow,
6 mm. 78 mm. The ant

Seeds. Several vines
were found all spring
near each other, but the dried foliage of last year, and the flowers a leaf enclosed did not come from the same vine. There was no flower as yet on the vine with the foliage, the latter I shall send to day to Dr. Engelmann, as also a few flowers, —

I had been hunting for this thing wherever I went ever since I wrote you, and had almost given up the search, when this morning happening to walk through my old burned lot — not all grown up — I was attracted by a young vine. The leaves and shoots made me as being more delicate a white vine.

The first time I sent you a short time back! I was satisfied I was on the track at last; and after hunting a little, soon struck the mother vine, which was just blooming, — turning around a young vine raising up had shot up near the crumbling bricks of my old kitchen chimney! None in bloom but this one.

It is very likely that these vines (whc I've found no where else about here) came from seed which I had collected just before the war! I think sometimes to you — that to oblige a friend who has promised some of our native seed to Mrs. Carson — I had collected foliage of a smothering - at a certain place, where this distance of time I believe that was
If you thought it worth while to preserve that Acalyphella, I sent you can place the enclosed slender shoot alongside of it as both came from the same root— which I transplanted. It has just shot up that, and without a doubt it is Anemotheria cornua. The parcel contains also two follicles of Podostema umbrosum— which I believe you asked me for some four years ago. If the Engerson turns out to be E. quercetorum I'd like to know. Wishing you a pleasant voyage and all manner of blessings, I am your most obedient servant.

My sincerely yours,

[Signature]
of what may be E. quercifolium, but I am such a poor hand at The Composite that I cannot venture to say! I suspect from the looks of the rest of one of the descriptions it does send up off shoots, and it may be perennial. I think it is. I send you with this what I found, and will now continue to look - and may be able to send other and better ones by the time I'm set back from Europe, or even before Sep.
To Miss Gray.

my dear Doctor,

It is late, but not too late. I hope for
me to join in the congratulations of those friends
of yours, who salute you on the occasion of your
labor birthday.

Of all the well wishes there is no one who
thinks of you with more affectionate regard than I
do, or who wishes you many years yet of labour
and happiness.

Yours truly.

Very truly & respectfully,

P.S. your friend,
no trace of the clusters of miniature leaves, and one small plant which has a good many.

This is the only condition I have seen that approaches what you refer to.

Pogonia and Anennanthemum growing near were thrust into the barrel, and also mulefors of Asclepias Michii. Because, what from last were a tiny distance off, also Peristephanum hub.

I have not yet read the article Blumm sent.

25 May 86

Mrs. Grey

My postal in reference to Sarsa =

as I read last night.

S. flava is not found here on salt water, so that I cannot join in anything of its. The =

harm. S. variolans has another set of

leaves, appearing sparsely later in the year which are well grown.
These secrete abundantly, and Captain insects even more largely than those in May to June - till cold weather. As for the small Hydnora; you refer to in S. Flora. I have never seen such just so fine 'tis hard in S. Kainolans, but I do see here and there a few plants with clusters of miniature, softest, inflated leaves, growing at the bases of an old large leaves. These are two or three inches high, whether when they first emerge they are flat and clasp me not know (I suppose they are), but as usually seen when still very small they are hollow and perfect in shape. They come to nothing. One does not see them especially in Autumn but at any time of the summer it seems to me. I saw two or three clusters today, but most old plants I v' thente to not show them at all, rather the youngest ones, but I have seen them on what appeared to be old plants. With this I send you a parcel put up roughly in the woods, (or hastily so) which will show you old plants with
24 May 1889

My Dear Doctor Gray,

My friend Mr. Thome Scott has but lately informed me of your having named a new genus for me—be having kindly transcribed the dedication.

It was a very great surprise to me but a most agreeable one—especially as indicating a continuance of that kindly regard which you have so long shown me, and which I value so much.

I thank you, my dear Doctor,

and ever yours,

[Signature]

Dr. Gray

Cambridge
P.S.

If I mistake not you said somewhere that fruit of Cimicifuga racemosa was wanted. I sent some collected this in a sandy flower bed and then in the hard winter fruit of Schizandra. Which may or may not grow in England.

[Signature]
Brog
21 nov 1894
My dear Carly
I trust you better
sent me the Shering
of Pteleum tri
= Spectrum at once,
though I don't know
whether you care
for them. I think
I sent you one a
good many years ago.
It seems extremely
rare — now I am
in it anywhere that
here — two or three

...
growing under five cases. Why seemed to disappear—but finding them and as the hogs were rooting around them I woke them up to save them (if of any value) for Herbert Printed. I never saw but one in fruit, and this was a single blind growing under a pine-talking in the pine trash! I saw through many with Mr. Fos's life and little

Don't know that I ever enjoyed a book more in my life—what a perfect life! To live, learn and understand—always keep up loving friends greeting him everywhere he went, and do well did he deserve all of this love—for he was a loving and tender heart—yet oh my! he was half a (no! don't say that!) but in the ally of duty or what he considers such—to this County—he
À Mons. Ala Gray, secrétaire de l'Académie Américaine des Sciences et des Lettres de Boston.

Monsieur le Secrétaire!

La personne qui vous présente par cette lettre, le grand vol
de une Théorédrie et ses deux derniers exemplaires des mémoires,
sur le Magnétisme des Volcans, que j'ai laissé découragé à l'Aca-
démie des Sciences de Toulon, est M. Bertolozzi, pensionnaire
tot distingüé et excellent professeur de mathématique, qui part d'ici demain, l'inten-
tion de s'établir chez vous. Mon excellent recommandation
politique et financière, celle d'un simple académicien, la reçoit
parmi, superfluous sans aucun doute, de lettres (ou plutôt qui vous
restent depuis, longtemps, ne l'avoir partagée du courrier). Vous
êtes, Monsieur, et honorable confiance, mon seul correspondant à Bo-
slon... il faut donc que, je vous, prix instamment de la promesse de la
relation, qui pourrait leur être, la plus avantageuse dans cette capitale.

Outre, son habileté bien reconnue, M. Bertolozzi est un jeune
homme, extrêmement honnête et fort aimable en société; les, mets tant
irréprochables... il porte, toujours, bien, qui, de ses amis, et connaissait
le mot tendre, très recommandant, destinent ce que vous pouvez, avec à son avantage,
eant pourra complètement vous rendre dans le cas où il s'éprou-
verait, ici, les mêmes services à l'un de vos recommandés...

Veuillez en attendant recevoir d'amples mes plus vifs remerci-
ments et ne croirez tel que je suis, efficacement,

Monsieur le Secrétaire,

[Signature]

Sous le 15 février 1854

[Signature]
L. Mowants
Menand Station,
Albany, N.Y. Sept 25 1859

To Miss Watson.

Dear Sir,

Would you have the kindness to tell me the names of the two Composites mentioned. I have raised them from Colorado last spring. One grows 3 feet high, the yellow one, the other about 1 foot.

Sincerely yours,

L. Mowants
My Dear Ward,

I shall be very happy to receive you & Dr Gray to dinner on Saturday next — come as early as you can and if you bring Mrs Ward with you it will greatly add to Miss Brown’s pleasure & mine — I remain

Yours most truly

Strewn of

Ladbroke Terrace

20th Feb’y 38
Hammermollt

My dearest 6 June 1844

I am much obliged for
your kind apologising me that you
have received some plants which
W. Harvey has kindly sent for me
and that no doubt I shall judge very
highly. I shall be engaged all the
beginning of the week and cannot
leave the pleasure of writing to you
before Thursday next. Then I will
date on my return from Ralcliffe
about 2 and then I shall hope to
find Mr. Ward quite recovered.
Mrs. Miers desires to speak with me & had agreed with me to invite your family aside.

I am now here to give my respects.

John Miers
My dear Sir Gray

In putting away my papers preparatory to my

usual trip on the Continent, I have discovered to my great confusion

a letter I had addressed to you in the beginning of March last which I

thought had been forwarded to you at that time. I must therefore now

beg your indulgence for this carelessness, and hope you will

excuse such a heedless want of attention. That letter was

written in reply to your very kind letter of the 20th December,

thanking you for your remembrance and for the information

of a manuscriptous plant you enclosed while I examined

and found not to be a St. Johnswort as you concluded, but to agree

with Nepeta as I defined it. It will be interesting to know whether

this species grows there. I have received a packet of original plants

from Tahiti, and would like to have some

of you can add your leisure notes on a bit of the paper, merely to give

a rough outline of the leaf and general habit of the plant, with such facts

concerning the plant as you may think necessary, and this clement

will be more of interest when the paper comes in, as it may interest

Dr. Dufresne with whom I have lately been much engaged with the Manuscript, as Mr. de Rovetti

and some others. The plants of this family, out of their houses for me to determine, and I gladly avail myself of the presence of the

plants before me to work out many details that have not hitherto been

able to do them. I shall examine specimens as a departure from home.

For conclude, rightly that in my paper on Monopera & Linnacea, I stated
...a topographical error for Plate 6. I hope to determine, Cunning's
no. 3402 to be Leucemia selaginella. For enquiries to卓和 400
Leucemia Selaginella - there is one in Allen Cunningham's collection
therein and at the Herbarium, and also a Cyopsis from Dr. Jackson in M.
Blandford's collection, by Dr. Eichardt, there is a plant from
M. Arthur in the Botanic H. all of these very welcome to the species
of Leucemia and another of Stenolepis, both found on foot on the
Borneo that constitutes, apart from the Cyopsis, an important plant family
which exist coextensively with the high climbing Adelapogos and the
sandwiched Pennisetum. The line of Mrs. Park's nail was gone and part of
your sentence was transcribed as pencilled. I hope it went well and a few days
ago you sent the line. Last Part of your letter. Most kindly thank you
for the present & present. About 140 feet very high.
On my return home I much apply myself intensively to bring of the long
setting memoirs & finished the third partly done towards the completion
of the 2nd. Of my illustrations. Which shall be forwarded to you as soon as
possible. Further often that I shall get an intimation of more
money in accordance.Mrs. Beers and my daughter write with
much love regards to Mr. Gray and to your son the letter he wrote to
her. Your health being in the enjoyment of good health, etc.
Adieu in my name. Peters. Fow. - my uncle. poured in a
number of dates - none of which name is in any way connected
with the names of the members of the Society.

John Murr
Doctor A. Gray
Cambridge, U. S. A.
My dear Sir,

I found yesterday at the Linnean Society your "Character of new Genera" which you kindly sent for me, and there were also your kind presents of Part. 2. of your "Planta Miqueliana" to me. I am very much interested in the "Nyctaginaceae" which have been long since for me. For these I beg to return my cordial thanks.

I have hastily glanced over your "New Genera" which have much interested me. When at lecture I will refer to my notes respecting the Chilean Nyctaginaceae, which have, I think, obtained my attention. In addition to which, I was acquainted with the genera in Eugenia, and agree with you in separating them but I do not perceive in what respects your genera differ from true Nyctaginaceae, unless it be in the thinner testa, but then, L. (Eugenia) and some others have a crustaceous testa as in Nyctaginaceae. I am not a little curious about the genus Nyctaginaceae, and judging from the characters there given, am led to doubt the propriety of placing it in Olacaceae. At least, I will take the liberty of suggesting these doubts to you: 1. because of the stamens being alternate with the petals, 2. on account of its epigynous corolla, 3. from its calyx being adnate to the ovary, and 4. for reason of its solitary suspended ovule in a single-celled ovary. You are probably aware that I have separated these genera from the Olacaceae, all Platanus's tribe of the Dacineae, andplants allied to them, placing them in a separate order, near Calycotrichae, they differ from Olacaceae in having their stamens alternate with, not opposite to the petals, and in having 2 ovules collateral and suspended from near the summit of the cell of an ovary, which is usually only 1-celled, but this arises wholly from abortion, for occasionally they found the ovary 2-or 3-celled and normally, it is probably 5-celled. You will find these ordinal distinctions briefly described in the late new edition of the "Botanical Kingdom" of my friend Lindley (p. 444), than already published monograph of the 13 genera of the Dacineae, and will furnish illustrated drawings of each in my "Contributions". You will also see there (p. 444) my views of the structure of the true Olacaceae, monograph of all the genera here given are completed but not yet published: here the ovary is also superior in respect to the calyx, but in Scrophularia, the ovary is half inverted by an adnate fleshy disk, on the margin of which the corolla is supported, and this disk or cup is quite free from this calyx, so that the ovary is quite inferior with respect to the calyx.
and only half superior in regard to the cannon: here as well in the rest of the
family, the maximum is undeniably at the summit, and half-eclectic at the base,
and the sources are detached from a few isolated plants — not suspected from the
summit of the hill; here likewise are sometimes occur icae (saccariae) there is a
remarkable large epigonomy itself, beside the 447 Senioris folio hypogynous
data; the same fact noted also in Hypogynae and in other cases - a fact of
inexplicable occurrence, though contrary to the received Theory. — The circumstorm
of the suspension of a solitary source from the summit of a single-areolar maximum,
combined with many other obvious features, appear to me to bring your genres
much nearer to the Lignaestruces, which family I have also briefly characterized
in my work. Your character of Hypogynaeae, I agree with those of
Myrtaceae, except in the circumstances of the position of the elements, which are
often in the latter genus, not alternate with the petals; the peculiar situations
of the author, which must be found also at that genus, and it is somewhat analogous
what of the Myrtaceae. I will look with attention on the Antilles, Wight, to the
Myrtaceae, for there is much analogy between these groups and the others of the
potamidia in America, and we shall see if it is not sufficient to add more than an
ordinary degree of interest in them.

During the last 2 weeks, there has
been a tendency to become somewhat more engaged in matters of business, and the
work on these has been quite frequent in 2 or 3 months. Those have therefore been to the Lignaestruces, and with more esoter
etc, which have not been the last 6 months. All have been able to do,
and complete a manuscript of the genus Hypogynae, of which 2 copies are included
and the third, the 2nd of my illustrations, which
have found a better than expected. After that I will get on with the illustrations.

The plates of which are entirely in progress, and which I shall have the pleasure of making
presently finished - I must also note some efforts to perfect the Myrtaceae.
This brings to mind a request, of which it is the duty of reminding you.
You will remember I wrote a few weeks ago to Mr. Gray, giving
information, which I would like to have if it is different from Foster's plants. Should you
have an opportunity of referring to it, I shall be glad to know what you will introduce
on your letter, as I wish, if possible, to include the plant, with some
short statement on its position, as it may be necessary to identify it.

I am now at work, whenever I am free, to examine the
flowers in the collections of the British Museum, and to
assemble several observations, and collect many facts, during my residence in Brazil.
I shall be able to show them demonstratively, that Pterostigma all along, have been
quite mistaken in regard to the structure of its leaves: that the parts which
has been considered as obsolete, consists of 2 minute 12-glands. It is true, while
being the large scales of the maxima, which has been held to be the concomitant
scales by most authors, and albinos by Sowerby and others. This
structure brings the Rhigodelae, at least very closely connected, in
close contact with the Raf family, and draws still closer its connection with Hypogynaeae and albing males, at least. As you do not describe
any member of the Myrtaceae in your "Genus of the Myrtaceae,"
I conclude you consider that no plant of the order is found within the limits
of your extensive region. But can I assure that any one plant to be met with,
with the Abhine of Stenacae. Still however, Clinton's vision is figured in
Catalpa, Ailanthus, and quoted by Hadfield as being a native of Brazil, as well
as in America, very much to know the actual structure of this plant.
It is impossible to there any accurate description. I suggest that all our
Persea Clinton will be found generally distinct, not only from this,
but from several others in the genus, bearing to these names, which
again appears to me essentially different from the original and typical species
of Catalpa, if this latter plant, no specimen exists in the collections of the British
Museum, but hope to be more successful in your search for it at home, when
there find time to get there.

I shall be glad to hear that Mrs. Gray and your health
are both well, and your daughter desire to write with me in
the dear lady, and to your health. I trust to have the pleasure of meeting you
again at some distant time.

Believe me to be your dear the always most truly yours,

John Ellis.

Date: 1816
Temple Lodge - Hammersmith
London 28th October 1854

My dear Doctor Gray,

Your two greatly esteemed favours of the 18th and 21st of July reached me 2 days prior to my departure from London, when being much occupied with business and putting in correspondence to finish for the Brazilian mail, I was unable to write to you in reply. I now however avail myself of the first leisure moments since my return to thank you for your kind letters and their enclosures, of a seed of Calycocarpum loricatum, a specimen of Ducleyeia, your recent additions to the genus, and a fragment of "Stephania Forsten," all which have greatly interested me. I made an analysis 4 years ago of a seed said to be of Calycocarpum, which I owed to the kindness of M. de Queiroz: your specimen, though not large enough to exhibit the form of the embryo, has been of much value by showing that I have been correct in the results formerly obtained. This seed differs essentially from that of Calycocarpum, which wants the singular cristate margin, and has only 3 prominent teeth at each extremity. I thank you for the leaves of flowers of your Polynesian Menispermacous plant, which on account of its curious arrangement of parts might be referrible to the genus Elypea of Blume, but this cannot be - in Elypea there are the flowers crowded on a petiolar tube, the petals singularly bellated and winged, and the nutlet only 2-celled. I have observed the flowers of Stephania constantly 3-mucronate, but if as you say, the flowers in your plant be both 3 and 4-mucronate, it may throw some doubt upon the correct reference of some species of Elypea, from which the male flowers differ from those of Stephania only by the number of the parts. In the seed, their great difference is at once detected: the nut of Stephania is flatter, always with a distinct perforation in the middle of the Cordyle, or central hollow, flat disk round which the cell is coiled - in Elypea, the Cordyle is very thick, convex, without aperture, and forms a close chamber. Your plant from its habit and its usually 4-mucronate flowers, can therefore disposed to refer rather to Elypea.

The specimen of Ducleyeia is extremely interesting, and its value is much increased by your observations of that genus. The presence of a double row of floral envelopes, with the outer series so many times longer than the inner short, is very curious, and quite at variance with the usual arrangement found in the Santalaceae, to which order I fully agree with you in referring the genus: we have however in Ulosa, Chorterium, and Nanoclea, something
approaching to the nature of a double envelope, for the segments of the border are articulated on the margins of an absolutely 5-toothed lanceolate leaf, the teeth alternating with the teeth. I can perceive nothing in the activity of Dicksonia, at variance with that constant of Scleranthus, and it appears to me completely that of Cheironium, and some other genera, not exactly similar, but somewhat similar in addition. The dried specimen appears to be dry, and not imbricate ( ). But in general cases, I attach little value in itself to the character of activity, however important it may often promise an accessory feature. This partially imbricate mode of activity, which exists also in Scleranthus, is general through both families, and often Scleranthus, and is frequently carried to such an extent near the summit of the petals, that their several offices are often sometimes infected in the shape of a pendant, partly extending half way down the petal, and these appendages meeting in the axis of the flower, have a tendency to gather in a central mass, hanging in the middle of the petals before bursting. ( )...you allude to the circumjacent of my having excluded, Rupicostata, from Scleranthus, on account of its activity. I believe it was in error in doing so; it was not however, because of its activity, which is exactly that of Scleranthus, but of the standing team, of the flower at that of a Scleranthus, a genus referred to botanists to Agrostis, Platanus, to examine, it may appear more tenable and less probably to refer to the Rupicostata, which is the same as the former, although the latter genus has a decidedly imbricate activity. This family, it is now evident, has no relation whatever with either Baccaceae or Scleranthus, and is distinguished by a normally plain to nearly ovary, with 20 or more petals, from the true, which is much less numerous, though generally only one seed becomes developed, and only a single seed matured. You kindly promise a sketch of your genera, to be printed, and a tracing of a new species of Rupicostata, which I shall be very glad to see. The few characters without published by Plato's, Platanus, refer to Agrostis, Platanus, and I have always considered the former to be merely a synonym of Platanus; if this be the case, Rupicostata must remain valid for Platanus genus, long since published, and clearly belonging to Portulacaceae, but which was I think, without reason, referred to Aizoonanthaceae, by Baldick and Aubrey, and by the latter, injudiciously changed to the name of Aizoonanthaceae ( 1871, p. 14). Referring to that subject, you say, if my species of Rupicostata, the structure of the ovary will at once decide, whether it belongs to Scleranthus, or not. If you find much external resemblance in the flowers of this family, with those of some genera of Lycanthraceae, so much so, as to have decided the experienced botanist as Portulacaceae.

In my last letter, I mentioned that my attention had been directed to a more extensive examination of the Scleranthus, upon finding that my observations made in Brazil, upon the plants of that family, were not in accordance with the facts upon record. At the last meeting of the Linnean Society in June, a portion of my paper was read, on the structure of the leaves in the Scleranthus, and the thistle-like teeth, which I have discussed, that has naturally directed my investigations, where I have taken the liberty of offering an opinion on a point of structure which I seem to have adopted on the authority of Fideckon, in your most excellent work (Gen. Pl.) you have described the exterior tissues of the leaf of Magnolias, and of these, and the thick cuticula covering the tergum, justifying this conclusion on the appearance of spiral vessels in the anastomotic attachment of the anastomotic circles. There can be little doubt as to the fact, and I am anxious to prove the same, and especially to the thistle-like teeth, because these exist in a distinct, free raphes within the excision, from the basal point of attachment, and lying between the arteries, and one side of the cuticula coating, and disappearing at a point in the summit of the leaf, having the thistle-like teeth, which I consider to be the leaf. Through an opening, which I have called the prickle, in contradistinction to the microspore, at the opposite extremity which it penetrates, and where it terminates in the darkened thorns of a more internal integument, which is a covering which you and others seem to have overlooked: this simple, obvious points to the cuticulainy extremity of the leaf, such as in an analysis many years ago of Tolmeo, species little differing from Alpinia. It was of some importance to me to settle this point, in reference to my investigations of the Scleranthus, because there exists in the seeds of the tribe, a strictly similar coating which I hold to be the plating, investing a cuticular coating, which I conclude to be the leaf, for whatever form its nature in Alpinia, must be the same in Scleranthus, and Scleranthus, can be called, because it is always exterior to the raphes. It is clear that if the leafy arteries were a development of the primerv, and the thick cuticulae coating, or the barb, continued from the bounding the raphes could not exist in a free state. Large things, it must necessarily be exterior to the production of primerv, and whatever coating exists in the leaf, exterior to the raphes, must be of subsequent extraneous growth, and therefore of less. This view of the matter is further proved, by the condition of a corresponding thorn in the tribe, Deinrura, where it is also thin and angular, but not entirely, as in the Albins, the margins being free, and overlapping each other.
along the dorsal line of the seed, which it encloses entirely; in the Galiaceae, the same development assumes the form of pulp. Hence it appears to me clearly demonstrated, that in the Galiaceae, as in the Magnoliaceae, the external crimson testa-tunic is arillous, the hard crustaceous coating the true testa, with a well-marked perforated diaphragm near its summit, through which the extremity of the more external xylem penetrates, to terminate in the chalaza of the inner membranaceous tegmen. In all this there is nothing out of the ordinary rule, such being the usual structure found in dicotyledonous seeds. This matter became of additional interest to me, because Dr. Thomson informed me, that he and Dr. Hooker, in their forthcoming Flora Indica, had adopted your view of the nature of the tunic in the seed of the Magnoliaceae.

On my return home I had the gratification of finding a letter from your friend and able collaborator, Dr. Torrey, which I have much pleasure in appending. This brings to my mind another enquiry. There is often, as you well know, a great analogy between the plants of Chile, and California, or other parts of Northwestern America, and as you have studied the botany of these regions, particularly the Composite, you will probably know whether any of the Cymaeaceae have been found there. I make the enquiry, because I have collected the materials for a more complete monograph of the family, which I propose to illustrate, one of these days.

We have had a most delightful trip to Switzerland again this year, and were greatly charmed with parts of the Valais, we have not before visited, especially the valleys of Zermatt and Saas. There is here a perfect circle of extensive glaciers, and snow-capped peaks of extreme elevation, all close around, including Mischabel, Monte Rotta, M. Cowan, Weisshorn and many others, the panorama being rendered complete, by the more distant Jungfrau and highest Bernese Alps. In my estimation, it is the most magnificent scene I have ever beheld, far more striking than the justly celebrated views about Chamonix. Mr. Miers and my daughter reunite with me in very kind regards to Mr. Gray and yourself, and believe me always, very sincerely yours,

John Miers
Hammersmith, 5 June 1855.

My dear Doctor Gray,

I have long delayed writing to acknowledge the receipt of your kind favour of about Christmas last which I had carefully put aside for reply, but which has somehow got mislaid: the delay arose from the desire to acknowledge the receipt of the books which you then announced you had kindly sent to me through the care of Mr. Lee. I was at Kew three times in January enquiring for them, but they had then been mislaid; since that time I have been confined within doors first by indisposition and secondly by a sprained ankle which accident kept me a prisoner nearly three weeks so that I was not able to renew my visit there till very lately, when I was glad to obtain your note on Vávda and "N. her. Thurberiana" which had at last turned up and was been put aside for me and which I return you my very best thanks, as well as for the seeds of Calycoearpus which you kindly sent me some time ago. I have also to thank you for your last kind note of 9 April last announcing your having forwarded through Mr. Trinche the memoirs Doctor Torre had so obligingly sent to you for me which have come safely to hand, and for which I have written to offer him my very best acknowledgments. Your "Note on Vávda" especially interested me as you have there commented on some observations I made on Styraeae, in which I will here rejoin in the same cordial manner, that I much regret you have not been able to coincide with what I have so distinctly met with in the structure of the ovary of Styrae. My observations on that family were only general, and in relation to its affinities with Olacaceae, it being my intention at some future time to examine more in detail the plants comprised in it: all I can say is that I certainly met with the structure depicted in the "dep flAnna": and that you will find it confirmed in other quarters and will presently show. I think you have not overlooked all I have said on the subject, or you would have looked at the matter differently. The circumstance of the normally incomplete review of the carpels in that family, which you appear strongly to doubt, is think rendered even more manifest by your own argument; you have confirmed my observation of the upper row of ovules being erect, the middle horizontal, the lowest most pendulous in the several species you
examined, but you add this is not true of the whole genus, as Buechmiiji points

Theman exist in one instance, as you seem to think may also be the case

in some American species; a consideration that appears to have much influenced

your opinion, but it appears to me that mere direction of the ovary in no way

affects the question, and that this is of little consequence, even in a genus, much

less in an internal point of view—-for it in any species the placental column, which

unites the bases of the incomplete developments, is broader than usual, so the more

for those of the ovaries on either side would cause them to assume an erect position, in which

case the ovary would still be 3-ovular at base and 1-ovular above. TheI am

surprised, you say, you do not find the case, which is the more reasonable, because you admit the same, the accuracy of Buechmann's definition of

"placental incompleteness of an erect column distalibus" is not; these

seem positions are thick and solid, and would not become "ovary distalibus", if there was any real union of their sides with the axis—-indeed the distinctiona

of the placental column along that line, which you admit, does not confirm the view I have taken, that an absolute union has been, at any time, to be found

in every species. I have examined Pololls ottomae, which are comparatively <9>.

find no union of the incomplete developments above the line of the columnar column.

This structure so closely approaches Humistraca, and differs mortally, and so

widely from that of Symplacca, that, taking into consideration the constant

and striking differences of the structure of their Stamens, the great dissimiliarity of their limbs and seed, and other Characters, there is not the slightest evidence to warrant the separation of Symplacca from Symplacca, and no solid foundation whatever for their union, upon nearly similar

grounds, they differ as widely from each other, as the Humistraca from Orestes, if

the facts I have stated, be founded on truth.

You do not admit the existence of the epigynous glandular tubercle in the

ovary of Symplacca, although you confess to a thickening of the epidermis there.

The epigynous appendage, which I have observed in Symplacca, is very different in its nature

from the epigynous disk found upon most inferior ovaries; it is without doubt a

distinct piliferous deposit, not a mere thickening of the epidermis, as you suggest, and

is of a more peculiar occurrence, upon completely superior ovaries, as I have frequently pointed out; it often exists concurrently with a thick

epigynous disk, and has called it on all occasions, if it exist, on any

epigynous gland, to distinguish it from the true epigynous disk, whose true

nature was I think pointed out by St. Béronais. It is largely developed in

Symplacca orthoceras. Where in Symplacca it forms a culture as large

as the ovary itself. There also described its existence in Symplacca, which

to this cause, and the circumstance of all its cases, its species, the

epigynous disk is largely developed in the Brazilian Symplacca /Symplacca superba /

is seen in the living state much larger than in the case in W. Brazilian Symplacca

from the United States, a much higher, and of much longer diameter than

the ovary inferior ovary, and about 20 anyprocesses, or if a dark orange color.

Among Symplacca I have also a genus not unfrequent, where the parts

are 3-ovular, the Stamens 15, vectorial as in Brazilian, but the ovary is

3-ovular, with a single suspended ovule in each cell, one of which always

becomes abortive, the base is a flat, indurated, short, amorphous, columnar-

plantation with a single leaf; in another species the ovary is 3-ovular, the

Stamens' under the ovary, are 6 or 9; the ovary is 3-ovular, with a single suspended

ovule in each cell.

You object to my statement that the ovary is inferior in Symplacca—

and superior in Brazilian, and argue that this is not true at least in Brazilian,

where you say, the ovary is not inferior; but on this point it appears to me,

you are entirely under misconception, as you will best convinced if you

refer to Schott's Nov. Ed. 8 tab. 42, where you will see in the analysis of

Symplacca orthoceras, the ovary depicted as being equally superior, the

cell 3-ovular at base, unicellular above, and the ovaries all erect, as I have

above stated, would happen, whereas, as in this case, the placental column

is shorter than usual. You will also observe a repetition of these same

facts in the following plate of Brazilian, all confirmatory of the views I

have stated in regard to this family. You remark moreover, that a

superior ovary is denied in the case of Plastogyra and Helicia; in the

former, it is however described as being "semi-inferior", which is about the

same thing; while the latter genus was considered so abnormal by Dr. Liliane,

both from Plastogyra and Symplacca, that he formed it into a distinct suborder

(Belicera). Have never seen my specimen of either of these genera, so that I have

no positive evidence on the subject. — In noting that the Stamens are few-
and uniserial in Hyracca, and pluriserial in Lym庇ocacco, I asserted only that which is generally the case, for I was aware of occasional exceptions to this rule in the latter family, as in the instances I have abore described within my own experience, and even in Barborina, where though apparently in a single row, the 15 or 20 Stamens were in all probability 3 or 4-serial, but owing to the narrowness of the filaments, obliged by pressure to assume an uniserial position. The fact, one way or the other, is of little importance to the main question, but the circumstance of the adnate character of the anthers in Lym庇ocacco, and their being oscillatory in Lym庇ocacco, is one of far more consequence, a difference which I believe to be remarkable. Constant, for although as you remark, the anthers in Plano Lym庇rac may be shorter than in Lym庇rac, they are still described as being adnate. You will therefore I am sure excuse me when I say, that I do not perceive in your several observations one tenable argument, or fact, that tends to invalidate the evidence I have shown, or that can annul the inferences which naturally follow as a consequence.

You express surprise that I should have ignored any affinity between Lym庇rac and Melliaceae, but if this was not shown, it was manifestly inferred, by my conviction stated in positive terms, of its real affinity towards Melliaceae, as indeed had been long before suggested, though upon different grounds, by Martius and Bentham, and thus of course to the Melliaceae, between which families there exists a most obvious relation. I was well aware that Bellandolle in his Prodromus, had placed Foveolasia in the latter family, as had been proposed many years previously by Justicia where he also associated Lym庇rac. It escaped my memory at the moment, but it soon occurred to me, that this connexion was rendered still closer by the observations of Justicia, showing that in the Melliaceae the cells of the ovary are in the manner uninocular, at the summit, where the sepiments are incomplete, or as he calls them previous. You must not imagine that my suggestion for the removal of the Lym庇rac from the position hitherto assigned to this family, to the neighborhood of the Melliaceae, was either arbitrary or speculative, and I will endeavour, in as few words as possible, to show you the reasoning that led me to adopt it. — In seeking for the soundest views
of systematic arrangement, it always appeared to me that the characters
drawn from the organs of reproduction (as being most constant) should
always claim the preference, and the most important of them certainly
the ovary, the organization of which is generally believed to be either simple
or formed of various combinations of different carpels, each carpel resulting
from a peculiar modification of the leaf, some portions of which, generally
the interfolded margins, are supposed to become ovuliferous. Upon this
hypothesis I have readily explained the source of the numerous kinds of parietal
ovule or projecting placentations but does not account for the free central placentas.

I have imagined to proceed from the point of junction of the petioles of the
combined carpellary leaves, the margins of which remain sterile; while in the
case of the partially many-celled ovary unicellular only at the summit, I
have assumed it is the basal portion of the carpellary leaves alone that are
sterile, while the upper portions remain sterile and disunited. Upon this
hypothesis the proper place in the system of the group I have called Cynodermata
and which is now become extensive, will naturally fall into order, following
those families exhibiting a gynoecious and bicarpellary ovary
near the head of which and preceding those possessed of a more perfectly
many-celled ovary near the head of which the Osaeceae and Angiospermae
have hitherto stood and close to where the Meliaeae have been located.

A somewhat analogous structure occurs also in Bombaceae as was long
ago indicated by Pachira to exist in Riodendron and Chorisia / some-
an ovary always more or less unicellular at summit and 5-celled at base
that of Pachira he also described as being "varius quasi unicellular", and this probably
to also in Bombax. While in Rio I had an opportunity of studying the unicellular
structure of the ovary and the remarkable dedevelopment of the carpelles and seeds
in Adansonia - it is very singular and is most imperfectly known and described.

But I may some day publish it from the complete drawings I made many years ago.

All that I have hitherto stated in regard to Hypaeceae, Sympeleaceae, &c.,
I have been simply indicative my object being to show they have been sadly
misplaced in the Prosomata of Sentendolle. A more critical and patient
investigation of these families is necessary that I think they have shown enough
to point out the direction into which they will generally fall.
Your observations on Rhododendron are very interesting and excellent, and your references of the genus to the Anthocarpa—doubtfully approach the truth. From the short characteristic you had given of the genus, the eminent habit seemed to point more directly to Leanischer than to Anthocarpa or Alstonia, but in considering my suggestions of its probable relation to the former genera you do not appear to have borne in mind that I had previously said of that genus, "Anthocarpa," as suggested by Dr. Hook seems to approach nearest in its structure both to Rhododendrom and a close parallel with Laxiflorum, and that the writer of the latter species supposed the flowers of its own and the same genus to be similar, as in size, shape, and color. The same genus may be also adduced as similar to Anthocarpa, with a single corymb suspended from its summit in monopodial form, with the stem suspended having an ornamental effect.

I am inclined to assign a variety of Rhododendron, one of its most singular features, to the structure of the authors' description. Indeed, I have noticed a number of these with formerly unclassified flowers, which I have subsequently added to alternate, frequently prominently leaves and conspectious flowers (Hook. Ill. 130).

In describing its genus, I propose to name five whole genera of the structure of the writers' description. Indeed, I have noticed a number of these with formerly unclassified flowers, which I have subsequently added to alternate, frequently prominently leaves and conspectious flowers (Hook. Ill. 130).

The same structure occurs also in the class Arecacea and in the class family

I have already adduced remarks here for what I have before said, the misunderstanding. In all cases of botanical singularity I have endeavored to avoid being guided by mere external appearances and seek, whenever practicable, to trace the development of these species through several stages: this rule has proved useful in the present case, for we have many instances where the ovary is in the form of a single or a number of distinct locules, but this exists in the normal structure of the flower of the same family and of the others to which I have already referred, such as Sycamore.

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My dear Doctor Gray,

I take the first spare moment after my return from Birmingham to acknowledge the receipt of your greatly esteemed favour of the 8th. by which I was surprised to hear you had been so lately in London and Paris; we must have been in the latter city about the same time, where it would have given me much pleasure to have met you. I returned home in the beginning of October after a delightful tour in Switzerland & Piedmont stopping in Paris both ways, and had hardly been a week at home when I was taken ill and confined nearly 2 months so that I have not yet had the satisfaction of seeing any of my friends at Kew, or I should have learned from them that we had been in such close proximity to each other. I am obliged for your remarks about the Ovula of Magnolia, and although we are still at issue about the nature of the fleshy coat of its seed, it is gratifying to me to have to contend with so able and generous an opponent: I feel confident however that as the discovery of truth is our sole object, we shall soon be of one accord & if I am in the wrong I will hasten to acknowledge my error. I had already combated your very able and ingenious arguments published in Hooker's Journal for August, in a paper now in the hands of Mr. Bennett, which is not yet read before the Lin. Soc, but which I will not withdraw, as there is nothing in your letter that induces me to alter any opinion there expressed. I have here taken a wider view of the whole matter, and have entered into the question of the Raphe under its many varying circumstances of development. I was led into this by the puzzling question that occurred to me in the investigation of the Cottons - why should the Raphe be quite free from the Testa as a simple Cord in the Celastrea, while in the Tovomites and Garcinias it appears as branching nerves spread all over and imbedded in the substance of the Testa? These appear here an incongruity in the position of the nourishing vessels of the future Raphe in regard to the primine which at first sight is inexplicable: the same difference...
has always puzzled me from the earliest period of my acquaintance with structural botany, and the explanation of which I have sought in vain in botanical works, or in various memoirs that treat on the development of the ovule and seed. I think, however, I have reduced these apparent incongruities to one simple law of development, but I cannot give you in this latter an idea of my conclusion on this subject, let me add that our discussion on the nature of the cell-form coating of Magneta leaves, has led to the solution of this difficulty. I make a distinction between the different kinds of epidermis in various seeds - those which, as in the Tournesol, are produced immediately from the placenta or female, consider to be a true and, in which sense, the cells change into tissues, but scarce employ the name of drilium to such coatings as exist in the Cicada, Magnetae, etc., which I hold to be a development of the placental sheath, containing the nourishing vessels of the ovule, which you figure in Magneta fig. 56. Although you failed to notice the extension of this sheath [for looking at the ovule from the expansion of the placenta] I know that such extension must have taken place as it has been seen to be developed in other cases. I believe it to be identical with the chalazae of Planckon which, when they produce growth, are analogous in the coating of the ovule, because the seed coats are quite analogous to that of Magneta and where the sheath is found exactly under the same circumstances, as the ovule, it must have occurred in an extension of the formers of the ovule, though he states that the latter was hidden at the time by the placental sheath, but if he had consulted a little more, he would have gained more valuable information that the facts which he has given in different stages of the growth, proceeded from the extension of the placental sheath, for example, to the right side of the figure (where the seeds are supported with some modifications by his friend Neumeier) the incomplete formation of the ovule in Tournesol, proceeding from the upper portion of the ovule or placental sheath, its lower development being still visible as a cord attached to the latter, and both Planckow and Tagliaron at different times actually investigated the progressive and complete formation of this covering in the ovules of Goutin, which they describe with much minute care under remarkable similarity of circumstances - they both describe the extension of the placental sheath in the form of an extremely delicate membrane, or a very minute drilium, and other progressive thickening as to form constituent to its entire thickness. - In cases, however, where the drilium is formed as a mere particular extension of the placental sheath, and where the quantity of nourishing vessels is small in extent, these branch out and are extended with the growth of the drilium over the surface of the primord, now encased within the extended sheath; where the latter has no secretion of fleshly tissue to thicken its substance, but where mucous deposits intervene between the drilium and primord, the result is such as we see in Tournesol, forming an apparently simple, but really compound, tissue in the seed, with a branching sheath, interposed between the different membranes of the drilium and true sheath. A very destructive root of this exists in the seeds of Tournesol and other cases, where the kernel is enveloped in a somewhat thin Chalazae, which appears to be a single sheath, but if it be well macerated, it is found to be double, the two coatings being easily separated, having a distinct network of delicate white threads, intertwining between them, which may be seen on the surface of beautiful spiral vessels, thus exhibiting a peculiar modification of the sheath. You will see we are somewhat approximating in our ideas as to a difference between the ariled ovule and the true ovule, you consider the latter not as formerly as the developed primord, but merely as an embryo of the seed, which latter you never name to be the seed; while I regard the true ovule, because of the distinction of the sheath. At the same time I hold the larval covering to be different from a true seed, though both are of placental origin, the one being a development from the placental cord, the other the placental sheath, both are distinct facts. In my view there are many phenomena attendant on the growth of the external coatings of seeds, and of the varied form and position of the sheath may be satisfactorily explained by one simple rule without violating any known law of development. When you take all these facts into consideration, I think you will come to the same conclusion. The fact you notice of the deposition of delicate cells in the substance of the primord, without any relation to the growth of the sheath, is of great interest, but it tends rather to confirm me rather than to rebut any arguments. I am greatly obliged for the flowers of Tournesol you enclosed for me, and I have some time ago prepared a memoir to show the very decided structural
differences that mark the Stryaceae which will be followed by another on the distinctive characters of the Symphloegia. In the former I have noticed the observations made in your "Notes on Vasco." I will not publish this until I know something of Halesia & Styrax; neither of which I have yet seen, and shall therefore gladly avail myself of your kind offer to send me seeds of Halesia and better still, if you can spare me a fragment of flowers or it. Now enclose you a tubular view of the leading characters of these 2 groups families placed side by side by way of contrast, and this is accompanied by sectional analyses that illustrate their extreme differences. There may exist a shade of variety in some of these features at times, as you have shown, but the main facts are true. I believe in all cases the only doubtfull point remaining in Halesia, but that incongruity may perhaps be solved by an examination of its structure. Don't know made a distinct family of it apart from Stryaceae but even if retained in the latter it would not be fatal to my views, for in the very natural order of Malacostomataceae, as well as in others, we find both superior and inferior ovaries: I enclose you a flower and section of Styrax loccis when you will see the vase shaped superior ovary found in all the Brazilian species you will there perceive the thickened apex which I have figured, protruding in an embracinal form over the lower tubular portion. The very fleshy apex is however still more strongly marked in one of Gardner's species (his No. 4892 from Minas Gerais) which I also enclose. I had no idea of intimating that in Styrax the ovary is surmounted by a fleshy disk; all desired to show was, that the summit of its shell is thicker in substance than the lower portions and glandlike in appearance as in Flacourtiaceae and in many Malacostoma, but this wants the difference of colour and texture and is of a nature wholly distinct from the conspicuous fleshy lobes Cibicium that crown the inferior ovary of Symphloegia (Stenocarpus florib.); that is a true epigynous disk which is never found in Styrax. In the flowers you sent me the ovaries were completely crushed, but enough remained of the fragments to show the existence of the same form as in S. crispus. The circles
we regarded about the central placenta, and that the ovary is distinctly
spherical in the summit. I did not mean to say that the direction
of the ovules influenced your opinion regarding the affinities of the
Typhaceae, but as you remarked that my statement of the ovules
being sometimes pendulous or horizontal as well as erect, was not
borne out by your own observations, and that Luccarini had
figured them all erect, I wished to show this might (as indeed does)
sometimes happen, without affecting the nature of the structure,
for if the placenta be short, all the ovules must necessarily be erect,
if longer, they partake of the several directions, as shown by me,
mere from the effect of mutual pressure. — The fact you
noticed of the deposition of thickened cells in the substance of the testa
of Leguminosae, simultaneously with the growth of the fleshy envelope,
is very interesting; but it tends rather to confirm than to rebut
my arguments.

28th Decemb. Since writing the above, having many other matters to attend to,
I have been pondering upon Kalarea, and have looked to the descriptions of
Gaudin & others, relative to the structure of its fruit, and if I understand this
rightly, the 4 cells are complete, both the dissectiments and the axile column
become greatly thickened and odorous, the latter hollow in the center, showing
a disposition to separate into 4 carpels, probably from the introsion of its
dissectiments: indeed Endlicher asserts it to be "4-fyrena, pyrenes inter se distinctis,
and Gaudin so figures them: the seeds are erect or pendulent (according as an erect,
or suspended ovule has become perfected) and they are attached to the axile placenta.

Assuredly there is here no analogy whatever, with the fruit of Typha, indeed it
shows that of Kalarea derives its origin from a very different kind of ovary. If
might be allowed to guess at the affinity of a structure which I have never seen,
and which I know only from imperfect description, I would suggest a greater
analogy with Dactylis, or other genera of the Phaidelephiaceae: in the shape of its
multilocular pericarp, terminated by the hard and persistent style, it agrees with
Dactylis & Dactylis, with which it also accords in the form of its albuminous embryo.
Even in the characters of its floral parts, there seems a greater approach to the Philadelphia than to the Styracaceae; it has a similar superior ovary with a free divided limb, apparently voluble in oscillation; numerous petals slightly echoraceous, and with a similar continuous activation; stamens also 2 or 3 times the number of petals, and with a similar origin and form, and anthers similarly affixed; both of like structure in nearly all its details. It is true that the leaves are alternate, not opposite, but then Styrax has sometimes alternate leaves; the hairs in Philadelphus, as in the Phil., are sterile. There are certainly very striking coincidences. Now you have paid considerable attention to these groups, and therefore venture to make these remarks, I will endeavor to get a sight of your Philade. Foundiniana, to which Willans refers on the subject. I shall add that Persea americana, another true Philadelphus plant, appears in the manner most allied to Persea, with which it agrees in the peculiar presentation of its seed, and the activation of its corolla; taking its characters as your in St. Roc, there appears a fair reason, in fact, to the Philadelphus.

These must close this, which has been much longer than intended, and you must excuse me for having been carried too deeply into the subject, tending to lose your patience. I will deliver a little more candidly regards to Mr. Gray and yourself, with our best wishes on the approach of the new year and our hope that you may enjoy many future happy ones. Believe me always, with the most loving esteem, yours most truly, John Muir.
Ovary wholly inferior - crowned by a large pubescent grooved coloured fleshy disk; rarely 3- or 2-celled. Cells complete, their axis being continuous with the style. Cells always inferior; but the ovary appears often semi-inferior by the protrusion of its large epigynous disk.

Ovules 2 to 4 in each cell superimposed, suspended from the internal angle of each cell near the summit of the axis.

Style simple elongated.
Stigma capitate with many lobes as cells in the ovary.

Drupe inferior, generally somewhat fleshy, the scar of its being formed by persistent calyx, and crowned by its small persistent limb, having an emarginated depression in the centre remaining from the epigynous disk. Nut generally hard and long, mostly 3 or rarely 3-celled - sometimes concave and 1-celled by abortion, indehiscent.

Seeds always suspended - 1-often 4 in each cell, long, cylindrical pointed at each end.

Tests and integument in one simple and membranaceous tissue.

Albumen copious, fleshy.
Embryo slender, testa with radicle nearly the length of half the whole, inferior pointing to the hilum.
Coiled very minute, equal in breadth to the radicle.

Ovary wholly superior, turbinate, apex conical or flattened, and projecting in an umbilical form - often thickened and gland-like in this part - briefly 3-locular at base, above this the dehiscences are incomplete and continued to near the summit like narrow projections, leaving the middle and summit of the ovary 1-locular, the placenta springing from the base of the ovary and above the line of the complete portion of the partitions in the form of a free central column to which the ovules are attached; this column is never in connexion with the style. Ovules 6 to 10 in each division; are 3-ovulate, erect when the column is very short, but also horizontal and pendant when it is longer; each ovule is attached by a capsule-form tubercle to the placenta.

Style simple elongated.
Stigma almost obsolete.

Drupe superior very fleshy, surrounded by the free persistent calyx.
Pericarp thin and coriaceous, 3-grooved 1-locular, by abortion 1-seeded rarely 2 or 3; dehiscence 3 longitudinal lines alternate with the 3 grooves that correspond with the compressed nerve-like incomplete dehiscence. It is transparent, beautifully marked with interrupted red lines radiating from the havens.

Seeds always erect, generally solitary, erect, 3-grooved by the pressure of the incomplete dehiscence, oral, hilum basal, subterminal; somewhat flattened by compression of the ovule placenta, about which the abortive ovules remain visible. Aril thin, light brown, opaque, distinct longitudinal simple running from base to apex, where it branches and is lost in the summit of the ovary.

Tests thin, membranaceous.
2 internal integuments distinct, extremely thin and transparent.

Albumen white opaque, oval, fleshy, subterminal, with an embryo white, globular oval, fleshy, subterminal, with an embryo white, globular oval, fleshy, subterminal.
Humbersmith 29 Novem' 1856.

My dear Doctor Gray,

I was very glad to receive your favour of the 20th ult., and the copy of your Manual of Botany, for which I beg to offer you my best thanks. I find it a great improvement on the former edition and an excellent text-book, but the faithful and characteristic drawings of the flowered plants which are so ably executed, render the work still more instructive and valuable. Since I last wrote to you, I have not been idle. You will see in the part of the Linnaeus' Trans. just published, all I have stated in regard to the nature and development of the cerasus in plants, an investigation to which I was led by our difference of opinion as to the nature of the outer covering of the seed in Magnoliaceae, and you will notice the curious phenomena I have described, in connection with the development of the nucellus and my detection of the frequent occurrence of a crustaceous cerasus in plants, which term has hitherto been considered to be the testa, understanding by that term only the tunic resulting from the growth of the primine. Similar inquiries have been followed up in other papers, I have now in hand — one on the Winteraceous Illiciaceae, where I was also able to verify what I have long more than suspected, that canella is closely allied to Drimys and in no way related to Platanus; it agrees with the former in the structure of its seed, which is a very characteristic feature: in the Winteraceous, the outer tunic, generally taken to be the testa, is also an arillus. I have likewise drawn up my notes on the Styraoe, and have brought fresh evidence in support of my views respecting this family, acknowledging that in your notes on Winteracea you are right in your statements in some instances, but mistaken in others: my earlier notions had been framed principally on my examination of the So. Amer. Species of Styraoe, for which I restore Cavendish's genus 'Argilia,' which differs in many respects from Styraoe to which I suspect your No. Amer. Species belong and where the Corolla is more or less imbricated in ostivation and as occurs also in 'Halesia T. Merr.' styraea.
a conspicuous feature occurs in most of the genera of this family, where the outer coating of the test is a hard cuticular material and where an apparent, more or less, consisting of a peculiar spiral thread, that in diverse as a hollow tube, between the arillest and the true teel or intermediate coating. Helicea shows us an exception, and this has much to tell us about the common cephalopods. Thanks for your kindness. I have been able to comprehend the entire structure of this figure: at an early stage its many quite corresponds with the well-marked structure of Helicea, but it subsequently undergoes a singular metamorphosis which I have been able to trace in its growth. By its peculiar mode of growth its pseudo-cells are brought into the position obtained in the true face, and these becoming covered over by the first deposits they are converted into perfect cells.

Lately I have been examining some Rhamnaceae, principally with the view of determining the genus of the tribe. Helicea, to check my notes, and I am able to do this by referring to a book on Rhamnaceae, especially to Reut. and other books on the family. The stem is a small one, and has been compared with the preceding phenomenon by all botanists, even by Gillies, who ought not to have done so, as he had seen my original notes. In the tribe of all the Heliceae, we find a hard cuticular material in the testa, the testa being perfectly free from it, and adhering to the intermediate to the arillest ones. There are some phenomena, however, connected with the structure of all the Rhamnaceae that are peculiar, showing there has existed a motion of the testas independent of the testa of the whole. This you will partly obtain from your own excellent analysis of this order in your "Genus." I know that in Helicea and Boronima alone, the testa is seen on the dorsal face of the test, as in Boronima; in all the other genera it is found always on its lateral margin.

I am well aware of the importance of Helicea, but I am not satisfied with the statement of Gillies that the testa is always in the ordinary position, i.e., on the ventral face, and of course opposite one of the colutinal faces—but that by the tendency of the furcillum, Helicea afterwards becomes either dorsal or lateral, but has not been one and the same as is seen in the other figures. In the result, Helicea is the same as much as the coatings of the outer testa, and where we do not find this to be the case, because in most of the genera of the Rhamnaceae, the testa is not opposite one of the colutinal but opposite to its commissures, showing there is a motion of the testa, one and the same, independent of that of its coatings in these cases amounting to 360 degrees, analogous to what I have shown to exist in eustemous circumstices, only that there the revolution is in a vertical direction. This phenomenon, on the whole, is the more interesting because of the testa, which has precisely such a cuticular material, which is in the circumstices, the testa is a smaller diameter and loose cellular tissue interposing between them, and the arillest, which renders it easy of separation, it is a discovery of error.

The testa, painted at its base into a third terminating in the helix, on each lateral margin as a projecting, membranous sheath, starting which starting from the helix on one side, you roll around it, and the sheath is attached to the testa, in no way connected with the arillest and filled with fine spiral vessels seen nowhere else, and is therefore, not only the Rhamnaceae, but also by the Boronima. There is no case, not marked, indicative of any former connection with the arillest of the arillest of the testa, and the sheath, which there would necessarily be, if the former had its origin in one of the testa of the testa; from this circumstance, and the position of the sheath, I have shown that it may be a natural product. There is an indication of an area of the long testa, between the testa and inner testa, but otherwise these 2 testas are free from each other, determining the position of the testa, the testa, the testa being larger than the testa, more contracted. Below, can terms posteriorly, it is more expanded. You will see the difference here, nearly a perfect resolution of the phenomena, which described as existing...
in the present need, and a satisfactory corroboration of my former observations. I have also made much progress in my investigation of the Clusiaceae, a long and difficult task, of which much yet remains to be completed. I availed myself of the opportunity on my return from Switzerland lately, of examining the collections of the Paris museum to which I devoted a week, but I must return there and shall probably visit Berlin & Vienna with the same object, as I am anxious to complete as far as I am able, our knowledge of this interesting family, which has been so imperfectly described and so little understood. I have also looked up the different groups that must be excluded from it — Hierone bee-le, Colophyllum, de — Helamnea de — Platania and some others.

The Platanus Pendulina, which you say you kindly sent me last summer have never come to hand — I have parts 1-12 of the Journal or Wightman, and your "Notes on Vavoa" which long I thankfully acknowledged. We returned well much improved in health from our annual tour on the Continent this time, we visited the falls of the Tosa, and explored the Southern Flanks of Monte Rosa and its gigantic glaciers — for 2 years previously we had examined all the wonders on its Northern side, in the Valleys of Vermand and Saas — certainly the most magnificent Alpine scenery I have ever seen. As you say nothing of the health of Mr. Gray or yourself, I conclude you were both well. Mr. Miss and my daughter most cordially join with me in kindest regards to her and you. Believe me always,

Your truly yours,

John 

2 Dec. — I have examined with the greatest care the seeds of Retama et Vavosa and other Collocies, as well as that of Reseda, which all have a thick crustaceous cotyledon — the testa though separable adheres closely to it, but the inner integument is free from the latter, its chalaza being connected with it only by a single point and this latter lamina again connected by a suspensor with the hilum. Here the raphe runs all round the periphery of the seed, not on one side only as always supposed; it bears the form of a dark line upon the adnate testa and is filled with spiral vessels which are seen nowhere else, and the same phenomena occur in all the instances I have examined. That the raphe really belongs to the intermediate stage is rendered evident by the case of Lycophyllum where it is quite unconnected with the cotyledons. No this structure peculiar to the cases mentioned, or is the prevailing rule in the Flora, and we have always been puzzled to explain. You will excuse this long letter but thought you would like to have the earliest notice of these interesting facts which I have just communicated.
Hammer smith 30 June 1857

My dear Doctor Gray

I ought long before this to have acknowledged your welcome letter of 23 March last, but I am always a lazy correspondent a fault which I feel it difficult to conquer. I cordially reciprocate in all the kind feelings you express and if I differ widely from you in my notion of the nature and origin of seminal tunics that can in no way lessen my appreciation of the value of your friendship: I think however we are coming closer to a solution of that knotty point. Your valuable investigation into the growth of the ovule led you to give up one position in regard to the bony nature of the bony shell of the seed of Magnolias which you acknowledged to be testa and not tegmen, but this rendered it necessary that you should account for the origin of its fleshy covering which you considered to be merely its epidermal coating. This happy thought would have made your case unanswerable but for one point which you in common with all other botanists have overlooked, and that very point is my strong arm, as it shows that your solution is only a hypothesis and I think you will perceive before very long an untenable one. Perhaps after all we are nearer in accord than we imagined and that our difference is more in words than in fact.

Putting aside for the moment the question of the origin of the outer tunic, it appears to me we have evidence that it must have grown over and with the raphe subsequent to the act of inversion of the ovule, as in no case could the raphe have penetrated the primine (or testa) at any other than the chalazal point: but what is the fact — we find the nourishing vessels enter this outer tunic at the very opposite extremity and to have become so circumstanced this tunic must have been moulded round it. If you look attentively at any inverted seed, where a raphe is visible, especially a branching raphe subdivided on its surface you will find the ramifications begin to spread themselves, not from the original base of the ovule, as they ought to do upon your theory, but from the opposite or hilar extremity and you will see it imbedded in the outer integument which you call the epidemis of the testa: if that integument be nothing more than a development of the outer tunic of the ovule; then I contend, that it is against
The laws of nature to assume that the process of circumcision could possibly have penetrated the primate at that point: botanists have too carelessly jumped over this consideration, and have satisfied themselves with the theory so beautifully illustrated by Mr. Bowd. Without noticing this stumbling block to its correctness. Perhaps you will say that the external covering is modified by the subsequent production of an epididymis which tends to embue the spreading tibbets of the raph having the tesse of the primate and if you do, you grant all that. Contended for — viz. that whatever be its nature or origin, it must be of subsequent growth and therefore not a simple development of the primate of the ovale. But the stellate epididymis represents itself, but let it not in consequence be understood to be the epididymis of the primate, or a mere development of that primate tissue for such cannot be judged from the facts as we find them. Furthermore, when a long time was known of the origin of the internal coat of the seed, was evidently impressed with the notion of its subsequent growth, even in cases where it is adherent to the testis; he also adopts for the same reason that it is (epididymis) and dedicates itself with the avellis, both of which he considers additional integuments superadded to the testis (John 15:15 ff). — Take again the instance of a very great length of time the external covering instead of being fleshly in a very avellis, where you will see the tuberous sheathing covering the ovale as it slants from the base of the ovale to the base of the covering of the ovale. The same thing is at the point where the micropyle embryo is imbedded, without once coming into contact with the external covering. If that covering be the epididymis of the testis, how could the fluid and placental matter get out of it? Indeed it is clear that it must have been formed at a period after the ovule as well as its fluidic coverings had formed themselves into the shape displayed in the testis, and I can have derived its origin from no other source than from the placenta or from its jumeral emanation. — I may perhaps be considered too mechanical in my views upon the subject, for it is in the fault of my education in order, however, to show you the progress of my conception, I shall give you a sketch of the model I made to demonstrate this matter, and would recommend you to have one made like it in order to consider the point well, when you will more convince yourself of the infallible result of nature's laws that the Raphy however it may be attached or agglomerated to the ovule, the ovule must necessarily in every case of its inversion remain outside of the original ovale and must remain so in regard to it, during the progress of the
growth of the ovule into a seed. — All botanists in treating upon the ovule have almost ignored the matter of the Raphe, but the day will come when the important and essential element in the structure of seeds will receive the attention it merits. I see how completely it has been overlooked and misunderstood in Rhamnaceae; yet how singular its development, showing that changes must have taken place within the ovule, the nature of which is yet quite unknown. We have indeed yet much to learn of such matters, and our long received theories must undergo great modifications. I think I mentioned to you that I had prepared a monograph of the Botellia, where I show many curious facts relative to the seminal structure of the whole order. I have also worked out the Winteraceae and the baneblaces — the So. Amer. Eucarpicaceae, which can hardly exist as a separate tribe of the Tiliaceae — Hardly know when I shall finish up Calyceraceae, Staphyliaceae, Menispermaceae, Haploaceae, &c., for which I have such abundance of materials ready. I have been for some time devoting myself almost exclusively to get out the 2nd Vol. of my Illustrations now so shamefully delayed and have been obliged to lithograph the plates myself, having failed in all the professional assistance I obtained and been obliged to do all over again. This is a kind of labour I am ill disposed to give my time to. Instead, I should like to bring up the power of my promised "Contributions," which some to illustrate my monograph on the Rhamnaceae. I have lately been examining the Malosma of Nuttall, referred erroneously to my Lithraceae, from which it is essentially different. As perhaps there is no family where the characters of the genera are so ill defined and the seminal structure so little understood, I have thought it might be useful to publish what I have observed in the So. Amer. species of the Rhamnaceae, and these I will illustrate by numerous analyses. There again we fall upon the important feature of the Raphe, and upon the result to which an examination of its position leads us. The long, ovoid, conical nut, and leaf, Salvia, Malosma, &c., he cannot be as I had imagined it, the leafcup of the fruit — it is certainly an adventitious coating of the seed, and no doubt of reticulate origin. I fancy you will smile when I say so, and will declare I am riding a hobby, but we must not be hasty. Disregard facts, because they are at variance with all our preconceived notions, and with our settled conclusions: the evidence on this point is curious and positive.
I am desirous of seeing your No. Amer. genus Strophonemum: from the published description I see no single character at variance with Malosma. What is really the genus Rhod? The admirable figures in your "Genera" pl. 159 & 160 show a very distinct structure from any tlie to Amer. forms (R. angustifolia HBR) and different from any I have met with in the Anacardiaceae. For I have not yet seen a free funicule unattended by a complete tunica attached to it, arising from the lower part (and then never from the base itself) and arising to the summit, there holding the ovule in suspension, a character attributed to the whole family in all botanical books. The nearest resemblance to such a structure I have noticed in Anacardiaceae, but I have found what is there figured for a free funicule in a prominent placenta partially attached by its edge till it reaches nearly the summit of the cell and from its termination the ovule is suspended by a short funicule you will easily see the proof of this in the ripe seeds of the Common Caju but.

the distinction is important: there are many novel points of structure in this family which appear to me of extreme interest—especially the facts upon which I divide the Order into 2 well marked tribes.

I have here carried this letter to a much greater length than I intended which I must beg you to excuse. — You do not hold out any prospect of our meeting this autumn — in what direction do shall this year ramble is not yet determined — it hopes I should like to visit Berlin again and certainly spend some time at the Jardin des Plantes in Paris, to work out some points I have in view. Mr. Allan and my daughter desire to join with me in very cordial regards to eh? Gray and yourself. Believe me to always very truly yours

John Allan

3 July, I will add a few lines to say that I shall have, one long, something to remark about Myristica, and give my observations on the carpellary structure in that family, which has not been well understood: it offers another exceptional case against our ordinary laws of seminal development. In Carya since you will find the integuments of the seed erect and antitropous but the embryo is transverse and heterotrophic without the least trace of Rafhe, the Chalaza being found at the basal hilum: the albumen, when the integuments are removed, is pervious and open at the
radicular extremity; the integuments, in drying, show a very small depression over this vacuity, which has been erroneously considered in the microstyle, but they have show no vestige of any foraminal aperture. The outermost pericarp, called by De Candolle an involucrate membrane of the placenta, (by you it should be called epidermoid) is simply a true arillus, though membranaceous and entire. In the Thesapharaceae (I speak of Clavija) the seeds perfected are B—all attached by threads to a globular free placenta upon a long style; the seeds do not stand erect upon a broad hilum, as in the Lumyriaceae, but are attached by their ventral face; the embryo is parallel with the axis of the fruit—

the radicular pointing downwards, as in the other tribe, but there is no vestige of any hilum upon the integuments. The testa of each seed is closely invested by its own fleshy arillus, in the soft substance of which, the short digitate arms of the funicle disappear, and furnish in this manner the support of the seed, and to crown the anomaly, the whole B seeds are alliform, again covered by one common involucral soft fleshy membrane, quite entire, which encloses all as by a single envelope!! Here again the embryo is transverse in respect to the point of the support of the seed, but the nourishing vessels do not extend as a nephro, either towards the radicular, or cotyledonary end of the testa, as they ought to do according to theory... The embryonal cavity of the albumen is larger than the embryo itself, which lies loosely within it, somewhat after the manner I have pointed out in Stigeonora, Diospyros, Anona, Thesapharaceae...Turn XXII. 103 where I have suggested that the embryonal cavity is due to the persistence of the embryo-sac, and the extraction of one of its extremities, which has probably broken away from its usual point of attachment to the integuments, and hence the cause of the anomalous position of the embryo in such cases. In Lumyriaceae the albumen is absolutely perforate about the radicle—3 in Clavija it is not so, but it is reduced to the thinnest possible degree of attenuation— the embryonal cavity is also lined with a distinct helicolar membrane——
A piece of macking board 1/8 in. thick proveable upon a fixed centre - a vividly yellow paper to represent the Primrose with white for the Petals kept with blue for the inner Succulent, and with white for the outer Succulent. (The elastic scarlet band is fixed into the original place of the leaves to calyx) and represents the nourishing vessels from the placenta.

This represents an embryonic mass connected with the mother mass with placenta &c. in continuity without any interruption.
The same with the ovule turned round on its centre half a revolution - it represents an inverted or annihilated ovule - during the act of inversion, the nourishing vessels or raphes are drawn out from the placenta and must always remain outside the primord and testa - and can only penetrate the enguments at the chalazal point - any times afterwards found exterior to the raphes cannot be tested - it must have been of subsequent growth and an emanation from the placenta.
a = black chartaceous outer lemnis (arillus) = histura
considered to be the testa of the seed.
b = hilum or orific foramen through which the nourishing
vessels pass from the placenta. (raphé)
c = chord of spiral nourishing vessels, running from the
hilum round the large deep hollow vacuity in the middle
of the albumen and terminating in a spot near the embryo
d = a flaky lamellar plate dividing the hollow vacuity into 2
spaces and round the periphery of which the raphé is found
tém - it is an emanation from the placenta with which
it is connected through the hilum.
e = 2 distinct integuments, membraneous, the (testa & tegmen)
that closely invest the albumen, both over its external
superficies, as over its internal hollow space - they adhere
together to the more external raphé.
f = embryo - radicle pointing exteriorly - cotyledons directed
to the termination of the raphé.
g = concave albumen.
My dear Doctor Gray,

I was very glad to receive your much esteemed favour of the 10 ult., together with the sound lecture you gave me, which I received in the same cordial good feeling, with which it was written. You will however perceive in the meanwhile my subsequent communication in the May number of the annals, that I have ascertained the source of our difference and that there is now but little variation between us. I fully acknowledge the accuracy of what you have so ably detailed especially since I have myself watched the growth of the ovule in general, with a more critical eye and I admit that the fleshly coat, which is the growth of the ovule you call the primine and that the toko in most anatomical works is derived from the same source. But with this full concurrence I cannot conscientiously retract my inferences from the data upon which my previous argument was founded, for those data were not my own, but such as are to be found in our best elementary works. Therefore still contend that my fig. 1, p. 271 be an exact representation of the manner in which an anatomical is developed, and which I had been taught to believe, it is perfectly clear to me that any fleshly toko in which the epidermis is indurated could only be formed by the subsequent extension of the periclinal sheath of the toko. As it is clear to me that the data of our most eminent authorities are founded in error, then my inferences from them share the same fate and is also wrong. I feel myself bound to explain to you how this is, and to show you that I have invented nothing nor have I strained any asserted fact. — I Périn (morphol. p. 24) thus details the process of development: the first appearance upon the placenta is a small protuberance, the nucleus of the future ovule, which by degrees becomes covered by two lips rising successively from its base; the future primine (secundine) are the co-formed (ulres Anatropes par la courbure graduelle de la base de leur axe) de rapprochant peu à peu du cordon ambulical, et après avoir décrit un demi-cercle, la rencontrent le soudant avec lui, et le confluentes en quelque sorte dans leur substance, souvent le long, et en

"forme, de montrer contre une pression immense externe, mais souvent il se laisse point apercevoir" la partie ainsi sourdée porte le nom de Raphis." And in order to express this action more forcibly upon the conviction of his readers, Périn compares this ovule and the relation which the helical Chalaza and toko bear towards each other to a flower inverted by being suddenly bent down close to the base, with its medical agglutinated to the calyx and corolla: here the corolla represents the outer toko of the ovule, the calyx indicates the Chalaza, and the pedilum shows the raphis, just as I have represented it in my fig. 1.

A similar explanation is given in Subtis's admirable book "Courts Elementaires" p. 343, where he defines his meaning by figures: fig. 1 is said to be the nucleus first developed — fig. 2 the same with the primine — fig. 3 the same more advanced with the addition of the secondine — fig. 4 the same inverted, "alors avoine le fait un demiment de rевolution" — fig. 5 the section of the same when it is explained, how "a facetten"

"voulu" the abutement à la Chalaza la suit dans sa révolution en son allotrement et cette prolongement forme le raphis." — Lindley (Intro. 185) expresses the same in somewhat different terms: "an equalateral ovule one of its sides grows rapidly while the opposite side does not grow at all, so that the point forming "of the ovule is gradually pushed round to the base" while correspondingly the base of the helical
I am unable to assist with the request.
indebted for the present advanced state of scientific botany, he having pointed out the path which others have successfully followed; those only who had the pleasure of his acquaintance can know, his many rare attainments, and the vast extent of his talents.

You do not say anything that may lead to the chance of our meeting you and Mr. Gray this year - be assured we shall be glad of that pleasure, when it may take place, for all desirous to be more really remembered to him and to you. Believe me always,

Very truly yours,

John Miers

June. The forthcoming number of the annals will contain the first part of my paper on the Winteracea in which you will perceive what I have said on the feed-coats in that family. The second part there is analogous to the fleece-tunic of magnetis, but what is the outer brittle coating? is it not cellulose? it cannot be analogous to the horny shell of magnetis viewed in the light in which you regard its origin. From this difference, that in the one case helicospin is developed in the endoderm of the primine, and in the other upon its epidermis - because we see the shell in the Winteracea line with a distinct calcified membrane, both separable from it, and because we find some layers of large loose cells filled with oily matter lying between that membrane and the helicospin which encloses the rays. This difference of origin is still more palpable in Lantigabalus, where there is a similar excretion of the epiderm, on one side and the helicospin containing the rays in a large Chrysogalea wound which the rays line an almost Spinal course circling course. You wish I will give up my botanically notions about cell-tunics, but how this potable with such facts staring us in the face? By describing them however, I have tried to avoid any speculations about them and have endeavoured to confine myself as much as possible to the simple details of the several structures. The analyses, I have repeated hours of times carefully, always with similar results.

I am just prepared the American specimens of Hygrophylax grandifolium in Dr. M'Intosh herbarium, and find they do not correspond with ditton plant growing at Kent. These American specimens quite agree with a leaf of some flowers you sent me in a letter under that name - they are acute! not with a long attenuated point. They are mostly entire, sometimes slightly dentated, not serrated with a small gland at the tip each tooth. They are commonly two entire hemes not smooth glossy, sparse pubescence being only distinguishable by a lens. The ditton plant the leaf is double the length. The leaves larger - the flowers fully double the type of these. I & Lophophyllum longer than in S. officinalis - the author of S. Stansard much shorter in proportion, and the parallel leaflets widely separated - the ray half superior with the long style & pod. Lophophyllum much shorter by slight pressure to 12 the one into 3 distinct styles in which respects it differs from the tetraphila. Its calyx is short, it is equal or a notch, not tubular and not divided on 5 grandifolium - it is very different from H. tetraphila & H. dichotoma and appears to me a very distinct species.
My dear Dr. Gray,

I was pleased to receive your welcome letter of the 19th, which I found at the Linnean Society together with the 2 first sheets of your "Remarks on Rubiaceæ": these have greatly interested me and I truly thank you for them.

Your notes on Timonius brought to my mind similar conclusions I had formed concerning biometameres more than 20 years ago when I examined several Brazilian species of the Cattleya genus. Although, as you justly observe, this feature is often an accessory feature of value, it is not of so much importance as that of the structure of the ovary itself, and I quite support your view and differ from Mr. Bentham who considers the Guettardæs and 

coffee as mere divisions of one tribe: the peculiar plug-shaped staminal column you so accurately describe in the Guettardæs, certainly offers too strong a character to be missed in quite another development.

I am glad you confirm or rather anticipated my observations on the centrifugal mode of growth of what is called an annulose or Müllerian rule – I mean that it is not due to any inversion of its coats by a one-sided growth, as hitherto taught, and you wonder I did not acknowledge this in my memoir on Winteraceæ. That paper was partly in type, but left aside for the description of the question of the rule. In my reply to Dr. Hooker which was to have appeared in the June number of the Annals, I fully acknowledged my previous error in regard to the mode of development of the flaky coat in Magnolias and admitted the correctness of your details, though I differed from you, in regard to the origin of the brown shell. I am 

Heny Ray in defending your case, persisted in ascribing a one-sided growth to the maternal rule. I referred to his own papers on the rule of ortho, as proving the contrary. Dr. Hooker, however, in his article on the rule ascribing the one-sided growth of the mantle to the maternal rule, in contact with the unchangeable point of its placental attachment, so raised no inversion at all, and no one-sided increment of its tunics as heaves, nor precisely as many cells are here depicted as produced on the opposite side of the primitive, as one side of the ovary, the growth being equally downward in the manner I had attempted to prove.
At the same time I continued to work on the loam and plot its course from a point opposite the sort, as long ago shown by the author, and that the loam is a natural formation of the earth, not of its members, as argued, this being formed by the position of the

rhaphis and the presence of the rhinogene and the original carpel leaf within the thysbe. This paper was printed, and the proof corrected, when it was withdrawn

at the request of Dr. Francis, who had shown it to Longfellow, who had taken some time to read it, though I had been careful to avoid giving cause for this. I have now however

avoided myself of an opportunity of recalling some of these points in 2 notes in my

rhaphis on Thysbea, which will appear in the February number of the American.

and where also I have proved, and hope you will see, fairly examined the facts

related to you in your notes on Thysbea, that you object to my exposition of the theory

of that family, — my next paper will perhaps be a monograph of the collection

in which some novel points in the structure of the seeds will be detailed, that I may

evidently in Rhannus and Thysbea and probably in all Thysbeaes. It appears

time certain that the position of the rhaphis does not become dorsal or lateral, by the

manipulation of the rhinogene as Mr. Bennett suggested, for where found it is normally so in its place.

The structure has remained as it is. But the most singular to be noticed is that

I have invariably found the rhaphis in these cases peripheral. If you take the test of Rhannus catharticus and easily remove the outer corseous

covering (which is an amusing you will find upon the inner corseous leaf immediately

beneath it), a very distinct cord running down the central face from the original

rhaphis to the micropyle base, and which is continuous across the rhaphis with

the ordinary dorsal rhaphis found in the spore, and you may, by careful dissection, tear

both apart with a portion of the cord, away from the tegumen, in one continuous Cord,

joint comes off from the rhaphis without rupture. In all the collections this

cord, peripheral or invariably lateral, as in Thysbea, and opposite to the margin of the

flat thysbea ; in Rhannus whether the rhaphis be dorsal or lateral, it is always opposite to the middle of the thysbea. In Rhannus rhinogene, as a matter, as you have detailed it in Allophanea, is membraneous or superficial — in all ordinary cases it is cirriforme

and always composed by a thin, more or less considerable in size — I find the persistent portion of the rhaphis, with its visible spiral vessel, developed in the spore of the ovule, as well as on the testa of the seed. These facts will be illustrated by numerous analytical figures, and I am

tell me it is a subject that will interest you

My long suffering investigation of the Rhynogene goes on slowly but gradually as I can collect fresh materials. — I have also brought together many curious facts

connected with the development of the spore and testa in the same group, the South

American species of which was not observed to reduce into order. — Valuable

about novelties, what do you think of a new system of arrangement? I venture

to break all my previous notions on this head, but I am convinced that this method of

to the extent supported by all the rules of time affinity. Thus already concluded his

facts as well as observations concerning Allophanea, which he placed near Thysbea, especially

Allophanea, which he considers (though without any good reason) as

ordinarily distinct from the latter tribe, was carefully investigated by me many years

together with the Chilean and American Allophanea (which are hardly distinguish

as a tribe in Thysbea): here details I may probably some day publish. The plates

attached to this work are from a valuable collection of facts showing the different modes

in which the sexes are developed. — In regard to the so-called "Thysbea philoxeri,

which I mentioned as growing in New Gardens, I found one day after I had written to you, that it is identical with Allophanea philostoma — as compared

with original American Allophanea. I notice that as leaves were also those

of the rhaphis have the points of their teeth furnished with a glandular often detached,

which I do not find in any of the Thysbeae.

Mrs. Perry and my daughter join me in heartiest remembrance to Mr. Gray and yourself and cordially wishing you may both enjoy a happy new year and many interesting ones. I believe me to be always very truly yours,

John Allen.
In P. fimbriata the segments are thick and fleshy as in my Brazilian *Gymnastera*, but they have in addition the fimbriated processes, just back (not on the margins) of the segments: in *G. levella* the segments are extremely thin and semi-transparent.

What therefore is the distinction between the 2 genera? For the several differences I have pointed out, run into each other through all the species enumerated by Dr. Dehnh. and I must confess that I see nothing constant in them, nor any thing of sufficient importance to constitute a valid generic distinction.

I should sooner have complied with your request, but your plant reached me a week ago, the day after the arrival of my son, who has come home from Brazil to seek medical advice, so that I have been most occupied and my answer about time. You will no doubt have received the letter I wrote to you about a month since. With kinder regards to Mr. Gray below me, always my truly yours,

John Kirk.

Hammer smith
23 Feb 1859

My dear Sir Gray

I now return enclosed in this, some pretty little *Pernicandicaceous* plants kind for me to examine. I regret it is too old a state to ascertain its structure with any certainty, for when these small fleshy plants become crumpled by desiccation, they are not easily unravelled. I have endeavoured to disentangle one flower, as you will see, but have not liked to injure your specimens if you have a younger flower, you will probably be able to determine the form of its segmental divisions. I am always found in these plants, after expansion, the boy one of the pollen grains enter so deeply into the stigmatic tissues, and cement firmly together the
Author talks with the stigma, that they seem from an indescribable mass. I fancied I could perceive the peculiar stigmatic appendages in this decayed mass, but in this I cannot be certain: the stamens are certainly those of Cymbocarpus, while the gibbous shape of the fruit and its mode of dehiscence most decidedly correspond with that genus, so that I am not a doubt you have named it correctly. You will observe what I have always found in the Brazilian species, that after impregnation, the border of the perianth becomes shrivelled and breaks off by an irregular line across the point of the insertion of the stamens.

I will however direct your attention to the greater length of the tube, in which respect it resembles that of Symchomoria of Prince & Bentham. This name was suggested by the former on account of the inflected mode of derivation of its segmental divisions, 3, but it does not differ in this respect from Cymbocarpus and Aplonia, as shown figured in the latter. It is to be regretted that Dr. Bentham has given no figure of his genus, nor has he indicated the differential features separating it from Cymbocarpus. I find no distinction but in the 3-lobed shape of its 3 principal segments, formed by the veins, a character quite wanting in P. cornuta, and only well marked in P. tenella, and his Diplomoria section, but in the latter the internal segments are present as in Cymbocarpus, while in P. tenella they are obsolete; in Cymbocarpus the border expands thus:

in Symchomoria tenella

as in thus;

The stamens are alike in both genera thus:

In the stigma of P. cornuta you will find precisely the same long filiform appendages as shown figured in Cymbocarpus; these appendages in other species are less developed, while in P. fimbriata they are so much reduced as to form merely sharp, angular prominences.
One of the most curious of these genera will be dedicated to our friend Rumphius whose name has not yet been honored in the nomenclature of botany. As at least 17 plates will be required to illustrate the memoir, I am not certain whether the Council of the Linnean Soc. will consent to publish it - if not I must have recourse to a hitherto unprinted 'Contributions' which I wish to avoid if possible, though I have plenty of matter in readiness to fill a volume - but I feel I am getting too old for such a task.

I have not yet published the result of my examination on which you saw me at work on a new tribe of Convolvulaceae - I announced - my analysis of the carpal divided structure of the Convolvulaceae which will require another arrangement of the family. I have some observations to make on Leea rhizophora and to point out a peculiar structure in Leea hispida not recognized through Martius alludes to it - it is only seen in the living state, and I fortunately made a drawing of this while in Brazil - it will complete the severance of Leea hispida and Rhizophora which Berg has erroneously welded together. I have besides much other unpublished matter which I cannot expect will see the light. Please offer my very kindest regards to Dr. Gray and believe me to be always very truly yours, John Miers.

84 Addison Road
Kensington
30 March 1871

My dear Dr. Gray,

I have not been able to reply to your kind favour of the 24 Jan last because I have shrunken from the extreme pain of telling you, in answer to your kind inquiries that I had the sad misfortune of losing my good and excellent wife in September last after a long illness. I will not dwell on this painful subject, for which there is no help but resignation, and though I summon all my courage I feel the infliction not easy of endurance.

I must add that my dear and affectionate daughter does all she can to alleviate my distress.

Thank you kindly for your Contribution on Diaphytis Poliomeniae to which we are very valuable accession to our knowledge regarding them. I was also greatly pleased to have your carte, especially as it is so good - likewise I will not fail to send you mine, as you request by the first opportunity. Even ask me if I...
element Load from the version of the E Uhrig

ical to separate the sundered
the putative 3rd, 4th, and 5th; the 3rd, 4th, and 5th are not

flows of events in general and species. The result

be divided among the family. Since then, ever since the

any resolution of the family, or to ascertain the

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various - I past quote here in Bio with importing on 3rd, 4th,

and appears as a different organism, which is my

and although the thalassite are opposed to the

ammonites, and all others, and the
Steps in the publication of observations made long ago: I have still a large amount of material which I cannot expect to see published - I will however persevere as long as I am able to do so.

Repeating my kindest regards to you and Mr. Gray to clinch me my truly yours

John Herschel

84 Addison Road
Kensington 4 May 1871

My dear Doctor Gray,

I thank you for your note of the 18 ult. expressing the sympathy so kindly felt for me, but by Mr. Gray and you, under the very sad loss I have sustained, and which I still find very difficult to bear.

My daughter writes with me in cordial thanks for this expression of your friendship. I have also to thank you for your Memoir on Eriogona, the most recent and a more careful revision of this interesting group. I have enquired at the Linnean Society for the copy so kindly sent on a previous occasion, but it is not there: it may still be remaining at Kew, where I have not been for nearly a year, so little have I been from home,
You speak of having only two volumes of mine—these must be my Illustrations of S. Amer. Plants: These were followed by my Contributions to Botany, which I must have omitted to send. I have therefore now three volumes of the latter left in Prof. Olivier's care—today with a copy of each of my communications in the Linnean Transactions: among these you see a plate and my remarks on the Chilean plant I collected of Myrtica. I send these loose in order that my forthcoming memoir of the Neogean Hippocrateaceae may be bound up with them in case it should be received by the Linnean Soc. Council. I will also send you a number of the first Soc. Journ. containing a communication which you probably have not seen, upon some Brazilian Bignoniaceae: I had long studied this family and had begun to describe some of the genera, so you will find in my Contributions, which I intended to follow up by successive descriptions of the genera under my new arrangement, but this I gave up when I found Dr. Scenmann had undertaken a similar task: the latter however never persevered continuously and I thought I was absolved, when learned that M. Bureau had accepted the offer of completing a monograph of the American Bignoniaceae for Von Marten's Flore Brasiiliensis. Accordingly I sent him my specimens, all my notes, and analytical drawings—in fact all the knowledge I had obtained by long and careful observation concerning the family, which he was very glad to obtain, but I have not heard from him for 2 years or more, and do not know whether he has completed his task.

I enclose my Carte in this, as you requested: you will see from the date at the back, that I ought to be thankful at this age to be able to proceed by slow and careful
Dear Sir,

The enclosed flower has been received from a customer, said to be a wild flower. I cannot give you the name of the place where it was found, can you give me the name of it? By so doing you will oblige me greatly.

I can get the locality by writing to the one who sent it in.

Yours truly,
E. S. Miller
Dear Sir,

Your note this morning with your naming the Echinacea, I sent flowers of another we bought for Amaryllis, two seed sent last but had not any inhabited. The bulb from which this was cut is that shape 1 1/2 inches long and about 4-5 inches through at base. Mr. Pike I think is the same as the one he has which came from China, I am sorry to trouble you but hope I can return the favor. I hope Mr. Cameron will not go to any trouble to get the Echinacea.

Yours truly,

E. H. Miller
Floral Park, N.Y. Oct 6, 1891

Dear Sir,

I send yours by this mail a box containing a little Erythrina from Mexico. Will you kindly give the name or species. It is a constant bloomer winter and summer.

Yours truly,

[Signature]

P.S. I send in another box Specimen of a Solanum which I would be glad to have you name for me.

[Signature]

Solanum alpinarium

from Perugia.
Mr. Pleasant Dr.
Aug 11 1890

Dear Sir,

I have formed one plant that has every characteristic of an Empatorium, except that the leaves are in whorls of fives, carnate, instead of opposite. There are other Empatoriums around it, but no other Empatoriums. Does such a freak sometimes happen?

Yours truly,

J.W. Miller
Sereno Watson
Cambridge, Mass.
Sept 16, 1900

Mr. Pleasant, Jr.

Dear Sir: Gray's Botany under the
head of Cupressaceae says, "Seeds anatropous."
I send some seed pods of Ephedra coloratum.
The seeds seem to me to be orthotropous. I
would like your decision.

Sincerely yours,

[Signature]

A. W. Miller
McPleasant, Pa.

Screa Watson, Dec 3 '91

Cambridge, Mass.

Dear Sir, Could you

send me The Botanical text of

"Dimpleweed." It grows on prairie

and, newly broken, is three or four

pains. The heads form a cone

eight to twelve inches wide. The wind rolls

it into fence corners and hollows.

None near here, but I remember

to have seen it in the north

of the state.

Yours Truly

Wm. H. Mills
Mt. Pleasant, Jr.,
Serena Watson. Apr 10'71
Sombrada, Mass.

Dear Mr. Jones,

Are these sympotomous rhizophyllous?

I am busy.

M. Mills

I found a Trillium drift with several young tubers growing from the root stock of a flower in bloom. The books do not mention whether it is common. The infertile tubers were spherical.
August 7, 1911

Mr. Dean M. Walden,

Polyspermum lappathifolium?

Achene lenticular with concave sides or concave triangular.

Yours truly,

J. M. Mills

Amarantho rhotiferus?
Leaves are peculiar.
My herbarium of the genus Euphorbia contains 87 species so far, represented by 254 individuals, the bulk of which are from the collections of Dr. EdsPalmer, Dr. Geo Vasey, and Mr. Ainsor. Rendereh, Mohr, Curries, Orsone, McJones, Lemmon, Grimes, Pimble, and Addish Brown (taller plants). I have addide about 200 mss. letters from U.S. and Mr. Bollanists noting the sp. in their localities, these to aid in geographic distribution of sp.


Thus much for a ground work.
Haverly N.Y. Oct 19th 1888

My Dear Sir:—

I enclose you a viola that has both puzzled and interested me. It grows profusely upon an unusually overflowed meadow in Cayuta Creek near here. This individual is one of the largest found, the usual growth is but little over one inch. My sp. striata in other localities are generally from 4-8' high.

I have no literature of this revised Viola.

The character of the two bracts opposite on the sepal is in none of the violas in my collection. If it proves rare please remember the pride of an amateur in nominating it.

Yours Very Sincerely

C.F. Millspaugh
Waverly, N.Y. Dec 15th 1888

Un Sorno Watson

Dear Sir:

I have, for two years, been studying in the genus *Euphorbia*, with a view to publishing a monograph upon the U.S. and Mexican species. This I intend to illustrate with photomicrographs of the parts. I am endeavoring to make a really valuable work in this my favorite genus. Can Cambridge help me out any with duplicates; my collection is already large but not complete. Can you as a personal favor furnish me with either, some "debris" from, or duplicates of, your species, that I may have authoritative data on them? viz. *E. plumerae*, *E. pulycarpa*, var. mistress, *E. Rattani*, and *E. lineata*.

Species sent to you for identification, and then must be many, I would be glad to work out for you, thus saving you the use of much valuable time.

I am not playing with this genus, but have my whole soul in it. Yours sincerely,

F. Millsbaugh.
CHAIRMAN's RESOLUTION.

CHAIRMAN of the Board of Directors.

December 17, 1888.

In my letter to you of the 15th inst., I asked for a special favor, that you might send me either authentic duplicates, or some detail containing the actual information you have in the genus Euphobia. I should have included the following:

- E. gracillima
- E. felicitana
- E. subintegens

E. retusa
E. Guadalajara
E. temsukula
E. multiglandula

E. retusa
E. Guadalajara
E. temsukula
E. multiglandula

For the Mex. Board, &c. with you, or at the

Historical Columbia New York.

Sincerely,

F. Millsbaugh.
In Amoebition

Waverly, N.Y. Dec 18th 1888

Dear Sir:

Your favor in answer to my request for duplicates of the genus Euphorbia, does not in the least discourage me, though I had indeed hoped that Cambridge would have a few spare duplicates. I fully realize the labor I shall have to perform to make my work in the genus can assume authentic shape, and have planned that when my acquaintance and study in the genus, shall have become sufficient to spend some time, not only at St Louis with the Enquellungmann collection, but also at Washington, Cambridge, and in Europe with those of Berlin and others. In the mean time I am not only to get together material for comparison of these kinds. Accept my thanks for the packets you kindly send pointing to Hummeræ sacharia and Polyartha rutilana.

My work is earnest, and though it may take a number of years, is to be completed nevertheless, if I live.

Sincerely yours,

C. F. Millsbaugh.
CHA\& F. MILLSSTAPH, M.D.
HAEHEMANIAN.

Harmony, Vt., July 18th, 1854.

To Enoch Hudson
My Dear Sir:

Please accept my warm thanks for the "Contributions xvi," which contain much of interest to me in my favorite order Euphorbiaceae.

I have had the pleasure of studying Dr. C.C. Snelling's Godboldia [sic] THOMS, in this order, 1888. I found in it your beautiful Euphorbia Quadrifolia [sic] (pl. xiv. 1891), which is allied to S. [phylides]. Mathias citrifoilis, glandulose, also 1891, and 

"...characteristics that you mention in Palmes 92 93. Your Euphorbia quadrifolia [sic] is a new species which I have named S. multiflorae, together with others of similar interest."

I was occupied in writing out all these Euphorbias [sic] upon the descriptions in your "Contributions xii., xiv., and xvi.

J. A. not your XV, which I expected, & Dr. Mathias's [sic] "and Dr. Goodrich's xvi," containing revisions and modifications in the order. Several no species to return in any case. I hope therefore that the above will excite your determinations. I greatly regret my being so far from the larger [phylides].

Yours Very Truly,

[Signature]
Waverly, N.Y.
Oct 21, 1889

My Dear Sir:

In nominating the collection of Euphorbiaceae, made by Mr. Brandegee in Lower California this year. I find a specimen of E. magdalenæ. Benth., upon which in place of involucres in many instances I find such growths as I enclose you. They look like malformed involucral cups, I never have noted such before. Will you kindly express an opinion. Sincerely yours,

[Signature]

CHAS. F. MILLSPAUGH, M.D.
Nov 5th 1889.

Mr. Ernest Watson
My Dear Sir:

Accept my thanks for the "Contributions" and fragment of C. internixa so kindly sent me.

I have now completed the 44 species collected this year by Mr. T. D. Brandegee in lower California, and found among them some peculiar Sebastiana (S) bilocularis, also your C. tomentulosa, petrina, polycarpa x rotula, Bernardia nigrifolia, and Stillenia linearifolia. This collection
gulded one quarter % nor species as follows:
Euphorbia Purissimana, Brandegei, conjuncta, and Comandranza; Phyllanthus Brandegei, and ciliato-glandulosus; Anton Magdalenae; Argythamnia Brandegei; Bernardia Brandegei (and Euphorbia involuta).

Sincerely,

[Signature]
Harvard Medical School
Boston, Jan. 15th 1890.

Dear Sir:—

I enclose a circular of the Elizabeth Thompson Science Fund, you can see that it is necessary if you wish a grant to apply for a specific sum for a specific purpose. It is improbable that we could make you any grant before Dec. 1890, but it would be perfectly suitable for you to make application now or later.

Yours very truly,

Charles S. Minot
Sec.

To Prof. Dennis Watson
Botanic Gardens
Cambridge, Mass.
Morgantown Wva Oct 19, 1891

My dear Sir:

I am just in condition since a severe attack of sickness to compare specimens and thank you heartily for your favor pointing out the resemblance of E. Neallyi to E. augusta which it really is. I had only a little scrap of augusta heretofore for identification. I stop the publication of Neallyi. My sincerely yours,

C.F. Millspaugh
BOTANICAL DEPARTMENT
West Virginia
Agricultural Experiment Station.

C. F. MILSPAUGH, M. D.,
BOTANIST AND MICROSCOPIST.


Dear Sir:

I am preparing to publish this coming winter as complete a preliminary
list of the Flora of West Virginia as possible. Can you assist me in giving me such
bibliography as you conveniently can upon publications concerning the Flora of this region,
and the names of such Botanists as you can recall who have worked here.

In the Manual you credit W. Va.
with Arceurus flavus, C. van. purpureum,
gray. to whom should I credit the species
in this state?

Shoule it be necessary to put an
assistant or clerk upon the references,
I would willingly pay for the time thus
expended in my behalf.

Hoping that you can help me
our in this matter I remain

Very sincerely,
C. F. MILSPAUGH.
My dear Sir:

Several years ago Dr. Gray took a botanical trip through the mountains of West Virginia. Do you know whether he published notes of the trip, and where? If the notes were published, or are in any compilation of his notes, essays &c., please consider this an order for a copy if for sale at Harvard, and can be procured without too much trouble to you.

Yours Sincerely,

[Signature]
Ce temps était temps, je
reprend mes frais, pour publier
une biographie consacrée à
l'on enquis meilleure par
volant facile pour
jour plancher. Puisque
le parti d'arrêter de toute force
jolly proprément bien de l'art,
espérée de cette façon.

Avec les lettres de haut
citoyen et d'officier

Trommè.

T. H. L. McHenry

Le parle plus encore long. L'ougr.

A. G. Th. of cartes clairée.
et je me reçois à en croire vos lettres, que vous très beaux, très conseigns, donc que je suis très heureux de vous le demander. Je vous dirai la lettre de l’an dernier, que j’ai fait la première fois renommé a ce moment la carte avec cette belle collection de plantes, intérieures et de fleurs magnifique ! J’ai obtenu cette année de l’école et la lettre d’observation et de mes manifestes. Mon très cher ami ! Maintenant je ne le laisse pas encore de plantes belles, mais je vous promets une belle quantité ! Je le voudrais pour cette année, à la couronne, pour l’agencer par le gouvernement. C’est si fort de la belle école et des plantes de l’école. Tant de bien, si la graine de bons parcours. Je suis en fétal et je vais bien à la bonne, ce qui me rend si heureux. Je suis renommé dans le des plantes de Bonaparte, et il est obligé de lire à ce sujet.
Mon cher Collègue!

A la fin j’ai trouvé le temps, de faire un choix des nos plantez et je suis très ravi de les voir en fleur. J’ai suivi vos instructions et j’ai sélectionné les meilleurs. Je vous envoie aujourd'hui une petite boîte de nos meilleures graines. Je vous réserverais que ces graines se sont très bien acclimatées et que les fruits seront de bonne qualité. Je suis impatient de les recevoir aux États-Unis.

J’espère que vous allez bien et que vous avez repris la plante de différentes réussites. J’ai également ajouté quelques graines de la même plante, mais à une autre époque. Veuillez les conserver jusqu'à la prochaine saison.

Je vous prie de bien vouloir agréer, de la part de la Compagnie des Plantes, mes salutations distinguées.

Amsterdam, 4 Novembre 1895.
No visible content.
Chez Ami et très hume Collignon!

C'était, si je ne me trompe pas, ma faute, que vôtre lettre, pendante l'est conduite depuis longtemps déjà. Tes travaux envoi de différents brochures et livres n'est jamais dépendant, que Votre amitié n'étant pas perdue pour moi, et qu'en m'ayant dit être, que mon silence était pourtant involontaire, c'est...
à dix une des trois heures
l'occasion trop sombre.
Veuillez donc me permettre, du
compte la nuit et d'agir
accueillant. Il en est prévu de
quelques plantes de l'entrepôt
malin. Vous y trouverez
plutôt qu'actif, mais charmé
vivrent de récoltes
de Sardaon, ou le voyageur
est exposé à de grandes
difficultés, de la part de la
population et du climat,
it est difficile d'avoir
de chantillys fraîchets.

Le chemin encore long
men est de la ville des
de son calme, du orientale,
de bout en bout plusieurs lunes
men vivront longtemps.

Depuis le lever de la lune
Je publierai ces lettres à la presse,
Ainsi de jour en jour
plus facilement, de telle
Cher ami, je vois
d'espéder un caire
de plates pour votre
notion à Van trouver
ma lettre dans le
Caïn. Mais à propos
je dis qu'il est encore
perpète de Van faire
part du Bill of Lading
qui le coûte, dépit
fais ta part de mal
Brûle ton jardinière.

H. V.

F. W. Hollinger

Utrecht,
3 mai 1862.
Vogez donc, cher ami, que bien des épineux se rattachent à ma direction de l'histoire de Limeil.


Aadie! Croyez moi toujours,

votre dévoué ami,

T.Bulliguy.
dans les mains. Le poêle couvait de doux, des exotiques, de petits du Japon.

Je me suis aperçu à l'endroit de l'autreコー

le repos éternel de l'oiseau qui vient occuper ainsi les œufs de l'autre monde.

N'offusque, n'interfère et qu'entre ainsi font du prêche. Cela montre un certain de l'animal qui est pratiqué de personnes qui le prône avec l'animal est, loin de la réalité. 
Alors, je voudrais presque tout savoir. Peut-être.

Mr. P. Il, l'histoire de l'histoire, enfin la détention des Grands, et dans le cœur de 2%, il n'a pas encore détenu une dévotion.

Le Gouverneur a décidé que l'histoire de l'histoire, l'une révélée de cette histoire.

La première en ligne de cette collection, je trouve que le prêche fait des Littératures, publiées sous le Dr. Jung, y mangeure !

Mr. P. Il, l'histoire, à la maintien, après la mort il est tombé par habitude, tombe la main de Mr. P. Il, qui avait partout des hiboux, et maintenant on dit, que ce n'est pas que des oiseaux !
1864, lors la fin de cette lutte si triste, mais si
condamnée, que le bon et
très pricier de la beaute
y fut un beau gaine!
Oui, glorieux envers la
beauté, à l'abolutio
de l'endurage serait la
dernière écrit de ce livre!

Ah ! cher ami !
La couron toujours une
tout devenu en...

Fried. Allgemein

Cher ami !

Dans ma querelle de Bonmarché,
d'abonder aux chefs de l'endurage,
contenant des échantillons
d'objets de cette collection. Je
me propose de la faire diverses
par d'autres envois, car le
grand art du peintre, est de
placer ses œuvres é законим, et
est l'œuvre !

Cher ami, nous avons
condamné, je lui prodigue
fort intar, de mes journées
non communiquer plus...

Utrecht 22 Dec. 1864
chance des richeurs botaniques qui, nous avons dit.

Je suis maintenant entré dans deux collections botaniques,
1er de grande valeur botanique du Sol. On ne rend
chaque semaine pour un ordre
jour), et le 2e de l'Académie de l'Amiral de l'Académie
qui est mon domicile.

Je me permets de vous dire les noms éminents des docteurs
du Sol. Aux écoles de l'École des Arts et des Sciences,
distingués. Plusieurs d'entre eux sont dans l'état de
pour pouvoir obtenir l'état de santé, dont ils ont

et qui depuis 30 ans, y sont
ont été conservés. Les résultats
de ces recherches, je publierais
les, ont pour condamner pas
plusieurs essences, par
l'Académie du Royaume,
à la République, destinées à
différents endroits, cher vous.

Le terrible incendie
de Notre-Dame de Paris,
que j'ai été frappé depuis longtemps
de M. l'académicien. J'espère
de tout mon cœur, que
Le 18 Harts Academy of

Chère et tendre mère, je te prie de considérer avec indulgence et compréhension, il te faudra m'excuser pour ce silence, c'est que le moment le permet d'une seule de tes lettres, et c'est l'époque où l'on doit écrire la lettre, c'est le moment.

Puisque c'est la fin du mois de juin, je suis d'accord pour ta lettre, et c'est dommage que je ne la recevrais pas demain. La mienne d'ailleurs n'est pas dite, c'est le billet que je t'écris.

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... terme historique d'être exposé... de les expéditions... Mais je ferai un même, que cela m'aimerait... pour le rendre l'effet... les transport à main de fer... par le vrai que vous n'attendez... ou par un conte encore moins... chère.

Tant ce que vous pourriez annoncer... sur le petit château de Londres... qu'il sera avec la plus grande... délice, dans le plus grand... traité... La plante de S. Cerf... sont... est elle de jaspe tout... en trois tiroirs pour trois...

Au bon de notre gouvernement... j'ai encore plusieurs cédains... du Prince de L. O. à défendre... des frontières du... institut et société savante... chez vous...

À votre desir aussi, j'ajoute... de mon part en remerciant de...
P.S. de l’dem. J’ai pris des informations sur les meilleurs moyens de ces expéditions, chez un Montier de ma famille, Marchand à Rotterdam. Alors il est sorti en rapport avec le Compt. des États-Unis, qui a dit que nous avons payé un si grand dommage l’année dernière. Vous avez payé un si grand dommage. Il est connu probablement à cause des importations. Il est connu probablement à cause de ces importations. Nous avons payé 200,000 écus en un instant, à cause des importations. Nous avons envoyé nos caisses à Vaskers et fils, expédiant nos caisses
à leur correspondant à New York, et en
lais. Ma ces font prendre en rec-
rois la lettre pour la emporer à
Rothem. — Ami, — dit-on, mon
commissaire dans tier et un marche!

Arié H. N
Mr. President

General R. of the U.S.

Vol. I & II.

Troy & Arro, New York


In addition to parts d'armes, these memoirs published in the American Academy, and the conference titles of Proceedings.

Mais je ne propose pas les Tules of Plancie R. des famins, c'est rem un combat pour moi, de ces popolans.
Utrecht 10 Sept 1878.

Mon cher ami !

J'ai le plaisir de Vous annoncer qu'en avril de prochain, la Belle-Vive d'Ide esquittée à Paris a été acquise par elle. Kalthoff, fils de Brandt le 2e de New York, la paye par chèque. Je parle par cette lettre, que je ferai pour vous faire marcher.

Je m'anime envers le paquet qui contient du chapeau pour moi et pour de Poul. Je vais bien maintenant de Leipzig.

Ce jour-là, je pourrais le terminer en
J'ayant vu certaines lettres d'agent. Je n'ai pas réussi à les payer, mais a obtenu que le jour de l'asile pour cœurs arrêter.

Puis l'envoi de vous par le, ce qui concerne nos amis, surtout, de la espèce, vivent New York par le courrier, pourront de nos amis, Hasha, et de ne jamais ajouter aux projets destinés à moi des choses pour d'autre courrier, fondant, d'ajouter les choses, destinées pour moi, directement à moi, et le souvenir destiné pour la fille de la.

Je continue à l'admirer à Château. Je faisant autrement, il y aura ta.

Le dernier envoi du liquide, pour les deux, est déjà parsemé. La gésièrul IX et X, finissant parfaitement.


Avec, cher ami, de cœur toujours votre

Très aimé,

T.J. W. Mignon.
Cher ami !

J'ai réçu votre lettre d'introduction pour le Dr. J. pour sa thèse.

Mon grand regret est de ne pas avoir de nouvelles de lui. Je suis désolé de ne pas pouvoir faire de nouvelles pour vous.

J'espère que vous avez bien reçu les lettres et les documents que vous avez envoyés.

Je vous remercie de votre patience et de votre confiance.

Bien à vous,

[Signature]
la collection provenant des étrangères et des diverses ethnies. Pour
les derniers jours, en effet, la ]
elle, par Béziers, mais pour les
Lisiers pour cela rien ne tarde.
Vos vêtements sont excellents
monseigneur, mais vous mangez
les osmonors. Alors, à la
recherche de cet osmonor, tout
public !? Au moment où elle
Béziers, a tiré le catalogue
de l'osmonor, non plus
était publié, mais peut-être
ailleurs, il est fait après. —
J'ai trouvé un genre mixte de
Mme Hélinguer et je l'ouvre
en idéologie, et pourquoi encore ?
figur, mais la gravure de notre
jour, c'est l'État d'après notre
dessin, totalement différent.
Le même de mon genre, tout
ce vêtements de son genre, sont
des "genres antiques", n'ont
d'un thème, d'un thème, de
un thème, mais aussi plus,
tout trouvé dans chaque
esprit, long s'éteint... —
La finir grandifère. 
D'après les
formes de 1174, d'après... —
Après je suis entré à l'Académie
Latine. Thi, qui fait sûr un
esprit fort rare. Si mon petit
esprit, l'imagination, un
petit esprit, l'Académie
Latine (qui en dehors) des...
Cher ami !

Longtemps je n'ai pas son de Mus

décime et vos esprits profonds

générés par le magnifique car

de l'or, l'anneau de l'éternité, de l'or,

qui annonce ! — Mais l'autre

cheval, ! je vous trouve à l'au

à l'œuvre de ces grands événements

politiques qui le but profond chez

mon ami — Vous connaître à cet égale

maîtriser, et vos premières

futurisme, ma foi. la la

Victoire de la bonne cause, de la
activité dans le Tyrolis, à la hante
sur l’entourage. Chacun, parfois
libre, chérit un sympathique étranger
haut pour nos affaires, et nous
bons partage cette profonde âme
du libéro de grand et noble
Lincoln.
Pour l’humble citoyen, le victorieux
de Notre cause est le plus
grand impertinent, car le citoyen
n’est pas l’usage de la liberté
mol et non l’usage de la liberté.
Les gens de tyrannie
sur les mélènes qu’ils ont formé
en commençant la guerre, le
dénoncent et je pele que les
d’autres circonstances en 8 années
pour mi d’érablé en nouvelle
Pologne dans le mieux derrière,
que et de martre le libre
pays, du Sénat: —
Mais revenons à l’Allemagne.
Je n’ai d’espoir à être coupé
du formeur de Adolf, et n’ai
d’être seul que sain en ce péche.
J’ai pris les mots de ma connaissance
aux de plus voisins citoyens,
je ne crois pas. Mon ami de
ma liberté de pays est j’ai
commencé la prévention. Don
commencer la prévention. Don
mais faire un côté à notre
feu, au côté de notre
colère, parler que je trouve
éleve, p. ex. 120 C.F. 50 S.O. 120
Je n’ai pas retrouvé toute la
espéance que van avons espéré, van
mais presque. Mes collègues pensaient
au château. Vous trouverez de
la poésie term. la rédact.
de la copie.
Le Philos. F. J. de, édition
supérieure. Vous pourriez à
entre ouvrir. Traîner à
termine d'abord et qu'elle
pour vous être enlevée de confait.

Ainsi cher collègue,
l'expulsion de ses sentiments
d'estime et d'amitié.
D'amour.
Fort à vous.

F. J. W. Higby.
Mon cher Collègue :

J'ai le plaisir de vous envoyer ci-joint les notes des Annals et un petit volume ; la paire de temps vous trouverez plus mince, plus détaillé que le Géographe Estamiga de Lyon, rédigé en français.

M. Profilo les termina du l'Année 1818. Je me souviens alors les avoir vus. Mais j'ai oublié quel fut le dernier volume de cet ouvrage, que mon cœur veuille et
à celui de la pipette, de
Vau qui est encore un indigne
le dernier homme qui nous
est connu.

Il s'y est bien terminé bien
pas le commissaire de Ch.
malvieu; l'après-midi,
ne diffère pas particulièrement,
沸i elle est harmonieuse,
alis de l'Amérique étant
physiquement d'ailleurs.

Son ne s'est tenu calme
été hicinual aimable.

Tout à tour

E. D. Lépine
Cher ami,

je viens d'expédier les premiers de Prof. Fr. Jpp. qui Vous manquent davantage et que j'agirai pour vous. Si j'ai oublié de dire à Vous, mon petit germe (sans. Mes anciens de pumépis), patère, je suis bien prêt à m'attacher à vous, tout comme l'arrière de notre amitié et d'amitié.

Il est peut-être que tout le géorgien botanique de Jpp. est gravé sur le vieux papier. Je me suis beaucoup de attendant. Je me réjouis beaucoup de connaissance.
que vous arrivant dans votre lettre. Tant ce que vous de votre
lettre me interpelle toujours beaucoup.

Peut-être que vous, au cours
de ces derniers mois, avez formulé
tant d'événements, que le départ
de votre pays ? J'ai l'idée que
paris de l'Antiquité de
Jérusalem et celle de l'homme, c'est
tout ce que plus intimes

Ami libérateur sur qui de moi,
forme de la de ces libérateurs
selon mon style, de cette
ou l'ajourn en lui, et de
charges de la carte des libérateurs
qui de pelllement d'en vous ? Si.
Hurstperpoint
30th Oct 1879

Dear Mr. Gray,

I have not addressed to you on Thursday afternoon last a parcel containing everything of which Mr. Sullivan desired to see, and by this time I expect the Railway will have delivered it to you safely.

The parcel contains all that of the Museum he wishes to examine, and I only regret that I have nothing more at hand ready to lend him beyond a small collection of Torosan specimens which is
included in the parcel.

I regret to find that I overooked
in your note of 4th Oct., that
Sullivant wishes to have a set
of Spruce's boxes. Longlet to
have written to you at once to
tell you that I did not have
anything to do with the disposal
of these, Spruce did this himself.
I assisted and catalogued all
the specimens and made up
a certain number of sets,
the duplicates were all here
until quite recently, but they
have now been sent to him.
I will write to him about this
matter.

I send with this a letter for
Mr. Sullivant which will tell him
all about the contents of the
parcel and you can forward
it to him at your convenience.

I am sorry that no opportunity
has offered me the pleasure of
having seen you and am

Dear Sir,

Wm. T. Wirt.
Dearest Mr. Watson,

I owe you many thanks for the copy you so kindly sent me of your book on American Mauve, what a fortunate chance it was that Lequenoy obtained your address; nothing so original and independent has before been done, of course in all books of this kind it is possible to say here and there something may it have been put different, but that does not at all detract from value of your remarks in our own good English, which is often more to the point than the regulation Latin expressions. I came here and spent a Sunday here on his way to Schimper. I think, at that time he seemed in pretty good health. I find but little correspondence with Sullivan being, in fact, always over done with other matters, and so have

Huntspierpoint
Surrey, England
10, Sept 1891

With
continued. He sent to the Duke two or three years, so many specimens came
to me from Kew, that I have been kept fully occupied; for I have only the
intervals between business matters that sometimes consume all my
time and then I get too fagged to do
much good. About five years ago
having to move my collections from one
room to another, I determined to
overlook all the surplus of things
that had accumulated for many
years, for I was sure many things
had been overlooked for want of
knowledge and this I am now
about twothirds advanced in, for
I geton but slowly.

It was Gray when he was here at
Kew who apprised me for the Muses
for Ballantyne and lent them to him,
survived the figures in his Scores and
when he died Gray found the packet
and in a note promised to return
them on the first suitable occasion,
my own impression, until your letter
came, was that he had sent the packet
to Kew and that it was there put away
and forgotten, I have experienced something
of this kind before. The specimens were
mine and had never been in the Kent
Herb.; the most interesting having been
overlooked as common species by
Hooke and Wilson before my time.
I have two sets of his specimens, 144 American
Muses which being unlike I have to
keep both, Douglas's names some of which
were afterwards figured in his Muse. Exp.
were taken from the collections belonging
to the Royal Agricultural Society, many
years after the true dried plants were
sold, and this collection was sent home
to put names to the specimens and
divide them for a dealer who had
bought them to make what he could by
them; thus I got some specimens
which probably no one else had seen,
and many that were unknown except
by Hooke's figures. In one in all
this became dry; Britton talked about
the specimens last to Gray when he
was at Kew and they have since asked
for a return of them; but there is no one
and not Wilkes, it is in the "Records of the American Academy of Arts & Sci., Vol. 11, 1859-1860" p. 245. I have the paper and copy from a Mr. Cobl, I was obliged to give up the three species of Fissidens as hopeless. I think all the Hypnum are in doubt. The whole number of species mentioned is 33.

There still lay one or two copies of my " Araucana American Plants" and some other small papers also some of Powell's Notes from Samoa, he being dead I shall dispose them they are not all fine specimens Powell himself did studying them but only considering them and gathering them because I asked him. I have also a great many Australian things of mine any thing with your acceptance I shall be very glad to make some return for the book you so kindly sent me.

We have experienced a cool and dull summer this year, a whole fine day absent once in three weeks, just now it seems improving and we
have had fine days with sunshine and at last it is warmer, several American plants which I hoped to flower have been lost from the continued wet. Pracelia seems I have hope saved until the second year and then look they go wrong in the central bed but there is however always something to find and by those who try to grow plants.

With best regards,

Yours ever

William Mitten
I shall probably take a French mail steamer from Marseille on the 28th, and shall then be in Yokohama on the 1st of Sept.

Please remember me to Mrs. Gray.

Very faithfully yours,

[Signature]

11 Montague Street,
London W. 1.
June 23rd, 1889.

Dr. Serret Watson,
Dear Sir,

I arrived in England last Monday afternoon, and have been to Pekin almost every day and spent the entire day there. I have met Mr. Dyer, Sir Joseph Hooker, Prof. Alcides, Mr. Baker, Romesley, Clarke, Browne, Cooke and Milne. Prof. Alcides has been very kind in showing me any thing I requested him to examine.

I went yesterday afternoon to South Kensington Museum to see Mr. Carpenter and others. Being Saturday, and also a little past four, they were all gone, and I am intending of leaving England tomorrow evening for Berlin and St. Petersberg.
I left my paper in such a state, merely under your care, that I don't know how and when I can repay you for all your kindness. I undertook this unpleasant task because I found none that Herschel's Chinese plants have been already published as far as Centunculaceae.

Let me know that I will return it. But I am very much afraid of my paper, it wants to make a few corrections. I found that Eucereon mongolica is little different from D. grosseserrata in the character of scales of cups. and also in leaves. I have been able to examine a larger number of specimens from Manchurian and Khinda, showing a great similarity between them, but evidently distinct. My plant is still D. grosseserrata, until not mongolica. Please retain and write synonymy relating to this former. This cannot, as it is connection with Flago mongolica. Range of distribution is to be altered. stickless Manchurian.