

in the forthcoming rev. of Sill, Amer.
Appropos to Amer. Ant. Antiqu.

You should see the Proceedings
of the Academy, Ant. Soc. Philad.
phis - and its Journal, now
in large 4th No. There is not
much botany there; and what
there is with little.

I must get in the habit of
sending you all my miscellaneous
& scattered papers.

I could have the Proceedings
of the American Academy of
Arts & Sciences sent to you, or
to Ant. Hist. Rev., if you desire.
There are numerous historical
papers in it, other than my own,
e.g., Tuckerman has one on
Lichens, in the closing part of
Vol. V. - about to be issued. -
Printed last summer.

Excuse my hearty send
and believe me, as ever,

Yours faithfully
Dr. pamphlets address
"Richmond" reaches
you without fail.
Abigail

Cambridge, Mass. Oct. 27, 1862
Dear Prof. Oliver.

Yours of the 9th inst. has
just reached me; and as I am
writing several notes for my
British mail envelope this week
I will not delay to thank you
for it, and also very specially
for the precious materials of
the monstrous Geranium - flower.

I cannot look at this by
night, but I hope to examine
it before long with your letter
before me.

I hope you will keep on
working at authors, as you
propose. You will find it well
worth while, and you will probably
get rid of the clive in time,
even if you have not already.

Your remark that the current hypothesis, tho. agreeing very well with 9 anthers of 10, "explains nothing, suggests nothing", I will agree to.

The reason I say it answers the purpose of a sufficient hypothesis very well is that, having had it in ~~view~~ view for many years while working in Syst. Bot. at places of a great many families, I have met with nothing in the way of it. But, tho. this gives a certain probability, it by no means gives me much reason to insist upon it as true. All such morphological generalizations must be held with a certain looseness, and sub judice.

But if the author answers to a leaf-blade, and the pollen to paracymbium - a likely enough supposition may be entertained as to "why the 4 cells and the inter-nal plate of tissue. The paracymbium of a leaf is pretty distinctly in two strata, and it is likely enough that the transformation into polleniferous cells should commence in each stratum, and not at the junction

of the two, - especially as there you have ~~the~~ normally the vascular system of the leaf. Now starting with this as the type, it would be natural to expect that, in many cases, the upper stratum alone would be polleniferous, - but more commonly both. The common involution of a forming leaf should be put in comparison with the general intrase condition of anther, &c. &c. -

Now all this I hold sub judice, quite ready for a new view, when presented, with a certain clearness & completeness. The common view I have a clear conception of. Beetham's I understand, but do not at all take to it. Your view is yet to be developed. When developed, you will hardly find anywhere a more attentive pupil than I will be. But I still anticipate that the theory next to come will somehow embody the common one, as a case under it.

I think I see your hand in the Nat. Hist. Rev. on Darwin's Principles paper. I have a few comments



Gray, Asa. 1862. "Gray, Asa Oct. 27, 1862 [to Daniel Oliver]." *Asa Gray correspondence : letters from Asa Gray to various individuals*

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