

Harker

July 6 1879.

papers
into it. The unexplained
of this season. Never in
my mind.

them?
I have
reflected or undervalued
my hands
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my hands

My dear J. Dawson

I have carefully collated the
passages you refer me to in the Supplement
to Canadian Geology, with my 1878 Address,
& I fail to find that I have done you
injustice, or neglected your labors.

In the passages referred to you say,

"In E. America, as in Gt. Britain, the Conditions
of local ^{accumulation} formation seem to have set in
earlier to the Northward" (of Newfoundland).
& that "The Devonian Flora of N.A. seems
to have extended itself from the N.E."

Without undervaluing these local
observations, I cannot regard them
as contributing largely to either of Cook
Laugel's hypotheses, which are, that
Vegetable life originated near the N. Pole
& that its successive phases were developed

These by evolution, & were from various
distributed over

Mr. Glebe's

You speak of

my "putting forth

claims on behalf of Laporte which pre-
cede you of discoveries: - But surely I have not
done so. Laporte's claim is to have
written an ingenious Essay, involving the
discovery of a multitude of discoveries
& hypotheses, to the discovery of which he
lays no claim whatever; nor do I
claim any for him: & of, as you say,
Gray, Hale & yourself first worked out the
whole matter. I am glad to acknowledge
it. I am not well acquainted with your
writings, & I know Gray's well.

You observe that I have probably
learned before this that some Physicians
regard Laporte's idea of internal heat
as untenable. I have from childhood
known that this hypothesis was disputed,
but I have yet to learn that it is
rejected by many first-rate men, & by

any on irreputable ground. I have no
opinion of my own on the subject, no more
probably has Sabatier; but tho I hold that
in the present state of Science it is permissible
for him to write in support of his arguments.

I agree with you that the Veg. Paleontology
of the Anter. Regions has to be redone; but I
do not limit this redone to those regions,
I would wish it include all known regions;
for that we have as yet no approximate
conception of the extent, or composition, or
affinities, or chronology, or geographical
distribution, of the vegetation of any one part
of it. I believe that we are in the dark
as to the affinities of perhaps 90 per cent of
the enumerated fossil plants, & that our methods
of determining the age of beds by these means
are very feebly tentative. In saying that I do
not undervalue the definite results
obtained by Heer, Lesquereux, yourself & others.

In the best I conceive that I have, with
giving due publicity to the ability, interest &
originality of Sabatier's views, sufficiently
guarded myself against being supposed to

recourse, ⁱⁿ the adaptation, wholly or in part.

Lest you must allow me (through myself
as no less precedent) to remonstrate against
your attributing it to any one personally (as
you do to one personally in your letter) that
your Bakerian Lecture was not published in
the Phil. Trans. This contrary to the spirit
& action of the Society's Officers & Council, with
whom the responsibility of accepting or rejecting
papers wholly rests, & of the Fellows generally,
to hold any individual, or even the referees,
(of whom there are invariably 2 or more) as
responsible: & it is a point of honor not
to reveal the names of the referees, becaus
they desire to otherwise.

I was not a member of Council when your
lecture was described, & hence had no vote,
though, had I should have voted against.

The publishing of the descriptions of many
of ^{your} new species, both because I thought them
to write insufficient, & because I regard the
descriptions of species as far better published
elsewhere.

I trust my dear Sir that in this defence of
myself I have used no language which you
ambiguity might be supposed to reflect ^{unfavorably} on your
my & valuable labor; & shall be glad to hear

my address has remained unaltered & reconsideration