

73. Harley Street  
London

9. May 1869

My dear Dawson

Glacial  
Stipe  
& Cuv. Plans

Soon after I received  
your letter of March 1869  
& before I had time fully  
to study it, I made a tour  
of three weeks with Lady Lyell  
& my nephew Leonard to  
Norfolk, Suffolk & Yorkshire,  
chiefly to explore the various  
beds of glacial drift. I  
came back persuaded that  
the succession of events &  
the changes of level during

The glacial period were very  
Numerous & made me suspect  
that in former geological times  
we may speculate on a great  
variety of minor upward &  
downward movements, some-  
times amounting to 100 or 200  
& at other times more than  
1000 feet in opposite directions  
all taking place in a mere  
fraction of such periods as  
are measured by each of our  
important revolutions in  
organic life

But I will not dwell on this  
subject now but hasten to thank  
you for the great help which

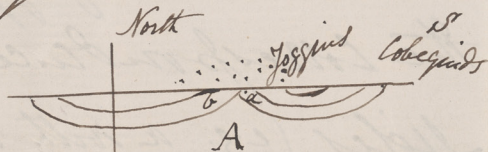
Your sections have been to  
me, though perhaps if I had  
studied your book more carefully  
I might already to have under-  
stood your views.

I think there can be no doubt  
that there was more sinking  
in the Cumberland area than  
elsewhere, & when one thinks  
of the diameter of the present  
trench between Minnie &  
the Cobequid hills as 20 miles  
in extent one may well suppose  
a great difference in the  
movement. (See New Zealand  
Earthquake. "Principles of Geology"  
10<sup>th</sup> ed. vol 2 - p. 85). The

different thickness of the  
Carboniferous rocks on the South  
& North sides of the trough is  
I believe supposed by you to be  
owing to the original amount  
of deposition being less in the  
South, which you attribute not  
to want of Sediment or what  
Hall calls attenuation, but  
to want of Capacity in the  
basin, on that side where  
the floor of the sea remained  
Comparatively stationary.  
There was no room for the  
accumulation of a great thickness  
of shale (?) Perhaps the conglomerate  
implies a single beach  
which was inconsistent with

the growth of coal (?)

I should like very much  
to give on your authority a  
section across the country  
from the Cobeguid to Dinuachie  
& as far as the middle of the  
Synchinal beyond



I must try & shew the small  
feature which the present sea  
cliff makes in comparison ~~to~~ with  
the thickness of the beds which  
are now below the level of the  
sea & the thickness of those <sup>above it</sup>  
which must have been re-  
moved by denudation and

which I may express by  
dotted lines.

Have you facts to bear out  
the great thickness of the  
Carboniferous rocks on the north  
side of the axis A as well as  
on the south Joggins side &  
the correspondence on the two  
sides (e.g. a with b)? I would  
like if this is a question which  
is already considered in your  
book. Your conclusions  
as to the denudation of the  
top of the Antichinal arches  
are entirely in accordance  
with those which I find

now generally adopted by  
geologists. But the new school  
are trying to accomplish the  
removal of solid matter too  
exclusively by rain & rivers,  
whereas the manner in which  
the upper side of the walls of  
faults has been brought down  
to the level of what must  
have been the lower side  
# Shows the intervention of  
an agent, which plucked off  
the surface instead of  
cutting it into valleys & ridges.

The South Joggins coast  
section which we saw together

Must imply as great a sinking  
as you demand for it, & as  
the S. Wales coal field is  
12,000 ft thick, & as Hall demands  
10,000 ft of sinking, in order  
to explain a bed of coal im-  
plying an old land surface  
in the Millstone Grit of  
Lancashire, I do not see why  
Lesly should have been so  
determined to question the  
thickness attributed to the  
S. Joggins coal field.

After reading your last  
edition I hope he regrets  
having disputed this fact.  
You ask no more for the



true coal measures than  
 must be conceded for those  
 of Lancashire & lately the  
 ground has been gone over  
 with such care in England  
 & by so many surveyors that  
 I have entire faith in  
 their results.

I believe that you suppose  
 the upheaval to have taken  
 place during the time when  
 the volcanic rocks were formed,  
 which agrees well with Darwin's  
 idea of the existing areas of  
 volcanic action & upheaval  
 coinciding in the coral regions.  
 Did we not see together an

unconformable junction of  
Triassic & Palaeozoic strata?

The entire absence of faults  
in the Joggins Cliff section is  
certainly striking, when we  
consider that we have to  
suppose a movement of  
subsidence of 15000ft - followed  
in the <sup>Minudie</sup> Antichinalx by another  
of upheaval to an equal  
amount. If this last  
accompanied volcanic action  
in the immediate neighbour-  
hood, it is still more note-  
worthy. ~~Must we not also~~ <sup>For we have to</sup>  
assume that <sup>the</sup> whole ~~carbon~~

-iferous strata about 2 rose  
to the extent of nearly six miles  
taking the two <sup>The uppermost</sup> judgements  
together. The coal measures  
along the Archival may  
have ~~been~~ <sup>remained</sup> all the while  
nearly at their original  
level?

I have not yet had time  
to show ~~Dunley~~ & others the photo-  
graphs you have kindly sent me of  
*Pupa vetusta* & the *Conulus* - but  
will speak of them in my report.

The vegetable origin of Torrell's *Eo-  
phyton* having been questioned I have  
sent for more specimens from Sweden which  
have arrived in the ~~box~~ but I have not

yet got them to examine. I will tell  
you more about them soon - believe me  
my dear Dawson

ever most truly yrs

Chadwell

P.S. Professor Williamson tells me  
that Comstock has visited him at  
Manchester & seen his specimens  
& has satisfied himself of his (W's)  
correctness on certain points which  
he had previously doubted about.

"<sup>1</sup> He is thoroughly convinced that the  
"reticulations on the fibres of my

"*Dadoxylon* figured in Elements Fig 484. 5

"<sup>5</sup> <sup>531. 18<sup>th</sup> ed.</sup> editions are those of a true *Composita*

"They are discs, and not mere ligens

"reticulations, and thus far the specimens

"is perhaps the only one yet found

"in our English coal. measures that

"is entirely satisfactory on this point

"All the other so-called *Dadoxylon*

"have mere reticulations in the interior

"of the wood tubes, hence their comi

"ferous character has yet to be proved.

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" Yet they are the ordinary & common  
" trees of the coal-measures here,  
" exogenous stems, supposed to be  
" Coniferous.

2? Cantharis is quite satisfied that  
" the woody wedges of my new calamite  
" your fig 478 5<sup>th</sup> ed. (L. fig 525  
" 6<sup>th</sup> edition) are identical in their  
" structure with corresponding wedges  
" of the beech or common type of  
" Dadoxylon. That they have true  
" medullary rays or exogenous growth  
" & a calamite like pith. I am quite  
" satisfied that I have got the cone  
" of the same plant, the internal orga-  
" nisation of its woody axis presents  
" the most important correspondence both  
" as regards its component elements, &  
" their arrangement. Yet it is a true  
" Calamitean cone, bearing sporangia  
" full of spores, resembling in general  
" features Binney's cones, but having

very distinctive features of its own  
"I know that Dr. Carnuthers following  
"in the steps of Robert Brown never  
"believed in my dissimularous coniferous  
"gibres. But he had never seen  
"my specimens till now. The  
"most points to be worked out are  
"the relations of the common retic-  
"ulated *Dadoxyleps* to the true  
"coniferae on the one hand, & the  
"Calamopitax to Binney's or Unguis  
"Calamodendron & Calamites on the  
"other."

Since I received this I have  
seen Carnuthers for a few minutes  
who told me that he had found  
that Hillebrand had been doing  
good work although, he, Carnuthers  
by no means thought that there  
was as yet, proof of the cone

with Sporangia belonging to the  
Dadoxylon.

Comethers at the same time  
told me that all this had  
not altered his opinion as to the  
true Calceites - that he had  
lately obtained evidence of  
Sigillaria being truly Cryptogami-  
nous - also that he had  
recovered the loop & shell spores  
of our living Lycopodium &  
found under the ~~microsc~~ microscope  
that they were actually identical  
in dimensions with the loop & shell  
spores of the Lepidodendron of  
comparative gigantic dimensions.  
I believe I am reporting correctly  
this last news which was given  
me in conversation.

I shall be very glad if you

will answer any of my queries  
in the first part of this letter  
or help me in speculating on  
the cause of plications or of  
linear anticlinals & synclinals.

You have probably seen that accord-  
ing to Lesley the folds of the  
Alleghenies cease when you go  
South of the middle of Virginia  
& a succession of parallel N. & S. faults  
each of 15,000 & 20,000 feet are  
substituted.

Can we imagine the same kind of  
force differing perhaps in violence  
or the mode of its application or di-  
rection or in the depth from the surface  
at which it is developed to have pro-  
duced these two distinct structures, namely  
the plicated & the faulted?

I cannot comprehend Dana's lateral shove  
by which he produces mountain chains (see Dana  
A 720 Manual 1863)