

8 John Bromley Kent

My dear Dr. Gray (I have divided 2 letters
to you Postage)

Your last letter, like all its
predecessors have been very valuable
to me; & every word in it has
interested me. When I said that

your remarks on your alpine
plants "riled" me; I did not mean
to doubt them, except in the opinion
sense that they went against
some theoretical notions of mine.

These notions are too long to give
& indeed not worth giving, as far
as America is concerned, & I can
see from your letter that we sh.
take very much the same view.
I am very glad to hear that you
think of discussing the relation between

of the identical & allied U. States &
European species, when you have
time. Now this leads me to
make a very audacious remark
in opposition to what I imagine
Hooker has been writing to your
own scientific conscience. I
presume he has been urging
you to finish your great Flora,
before you do anything else.
Now I would say it is your
duty to generation as far
as you safely can from your
as yet completed work.
Undoubtedly careful discrimination
of species is the foundation of
all good work; but I must

look at such papers as yours in
Lilliman as to fruit. As the
careful observation is far harder
work than generalisation & still
harder than speculation; do you
not think it very possible that
it may be overlooked? It ought
never to be forgotten that the
Observer can generalise his
own observation in comparatively
better than anyone else.

How many witnesses have laboured
their whole lives on the Assertions
& have not drawn a single
conclusion; I think it is Herivel
who has remarked how much
better it would be, if they had
paused in their devoted work &
seen what they could have deduced

from their work. So do pray look
at this side of question, & let
us have another paper or two
like the last admirable
ones. Then, and I not an
audacious dog!

You ask about my doctrine
which led me to expect the
Trees would tend to have separate
sexes. I am inclined to
believe that no organic being
exists which perpetually self-
fertilises itself. This will
appear very wild, but I can
venture to say that if you
were to read all my observations
on this subject, you would
agree it is not so wild

as it will at first appear to
 you, from flowers said to be ^{always} fertilized
 in but 20 or 25. — It is a long
 subject to which I have attended
 to for 18 years! Now it
 occurred to me that in a large
 tree with hermaphrodite flowers,
 we will say it will be ten to
 one that it would be fertilized
 by the pollen of its own flower,
 & a thousand or ten-thousand
 to one that if crossed, it would
 be crossed only with pollen from
 another flower of same tree,
 which would be opposed to
 my doctrine. Therefore on the
 great principle of "nature not
 lying" I fully expected the trees

would be apt to be divisions or
monocotyledons (which as pothen has
to be carried from flowers to
flowers exactly, would form
a map for another individual
of the same species) & so
it seems to be in Britain
& N. Zealand. But can the
fact be explained of certain
families having this structure
& changing to be trees, for
the ~~Arctostaphylos~~ rule seems to hold
both in genera & families,
as well as in species.

I give you full permission
to laugh your fill at this
wild speculation; & I do not

pretend but what it may be chance
which, in this case, ~~which~~ has
led me apparently right. But
I repeat that I feel sure that
my ~~own~~ doctrine has more
probability, than at first it
appears to have. If you had not
asked, I sh^d not have written
at such length, though I cannot
give any of my reasons.

The Leguminosae are my greatest
opponents; yet if I were to throw
to Menten in insects made
during many years, I sh^d. fully
expect ^{in them} to take place;
but I cannot find the one
garden variety ever cross
each other. I do not ask

you to take any trouble about it,
but if you should by chance come
across any intelligent nurserymen,
I wish you w^d. enquire whether
they take any pains in raising

the class of papilionaceous plants
[I have seen statement of naturally formed copied Phaseolus ^{hybridus} ~~hybridus~~
apart to prevent copying.] The

worst is that nurserymen
are apt to attribute all
variation to copying.

Finally I inclined to believe
that every living being ^{with distinct individual} ~~varies~~
an occasional copy; & as
trees from mere multitude
of flowers offer obstacle to this,
I suspect this ^{obstacle} is counteracted

of tendency to have sexes separated.
 But I have forgotten to say that
 my maximum difficulty is trees
 having papilionaceous flowers: some
 of them, I know, have their flowers
 when ready for fertilisation,
 keel-petals expanded, but Bentham
 does not believe ^{that} this is general:
 nevertheless on principle of nature
 not lying I suspect that this
 will turn out so, or that
 they are eminently sought for
 bees dusted with pollen. Again
 I do not ask you to take trouble,
 but if stirring under your
 Robinias when in full flower, just
 look

at stamens & pistils better
permeated & better Bees visit
them. — I must just mention
a fact mentioned to me the
other day by Sir W. Macarthur,
a clever Australian gardener,
viz how odd it was that
his *Erythrina* in N.S. Wales
would not set a seed, without
be imitated the movements of
the pistils which Bees
cause. — Well, as long
as you live, you will never
after this fearfully long
note ask me why I
believe this or that.

I am particularly obliged for your information about Protean genera; as this is one of the greatest of my many great puzzles; viz to know or conjecture whether the great variability of such genera is due to their condition of existence, or whether it is innate in them at all times & places (I am aware that this cannot be strictly predicated of any genus, for all have some fixed species). - Now I have thought that you would not object to my sending the latter half of your note ~~on this~~ with list of such American genera to Mr. H. C. Watson, of whose great clearness of mind & acuteness I have for long ~~known~~ ^{been} the highest opinion. - I have sent his

note (1) + 8p of (2) for the chance of your
liking to see them; but if note
longer than usual, do not
send them; & I havey in course
of half-year, you will be able
to return them together with your
own list; but pray do not lose
them. There is of one point, to which
for myself, I wish to call your
attention, viz. whether you rightly
understood the question I did not
refer to genera having very close
species, but to genera having very
variable species. Watson thought the
you might not have understood
me. - If you do read Watson's papers
& hear anything to remark on
subject, I need not say, how very much
I like to hear it. I am sure I

do not know whether I have acted (4
 wisely in sending Watson letters,

but I repeat again they do not
 need them, without you feel
 inclined: as, I suppose, in
 course of few months you would
 be sending some parcel to
 England, it will not cost you

trouble to return them.

With hearty thanks for all your
 kindness, & begging forgiveness for
 length of this letter, which is
 chiefly your own fault (— for
 as these are concerned), believe

me, yours most sincerely

(Ch. Darwin



Darwin, Charles. 1857. "Darwin, Charles Mar. 1857." *Charles Darwin letters to Asa Gray*

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