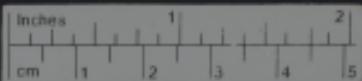


10571

In arranging these letters  
from C. Darwin to my father  
Daniel Oliver, I have  
divided them into the  
following batches

1. from Down  
(month + day given)
2. undated, from Down
3. from 15 Marine Parade  
Eastbourne
4. from Cliff College  
Bournemouth
5. odd pieces of <sup>detached</sup> ~~prosecript~~
6. letters to D. H. -

Notes by Professor F. W. Oliver



2

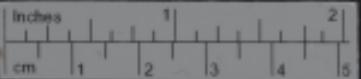
10571

The collection will run except  
fails betw. the years 1860-64,  
a period when Darwin was  
very actively working at  
insectivorous plants, pollination,  
dimorphism, orchids, Tendrils,  
the variety of other subjects.

He had evidently heard  
from Mr. Hooker, but may  
have been influenced by his  
father, Daniel Oist (recently  
appointed Keeper of the Herbarium  
Prof. of Botany at University  
College (1860), FRS. at the  
same time) was young,  
enthusiastic, well-informed  
& methodical, & wd. be a  
useful correspondent.

As the letters show  
he was not disappointed.

Indeed, reflecting as I do

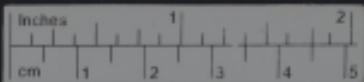


where my father stood at the time, the connection was almost severed - so would Ben the D. used be father almost to the point of exploitation. One he was continually pointing out advantageous fields that my father might take up, & warning him not to let his professional duties over-welm him -

But of course my father had to make good his appointments to Kew & U.C.S., leaving his lifeblood to Darwin, his livelihood to

etc -

He enjoyed the associa<sup>n</sup> & I have heard him complain -



10:71

Order of the letters

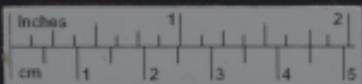
<sup>3-</sup>  
They are not in chronolog<sup>c</sup>-  
ical, & in the types copy  
the arrangement is anyhow.

I have left them so  
as I am sure anyone  
accustomed to Sandine  
Darwinianas will put them  
in proper sequence much  
more easily than I can.

a few are endorsed with  
He was by my father, - such  
may be accepted -

The pencilled nos. as  
"One no. 11" can be ignored -  
They were probably added by  
Francis Darwin when doing  
the "life".

Kemp has help in setting



and the correspondence  
chronologically - are

① The addresses (it will be  
well known when D. was  
at Eastbourne or Bournemouth)

② The manner of beginning -

Dear Sir  
— M. Sirs

— Ollie

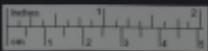
becoming more intimate with  
passage of time -

③ The subject matter

= On the typed copy I have  
added notes, wh. are all  
right so far as I know -

But apart from this  
more technical value there  
are lots of "Good Things"  
in the correspondence.

J.W. Chisholm



Dam House  
MSS. 10

Contained  
BAR 261  
DH/MS. 10:1-70

Packet 10

Charles Darwin

69 letters to Daniel Oliver  
3 letters to Joseph Hooker

1 letter to William Sharpey

Note by Atkinson, 1996 May 22

There are

66 letters CD to Daniel Oliver  
3 - CD to Joseph Hooker  
1 letter CD to William Sharpey

From

THE ROYAL COLLEGE OF SURGEONS OF ENGLAND



60-1.

Down,

Bromley, Kent

12th

My dear Sir,

I seldom see anyone, so it is a great pleasure to me to receive a scientific letter and I thank you for your very interesting one. First for Primula; I should like to see pollen of *P. farinosa*; but it really is of little importance, so tell your correspondent not to give himself much trouble; I send copy of my paper.

I had noticed difference in size of ovules, but slurred the case over, and concluded that I had taken buds of different ages; I have just looked at a couple of large flower-buds of Primrose; in these the ovules were largest in the short-styled, just the reverse of what you say! Have you written "long"-styled for "short"-styled? Or is the size a variable point? The short-styled certainly produce a greater number of seed, but I did not attend to number of ovules; I will look at a few more of buds of equal age.

Your discovery about *Campanula* seems to me extremely interesting; and I am especially surprised at the gradation of the two <sup>states</sup> ~~on~~ the same (?) plant: I am very glad you will read a paper on this. Since I have had *Campanulas*



- 2 -

and Violets for experiments, and twice I have been  
observing. I can see at least 3 classes of *dimorphism*  
Primula & CO; Thymus & CO.; Campanula. Violets & CO. For  
the latter cases I have theorised from what I have seen and  
read on Violets that the final object of the imperfect flowers  
is to produce seed safely, without any crossing (so your remarks  
on the structure please me); the perfect flowers being  
adapted for getting an occasional cross; but I have not space  
here to give my reasons for coming to this provisional con-  
clusion. You will see that, according to my notion, the final  
object of the *dimorphism* in Violets etc. is exactly the  
reverse of what it is in Primula. With respect to *Fumariaceae*,  
I had attended a little to them, when 3 or 4 years ago Asa Gray

(and so does Vaucher) advanced this order as one  
with perpetual self-fertilization; this made me attend more  
carefully to it, and I am convinced the whole structure of  
the flower is beautifully adapted to forming an occasional  
cross.

I have many recorded observations, but God knows  
<sup>ever</sup> whether I shall have time to make use of them. I write this  
letter only from memory.

Insects are not indispensable, for all that I  
have tried set seed without insects' aid. It is really pretty  
to watch a bumble-bee show neatly its hind legs rest on the  
(visit Diclytra (+ allied forms Asturia &c.)  
& See

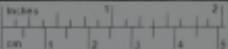


- 3 -

projecting plates on each side of the hood over the stamens and pistil, and push it to one or the other side, and gets its body covered with pollen, against which the stigma is rubbed.

This tube has nectary on both sides and the "hood" can be pushed either way, and the pistil is straight. Next look at *Corydalis heterocarpa*? and allies (purple flower with white tube and now in blossom) here secreting nectary on only one side, and the hood can be pushed off only on one side, and the pistil is curved (it is thought in *Dielytra*) towards the gangway into the nectary. Pray look at *Corydalis lutea* later, with nearly similar structure; but with additional curious continuance; when Bee (as I have often seen) visits flower, it pushes back the hood, and then the pistil moves with a jerk or spring forwards. This movement determines at once the act of fertilization, though it will take place if the flower is never visited and the pistil never snaps off (but flower in the case <sup>are not</sup> ~~on top~~ ~~pistil~~). But I must not run on; and I have written so briefly that you will not understand me.

With respect to Bees biting holes; this occurs in almost all flowers occasionally which are difficult to enter; I have never seen it occur in any *Thomomisidae*, at least I think not. But I know of no single instance of this always occurring



- 4 -

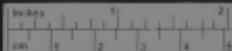
in any plant. When it does occur, there can be no crossing as you say, and I have some reason to believe that the fertility of flowers is diminished; certainly in some cases it would be quite stopped. Oddly this biting of holes occurs chiefly when large masses of the same species are grown together; I should like to hear what species of Fumaria you have seen with bitten corolla.

I am pleased to hear that you are going to discuss Dimorphism in Nat. Hist. R., for I have no doubt you will make a very valuable paper.

By the way, I have been much interested by your Atlantis paper, and rejoice at your conclusion; I think, however you might have made it more popularised, and I will add another criticism, viz: that you ought (as I think) to have laid much more stress on migration having taken place during former warmer periods. → a lecture at the R. Institution.

I wish that your lectures did not take up so much time, so that you might write on various subjects. I do not know whether the diagrams of *Catastoma* would be worth your taking away from *Linn. Soc.*; but if so, they are at your service; and there was a diagram of Primula there. This reminds me that Mr. Fitch drew for me cuts of Primula, would you be so very kind as to ask him what I am indebted to him for his

S. They failed to find these diagrams for *Banana*. They are still among the diagrams at V.C.



- 5 -

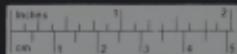
kind assistance; though perhaps the *Linn. Soc.*  
paid for the drawings on the wood. X

I am nearly certain that *A. Gray*  
(perhaps also in his Manual) told me that the imperfect flower of  
some N. American Campanula produced more seed than the perfect,  
but I keep my notes in such a stupid manner that I cannot refer  
to single points, only to whole large subjects.

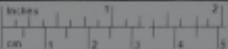
I have written to you a frightfully long letter  
not worth the trouble of deciphering.

Another thing just occurs to me which I meant  
to say whenever we met; viz: that if ever you had the time,  
you might make a grand essay on the character of water plants,  
taking, I presume, those whose roots were always covered with  
water. I presume no one could treat of more than Europe and  
N. America, though some Tropical country ought to be included.  
It would be interesting to see how many had sent one or two  
colonists into the water; but what would be most curious would  
be to consider the nature of affinities, and degrees of organisa-  
tion of these groups in which all the specimens were aquatic;  
I strongly suspect they would come out a queer set. Do think  
of this. Forgive me rambling at such length, and believe me,

X This is the famous wood cut. Yours very sincerely,  
showing long & short styled flowers. C. DARWIN  
"Primroses?" said by Sir W. Darwin has lost my father's a rough sketch  
which he wanted for his herbarium. The sketch I gave to  
Professor de Candolle framed, & he hangs it somewhere at the Jardin des Plantes - Paris.



P.S. I have just looked at another pair of buds and  
this time the ovules seemed of same size.



102.

Down,

Bromley, Kent

30th November

My dear Sir,

As I have lately given Hooker such a frightful amount of trouble and as you have looked at ovules of male *hyacinth* will you be so kind, (the next time you have your compound microscope out) as to look at the ovules of *acropolis* (sent on glass slides by this post, and to be thrown away when looked at), which I found, after writing to Hooker lower down in the ovarium. Please first look at ovules of *Maxillaria* and *Orchis* which resemble those of all the several orchids, (including ovaria of buds) which I have examined. Now see what a vast difference the ovules (or rather more empty membranous fringes to the placentas) of *Acropolis* present. These fringes positively lie on the same ridge of placenta which carry the true pulpy ovules in all other orchids. You will see them also in a transverse section. I have examined 6 specimens of *Acropolis* and all the ovules on whole length of the 3 placentas were in this condition. Why I want you so much to look at these ovules and give me your opinion is that I have never before looked at an ovule; and I have reason to believe that if I can trust these observations, I shall make out sexes of *Catassetum*, *Myanthus* etc. Though I know nothing about ovules, the



- 2 -

difference is most conspicuous between the placentae of  
and of all (except *Cat'sbeany*) other orchids.

Besides this experience in the ovules, the  
stigmatic chamber of *Aeropera* is so small that pollen-  
mashes can hardly be forced in; the surface is but little  
viscous, and the stigmatic ~~chamber~~<sup>warty</sup> are very different  
from those of every other orchid except *Cat'sbeany*, in  
being empty.

From these several reasons I cannot avoid belief that  
the *Aeropera* flowers of two species which I have examined  
are males. Will you inform me whether all the several  
flowers of *Aeropera* sent me by Hooker came from  
same plant?? [See Note below - pencil note on letter to  
my father].

One other point in the ovaria of *Aeropera* has  
surprised me; but unless you have carefully attended to orchids,  
you will perhaps not be able to express an opinion: Lindley  
and Link say ovarium consists of 6 carpels. Brown says of  
three; and that three of the ~~carpels~~<sup>divisions</sup> have no homological signifi-  
cation. Now to my surprise I see three irregular double  
rows of ~~hyaline~~<sup>ridges</sup> projections, running parallel to the ovules,  
and belonging to the three ~~carpels~~<sup>divisions</sup> of the ovarium, which do  
not carry ovules: you will see a few in the transverse  
sections and on the ~~dissected~~<sup>longitudinal</sup> fragments.

inches		1		2
cm	1	2	3	4

- 3 -

What can they be? It seems to me to favour Lindley's and Link's view. According to Brown they lie on the midribs of his 3 carpellary leaves : according to Lindley, they would be, I suppose, representatives, separatives of ovules.

Pray forgive my writing at such length and believe me,

Yours very sincerely,

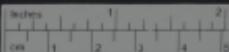
C. DARWIN

Is it not a rather interesting fact that you have observed in Lychnis, viz: ovules in the male plant?

I am glad to hear from Hooker that you are going to consider C. Heer, his Atlantis - map seems to me very wild.

The view, which I give in origin of migration of north during old warmer period seems to me much simpler and agrees better with geological facts. i.e. "origin of species"

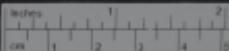
P.S. I should have said that ovules of the Orchis have been in spirits: ~~order~~ <sup>ovules</sup> of Maurilia would have been equally plain and ~~spotted~~ had they been in spirits: spirits make no difference to the ~~nativity~~ <sup>order</sup> ovules of Aeropan. I should have imagined that the rudiment condition of ovules and stigmatic ~~whorls~~ <sup>order</sup> ~~of~~ <sup>due</sup> might have been due to culture or bad conditions, had I found any other orchids in this condition; and had not solid edges of stigmatic chamber not been so much



- 4 -

contracted.

With respect to 6 *divisions* of ovary of Orchis,  
*long*  
are there ~~any~~ Monocots with 6 carpels and only 3 pistils?  
I suppose not, otherwise Brown would not so have objected to  
Lindley's view.



(63)

Down,

Bromley, Kent

December 7th

My dear Sir,

I am extremely much obliged for your two letters. The quazy-ovules in *Acropora* certainly were not in least dried and always differed widely from those in the orchids, young and old, (not fertilised) of all the main divisions; so I must trust to your own judgment, and make the statement exclusively on your own authority. Your former letter explained everything most clearly; I thought it worth while just to ask you the meaning of the spines. I supposed that Brown was a much higher authority than Lindley and Link; and the subject does not concern me; I asked out of mere curiosity. I have been interested by making out from state of ovules (according to your judgment) and of pollen, that *Catasetum Tridentatum* is male *Monocanthus Virilis* - female *Myanthus Barbatus* - hermaphrodite and you knew they have been produced on same plant.

Pray believe that I am truly grateful to you, so busy as you are, giving up so much time to me, and

I remain,

Yours very sincerely,

C. DARWIN



(o 4.

Down,

Bromley, Kent

23rd March

My dear Sir,

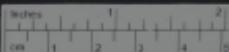
You told me formerly that you did not much care about my troubling you; I hope to Heaven you keep of same mind.

+ Will you ask Mr. Fitch to make two little diagrams for me for woodcuts, as by enclosed paper. And will you kindly give him the specimen, viz: two common primroses, one pin-headed, as the Flirists say with stigma at mouth of corolla; and the other with stamens at the mouth; and cut them longitudinally with sharp scissors. Mr. Fitch could keep a little against me.

I think I shall publish an account of my observations and experiments on Primula; i.e. on its dimorphous condition. By the way you will find the pin-headed and non-pin-headed in any bank of Primroses. I find that with *P. elatior*, the short-stamened flowers fertilise themselves, whereas the long-stamened flowers will not set without man's or insect's aid.

I am now crossing largely cowslips and polyanthus ~~and~~. And this leads me to beg a second favour, viz: to send me 2 or 3 flowers (not whole ~~twigs~~) of pin-headed and non-pinheaded of any species of Primula, except *P. Vulgaris*, *Sinensis* and *Auricula*.

+ Mr W. W. be the & Fitch, referred to in a note on another letter,  
will speak to Patterson for the /Dr James Smith, as a  
servant



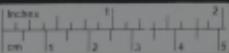
- 2 -

flowers in double green leaf, and pollen would come neither too dry or too wet for measurement.

If you can send, please send names of species.

Yours very truly,

Ch. DARWIN



65

Down,

Bromley, Kent

22nd January

My dear Sir,

You mentioned to me Kurr on Nectaries, and in doing this you did me a great service - that is if helping a man to ride his hobby-horse can be called a "great service"!

To watch flowers and insects has been a very old hobby of mine. Some statements in Kurr lead me to ask you, to give me some information on one point, if in your power, and if it can be done without much trouble. But I have no idea whether you have the plant alive at Kew, or whether dried plants would serve. Half-opened flowers do not do, for pistil bends subsequently. The point is, that I as yet find it a ~~rule~~ that if the pistil is curved to one side, it curves to the side of the nectary, so that the stigma lies in the gangway to secreting nectary ~~or~~ to the honey receptacle. The converse proposition does not hold good; the nectary may lie one side, without the pistil being curved, but in this case it ~~habitually~~ <sup>rule</sup> lies in ~~near the line~~.

One of the best cases in *Saxifraga*, in which in 2 or 3 species (names unknown to me) the nectary goes all round and the pistil is straight; but in *S. sarmentosa* the pistil is

inches		1		2	
cm	1	2	3	4	5

- 2 -

*curved*

~~curved~~ and the nectary is one sided and lies on side towards which pistil bends. Now Kurr, as you will see in enclosed paper, gives name of some species with one-sided and others with circular nectaries; can you find out at any time for me how the pistils are? Analogous facts occur in several genera of Proteaceae and Caprifoliaceae; can you aid me? The rule interests me in several ways, and only the most trifling generalisation, such as this, seems worth making out. I have already got a good many facts. But you must not waste much time on it.

Ever yours very truly,

C. DARWIN

inches		1		2
cm	1	2	3	4

(b6)

Down,

Bromley, Kent

9th April

My dear Sir,

Anytime when you happen to write to me, but not write on purpose, please tell me name of enclosed Fumaria or Corydalis - it grows in my garden in open air. It exhibits a very little, pretty adaptation to Bees. The long nectary alone secretes honey; and when bees are sucking, they push off the hood over to pistil and stamens; and then the pistils and stamens (curved to side of nectary) are rubbed against Bee's belly, and so cross would often be effected. As long as hood is kept pushed off the pistil and stamens, it keeps open and when it springs back, its beautifully snaps together and again encloses the pistil and stamens. Now if you push the opposite way, which Bees never do, as there is no nectar on that side, it is with difficulty that the hood can be pushed off the pistil and stamens, but when this is effected, the hood does not keep open, and so can never again enclose and protect the pistil and stamens. It is worth looking at fresh flower, and trying with a pin. In Dielytra, where nectar is on both sides, pistil is straight, and the hood can be pushed off both sides with equal facility, as I have seen Bees do.

Here is much dry and little wool!

Yours very sincerely,

C. DARWIN



(67)

Down,

Bromley, Kent  
1st May 1866.

My dear Sir,

Very many thanks for your two notes.

I should be very glad of these plants of *Dionaea* (for which I could pay and for *Cypripedium*) and as I shall not go to sea for my daughter's sake till the middle of June, the sooner I could have them the better, as I might try few experiments soon on them. But how on earth can they be packed?

The best address will be:

Ch. Darwin, Esq.,

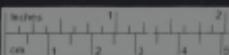
Care of Mr. Mitchell,

Postman,

per Railway. Bromley, Kent

Pray thank Mr. Croker. Also thank Mr. Fitch (and ask him to keep memorandum). I will not have the outline which does capitally, done on wood yet, for I have been very unfortunate in my experimental plants dying, and I many not come to any sure conclusion, and in this case should not publish.

Nevertheless, if some *P. farinosa* sends forth long-stamened flowers, I should be infinitely obliged for 2 or 3 flowers of each kind to examine folded up in 2 or 3 green leaves and sent by Post. Thanks for references about Nectaries.



- 2 -

work you told me of before, and I bought  
and have read it.

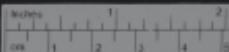
You will now be very busy with your Lecture,

With many thanks,

Yours truly,

Ch. DARWIN





10.8

Down,

Bromley, Kent

27th May

My dear Sir,

Will you show this note to Mr. Croker and ask him if he is inclined to try a little experiment for me.

~~Vinca major~~ I believe never seeds in England and is said never to seed in Germany; and I find pollen so placed it could never get on stigma without insect aid.

I have a pot of Vinca major and I have pressed fine bristles between anthers (not cutting or touching the flowers) in same way as proboscis of moth would press the nectary, near the sides of the corolla; pollen sticks to bristle and a bristle thus covered from pollen of one flower is used for another flower. I did this, and I have 4 or 5 fine pods <sup>vessel</sup> swelling, whereas every other seed-spel shank off soon after corolla dropped.

Now will Mr. Croker do this (and it will not take him 2 minutes) for any exotic Vincas, which naturally have never seeded; mark the  $\frac{1}{2}$  dozen flowers thus treated, and hereafter let me know whether pods swelled and whether seed was obtained.

My Apocynum (or fly-catcher) is coming up; I thought it was as dead as mutton; I have been trying Vinca in preparation for this plant.



- 2 -

You once told me that Mr. Croker liked experiments; if so, I wish he would insert fine brush, like a Bee, into a few exotic Polygalas which do not naturally seed and mark flowers and see if pods swell; I suspect they require insect agency. By the way you have not sent me *P. farinosa*; I daresay you have been so busy that you forgot it.

I beg and pray you not merely to acknowledge this.

Yours most sincerely,

C. DARWIN

I am surprised to find cowslips utterly sterile without aid.—  
Bears on origin of Oxlips.

+ JL rates us 27 very wh. would  
be full early. he feels puts a  
note on D's letter that the flowers  
have not yet, two-

inches		1		2
cm	1	2	3	4

109.

down,

Bromley, Kent

11th September

My dear Sir,

Dr. Hooker told me that you would be so kind as to observe rate of closing of leaf of some Australian Dossera, if it does close its leaf over flies.

There are two other points on which I should be infinitely obliged for a little information. Lindley speaks on the closing of the leaf of Dossera lunata of India, in a manner which makes me suppose that it closes in a more marked manner than our Dossera; I see Stendel in his Nomenclator refers to Buchanan and Night; would it give you much trouble to refer to these Authors, and see whether they say anything on the subject.

*lively*  
Lastly would you look at the Bioncea (if you have tiny specimen), and observe whether the hairs are viscid, for it almost passes my belief that the leaf can snap so quick as to catch a fly, unless it be in some degree entangled: when a fly is caught it is said to be bathed with a secretion, as in case of our Dossera. Please forgive me troubling you so much and believe me,

My dear Sir,  
Yours very faithfully,  
C. DARWIN

inches	1	2
cm	1	2

P.S. I believe one of your gardeners artistically stirred up pollen in the indusium of *Leschenaultia formosa* at my request. Will you ask whether it has made pods. My plant has made pods on those flowers which I thus treated. Whether my seeds will ripen I doubt, but I can see seeds through walls of pod.

P.S. I have written to Gardeners Chron. to ask for authority of movement of leaves of *D. lunata*.

You will think it incredible, but it is a fact that generally 1/1920th part of single grain of Nitrate of Ammonia (Nitrogen in both base and acid) causes decided movement of leaf. Even 1/2400th of a single grain sometimes causes movement, and I am myself convinced (though I do not expect anyone to believe without outer evidence) that 1/2880th of a grain occasionally produces an effect on very sensitive and young leaves. Are not these facts curious, i.e. if you can swallow them. Believe me that they are not stated without numerous and carefully repeated experiments.

+ *Leschenaultia* is a member of the Goodniaceae (not Campanulaceae) & the stigma carries a pouch (the "indusium") into which the anthers discharge their pollen before the opening of the flower. But its subsequent contact & clings to form the indusium squeezes out the pollen so that visiting insects remove it - cf. Syber. Bot. Lett. Mengenfassaden 14 Son. IV Teil 5, p 573. Atta C. barbata, Gard. chron. 1871, p. 116.

inches		1		2
cm	1	2	3	4

6.90

Down,

Bromley, Kent

15th

? May

My dear Sir,

I am very much obliged for your information.  
I will look to *Annales des Sciences*, and have ordered the German book.

With respect to the Australian *Dossera*, the rate or quickness of closing is the point, on which I am very anxious to have a few observations. I shall be surprised, judging from my observations, if the Australian *Dossera* can distinguish dry organic and inorganic substances. I found that closed equally (or nearly so) over any substance, but they released vegetable or inorganic substances much more quickly than flies or meat.

The most curious results which I have arrived at, is the recognition by the leaves of fluids containing nitrogen and not containing nitrogen.

This power of detecting nitrogen in fluids seems to me quite remarkable.

Findley does not refer to Buchanan or Wight, but he speaks in Vegetable Kingdom p. 433 bottom of the species of the Indian *Dossera lunata*; and I see in Stendel's Nomenclator the names of Buchanan and Wight in relation to this species.

inches		1		2
cm	1	2	3	4

- 2 -

I am surprised, if my instruction were  
*Leuchtmaltheit*  
followed by Croker the <sup>not</sup> ~~not~~ made pods.

I am sorry to hear that the plants in the  
"isosceles bed" have not seeded better. All experiments  
require a tantalising amount of patience.

With sincere thanks for all your kindness,  
Believe me,

My dear Sir,

Yours sincerely,

C. DARWIN

It might be worth while, if you like the job, to put a small  
drop of milk and of saliva on a leaf of the Australian  
*Dossera*. Our Dossera likes milk better than any other drink.

inches		1		2	
cm	1	2	3	4	5

1560

50 "

Down,

Bromley, Kent

21st.

My dear Sir,

I am infinitely obliged for your note. The first thought which occurred to me was endosmose (and I tried milk oil and wine). I have tried so many experiments that I cannot think I am in error. I rely much in the case of the saliva experiments on the diminishing effect of Weaker and weaker doses. Sulphate and Nitrate of Ammonia, Nitrate of Potash and N. of Lime produce none or hardly any effect. With respect to the Vegetable fluids, I rely on nitrogenous fluid placed on the same leaves subsequently causing contraction, whereas the vegetable matters produced no effect.

Nothing ought to be placed under the leaf as a support, for I have ascertained (in our Dossera) that irritation on the smooth under side of leaf causes contraction.

So small a drop ought to be put on as not to entangle the marginal hairs, as the act of drying of a viscid fluid mechanically draws the hairs together.

Are you sure that there is no glue with your gum? I tried pure white gum and it produced no action at all. Nor did syrup of white sugar or starch. For Heaven's sake try



- 2 -

some pure gum.

You have given me a panic. I tried  
milk on 9 leaves....9      }  
thin Solution on 4..4      }  
White of egg on 6...6      }  
Saliva on 8 .....8      }  
Urine on 11.....11      }  
Mucus on 4 .....4      }  
infusion of meat on 1      }

and all strongly  
contracted on  
different plants

43 leaves.

I have not tried vegetable fluid so largely -  
only 3 or 4 leaves for each; for absolutely no effect was  
produced.

Believe me that I am truly grateful for your  
cautions.

In haste,

Yours very sincerely,

C. DARWIN

I start for sea-side to-morrow, but letters will be forwarded  
to me.

inches	1	2
cm	1	2

P.S. Does not the sourness of your gum prove that it has  
fermented? And does not fermentation imply that there  
 has been some nitrogenised matter in it? I am nearly sure  
 that yeast is nitrogenised. Most cryptogamic plants  
 certainly contain nitrogen. *for Heaven's sake dissolve*  
 fresh pure, white gum and try again.

The inflection of disc of leaf itself, when  
 it occurs, is the best of all evidence.

I fully believe I have now ascertained that  
 $\frac{1}{2880}$  of grain of N. of Ammonia occasionally suffices with  
 young and very sensitive leaf, so *that scrupulous care*  
*is required*

inches		1		2	
cm	1	2	3	4	5

6.12.

1860

15 Marine Parade,  
Eastbourne

My dear Sir,

This is my address. I will write soon.

I am grateful for your last note.

I am dying to hear about your gum.

Yours sincerely,

C. DARWIN

inches		1		2	
cm	1	2	3	4	5

(6-13)

1860

15 Marine Parade

Eastbourne

per Brighton Railway

Wednesday

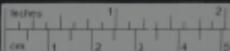
My dear Sir,

The morning post goes out immediately.  
Should like to give my address for Plants. Shall be  
delighted to try one or two experiments on *D. spectabilis*  
Plants could come with Dionaea.

Will write again more leisurely.

Ever yours,

C. DARWIN



13.14

15 Marine Parade,

Eastbourne

1860

5th October

Ans.

My dear Sir,

I thank you for two notes. It was a capital thought your sending me the gum itself (so ingeniously bottled or quilled); I tried it on 3 leaves, and it produced no effect; and I tried thicker gum on 5 other leaves with no effect, and I subsequently found that these leaves were good ones. If a leaf is feeble and does not secrete copiously, the gum dries and draws together all the hairs which it has touched; and this, I imagine, must have been cause of the apparent inflection in gum case. At last I have come to a puzzler, for I find Carb. of Soda causes inflection; but I have sent to London for pure C. of Soda, as that sold by Druggists is not pure. But I strongly suspect I have come across a poser. I am, however, trying my experiments in another fashion, which may throw light on the subject. I find the glands at end of Hairs are absorbers as well as secretaries. The change which takes place in the Hairs after inflection is very curious. The currents and movements in the cells strike me in my ignorance as marvellous.

You are very kind in your second note to say that I must not apologise for all the trouble which I have

inches											
cm											

- 2 -

caused; but pray thank Mr. Croker for enquiring about the Dionaea. Also please give my best thanks to Sir William for his wish to oblige me; I shall be intensely anxious to examine the leaves, if I can't get a plant.

If the Dionaea ~~commonly~~ catches only small fry, I should not be surprised at a fat fly being too much for its digestion; at least I have found bits of raw meat often, indeed I think generally, kill the leaf of the ~~Dossera~~. I should like to hear whether Croker is pretty certain of this fact.

I am very glad to hear that you are experimenting on the leaves in water. I have long thought that naturalists make far too few experiments.

My dear Sir,

Yours very sincerely ,

C. DARWIN

X Do you possess a copy of my "Origin of Species"; if you do not, I should much like to have the pleasure of sending you one.

X He did send it - a 1<sup>st</sup> edn, with a letter of dedication - Eventually it found its way onto the shelves of my Bot. Library at University College. When I left U.C. in 1929 (I asked all members of the Staff to choose one thing which I had given them) Miss Violet Agnew (now Mrs de Caen) took the "Origin" - a few years later I saw a 1<sup>st</sup> edn of the book quoted in a book bid at £50!

Now by 7.40 -

inches		1		2
cm	1	2	3	4

(10.12)

15 Marine Parade,

Eastbourne

Saturday

1860

My dear Sir,

I really thought that I should give no more trouble. But I have strongest wish to observe <sup>the</sup> ~~stem~~ of Dionaea, now that I know ~~Dossera~~ so well. Is it a very precious plant? Could a living plant be packed so as to come here by Railway, and could I keep it alive for week or two in sitting room? If so, would you be so kind as to give me address of my nurseryman whom I could purchase a plant. Or if there are several plants at Kew, would you read this note to Sir William and ask him whether he could lend me a plant, which should be returned (carriage free) to Kew; but I should require <sup>to take</sup> and dissect some leaves. I want to compare structure of hairs of Dionaea and ~~Dossera~~, and especially to see whence the fluid is secreted in Dionaea, which is said to bathe the flies caught by it. The fluid in ~~Dossera~~ is acid, and has curious anti-septic powers on meat.

Pray forgive me. I would just as soon purchase as borrow: I only mention Kew in case the plant is not to be easily purchased, and in case of there being several plants at Kew. How I should like to see a fly caught by it!

Yours very sincerely,

C. DARWIN

inches		1		2
cm	1	2	3	4

6.16

15 Marine Parade,

Eastbourne

12th ~~Friday~~ 1880

My dear Sir,

The Dossena and leaves of Dionaea arrived quite safe. I am very much interested by them. Pray give my best thanks to Sir William, and accept them yourself.

I am very busy and get my constant ~~very~~ headache; so will not write any more, but I have got a good deal to say first day I can.

Will you try one easy experiment: Drop on sensitive Hair of Dionaea one drop gently of pure water; and get solution of Carbonate of Ammonia six gr. to two oz. of distilled water (Perhaps safer to get four gr. to 2 oz. of water) and drop this, and see if it immediately or within hour or two causes movement. This will not injure leaf. Also one drop of the C. of Ammonia on disc of leaf. I want so much to know whether movement analogous of Dionaea and Dossena.

Yours very truly,

C. DARWIN



617

15 Marina Parade,

Eastbourne

14th October 1860

My dear Sir,

I am much obliged to you for so kindly telling me about the Australian Orchids (a subject which interests me greatly, and I have now examined nearly all the British kinds); but I cannot quite understand the description, and without examining the like plants, with reference to visits of insects, I believe their means of fertilization can never be understood. Even Hooker was led into considerable error, not of facts, but of purpose in his curious description of *Listera*.

I want to consult you on one point, you have so much knowledge: I have consulted Professor Henslow, but he knows nothing like it. - The elongated cells in the hairs of *Silfera* are filled, when expanded, with homogeneous pink fluid: after inflection this fluid always separates into colourless fluid and into thick, viscid, dark red fluid. This latter fluid undergoes a slow, never ceasing, endless changes of form. The enclosed diagrams (1,2,3, etc) represent the same cell full of colourless fluid, with the shaded dark red viscid matter: the outlines were done at the interval of about  $1\frac{1}{2}$  minutes, and you will see the changes of form and position of

inches	1	2
cm	1	2

- 2 -

the two original masses; they divide, coalesce, leave threads behind them; these threads change into necklaces, and one or more of the beads swell and enlarge, then often divide, coalesce and so on ad infinitum. I have never seen the same changes twice, they are infinitely varied. It is not due to ~~endurance~~, for it takes place in Hair cut off and placed dry between two slips of glass. Has anything like this been described? A very weak solution of C. of Ammonia instantly sets the process at work.

I hope that you will like to hear a little about *Dionaea*, - the examination of which I have much enjoyed owing to your <sup>Knowledge</sup> ~~knowledge~~. I have made out but very little. The sensitive Hairs have no gland or spiral ~~sepal~~ <sup>vesicle</sup> or stomata, so differ greatly from the Hairs of *Dossera*; but I have hardly any doubt that the spirally arranged cells are before the contraction of the leaf filled with homogeneous red fluid; for many cells were in the contracted specimens sent by you thus filled, and I saw them <sup>some</sup> turn, but very slow, movements, as in *Dossera*, and the red viscid matter was much excited by C. of Ammonia. This seems an important point of agreement with *Dossera*. On upper or inside of leaf, the whole surface is studded with <sup>minute</sup> projections on footstalks, just like the back of a tortoise; and with cells or compartments, they <sup>are</sup> pink fluid, which with C. of Ammonia acts as this matter in cells of

inches	1	2
cm	1	2

Dossera. From analogy of Dossera I infer that these tortoises are the secreting and absorbing glands. One thing surprises me, viz: under each footstalk of the tortoise-shells there is what appears a stoma (1). These tortoises do not occur on insides of marginal spikes. On back of spikes and on back of leaf, there are many petaloid or star-like little projections on footstalks (2) and beneath these there are apparently stomata. The brown matter within these stars is not acted on by C. of Ammonia. The marginal spikes have spiral <sup>veins</sup> ~~seals~~; and I suspect they may be looked at as the extreme marginal hairs of Dossera in a functionless condition.

Now these few observations would lead me to try the following experiments, if I had living plants; but I beg you not to try, unless you feel some little interest on the subject.

Firstly and chiefly, to gather one more leaf very quietly (or look at growing leaf) put it under simple microscope and observe whether the sensitive Hairs are uniformly coloured; then touch Hair with needle, and observe whether the colour does not become very soon (how soon?) mottled from the action of segregation as described, of red viscid matter from red fluid.

Drop weak C. of Ammonia on inside of <sup>marginal</sup> spikes, on disc of leaf, on back of leaf, and on sensitive Hairs, but in

inches	1	2
cm	1	2

- 4 -

latter case so small a drop as not to run down on to the disc, if this be possible. These experiments could tell much.

First try disc and sensitive Hair; if these do not act, other ~~trial words~~ tends to be useless.

In how many seconds or minutes does leaf close?

Lastly, can you tell me whether the stomata are ever covered by reticulated cells?

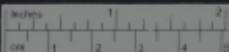
Have stomata and spiral ~~spiral~~ <sup>spiral</sup> any connection? or relation?

I have as yet tried out two experiments on D. spatulate

:-

At 9<sup>AM</sup>, we put on one leaf  $\frac{1}{2}$  minim of solution of Nitrate of Ammonia (3 gr. to 1 oz., so that I gave the leaf  $\frac{1}{320}$  of a grain which is much too strong), in 6 hours, hairs ~~were~~ increased; in 7 hours met; in 9 hours edges of the leaf ~~were~~ <sup>had</sup> much increased, so the leaf became semi-cylindrical. ~~Gum~~ has produced no effect.

I am in complete muddle about effect of various salts; and I am deeply indebted to you (in main part) for having made me try more experiments, God forgive me for having written you so long a letter. You will soon find out that I can be troublesome with a vengeance.



- 5 -

With cordial thanks,

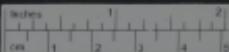
Yours very truly,

C. DARWIN

(It has been ridiculous in me, from my ignorance, taking up this subject; but it was purely accidental)

Do please touch the sensitive Hair; though my fear is whether with simple microscope you could perceive change from uniform to broken colour: yet I felt sure the colour was broken when I first saw - through simple lens - the Hairs. I expect the change to be almost instantaneous. It would be very curious if disc alone was sensitive to Ammonia, and the Hairs ~~out~~ to touch, but Heaven knows how it would be.  
*only*

Perhaps action of C. of Ammonia on the disc would not be rapid (as from a touch on Hair).



b R

15 Marine Parade,

Eastbourne

17th October

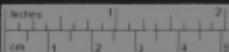
1860

My dear Sir,

I am infinitely obliged for your mass of interesting facts and extracts. The only at all full account which I have seen of Dionaea is by Mr. Curtius, quoted by author (Lindley?) in Penny Encyclopaedia. Lindley also refers to Dionaea having been fed and flourished on chopped meat. I think you could see with lens on growing plant whether colour in Sensitive Hairs became broken after being touched. That experiment and effect of C. of Ammonia on disc would be very interesting. A good dissector, I daresay could follow the cells with red matter from base of sensitive Hair, and thus follow its ~~near~~ (3)

Many thanks about stomata; it was uncommonly stupid in me not to think of your explanation, which I have hardly a doubt is right; for it was after removing the tortoise-like glands, which are mounted on very short footstalks, that I saw the effervescence of stomata: I have hardly a doubt that my so-called stomata are 2 cells of the footstalk.

Thanks for your various analogies and comparisons  
+ my father was a poor microscopic naturalist.  
J.W.D.



- 2 -

about the missing red matter; it is beyond me. I shall just publish what I saw.

I think that you have misunderstood me in supposing that this effervescence follows only from C. of Ammonia. It is better seen in the Hairs which have naturally contracted over a fly or other object; and this it is, which seems to me to make the case curious. I have been ascertaining this morning how quickly C. of Ammonia acts; and certainly 13 seconds suffices for the absorption and for a marked change in *Thickness* in the glands; in one minute the action reaches the upper part of footstalk. No other substance (such as acetate, oxalate, nitrate of Ammonia etc. etc.) acts nearly so quickly, though they do act after some time; the acetate of Ammonia is next in quickness. I suspect the milk, urine etc. and these salts do not act until they have become decomposed and yield C. of Ammonia. I cannot avoid suspecting that we see in the action of these substances on the leaves of *Dossera* what chemists believe take place - with organic manures on the roots of other plants. I am going to try to-morrow C. of Ammonia on the root of *Dossera* and on some other common plant.

Thanks about *Urtica*; and for *Gothins* plants. Please thank Croker for all great trouble which he has taken.

I have told Murray to send you copy of my

inches	1	2
cm	1	2

- 2 -

*Journal*  
~~foreword~~, which I am very glad you did not possess.

Yours very truly,

C. DARWIN

The day after to-morrow I shall begin and draw up my paper on ~~Dossera~~; for I have wasted a shameful lot of time on it, - and yours also.

The leaves of D. phalanthus will not open.

P.S. Will you be so kind as to dry a little of your gum used on ~~Dossera~~ and burn it; perhaps you could detect smell of animal matter if any gelatine or glue used, or size used. Any conserve left over??

Could you send me, when you write, one leaf (and name) of Australian ~~Dossera~~, that I may see it out of idle curiosity.



6.19.

15 Marine Parade, 1860

Eastbourne

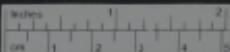
Wednesday

My dear Sir,

Just received your letter, which I have hardly read. I write before morning post, to say, that I did not suppose you would care to look at the movement on Dospera; (I am very glad), else I would have told best way, viz: to take Hairs lately inflected over a fly, cut off, and put either dry or in water between glass, under high power (1/4 object) make a sketch of shape of red matter, and look again in one or two minutes. There is, also, sometimes a strong circulation in the cells. I refer to cells of footstalk of round-headed marginal Hairs.

Sincerely yours,

C. DARNIN



10 40.

15 Marine Parade,

Eastbourne

20th October

1860

My dear Sir,

We return to "Down, Bromley, Kent" on 26th.

I have heard from Huxley about the Nat. Hist. Review, and it has my good wishes and I shall certainly take it in. I really know nothing whatever about vegetables ~~its~~ <sup>but</sup> itability (it is quite beyond my scope) except in case of Orchid. I have a large mass of notes with many new facts, but I resolutely, against my inclination, put them away a month ago with the determination to work them up and get drawings made at some distant period; for I am convinced that I ought to work on Variation and not amuse myself with interludes.

Dossera was an accident owing to my being away from home and having nothing to do. My paper on Dossera, from containing minute particulars of numerous experiments, would not, I think be fitted for the Review; and if I do not deceive myself some of the results are sufficiently curious to be published in some old standing Transaction. Where I shall send them I know not yet; as I cannot judge of evidence till all my experiments are tabulated and that will

inches		1	2		1	2
cm	5	10	15	20	30	35

- 2 -

be a long job.

I may add that I told Huxley some months ago that I really could not assist by writing. I may add that my health is so weak that I cannot above 3 hours a day; and I am at all times a most slow worker.

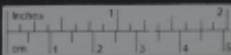
I am so much obliged to you for going to try C. of Ammonia on Dionaea. I should expect (but expectations are oftener wrong than right) that (action) of C. of Ammonia on disc of leaf would cause it to shut slowly; but would produce no effect on back of leaf or on marginal spikes. What it will do on the sensitive lamina, Degyen only knows. I should expect after leaf has closed that each of the little tortoise-shell glands on the disc of leaf would secrete a minute bead of liquor. I must have expressed myself badly, - it is the red fluid being broken up or segregated within the sensitive lamina of a closed leaf, as in Doskera, which seems to me the important point of accordance; and not so much the action of C. of Ammonia on the red fluid in the sensitive glands. I will try, and should not be in the least surprised at C. of Ammonia acting on the fluid in cells of common leaves.

My dear Sir,

With many thanks,

Yours very sincerely,

C. DARWIN



1621.

15 Marine Parade,

Eastbourne

Sund. 23rd. [out] 866

My dear Mr. Oliver,

What wealth of knowledge you have! But I have hardly read the extract about *Drosophila*, as we are in much distress, my poor eldest daughter has had a relapse. We cannot return on 26th to Down.

Excuse more at present.

Yours very sincerely,

C. DARWIN

inches		1		2
cm	1	2	3	4

10.22.

16 Marine Parade  
Downs,  
Swanley, Kent  
24th, Monday Oct. 16<sup>th</sup>

My dear Sir,

I should think you could make an interesting paper about Spring trees. I had a vague notion about plants varying by becoming more spinose in dry and hot climate, but I never put two and two together, as you have done, in relation to the species of such countries being spinose. This is a point after my own heart.

It is quite new to me about species of far removed orders being spinose in deserts. The labour would be, I suppose great; but I can hardly doubt that you might make a very interesting paper on the subject. The simple fact of bushes and trees being in so marked a manner spines in deserts alone struck me, from what I have myself seen and read. Livingstone for instance, was much struck with the contrast in this respect between the plains of the Southern parts of Africa and the more humid and intra-tropical parts. The explanation of that fact, seemed to me to be probably, that where vegetation was scanty, those plants alone could withstand the injury from browsing quadrupeds, which were protected by spines. Even our gorse is an instance of this, as being when bruised and chopped so eminently liked by horses. I have fancied that desert plants

inches	1	2
cm	1	2

- 2 -

were often strangely aromatic or strong-tasted for a similar purpose, viz: protection. This struck me much on stony mountains of Chile.

You allude to another interesting point, about unisexual plants having very fragrant and conspicuous plants. This would be a very curious point, but I should suppose very difficult to determine, as frequency is so indefinite. I have imagined or seen stated, that night-flowering flowers are often fragrant to attract insects, and often white. By your excluding orders in which all the species are unisexual you exclude in fact one source of doubt, viz: those cases, when wind is sole agent of fertilisation. I have just thought of one strong exception - the Common Holly, which is not fragrant or has conspicuous flowers, and is yet essentially unisexual. Bees can certainly smell any sweet excretion. I fear there would be too many elements of doubt.

I am not Botanical enough to follow out your ideas about definite and indefinite inflorescence. I am not sure that I mentioned "definite" and "indefinite".

I have long thought that dimorphous flowers (and told Dr. Hooker so) would be a very interesting subject for experiment. I once got the Russion violet to commence on; but want of time and my poor health prevented me doing anything

inches	1	2
cm	1	2

-3-

This very summer I have had ~~benutzt~~ ~~das~~ glass under cover partly with this object, but the wet season spoiled this and several other little experiments. I shall never take it up now. The view which I meant to test was this: that the apetalous forms were self-fertilised, and therefore seeded abundantly; whilst the flowering kind was alone visited by insects, and either required (as in case of violets) and I believe all Campanulas) and was much benefitted by the visits of insects for its self-fertilisation; and thus incidentally received the benefit of an occasional cross. This year I tried ~~the~~ tricolor, and it was marvellous the difference in seeding of the flowers, which were visited by Bees, or artificially fertilised by me, and those which were untouched.

I think that you would find this an easy and interesting line of experiment.

Very many thanks for your interesting letter.

Yours sincerely,

C. DARWIN

There is an N. American Campanula with nearly apetalous flower which seeds largely. How does pollen get on stigma?

inches		1		2
cm	1	2	3	4

16-23

15 Marine Parade,

Eastbourne

Thursday, 27th 1876

Oct

My dear Sir,

Your note of 25th about the gum has been a great relief to me, for I took a panic, as I know I am very apt to blunder and run away with things. After writing to you I looked through my notes, and found I had been rather more careful than I thought, for I had tried 27 leaves with non-nitrogenised substances, not counting saline solutions and simple water. The more I reflect on the experiments which I have tried, the less I think I am mistaken. Thus I fully expected that 1 gr. of gelatine to oz. of distilled water would have affected the leaves, but it produced no effect, whereas a little stronger solution produced a marked effect, and whereas 1 gr. to 1 oz. of several salts with nitrogen produced strong effect. Thick syrup on 5 growing leaves produced no effect, whereas a leaf gathered and put into the same solution suffused in extraordinary degree from, exosmose and the hairs and disc of leaf collapsed; so that the vital power seems to resist exosmose in the living plant. I think I have made out the simple mechanism of movement. But why I trouble you with all these details I know not.

I will give you no more trouble, except that I

inches		1		2
cm	1	2	3	4

- 2 -

earnestly hope you will try again the old gum, and if it acts, endeavour to find out certainly its composition. Thank you for the pretty leaf of the Australian *Dossera*; and for paper with gum; but I have a horrid cold, and must stop to another day to try it: a large drop or  $\frac{1}{2}$  spoon-full dried and then put on heated knife is proper way to try.

M. Toccol (I am so much obliged to you for telling me of that paper) disbelieves in any movement and accounts for flies being caught by their crawling under the young curved hairs! But he kept the plant in a greenhouse or hothouse. Could this have paralized them? I suppose not; anyhow, hot sunshine seems to make them act better. I imagine he looked out for sudden movement.

Vapour of Chloroform for 30 seconds paralyses them completely.

Thank you much for the unnamed Australian species. I will with permission quote your observations. Neither *D. Spiculata* or the other species <sup>or *D. longifolia*</sup> seem to make ~~detache~~ quicker than *D. rotundifolia*.

I am very glad to hear that you intend to collect ~~spring~~ plants. Your note shows that it is a complex problem.

With many thanks, My dear Sir,

Yours sincerely,

C. DARWIN

inches		1		2
cm	1	2	3	4

I have just received a cargo by post of living plants of  
*Dossera* from Down, and I will try more gum and starch myself.

In the unnamed Australian *Dossera* the <sup>incarnation</sup> of the leaf itself was terminal I suppose by your sketch. In *D. longifolia* sometimes the <sup>incarnation</sup> is terminal, sometimes lateral, and sometimes termino-lateral, so is to be quite variable: as far as I saw in *D. longifolia* it was always terminal.

inches		1		2
cm	1	2	3	4

10-24.

15 Marine Parade,  
Eastbourne  
3rd November 1860

My dear Mr. Oliver,

My daughter has escaped the imminent danger she was in for a week and our minds are now tolerably at ease; and we hope to get home in a week or ten days, if she progresses favourably.

I shall be anxious to hear the result of the election for the Professorship. I presume your time will have been fully occupied in this important matter, and you will not have had time for any experiments on *Dianaea* with the c. of Ammonia; nor had time to work at the moving red matter in the Hairs of *Doskewa*. I am so greatly obliged to you for taking so much trouble to copy the extract from Bot. Zeitung. I presume there is a paper in extenso. Is this so? The author has missed the point which seems to me most interesting, viz: that though the Hairs close equally (or nearly so) over animal, vegetable and inorganic dry substances; they remain closed much longer over animal than over other substances. This fact led me to try the ~~nitrogenous~~ and non-nitrogenous fluids, for somehow the plant knows well when it has caught good food. You were right in anticipating (and I believe you did) that C. of Ammonia acts on the fluid in cells of other plants, as in case of <sup>Doskewa</sup> *Dianaea*: I find it so

inches		1		2	
cm	1	2	3	4	5

- 2 -

with Lenna and almost as quickly with the roots of a little Euphulia, as with the hairs of ~~L~~esseria.

I have asked Daubeny whether the action of C. of Amm. on the sap of plants was known; and he says he does not remember having heard of it.

Yours very sincerely,

C. DARWIN

P.S. There is something which looks odd to me in the roots of the Duck-weed; a new root seems formed within the old one; at least an old outer cylinder is cast off in pieces; but it is out of my line.



1625.

15 Marine Parade,

Eastbourne

7th November. 1880

(We leave here I hope & think on Saturday)

My dear Mr. Oliver,

I am heartily glad to hear that the Professorship is properly decided.

I have been glad to see the slide, but I am sorry to say the glass arrived cracked. I do not think that the structure stomata is so plain as when I made fresh slices. I shall be some time curious to know what you see so remarkable in the sensitive Hairs. I hope some day you will study the Dionaea and publish on it. I shall make only a few remarks in comparison with Drucearia. I can see nothing in sensitive Hairs more than I saw before, viz: elongated cells arranged spirally with a tipped point, with no spiral vein or stomata. There is a short footstalk making change of structure in cells and that is all I can see! I conjecture that the sensitive Hairs are homologous with the tortoise-shells or buttons, in a much elongated condition. I cannot help fancying that by holding footstalks of living leaf and retarding the movement by your thumb, with good lens, you could see whether the sensitive Hairs became mottled in colour by touching it with needle.

inches		1		2
cm	1	2	3	4

- 2 -

(I can tell with lens whether Hair of *Dionaea* are mottled)  
 It ought to do so!! I shall be intensely curious to hear  
 about C. of Ammonia and Dionaea. I have tried several salts  
 on the Hairs and Roots of *Dionaea* and on other plants. But  
 I know well I have not tried enough; nor intend to do so, for  
 the work is quite out of my line; and though I have enjoyed  
 it very much, I have been exceedingly foolish to attempt it.  
 I still think the action of the Carbonate and some other salts  
 of Ammonia remarkable, and worth anyone's following out. I  
 shall briefly give the facts in my paper. I put all the roots  
 of a little plant of Euphorbia in water with  $\frac{1}{2}$  of grain of C.  
 of Ammonia, and it was curious how it wholly altered the appear-  
 ance every cell in all the roots. The action is so rapid,  
 that you cannot put the root under the microscope, before clouds  
 of granules have shot some way up the roots.

Now I am going to beg a little favour of you. Does  
 not Mr. Fitch work at Kew? And will he draw for anyone for  
 payment? If he will be so good as to make 2 or 3 drawings  
 for me, I should be very much obliged, and I could repay by  
 P. order. First, a leaf of Dionaea with all the Hairs fully  
 expanded. It is for woodcut for any reader who is not a  
 Botanist. It ought to be 3 or 4 times natural size. Mr. F.  
 could judge this. It ought to be done neatly. The extreme  
 marginal Hairs are long-headed; whether on this scale the  
 difference could be represented, I know not.

inches		1		2	
cm	1	2	3	4	5

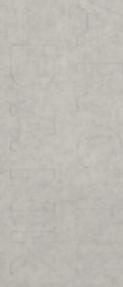
- 2 -

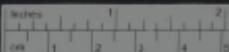
Secondly, would you put little fly on exact middle of leaf, and after Hairs are well clasped get a drawing made on same scale. Thirdly, perhaps, sketch of leaf of Dionaea, just to interest Readers. I shall have copied from *Tricul* the minute structure of Hairs.

My dear Sir,

Yours very sincerely,

C. DARWIN





10.26.

Down,

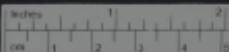
Bromley, Kent

16th November

My dear Mr. Oliver,

In writing out my paper yesterday I was so astounded at my results that I have got fairly frightened, and have determined to finish my paper, but to publish nothing until next summer. I shall have selected my results and tried some of them in another way; so that there is no hurry about the Drawings. If there are good specimens, I should be glad for Mr. Fitch to complete them; otherwise will you ask him to wait till next spring or summer.

Most heartily do I thank you for your most kind and valuable assistance. If I could get one or two plants of *Dionaea* I would experimentise on them, but I shall not of course attempt the anatomy; and if you thought you could undertake the subject I would not interfere in any way, except by bare allusions to what I have seen; but it is so important to me about the mottling on segregation of colour, that I should extremely much wish to ascertain that point, if in the spring, by the acids of Mr. Crcker I could purchase (at almost any price) one or two or three of the young plants which he saw.



- 2 -

I have been corresponding with some of the chemical physiologists, and as far as I can find out the curious action of the C. of Am monia on the roots of plants has not been observed.

With cordial thanks for all your kindness,

Yours very sincerely,

C. DARWIN

P.S. Can you tell me name of Plant, which grew out of doors in my Father's garden 2 to 4 ft. high, considerably branched, died down (I think) in winter, bore many minute almost white or very pale pink flowers, and which flowers caught a multitude of flies by their proboscis. I should like to get a plant to try to make out final cause of the catching.

P.S.2 Perhaps you would like to hear following extraordinary fact: I have placed over and over again minute atoms of paper, stick, cinders, meat, flies, etc. on glands of single hair of ~~brown~~ and they always became inflected; so I thought I would test how minute an atom would cause movement: I measured with micrometer several bits of woman's hair (and thread) which caused movement, and found that under 1/50th of inch amply sufficed. Of same hair I sent 6 inches to be weighed in London by best balance, and it weighed under 1/100th of grain, which shows that 1/50,000th of a grain suffices!! Prolonged pressure alone causes movement.

inches	1	2
cm	1	2

16.27  
See 16 Nov 1870

Undated

My dear Mr. Oliver,

Thank you for your note. I do not think the plant can be an *Asclepias*; as far as my memory serves the flowers were very simple, and I think the proboscis was caught between stamens and pistil. The minute white or very pale pink flowers were not in trusses or umbels. I have seen the plant fairly studded with captured flies. My Father called it the Fly-catcher. It died down in winter. The leaves were narrow. Stem thin; much branched smooth and I think slightly succulent.

You did send me the curious account of the *Asclepias* which surprises me much, considering R. Brown's paper on importance of insects in its fertilisation. The Conference seems a good suggestion.

Yours very sincerely,

C. DARWIN

In the Fly-catcher the orifice of corolla was small. It is odd that such a plant should not be in Kew. It lived 30 years ago for very many years in my Father's flower-garden.  
Will you ask Sir William if he can recognise my vague account?

+ The most famous insect-catching flower is that of *Araujia*, an asclepiad. In, e.g. S. Africa, it becomes covered with them, caught by their proboscis in the slots of the "columns".

F.D.

inches	1	2
cm	1	2

18-28

Down,

Bromley, Kent

20th December

My dear Mr. Oliver,

If you have Rafinesque's New Flora of N. America, will you kindly look and tell me the date of Part First. Drumond of ~~Seven Rivers~~ has sent me seeds of a <sup>Compos.</sup> Plant. Styloclerus Hanifurus of Labillard. As, after looking at them, I shall not want them, I have thought I would just mention that I had them, in case they should be of any use at Kew; not that I suppose they would.

He has sent me seeds of Distylis which I shall plant. You sent me a Godenia, also, so I shall have plenty of this order now.

I have been pleased at a prophecy which I made to myself coming true; viz: that Bees would open the ~~inflorescence~~ and get out the pollen and thus accidentally carry it from flower to flower, for Drumond writes that he watches a small Bee busily employed in extracting the pollen out of the ~~fact~~ <sup>inflorescence</sup> of a Brunonia. This ~~fact~~, would I expect, puzzle our European Bees

Yours very truly,

C. DARWIN

+ herewith *Angianthus* L. Hoffmann in  
Engl. Bot. Compositae - p. 193.

inches	1	2
cm	1	2

10-29.

Down,

Bromley, Kent  
4th April / 86)

My dear Sir,

Very many thanks for the Primulas ( and <sup>?</sup> common Primrose) which I received this morning and examined. Only one was dimorphic; but I have every reason to suppose that others would have been so, had you possessed a greater number of plants. *P. Siberica*, however, seems to differ from all other species in not being dimorphic.

I have been deeply grieved to hear about Prof.

Henslow.

? death in 1861.

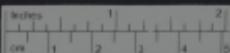
Yours very sincerely,

C. DARWIN

As *P. Siberica* is apparently so exceptional, would you ask Mr. Croker, if you have other plants, which come into flower to send me two or three flowers (not whole ~~Trusses~~) for examination, in order that I may see whether they present any incipient trace of dimorphism.

But if your plants have been increased all by offsets from same original plant, it will be no use to send them, as the character is permanent by offsets.

I do not know whether I shall succeed in making out the meaning of the dimorphism; but I have not been idle, for I have made much above 100 crosses with the pollen - of the



- 2 -

different sizes.

The lot sent has been most interesting to me. Pray thank Mr. Croker.

Do not trouble yourself to write if you can at any time send me other specimens of *P. Siberica*.

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inches		1		2
cm	1	2	3	4

18-36.

Down,

Bromley, Kent

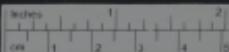
September 11th

My dear Sir,

I was pleased to see your handwriting again, and sincerely hope that you are quite recovered from your long illness. Take warning from me and do not work too hard. Very many thanks for information about *P. Ciliata* and *Var.* I was sure it could not be an ordinary variety; it differed too much and I was utterly perplexed what to do about it. - I should have much liked to have seen *P. Farinosa*, but I well knew that you could not send it else you would have done so.

I hate changing work and I have so many irons in the fire that I have stopped looking after *Dionaea*. I return your nice sketches which make me quite envious. I do not of course mean to attempt any minute work at structure; merely a few functional observations relating to *Drosera* (which alas I have resolved to put off for another year, for I have amused myself too long over *Primulas* and *Orchids*.)

I have made out very little on *Dionaea*; merely that the leaf behaves very differently when a fly, or only bit of cork or nothing is caught. When a fly is caught much acid mucous is poured out as in *Drosera*. And the leaf must absorb so as to perceive when it has caught a fly. The "tortoises" seem to be



- 2 -

the secretors and absorbers and appear to me to be strictly homologous with the glands in *Drosera*. It is odd that meat, flies, sol. of C. of Ammonia will not excite movement or secretion; the sensitive filament must be touched to set all in action. The sensitiveness of the filament is prettily adapted; drop of water falling on them, or strong through pipe produces no effect; but a touch of a woman's hair held with one inch length free will suffice. There is a pretty difference with *Drosera* the latter does not care for a single rough touch with even a needle, but a weight left on the gland ~~the~~ 1/70,000th of a grain will excite movement; and this is good for plant for it has to clasp an insect when resting on and sticking to the gland; it would be lost labour to *Drosera* to close when merely touched by large insect. In *Dicranum* a far lighter single touch by mere hair causes movement; but a much greater weight, if put on delicately, may be left on the filament without exciting movement than with *Drosera*. But I must not run on.

Pray believe me,

Yours very sincerely,

C. DARWIN

inches	1	2	3	4	5
cm	1	2	3	4	5

10-51.

Down,  
Bromley Kent

October 8th

My dear Sir,

I am perfectly ashamed to trouble Hooker again. Would you have the kindness to look at well-opened flower of Stanhopea Saccata and see whether any nectar is in hollowed out base or cup of Labellum or in other species of Stanhopea, if they have ~~exp~~ hollowed out base or cup to Labellum. I have particular reason to enquire; but I daresay the plant will have none, though it ought to have some. But Nature as Agassiz says, does not lie and therefore it must have nectar, else a theory of mine is wrong, which is clearly impossible! I

In haste,

Ever yours,

C. DARWIN

inches		1		2
cm	1	2	3	4

Life & Letters III : 270.  
10-32

Down,

Bromley, Kent

June 8th [1862]

Dear Oliver,

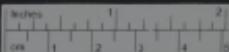
Very many thanks for the orchid which was new to me and interested me, but by Jove I must stop and go on with confounded dull old subjects. The orchid must be a *Gatsetum* (allied to *C. tridentatum*) and has no doubt its own *monocentrum*.

The stigmatic surface was more viscid than in the other species examined by me but not viscid enough to break the caudicles. The utriculi and ovules after spirits showed also very little contained pulpy matter. An examination of the tissue or utriculi of stigmas of utterly sterile hybrids after being kept for 24 or 48 hours in spirits in comparison with the utriculi of the pure and fertile parent species, would be a point worth attention.

But time time time, as you know doubt ~~explain~~ with your lectures, and as I often exclaim with my wretched stomach, though having no lectures or other disturbance.

That is a curious monster which you sent with its two ~~two~~ <sup>two</sup> ~~Pellion~~ anthers and two ~~intellamus~~.

I am glad that you have read my orchis book and seem to approve of it; for I never published anything which I so much doubted whether it was worth publishing and indeed I still doubt. The subject interested me beyond what, I suppose, it is worth.



- 2

Almost every day I get more convinced that insects (in relation to the marriages of distinct flowers) govern the structure of almost every flower; I have been led, from crossing, to look to Pelargoniums, and see how well the seven anthers stand and face, so that an insect visiting the nectary may take them all; and see the open stigmas in an older flower.

Ever yours truly,

C. DARWIN

inches		1		2
cm	1	2	3	4

10-33.

June 1662

Invited

Dear Oliver,

I have been much pleased by the Zeitung für Müller describes exactly what I have seen, except that he overlooks the furry minute indentations of petals. The V. Hirta belongs to the type of V. Odorata. Why I write is that you have marked "Your regards" on copy; but you do not mean me to keep copy? Does it not break set? I will keep it safe, and return it whenever you may have occasion to write.

With many thanks,

Yours very truly,

C. DARWIN

It is a pity that Müller did not know that the perfect flowers are fertile only when visited by bees.

P.S. By the way I must tell you that I had long letter from Asa Gray this morning, approving of my orchis book, and, what is better, comparing what I say with structure of living American allied orchids, and finds as yet all true. This pleases me. He likes the book, incomparably more than I ever ventured to hope.

P.S. What a wonderful scheme of reference you must have, always to know, what and when has been written on any subject.

inches	1	2
cm	1	2

1162

103.

Down,

Bromley, Kent

24th July

Dear Oliver,

My poor Boy rallied last night and the Doctors think he has passed the crisis and is out of danger. I have had a miserable month, and many of my experiments on *bimorphata*, are gone to the dogs.

I am very glad that your Lectures are nearly over, and that you will be a somewhat free man. It must be very hard work. Also Greg seems to be able to do nothing else when he is lecturing. He is now free and has been making some capital observations on orchids. He has got a self-fertilising just like the Bee, with their candle, but still more plainly than the Bee with adaptations for an occasional cross. I think in opposition to you, that some day the Bee *Ophrys* will be explained; I have been speculating, against evidence, that *Arachnites* may be the crossing form and the Bee the self-fertilising form of same species; but it won't do.

Many thanks for your reference to Duchartre. The Vanda must be something curious. By the way I find that the orchid mentioned by me as *Homodes unisexualis* sp. is *Cycnoches ventricosum*; it is hermaphrodite and I wish much to see a dark *Cycnoches*, of which I once received a flower from Kew, for I believe it to be a male.

inches	1	2
cm	1	2

- 2 -

Will you ask Sir H. Gower whether he could spare me one, when it flowers again.

Many thanks for Bot. Zeitung; it will be very useful. Dont take trouble about the Primula; if you stumble on it, I should like to see it. Cytisus adami is a strange puzzle; I have failed in fertilising C. pulcherrimum by pollen of common Laburnum.

If you can spare the time to notice my orchid Book, I have not the least doubt you will do it right well. There was capital matter in your Review of my Primula paper. I am always astonished at your knowledge.

Farewell. I am tired, so no more. I hope you will enjoy your holidays and be idle.

Yours very sincerely,

C. DARWIN

If you can remember, please remember that I want any peloric plants for experiments; i.e. any in pots, which could be sent me from Kew. I have been working at peloric pelargoniums; but whether I shall get any good result, I know not.

inches	1	2
cm	1	2

10.35

Cliff Cottage,  
Bournemouth,  
Hants.

2nd February 1862  
Sept

Dear Oliver,

I want very much to beg a favour. I have been working hard at *Sythrum Seitzeri* which offers a most curious case (beating Primula all to fits) of <sup>the</sup> Hermorphism, and I hope my numerous crosses at home will explain the functional meaning of all the differences? This work has given me an intense wish to see fresh flowers of any member of the *Sythrumaceae*. Have you anything in blossom at Kew? If so, would you be so kind as to send me anything in little tin b y Post - tying something damp round cut off stems. When you hear the case of *Sythrum*, I really think you will not think the trouble wasted.

We are here on account of a very miserable illness of one of my Boys from Scarlet Fever; and now my poor wife has caught it, but has almost recovered. I have in consequence done hardly anything this summer.

<sup>1862</sup> Can you tell me if any plants which bear differently coloured anthers? I wrote to Hooker, and he could not remember any besides the Melastomas and *Sythrum*, and I have just seen in a garden a small bush, which seems to me

inches		1		2	
cm	1	2	3	4	5

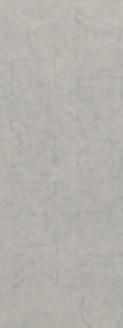
- 2 -

either a clarkia or *Epilobium* <sup>crimson</sup> with common tall anthers  
and short white ones. This difference I suspect would be  
good guide to functional dimorphism.

I hope you have enjoyed your holidays.  
Hooker tells me you are at Kew, where I wrote to him about

Dear Oliver,

Yours very sincerely,  
in haste  
 C. DARWIN





10.56.

Cliff Cottage,

Bournemouth

Wednesday evening

1862

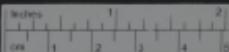
Feb.

Dear Oliver,

I am sorry you had trouble of writing two notes, but I thank you sincerely for the flowers of *Lythrum*, which I shall carefully examine with great interest. Unless *L. Hyssopifolium* presents two or three forms (which I do not believe) I am astounded at D.C. speaking of *L. Graefferi* as ~~practically~~ the same as *L. Hyssopifolium*. They are totally unlike! I am truly obliged for the specimens, for I feel a strange interest in this case. As you say it will be laborious to prove the case; but before I left home I castrated, marked and fertilised in 18 methods above a hundred flowers and protected them from insects.

I am much obliged for your photograph; I have none to match of myself but I have one made by my son, which I will send when at home if you care to have it.

Thanks, also, for diagram of Compositae in which the "etc." means I fear that all cases are not represented. By chance the day before I was looking at the Marygold and was puzzled by the apparently different stigma of the ray florets which do not seem to produce seed. But a man must be a Botanist to think about so gigantic an order; moreover, it would be hardly possible to experiment by crossing.



- 2 -

Thanks again about Belle or Rolle; which I will ascertain when I get my copy from Lyell; I fancied from some remarks that he might be a Botanist; but my wife read it Rolle as well as I did. Being nothing on earth to do here I have been working a little bit at the never-ending Drosera; as the glands absorb so readily and the hairs move so rapidly under certain stimulants it seemed a good opportunity to test how far this plant was sensitive to various vegetable substances which are known to act energetically on the nervous system of animals. As yet I can make out no sort of rule; but the difference in action is very great. Thus, strychnine produces no effect; belladonna causes movement as does veratrine; henbane does not cause movement, but does not in the least check subsequent and immediate action of meat. Opium on the other hand, does not cause movement but afterwards meat instead of causing movement in less than one minute, does not act for two or three or four hours - it puts the plant to sleep! I wonder whether analogous experiments have been tried on other plants; but anyhow I shall go on, as it amuses me and passes the time. But why do I waste your time? - I suppose for same reason it amuses me and passes the time, so forgive me and believe me

Yours very sincerely,

C. DARWIN

inches		1		2
cm	1	2	3	4

1037.

Cliff Cottage,

Bournemouth

14th September 1862.

Dear Oliver,

My sister-in-law sent me several specimens dried of *Lythrum hyssopifolium* to compare with the fresh specimens, which you kindly sent me, and amongst them was the enclosed; it is clearly not *Lythrum hyssopifolium* or *L. salicaria*; it has 12 stamens, large petals, smooth calyx, and flowers not in whorls. Could you find out its name? I fancy the genus is not large. It is a European specimen. The specimen sent answers to the "short-styled" in *L. salicaria*, but differs in many important respects. The stigma would not project beyond the calyx, and this perhaps led old Vaucher (who always blunders when that is possible) to assert that some species are dimorphic like *Primula*. It would be a very interesting aid to me if you could name this species for me, and at same time, when you find the specimens in the herbarium (if the species be not rare) pluck off a single <sup>young</sup> ~~growing~~ unopened flower from a few specimens, as I should very much wish to compare the pollen of the two sets of anthers in the "long-styled" or "mid-styled" form of this new species.

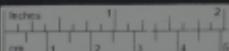
inches		1			2	
cm	1	2	3	4	5	

- 2 -

I hope you will not think me very unreasonable to ask all this; for I hope and believe that the species of *Lythrum* are not *homomorphous*; and I have been much perplexed, how any of the species could be *dimorphic*, as old Vaucler says.

Yours very sincerely,

C. DARWIN



1858.

Down,

Bromley, Kent

20th

Dear Oliver,

Many thanks. I have some analogous cases, but I am very glad to have what you have sent.

Have I not been very good, and not given you any trouble for an age?

Ever yours sincerely,

C. DARWIN

I have been this morning copying out References on subjects which concern me from N. Hist. R. - What an enormous benefit you have conferred on everyone by your gigantic labours.

No wonder that you are "omniscient".

Do you remember my suggesting that if ever you had spare time (so likely this is!!) that you might do good work by discussing F. Water plants. Now it occurred to me the other day (and I then much wished to know) whether there is not an unusual proportion of such plants with separated sexes. I merely mention this, as showing one little point which could turn up in such an examination.

inches		1		2	
cm	1	2	3	4	5

1839.

1863

Down,

Bromley,

Kent

26th February

Dear Oliver,

I am much obliged for your curious paper on the *Hannamelidæ*. The points which you indicate of the complex affinities, wide distribution, small number of species, seem all clearly to point to a ruined and bankrupt Family. And how curious the ~~structure~~ of the wood! But you well indicate how difficult it is in such cases, especially when one does not know the use or function of a ~~structure~~, to tell whether any point is due to inheritance or to subsequent ~~acquisition~~ in which case the point would only be one of analogous resemblance. It has pleased me extremely to see you allude to such questions in ~~so far from~~ a paper; it is the best of all answers to those who pooh-pooh the whole subject.

I long to be at Drosera again, but Heaven knows whether my health will last out, and whether I shall not have to retreat for two months to Malvern for Hydropathy. But if in your power, I should be extremely much obliged if you could procure for me a few plants of *Diancea* and keep them at Kew for me, and let me know what they cost, so that I may repay

inches		1			2	
cm	1	2	3	4	5	

- 2 -

you for them. I mean to attempt up a few comparative experiments with *Dresera*.

There is another plant which I want much; would it give any extra trouble to order it at same time; if it would, I am not so silly as to expect you to do it for me. But I have thought it might be ordered and come with *Diancea* and so give no more trouble. It is *Cypripedium spectabile* from N. America. A neighbour says it seeded with him; from which I infer that probably some insect visited it; so that I am anxious to make observations on it, having observed so many other Orchid *Scenaria*.

If you can easily help me, I believe you will,  
and pray believe me,

Yours since-rely,

CHARLES DARWIN



1883 1890.

Down,

Bromley, Kent

March 11th

My dear Oliver,

Will you be so kind as to give me your opinion on the following point? I was so much struck with the corresponding position of tendril and flower stalk in *Passiflora* that I got my son William (who can dissect under a 1/10th and can draw) to examine these parts in their earliest stages. In the enclosed drawing (which please to return) you will see fig. 1. the tendril and flower bud apparently quite confluent which seems odd. From fig. 2. to 7. you will see the gradation. The sort of horn to the left is the outer division of the involucell which in the early stages is the most developed. In one single case of an old bud fig. A. my son found the top of the tendril absolutely like a flower bud in an early stage as in fig. 3 or 4.

Does not this render it highly probable that the tendril is a modified flower with its peduncle? I presume that a flower would be called by you an axillary part?? And that in the vine the tendril might be considered a modified flower peduncle as I now see Lindley maintains that it is.

It would I have reason to think be a considerable relief to me, if I might view such tendrils as modified flower stalks instead of modified branches.

inches		1		2	
cm	1	2	3	4	5

- 2 -

Pray tell me is there any essential distinction between the peduncle and mid rib of a leaf, and a branch? For does not a leaf sometimes produce buds?

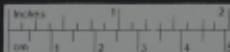
It would be a great kindness if some day you would look at the plants of *Tecoma Radicans* and observe if the branches spirally twine - I suppose they do not.

Also would you look at *Tecoma Undulata* and *Capensis* - how can they climb? Do they twine or do they emit rootlets like *T. Radicans*? My plants though four feet high show no signs of climbing. I am much interested about the genus *Tecoma*.

Believe me, dear Oliver,

Yours very sincerely,

C. DARWIN



16-41.

1943

Down,

Bromley, Kent

20th, Friday Night

Dear Oliver,

Many thanks about Phyllotaxy. Your cases seem sufficient and when next in London, I shall hear what Falconer has to say. Do not look for any more cases; but if you should stumble on them, please let me hear. I find the subject very difficult to understand; indeed I cannot understand several most simple points, such as whether there are ever more than one spine; but when Hooker comes he may be able to enlighten me. I must learn the elements to understand force of Falconer's objections; he considered the laws as fixed as that of the attraction of gravity!

I see what Treffanous says about *Primula angiflora*; I should like to know (if you are up in *Primula*) whether this species is closely allied to *P. Scottia*; because Mr. J. Scott of Bot. Garden of Edinburgh, has been carefully observing *Primulas* (and I feel a conviction that he is trustworthy) and he says *P. Scottia* is never dimorphic, and is much surprised, as he says it is so like *P. farinosa*; he has sent me plants of both, but they look very sickly.

By the way, I see Mr. Bentham makes *P. Scottia* var. of *P. farinosa*; would it not be worth while to tell him of

inches	1	2
cm.	1	2
	2	3
	3	4
	4	5

- 2 -

Mr. Scott's observations; for there can be no doubt that this difference indicates an important functional difference. Unless indeed *P. farinosa* presents 3 sexual forms; but then they all three would grow together. Trelease in his Review of the Orchids does not seem to appreciate at all the prettiness of the adaptations, which seems to me the cream of the case.

Yours very sincerely,

Ch. DARWIN

inches		1		2
on				
	1	2	3	4
				5

1042.

1863

Down,

Bromley, Kent

24th March

Dear Oliver,

I remember looking in such Books as I had for explanation of the ovary of Primula; which I found something like this; viz; a knot in the middle covered with ovules, which sent a thin stilyform process up centre of style. I could not make out from Books what this latter part was. Last autumn I potted some Primroses; a long-styled plant is now in flower which in almost all its flowers has <sup>Knot</sup> rather short pistils, with stigmas (5 stamens etc. as usual); the three styles become a little feliaceous at base, but are not united, so that by pulling them a little asunder without tearing, you can see right into the ovary with its ovules; and there is no central process from Knot. If capable of fertilisation the pollen tubes must go to base of Knot and then turn up to ovules. I have put a few flowers in spirits. The case does not concern me; and I mention it solely for chance of your caring for it, or for its throwing any light on morphology of ovary in Primula. It looks as if Knot with ovules was prolongation of Axis. If you can forward specimens per Railway, or want me to



inches		1		2
cm	1	2	3	4
				5

- 2 -

uncertain whether these flowers can be fertilised, please write; if not I shall understand that specimens may be thrown away.

Please tell Hooker I should like to see flowers in tin box of Edwardian tetraptera; and I should like to see 2 or 3 leaves or bracts of marcgravia which Lindley speaks of as approaching in structure to pitchers of nepenthes. Poda of Orchis - Poplars.

Dear Oliver,

Yours very truly,

C. DARWIN

I think I will put pollen right on to ovules and cut off the stigmas and see if I can thus directly fertilise the ovules.

P.S. Tell Hooker I have just received his interesting letter, and will write soon about Poplars' surprise with particular thanks; perhaps he would look just once again for Bees on fine days, and gently again shake branch. Can interest me on account of willows.

You shall surely have copy of Linum paper whenever I get copy.

I am badish to-day, so no more.



1/P(1)

1043.

Down,

Bromley, Kent

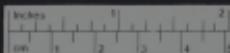
28th March

Dear Oliver,

You have indeed sent me some interesting specimens. Every part of the base of the flower of *Edwardia* seems to secrete a surprising quantity of nectar; I thought from what <sup>T. D.</sup> *Trelleanus* says it did not secrete till stamens or pistils were broken off, which would have been a very surprising fact.

What a very curious stamen you sent me; it would be fine sport to observe the living plant. Can it serve as protection to stigma; in a *Nieembergia* I saw the stamens formed into a sort of cupola over the stigma; but I neglected to get the plant for observation. What singular bracts of the *Hægaria*; if I can make anything of the Pitcher plant, which I do not suppose I shall, then *hægaria* would be interesting to observe?

I will send bottle with monstrous Primrose on Monday or Tuesday, with the Medallion for Hooker of Dr. Darwin (please tell him); I have seen one flower with 4 pistils; there is one in bottle - one specimen - with corolla removed. <sup>recess</sup> I remember I observed that 9 or 10 bundles of spiral sepals ran up in outer case of ovary and up pistil, and none ran up the central needle, prolonged up the middle of pistil in



- 2 -

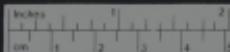
youth from the placenta. I concluded at time that the single pistil is really ten pistils. I had never heard of Cagpary; but speculate whether what you say is the placenta, was not an inner whorl of pistils without stigmas.

I have been looking at anther again, would it not force insects to approach only on one side and so rub against the side of anthers which dehisce?

With many thanks,

Yours very sincerely,

C. DARWIN



1844.

Down,

Bromley, Kent

March 31st

Dear Oliver,

I am infinitely obliged for the reference, I certainly should be very glad to hear what the paper is about and I beg you to be brief and not waste your time as I must ultimately read the paper. I am very glad to hear about Modocia, but I had given up the ghost from your former letter. Will you thank Hooker for his splendid long letter and ask him to read the enclosed from Scott about whom I am very sorry. I have just had another long letter from him with some most remarkable observations on the fertility of orchids with their own and other pollen. Incidentally he was led to estimate with care the number of seeds in one capsule of Acropelma and the number amounts to 371250 in one capsule, and the plant produces many capsules. Is not this stupendous? I heartily wish something could be done for this man.

Dear Oliver,

Yours very sincerely,

C. DARWIN



1845

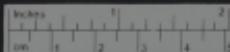
Down,

Bromley. Kent

April 15th

Dear Oliver,

How can you doubt about publishing your paper? I may be, but I do not think I am, blinded by the too much praise you give me. Why the lowest merit of the paper, the number of references, would alone make it worth publishing. Why do you not attach your name; pray remember it more than doubles the value of every remark? It is just the paper, with your original remarks and suggestions, to set men observing. Your observation of possible tendency to hermaphroditism is quite new to me and ancient <sup>2</sup> practices of dioecious forms. It is as I now see, certainly quite possible; but I cannot call to mind, at present, any case supporting this view. Yet we entirely agree I see in probable function of the imperfect flower of *Viola* etc. i.e. to make sure of a stock of seed. I am almost certain that the Frenchmen are wrong about the perfect flowers not producing seed; but I have not time to hunt up my notes. I should not be surprised that the observed plants were (?) in pots in greenhouse or in chamber! As number of seeds is different in two forms of *Primula*, you have a right to put the case in your first class; but I doubt (I say only in my paper possibly tendency to dioecious condition) whether the tendency is real - whether the difference in number of seeds is not merely some odd case of correlation like size of pollen grains.



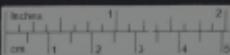
- 2 -

Would it not be well to keep your hypothetical or rather possible class of dimorphism altogether in the note; the sentence at present is not clear. - I have been looking at ovules again; one case seemed to agree with you, but others did not. Had you not better have one more look: the case, as you put it seems probable, but certainly in some buds of apparently exact same age, did not hold; I got one of my boys to look, as you did, and he gave verdict, without knowing which with me. I am forced to write in great hurry. For heavens sake publish and append your name; if you do not publish, pray let me make M.S. copy of your paper. The time will come I suspect, when all animals and plants will have to be viewed as primordially hermaphrodite; though confervae are opposed to this.

I am disgusted that your pretty discovery about Campanula has been forestalled; you are quite right about insects and Campanula - C.K. Sprngel long ago observed and found same fact i.e. on necessity of insects in this genus.

Do not forget to look at Corydalis Lutea: it will interest you: Vaucher observed the fact, but blundered greatly about all the details.

It makes me laugh at myself hearing you say that migration into N. America so manifestly depends on old warmer period; I believe I meditated for four years on <sup>the</sup> whole case before this occurred to me!! I than wrote to Asa Gray who used it in his paper. - Farewell - i  
days.



- 3 -

hope to heaven my vile handwriting does not break your heart -

Yours very sincerely,

C. DARWIN

I return your paper by this same post.

inches		1		2
cm	1	2	3	4

Mr. Letters 15

I. 333

[April 1863] 1046

Down,

Bromley, Kent

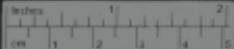
Sunday

Dear Oliver,

It is a shame to trouble, but will you tell me whether ovule of *Primula* is "anatropal" nearly as figured by Gray p.123 ~~Length~~ of Botany, or rather tending to "amphitropal" I never looked at such a point before. Why I am anxious to know is because I put pollen into ovarium of monstrous *Primroses*, and now after 16 days, and not before, (the length of time agrees with slowness of natural impregnation) I find abundance of pollen - tubes emitted, which cling firmly to ovules, and I think I may confidently state penetrate the ovule; but here is odd thing - they never once enter at (what I suppose to be) the "orifice", but generally at a the chalaza when they fall..

Do you know how pollen tubes go naturally in *Primula*? do they run down walls of ovarium and then turn up the placenta and so debouch near the "orifices" of ovules?

If you thought it worth while to examine ovules, I would see if there are more monstrous flowers and put pollen into ovarium and send you the flowers in 14 or 15 days afterwards. But it is rather troublesome, and I would not do it unless you cared to examine the ovules. Like a foolish and idle man I have wasted a whole morning over them.



- 2 -

When does Hooker return?

Yours most sincerely,

C. DARWIN

One line in answer would suffice.

In 2 ovules there was an odd appearance as if the outer coat of ovules at the chalaza end (if I understand the ovule) had naturally opened, or withered, when most of the pollen tubes seemed to penetrate, which made me at first think this was a widely open foramen. I wonder whether the ovules would (could) be thus fertilised! X

X. What is most interesting here is that Darwin should have anticipated the theoretical possibility of fertilization via the chalaza. The actual discovery of chalazogamy (for Casuarina) was made by Treub 30 years later (1891), & then found in many plants since - also Cycas, Ginkgo & Arecaceae. I know of no statement connecting it with Primula. It is doubtful whether Darwin did have the necessary technique at his command to demonstrate chalazogamy convincingly. It is possible that what he saw were fungal hyphae & the postscript above is consistent with this suggestion. I wonder what my father said! J.W.C.



1047.

Down,

Bromley, Kent

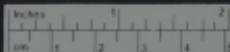
Apr 24

Dear Oliver,

Many thanks for Oxalis; of 38 flowers sent 24 are long-styled and 14 short-styled, just as in the flowers here; but whether this depends on mere useless variability or on useful dimorphism, I suspect experiment will alone show; I have got plants in pots for next spring. I am quite aware the other cestoid <sup>?</sup> flower is a very <sup>hard</sup> more curious case. I return 6d. stamp, which I suppose is a Kew affair.

Yours very sincerely,

C. DARWIN



[1863] 10-78

Down,

Bromley, Kent

May 4th

Dear Oliver,

I am very much obliged to you for your kind present of Elementary Botany, so profusely and beautifully illustrated. When I am able to read a little more I am sure it will suit me excellently and I thank you sincerely for sending it. In the back numbers of the Nat. Hist. Rev. I have read some reviews which I am sure are by you, with extreme interest and with amazement at your bibliography. Try and remember that it is possible to kill yourself with work. At any time when Botanists congregate thickly, or you come across anyone who has studied the order of Passiflorae, will you ask the assembly whether any member of the order climbs without the aid of tendrils i.e. spirally twines; I am really anxious to know; if I do not hear I shall understand no one knows. I am glad to say that my health is slowly and with oscillations steadily improving.

Believe me,

Yours very sincerely,

C. DARWIN



1049.

Down

June 15th

Dear Oliver,

Very many thanks about the diagram.

You told me before of Mohl's book and I got it in consequence and very useful it has been to me. He discusses homologies largely. I fancy he wrote when he was young. Anyhow he overlooked much and made many mistakes. Palm's Treatise is better. I have sent you a photograph of myself with much pleasure.

Yours very sincerely,

C. L. DARWIN

P.S. You sent me some time ago a reference about the roots of seeds, orchid seeds, drawing themselves into soil like tendrils. I have put this reference somewhere and I have looked and looked and cannot find it. Could you send it to me again whenever Hooker writes.

inches	1	2	3	4	5
cm.	1	2	3	4	5

/050.

Down,

Bromley, Kent

July 13th

Dear Oliver,

I very much wish you would observe one point for me, which would only require your looking once carefully at *Nepenthes*. Do the tips of the young leaves which catch hold of any support develop pitchers, or is it an alternative process of clasping or pitcher-forming? My plants will not grow vigorously, and will catch nothing; perhaps young plants do not climb. If you are able to observe this for me, please tell me whether the tips of the young leaves are naturally hooked or only curved downwards, before catching. I wish you could feel interest enough yourself on the point to put a twig under the tip of a young leaf and afterwards see if it catches hold and let me quote you. - I am much the most curious about the first point.

Many thanks for your paper on legumes which has interested me much. If I could make out a little about *Nepenthes* I think I should understand moderately well every class of climbers and I do not yet quite despair of my plants growing.

Dear Oliver,

Yours very sincerely,

C. DARWIN

inches		1		2
cm.	1	2	3	4
				5

18-51.

Down,

Bromley, Kent

18th July [1863].

See L.L.S. 305

Dear Oliver,

Would you have the kindness to look over the enclosed; it seems to me a valuable paper. Dr. Hildebrand has sent it to me with the request to get it read before some Society, or get it published somehow.

Now you will see that it is an abstract of a longer paper to be published in the Bot. Zeitung. Therefore I fear it would not do for Linn. Soc. How is this? Please consult Hooker. If it will not do for Lin n. Soc., would it do for the Nat. Hist. Review? And this is the reason I send it to you. If not fitted for N.H.R. I will send it to Annals of N. Hist. So at your leisure please let me hear, as I must write to Dr. H. Dr. Hildebrand pleases me by telling me that he has been repeating my experiments on Linum and has observed fertilisation of all German orchids and finds my statements correct. But he adds that he thinks I am probably wrong about sexes of Acupera and Cetnsetum, and I very much fear, indeed feel almost sure, that I have blundered fearfully about Acupera, and perhaps about Cetnsetum, which is horrid to think of. I wish my Acupera would flower; I shall soon have two Cetnsetum in flower and must look to them.



- 2 -

Dear Oliver,

Yours Very sincerely,

C. DARWIN

Tell Hooker I am madder than ever on Tendrils, and long to  
hear whether he has anything for me for observations

inches		1		2	
cm		1	2	3	4

1052.

Down,

Bromley, Kent

November 3rd.

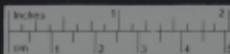
Dear Oliver,

I thank you most sincerely for all the references. Good heavens, how do you know such multitudinous references! It is a horrid bore, that I shall probably find all my recent work done and done better than I have. Anyhow I have enjoyed the work. If you had not told me those references how nicely you might have reviewed, "disgraceful ignorance", "highly presumptuous" etc. etc. I heartily thank you though I expect to be heartily disgusted with the papers when I get them from Linn. Soc. Please thank Hooker <sup>for him</sup> for information about ~~Dendrobium~~ Chrysanthella, and say that I should be very glad of a Cypripedium to have a look at the dusts of Labellum.

Yours very sincerely,

C. DARWIN

If Hooker sends any more orchids I should like one more head of Erezlina Carivata on account of its curious nectary.



16.53

*unstated*

Dear Oliver,

I think Wyman's very short notice in Proc. American Acad. of Arts and Sciences Vol. III (1852 - 1857) page 167, worth your looking at. I am proud for once to give you a reference.

I am bad so no more.

I am so sorry to hear about Hooker's boy with scarlatina.

Yours very sincerely,

G. DARWIN

inches		1		2
cm	1	2	3	4
				5

Wyman Proc. Am. Acad. III. 167

Trying to ascertain cause of contractn. in some veg. tissue. as in common balsam and Echinocystis Lobata.

"It was Dutrochet's theory that this action is due to the sudden transfer of fluid from the inner to the outer cells of the wall of the capsule. Dr. W. had demonstrated the impossibility of this by dissecting a way the outer layers of cells without impairing at all the contractility of the capsule. He had himself come to the conclusion that the motion is due to the spontaneous contraction of the cells on the contracting side in the way that motion is produced in the <sup>by Wyman</sup> polyps. In the contracted capsule the cells on the concave side are found to be shortened, while on the opposite are elongated. In the Balsam capsule the contraction is so sudden that the shortening of the cells cannot be watched, but in the Echinocystis the motion is so gradual that the change can be observed under the microscope. When gradually subjected to the action of anaesthetic agents the capsules lose their contractility; but when suddenly placed under their influence an immediate contraction is the usual result."

Above is my failed abstract  
of Wyman's paper referred to  
by Darwin in the Preedy  
letter, 200



10.57.

Down.

28th.

Dear Oliver,

Your note has interested me much. But I cannot look to notes on Fumitories, ~~has~~ not nearly strength enough. These plants are fertile in large degree without insects, but are all (or nearly all) manifestly adapted to visits of insects, which favour and increase their fertility. But I must write no more. I shall be very anxious to read sometime Hugo von Mohl on the little flowers. I did some work on them and on Violets this summer.

You may rely that perfect flowers of Violets except ~~the~~  
V. Tricolor are fertile only when visited by insects: I marked flowers visited by Bees and ~~presented~~ Bees visiting them, etc. The imperfect flowers are of course fertile without insect visits.

Yours very sincerely,

C. DARWIN

How curious about the pods.

The hydrostatic movement must be sometimes like that of the pollinia in ~~the~~ orchids.



16-355.

Down,

Bromley, Kent

29th

Dear Oliver,

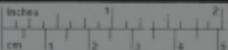
Thanks for Orchids. I liked to see the strange labelling of the Catasetum new to me. C. longiflorum is considerably different; there is a truly wretched sketch of the genus at the back of my Orchid Book. Do not take any more trouble about it; I only thought that as I had carefully examined C. longiflorum I should like to see its male.

Yours very truly,

C. DARWIN

I care more for Dimorphism than Orchids, and I think it is a most important subject. To-day I have been looking at and have seen the three forms, i.e. long-styled, mid-styled and short-styled; as each form has two sets of anthers, 18 different crosses are practicable within the limits of this one species!! As I ought to cross 10 of each, this would make 180 fertilisations and markings and countings of seeds. A nice job, Heaven knows whether my patience will last; but I should like to make out this wonderfully complex case.

I enclose stamps not to cheap Kew.



10-58.

Down,

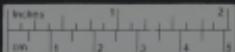
20th

Dear Oliver,

Hooker says you pass daily some *Oxalis Acetosella*. Will you oblige me by gathering a dozen or score of flowers from different plants, and if possible plants growing a little apart. I find some evidence of dimorphism in the plants here as in *Primula* and much want to see plants from some other station. The plant does not grow here within my walking distance. Would you send me the flowers by post in a little tin box with a damp blotting paper.

Yours very sincerely,

C. DARWIN



10.57

1864

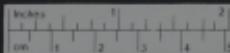
Down,

Bromley, Kent

23rd

Dear Oliver,

I am very curious to hear about *Epilobium Angustifolium*, both on account of fact itself and for following odd psychological case. I knew plant well between 20 and 30 years ago in my father's ground; well, this summer it flashed across my mind that there was something dimorphic in it, I tried my best, but could remember no vestige of particulars; yet I was very near sending to my sister for a lot of flowers; but as I could hardly remember anything of flower, except its colour I thought it too foolish. If it really is dimorphic, I shall always look at my flash of memory like one of those cases of persons in a fever who have temporarily remembered a language learnt in infancy and ever after forgotten? If you have pretty reason to think it dimorphic, ask Hooker to put it down in list of seeds required, if I can possibly get it. Seeds are much better than roots as same form may spread by suckers. For several reasons (*Clarkia Elegans*) I should very much like to experiment on this plant. About Strawberries, do you refer to American plan of planting six rows "pistillates" and one row of hermaphrodites? If something else, kindly inform me, as I have written a very little on Strawberries. Thank Hooker for very kind offer of dried plants; but I hate dried plants. I can make nothing of them and I profoundly pity all you Botanists. Will you ask

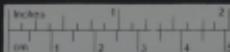


- 2 -

Hooker (to whom I shall be writing before long, for I never give him a long holiday) if he gets the missing vol. of Bot. Journal for Linn. Soc. kindly to send it me, (per Railway to c/o Down Postman) first and then I will send it in his name to the Soc. for I want pretty soon to consult it before I write my paper on Linum for Linn. Soc.

Yours sincerely,

C. DARWIN



Ans letters 11 . 338 . 10.58.

Down,

Bromley, Kent

February 17th [1864]

Dear Oliver,

Many thanks for the Epaeridae which I have kept as they will interest me when able to look through the microscope.

Dr. Cruger has sent me the enclosed paper with power to do what I think fit with it. He would evidently prefer it to appear in the Nat. Hist. Review. Please read it and let me have your decision pretty soon. Some Germanisms must be corrected, - whether woodcuts are necessary I have not been able to pay attention enough to decide. If you refuse, please send it to Linn. Soc. As communicated by me. The paper has interested me extremely and I shall have no peace till I have a good boast. The sexes are separate in Catasetum, which is a wonderful relief to me as I have had two or three letters saying that the male C. Tridenatum seeds. It is pretty clear to me that two or three forms are confounded under this name. Observe how curiously nearly perfect the pollen of the female is according to Cruger certainly more perfect than the pollen from the Gazania specimens described by me. I was right in the manner in which the pollen adheres to the hairy back of the humble bee and hence the force of the ejection if the pollinia. I am still more pleased that I was right about insects knowing the fleshy labellum. This is



- 2 -

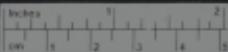
important as it explains all the astounding projections on the labellum of Oncidium, Phalaenopsis etc. Excuse all my boasting. It is the best medicine for my stomach. Tell me whether you mean to take up Orchids as Hooker said you were thinking of doing.

Do you know Coryanthes with its wonderful bucket of water? See what Cruger says about it. It beats everything in orchids.

Dear Oliver,

Yours sincerely,

C. DARWIN



105.

Down,

Bromley, Kent

March 18th 1864

Dear Oliver,

I am extremely obliged to you for your two letters. Your first letter was just what I wanted and I greatly prefer being treated as what I am quite ignorant of the rudiments of Botany. Your information about Tecoma though chiefly negative is of value as it will save me much useless labour. Thank you much for telling me of the book on orchids which I have not seen. I have been pleased and interested by the extracts though we are such bad German scholars we have had hard work to make them out. I can hardly believe the statement ~~about~~ <sup>of</sup> Catasetum. I have no doubt from facts communicated to me that Cat. Tridensatum does sometime seed in its native country, and that there is great difference in the degree of separation of the sexes. About the sexes of Aeropera I certainly erred, but it is a wonderful mystery how the plant can be naturally fertilised in its native country. You must not think of wasting your time in telling me anything more about the book; unless indeed you come across anything important; but I hope that perhaps that you will review it. Please to tell Hooker that the magnificent supply of plants is arrived safely and I thank him sincerely.

I am, Dear Oliver,  
Yours sincerely,  
C.L. DARWIN



1660

Down,

Bromley, Kent

October 24th

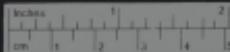
Dear Oliver,

I am extremely obliged to you for all the trouble you have taken in correcting so fully the paper which I sent you, and which I will now transmit to Linn. Soc.

Dear Oliver,

Yours very sincerely,

C. DARWIN



10-41.

18 64?

Dear Oliver,

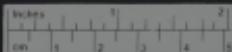
My paper on *Digitaria* is to be read on  
Thursday at Linn. Soc.

Will you grant me a favour; I know that you  
are a good hand at Diagrams; and I believe in 5 or 10 minutes  
you could copy a diagram in chalk on the board. The diagram  
is with my paper. The only point to attend to is the  
relative length of pistils and stamens, and the dotted lines of  
full fertilization. Without the diagram my paper would be  
unintelligible.

If you do not attend the meeting, <sup>pass</sup> this note  
on to Hooker.

Yours very sincerely,

C. DARWIN



18.42.

Down,

Bromley, Kent

December 15th

1864

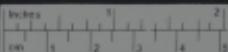
Dear Oliver,

Will you be so kind as to tell me, if you should happen to know the addresses of Flanchon, Hofmeister and Schleiden? Do you want a spare copy of my Ianthrum paper and of the three other papers communicated by me, as I do not send them to members of Linn. Soc. unless they are wanted. I fear you cannot advise me whether Dr. Cruger left a widow to whom to send spare copies of his paper.

Excuse brevity and forgery as I am unwell in bed.

Yours sincerely,

C. DARWIN



1064

Down,

Bromley, Kent

15th November, 1871

Dear Professor Oliver,

Will you have the kindness to give me a small piece of information.

Is it now believed that the spongeoles at the extremity of the rootlets secrete carbonic acid or any other element in a nascent state, which acts on bones and rocks so as to obtain the necessary phosphates and alkalis?

I remember years ago having seen a statement to this effect, and I am anxious to know whether it is now thought true.

Pray excuse my troubling you and

Believe me,

Yours sincerely,

Ch.DARWIN



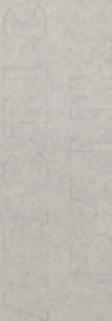
1865.

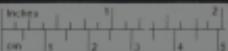
Post Card

December 15th 1875

Very many thanks for telling me about Cohn, but  
he sent me a copy of his article.

C. DARWIN





MacClellan

1876 10.66.

II : 408

Down,

Bromley, Kent

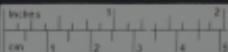
13th October

Dear Oliver,

You must be a clair-voyant or something of that kind to have sent me such useful plants. Twenty five years ago I described in my Father's garden 2 forms of *L. flavum* (thinking it case of new variation); from that day to this I have several times looked, but never saw the second form till it arrived from Kew. Virtue is never its own reward; I took paper this summer to write to you to ask you to send on flowers, that I might beg plants of this Linum, if you had the other form, and refrained, from not wishing to trouble you. But I am now sorry I did; for I have hardly any doubt that *L. flavum* never seeds in any garden that I have seen, because one form alone is cultivated by slips.

Secondly I raised a lot of plants from Kew seed marked "L. austriacum", but certainly different from the flower you sent, which no doubt, as Lecoq says, are dimorphic.

The Kew seedlings, 112 in number were not dimorphic, and were all self-fertile, and I strongly suspect were *L. heterotrichum*; now it would be of great use to me to know whether you keep seed at Kew of any other blue



- 2 -

Linums, besides *L. perenne*, austriacum and ~~westatissimum~~ for my plants were not the two former, and I could then perhaps know what they were.

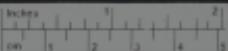
Many thanks for your caution about stigmas of long-styled *L. perenne*. I knew and think I said the position was due to twisting of style; but I will look more carefully at what period twisting take place.

You sent me some most curious plants; what in name of Heaven is name of the genus of which you sent me 3 species and especially of pinkish-purple flower with long thin nectary; I suppose you sent them from seeing their relation, like in Orchids to visits of insects. If a greenhouse plant I will get this plant.

I certainly thought you were author of the excellent review of the orchid book, especially from about unisexuality in high plants. The Reviewer (is it a secret, who?) I daresay is quite right about arrangement of Book; but I hardly know with my materials that I could have made it better. I wish he would criticise the last chapter; I should like to hear what a good hand would say to it.

I have not yet had time to examine flower  
most cordial thanks for them.

Yours very sincerely,  
C. DARWIN



10-67

April 27th

My dear Hooker,

Will you run your eye over other side and then tear up or keep as you like. What I want are facts to upset this apparent rule or (which of course would please me most) any strong cases to support it. Any one species in a group, with nectar-secreting surface on one side of flower with pistil bent toward that side would be, I should think, good case? - Do you think Mr. Oliver would keep and decipher my miserable notes, and observe a few flowers for me this summer. - Why I care about it, is that it shews that visits of insects are so important, that these visits have led to changed structures.

S.B.

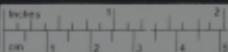
(On back side of sheet)

Leguminosae. In vast number of species pistil bent rectangularly upwards and Bees get nectar at base of standard: but in Kidney Bean and garden Lathyrus, they always alight on left-hand petal and the stigma by curving points to this same side.

Mem. Case of Corydalis and Dielytra alluded to, and Fraxinella.

-----  
Alstroemeria. (Common orange-flowered kind) two upper petals secrete honey, and the pistil is bent up to this side. -

-----  
Gladiclus. (Orange) much honey on lower side of flower, where the



filaments are attached; pistil when mature bends down to this lower side.

-----  
*Delphinium Grandiflorum*, pistil bent rectangularly so that stigma lies in gangway to nectary. (Contrast with Columbine).

-----  
*Tropaeolum*, stigma when mature slightly bent into gangway.

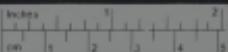
-----  
Several Scrophulariaceas, as *Antirrhinum* and *Pedicularis*; some Labiate, as the fine Blue *Salvia* have pistils more or less bent into gangway to nectary.

-----  
*Lobelia Fulgens* stigma slightly bent down towards gangway.

-----  
*Polygala*, (Common English species) has pistil directed at right angles to where Bees suck flower.

-----  
*Rhododendron*, *Viola Tricolor*, Horse Chestnut all have pistils more or less bent in same manner i.e. in path which Bees must brush over in sucking the nectar.

-----  
C. DARWIN



- 2 -

and not to the apices of the pollen-mass, as in *Listera*. I suspect, from one case that I saw the pollen-masses are ejected from anthers of *Listera* with force, before and independently of ejection of fluid. I can see that this viscid fluid is analogous to sticky gland of orchis, but it seems to me, in my ignorance, that in *Listera* the opposite end of pollen-masses are attached, to what is the case in *Orchis*. In *Cephalanthera Grandiflora* the pollen-masses are shed in bud and stand close in two friable columns close to upper edge of stigma; and this upper edge (which I presume answers to ~~st~~<sup>sp</sup>ellum) whilst in bud-state, ejects fluid (without touch) on each side and glues the pollen-masses there; as the rim with the glue turns reddish, it is probably homologous to the viscid secretion of *Listera* and *Neottia*. I am sure I do not know whether you care enough about *Listera* to hear this about *Neottia*.

Yours affectionately,

Pencil Note

C. DARWIN

It requires no sort  
of answer.

I can see externally to loculi in ~~sp~~<sup>ly</sup>ellum of *Neottia*.

I have got good facts about Moths and Orchids - good evidence thank heaven of pollen-masses of Bea-*Orchis* adhering to proboscis of Moth; and one Moth sent me with 13 pollen-masses of some *Orchis* adhering



3

- 2 -

to its proboscis, rendering it quite arborescent. - I could  
recognise pollen-mass of Butterfly Orchis on two other Moths.



1868.

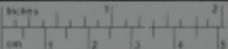
Down,

Bromley, Kent

June 17th

My dear Hooker,

I have re-read your paper on *Listera* and have been looking at specimens and have hardly ever been more amused. *Neottia Nidus - Avis*, the ~~Antellum~~<sup>(Tz)</sup> is thicker and the summit is so wonderfully sensitive the touch from a child's hair not held close to point was sufficient to cause the ejection of the fluid!! And what delighted me was that this fluid sets so quick, becoming slightly opaque, that in two seconds or even less when the touching object was withdrawn a mass of pollen was withdrawn with it. I send the hair in quill, with which I touched top of ~~Antellum~~<sup>(Tz)</sup> of *Neottia*, and almost instantly withdrew it, and you will see (if you think it worth cutting open quill) pollen glued to end. So the insect by this wondrous contrivance would be sure to carry pollen from flower to flower. The same thing happens with *Listera*, but in this case whole pollen-mass in a flower only lately expanded, is withdrawn. I cannot believe from what I have seen this pollen-mass can be ejected by the ejection of fluid matter for during first second it is not very viscid. A more important difference is that in *Neottia* pollen is shed in bud and is cast irregularly on to the ~~Antellum~~<sup>(Tz)</sup> and does not adhere to it until the fluid is ejected; and the adhesian is indefinite i.e. to any point of the pollen-masses



1069

Betty decidedly rather better.

19th, Down

Dear Hooker,

(I vow I will not bore you again). Have pity on me and let me write once again on Orchids for I am in a transport of admiration at most simple contrivance and which I should so like you to admire. How I wish I was a Botanist. Such brief books as I have say structures of Gymnadenia like Orchis, but I find the stigma consists of two most curious lateral horns; I must think them horns on the stigma from what follows - push thick needle down mouth of nectary and the two pollen-masses adhere by their long naked strap-shaped sticky glands, and then when needle is withdrawn by elasticity become nearly parallel to the needle. In doing this not a grain of pollen is left on the horn-like stigma, but here comes pretty fact, hold a needle in same relative position to parts of flower and push into nectary of another flower, and lots of grains of pollen are almost inevitably left on the two humid stigmas. In fact your conopsea would hardly ever get a grain of pollen of its own flower. I was led to observe this by finding that the 13 pollen-masses on the proboscis of a Accoutia Matsuura belong to this Orchis. I see Bentham puts this plant into genus Orchis but surely such a difference in stigmas, and naked and curious strap-formed glands must be good generic character. If, as is likely, you do not care or are too busy to look at this, as it seems to me really beautiful



15.12

- 2 -

of parts, ask Mr. Bentham whether he will. I see the stigmatic  
horns rise from spot where stigma lies in this Orchis.

C.D.