

Letter 2951—Darwin to Daniel Oliver

17 Oct [1860]

15 Marine Parade | Eastbourne
Oct 17th

My dear Sir

I am infinitely obliged for your mass of interesting facts & extracts.—^{f1} The only at all full account which I have seen of *Dionæa* is by Mr Curtus, quoted by author (Lindley?) in Penny Encyclopædia.^{f2} Lindley also refers to *Dionæa* having been fed & flourished on chopped m eat.— I think you could see with lens on growing plant whether colour in Sensitive Hairs becomes broken after being touched; that experiment & effect of C. of Ammonia on disc could be very interesting. A good dissector, I daresay could follow the cells with red matter from base of sensitive Hair, & thus follow its *nerves*!—

Many thanks about stomata; it was uncommonly stupid in me not to think of your explanation, which I have hardly a doubt is right;^{f3} for it was after removing the tortoise-like glands, which are mounted on very short footstalks, that I saw the appearance of stoata: I have hardly a doubt that my so-called stomata are 2 cells of the footstalk.—

Thanks for your various analogies & co parisons about the moving red matter: it is beyond me; I shall just publish what I saw.—^{f4} I think that you have is understood me in supposing that this appearance follows only from C. of Ammonia.— it is *better* seen in the Hairs which have naturally contracted over a fly or other object; & this it is, which seem s to me to make the case curious.— I have been ascertaining this morning how quickly C. of Ammonia acts; & certainly 13 seconds suffice for the absorption & for a marked change in structure in the glands; in one minute the action reaches the upper part of footstalk.— No other substance (such as Acetate, Oxalate, nitrate of Ammonia &c &c) acts nearly so quickly, though they do act after some time; the acetate of Ammonia is next in quickness.— I *suspect* that m ilk, urine &c & these salts do not act until they have become decomposed, & yield C. of Ammonia.—I cannot avoid suspecting that we see in the action of these substances on the leaves of *Drosera* what chemists believe takes place with organic manures on the roots of other plants.—^{f5} I am going to try tomorrow C. of Ammonia on the root of *Drosera* & on some other common plant.—^{f6}

Thanks about *Elatine*—& for *Goodenia* Plants.— Pray thank Croker^{f7} for all great trouble which he has taken.—

I have told Murray to send you copy of m y Journal,^{f8} which I am very glad you did not possess.—

Yours very truly | C. Darwin

The day after tomorrow I shall begin & draw up m y paper on *Drosera*; for I have wasted a shameful lot of time on it—& **yours** also.^{f9}

The leaves of *D. spathulata* will not open.—

Footnotes

^{f1} See preceding letter. The letter that apparently accompanied the note has not been found.

f2 Moses Ashley Curtis, a resident of North Carolina where *Dionaea* is native, was quoted by John Lindley in [Lindley] 1837, p. 508. CD cited Curtis 1834 in *Insectivorous plants*, p. 301 n.

f3 The stomata of *Dionaea muscipula* were apparently discussed in the missing portion of the preceding letter. For CD's views on the fine structure of the leaves, see the letter to Daniel Oliver, 14 October [1860].

f4 CD described the phenomenon of aggregation in *Insectivorous plants*, chapter 3.

f5 CD was familiar with the prevailing view of agricultural chemists that the roots of plants absorbed nitrogen derived from the decay of animal matter. A copy of Liebig 1840, which discusses this point, is in the Darwin Library-CUL. For CD's reading of the works of Justus von Liebig, see *Correspondence* vol. 4, Appendix IV.

f6 CD's experiments on the absorption of carbonate of ammonia by the roots of *Drosera rotundifolia* were given in *Insectivorous plants*, pp. 141–2. Further experiments on the roots of other plants were ultimately given in his paper on 'The action of carbonate of ammonia on the roots of certain plants' (1882) (*Collected papers* 2: 236–56). By that time, CD had decided that aggregation was the result of an accumulation of waste products in the cells at the tip of the root.

f7 Charles William Crocker. See letter to Daniel Oliver, 5 October [1860].

f8 *Journal of researches* (1860). See second letter to Daniel Oliver, 12 [October 1860].

f9 Although CD did not publish the results of his study of *Drosera* until 1875 (*Insectivorous plants*), he delivered a paper on the subject on 21 February 1861 at a meeting of the Philosophical Club of the Royal Society (Bonney 1919, p. 154). The index to his 'Eastbourne paper' is in DAR 54: 1.

Letter 3853—Darwin to John Scott

11 Dec [1862]

Down Brom ley Kent
Decr. 11th.—

Dear Sir

I have read your paper with much interest.^{f2} You ask for remarks on matter, which is alone really important, shall you think me impertinent (I am sure I do not mean to be so) if I hazard a remark on style, which is of more importance than some think? In my opinion (whether or no worth much) your paper would have been much better if written more simply & less elaborated,—more like your letters. It is a golden rule always to use, *if possible*, a short old Saxon word. Such a sentence as “so purely dependent is the incipient plant on the specific morphological tendency”^{f3}—does not sound to my ears like good mother English—it wants translating.—

Here & there you might, I think, have condensed some sentences: I go on plan of thinking every *single* word which can be omitted without actual loss of sense as a *decided* gain.— Now perhaps you will think me a muddling intruder; Anyhow it is the advice of an old hackneyed writer who sincerely wishes you well.—

Your remarks on the two sexes counteracting variability in product of the one is new to me.^{f4} But I cannot avoid thinking that there is something unknown & deeper in seminal generation. Reflect on the long succession of embryological changes in every animal. Does a bud ever produce cotyledons or embryonic leaves? I have been much interested by your remarks on inheritance at corresponding ages; I hope you will, as you say, continue to attend to this.^{f5} Is it true that female plants always produce female by parthenogenesis?^{f6} If you can answer this I should be glad; it bears on my *Primula* work: I thought on subject but gave up investigating what had been observed, because female *Bee* by parthenogenesis produces *males* alone. Your paper has told me much that in my ignorance was quite new to me.— Thanks about *P. Scotica*. If any *important* criticisms are made on *Primula* to Bot. Soc. I should be glad to hear them.^{f7} If you think fit, you may state that I repeated the crossing experiments on *P. Sinensis* & *Cowslip* with the same result this Spring as last year—indeed with rather more marked difference in fertility of the two crosses.^{f8} In fact had I then proved *Linum* case I would not have wasted time in repetition.— I am determined I will at once publish on *Linum* .—^{f9}

We seem predestined to work on same subjects: for 3 or 4 summers I have worked *hard* at *Drosera* & *Dionaea* & have almost a volume of materials, which I suppose some day I shall publish; ie if ever I have time to work my materials into shape.^{f10} Irritability of plants has been a hobby-horse to me. I **suspect** it will turn out that irritability of same nature, only intensified in *Dionaea* &c. is very common with plants.—^{f11} If your paper on *Drosera* is published, I should be grateful for a copy.—^{f12} Two German papers have been written on subject, but I now forget where; & there is good French paper on Structure, which I daresay you know.^{f13} I was right to be cautious in supposing you in error about *Siphocampylus* (no flowers were enclosed):^{f14} I hope that you will make out whether the pistil presents two *definite* lengths; I shall be astounded if it does.—

I do not fully understand your objections to N. Selection;^{f15} if I do, I presume they would apply with *full* force to, for instance, Birds. Reflect on modification of Arab- Turk Horse with our English Race-Horse.—^{f16}

I have had satisfaction to tell my publisher to send my Journal & Origin to your address.—^{f17}

I suspect with your fertile mind, you would find it far better to experiment on your own choice; but if on reflection you would like to try some which interest me, I shd. be truly delighted, & in this case would write in some detail.^{f18} If you have means, to repeat Gärtner's experiments on vars. of *Verbascum* or on Maize (see *Origin*) such experiments would be preeminently important.—^{f19} I could never get vars. of *Verbascum*.^{f20} I could suggest experiments on Potatoes analogous with case of *Passiflora*; even this case of *Passiflora*, often as it has been repeated, might be with advantage repeated.^{f21}

I have worked like a slave (having counted about 9000 seeds) on *Melastomas* on meaning of the two sets of very different stamens, & as yet have been shamefully beaten, & I now cry for aid.—^{f22}

I could suggest what I believe very good scheme (at least Dr. Hooker thought so) for systematic degeneration of culinary plants & so find out their origin; but this would be laborious & work of years.—^{f23}

I am tired, so pray believe me, yours very faithfully | Ch. Darwin

P.S. I have been thinking that if you do not complete your beginning on the nondimorphic *Primula*, I should like *extremely* to do so & would of course fully acknowledge your work.^{f24} What I shd. do would be to fertilise a dozen or score of flowers with own pollen & another score of flowers with pollen of others, the most different, & count the seed of each capsule.—

Now could you aid me (unless you resolve to do the work yourself) & procure me from any Edinburgh nursery-garden 1/2 a dozen plants (or fewer, if so many could not be got) & 1/2 dozen of any of the other non-dimorphic species. They could be sent in pots in box addressed. "C. Darwin care of Down Postman, Bromley Kent" & I could pay by P.O.— They give or lend me all plants at Kew; but they are very weak in *Primulas*. I am sick of ordering plants at London nurseries; I so often get wrong thing. Could you aid me in this?—

Footnotes

^{f1} The year is provided by the relationship to the letter from John Scott, 6 December [1862].

^{f2} Scott 1862a. See letter from John Scott, 6 December [1862]. CD's annotated copy of this paper is in the Darwin Pamphlet Collection—CUL.

^{f3} Scott 1862a, p. 216.

^{f4} In his paper, Scott aimed, by analysing the nature of fern spores, to shed new light on their 'apparently anomalous properties', and particularly on 'that peculiar facility afforded by spores for the reproduction and perpetuation of any accidental variation of the parts upon which they originate' (Scott 1862a, p. 210). Scott contrasted the reproductive organs in ferns with those in higher plants. Since in the latter the embryo was 'the *modified resultant* of two originally distinct organs' (p. 214), there would, he claimed, 'necessarily be a greater tendency to efface any individual peculiarities of these than would have been the case, had the embryo been the product of a single organ.' See also letter from John Scott, 6 December [1862] and n. 15.

^{f5} Scott 1862a, pp. 217–8. Scott prefaced his remarks by quoting CD's statement (*Origin*, p. 13): 'That at whatever period a peculiarity first appears, it tends to appear in the offspring at a corresponding age, though sometimes earlier.'

f6 Scott 1862a, p. 219.

f7 CD refers to the Botanical Society of Edinburgh (see letters from John Scott, 6 December [1862] and 17 December [1862]).

f8 See letter to J. D. Hooker, 23 June [1862], n. 4.

f9 Having discovered in 1861 that certain species of *Linum* were dimorphic, CD carried out crossing experiments in the summer of 1862 on *L. perenne* and *L. grandiflorum*. According to his 'Journal' (*Correspondence* vol. 10, Appendix II), CD wrote his paper, 'Two forms in species of *Linum*', between 11 and 21 December 1862.

f10 See letter from John Scott, 6 December [1862]. In 1860, CD began to experiment on the sensitivity to various substances of the insectivorous plants, *Drosera rotundifolia* and *Dionaea muscipula* (see *Correspondence* vol. 8). He had hoped to continue and complete the experiments in the summer of 1861, but subsequently decided to postpone them (see *Correspondence* vol. 9, letter to J. D. Hooker, 4 February [1861], and letter to Daniel Oliver, 11 September [1861]). He carried out further experiments in May and September 1862 (see DAR 54: 29–49, 74–5). However, he did not again work extensively on this subject until 1872 (*LL* 3: 322); his findings were published in 1875 as *Insectivorous plants*.

f11 See also letter to J. D. Hooker, 26 September [1862].

f12 Scott read his paper on *Drosera* and *Dionaea* (Scott 1862b) before the Botanical Society of Edinburgh on 11 December 1862. CD cited Scott's paper in *Insectivorous plants*, pp. 1 n.–2 n.

f13 In a brief overview of the literature on insectivorous plants in *Insectivorous plants*, p. 1 n., CD referred to five papers on *Drosera rotundifolia* published by German and French writers before 1862: Milde 1852 and Nitschke 1861a and 1861b (in *Botanische Zeitung*), and Grönland 1855 and Trécul 1855 (in *Annales des Sciences Naturelles (Botanique)*).

f14 See letter to John Scott, 3 December [1862], and letter from John Scott, 6 December [1862]. See also letter from John Scott, 17 December [1862].

f15 See letter from John Scott, 6 December [1862].

f16 CD apparently refers to the fact that an objection to natural selection based on the effect of blending inheritance on incipient varieties, would be particularly telling in cases where individuals paired for life (as with birds). CD had cited the gradual improvement of 'the whole body of English racehorses' by selection and training in *Origin* p. 35, noting that they had come to surpass the parent Arab stock. See also *Correspondence* vol. 10, Appendix VI.

f17 John Murray. See letter to John Scott, 3 December [1862], and letter from John Scott, 6 December [1862].

f18 See letter from John Scott, 6 December [1862].

f19 The references are to *Origin*, pp. 269–71, and Gärtner 1844 and 1849; CD's extensively annotated copies of the latter works are in the Darwin Library–CUL (see *Marginalia* 1: 248–98). CD considered the experiments particularly important because of their bearing on an objection raised against natural

selection by Thomas Henry Huxley (see letter to T. H. Huxley, 18 December [1862] and nn. 8–12, and *Correspondence* vol. 10, Appendix VI).

f20 See, for example, *Correspondence* vol. 9, letters to J. D. Hooker, 28 September [1861], 18 October [1861], 23 October [1861], and 1 November [1861], and this volume, letter to C. C. Babington, 20 January [1862], and letter to W. E. Darwin, 4 [July 1862] and n. 4.

f21 The reference is to the fact that in *Passiflora* it had been found that plants could be ‘much more easily fertilised by the pollen of a distinct species, than with its own’ (*Natural selection*, p. 400; see also *Origin*, p. 251). Scott reported the results from a series of experiments on sterility and hybridisation in *Passiflora*, begun in 1863, in Scott 1864b.

f22 See letter to J. D. Hooker, 14 [October 1862], n. 7.

f23 Hooker discussed this project with CD during a visit to Down House in April 1860 (see *Correspondence* vol. 8, letter to Asa Gray, 25 April [1860]). See also letter from C. W. Crocker, 31 October 1862 and n. 12.

f24 Scott had apparently sent CD an account of his experiments on *Primula* in the missing portion of his letter of [20 November -- 2 December 1862]). After further research, Scott wrote a paper on the subject (Scott 1864c), which CD later cited in *Forms of flowers*.

Letter 8113—Mary Treat to Charles Darwin [f1](#)

20 December 1871

Vineland, New Jersey,

Dec. 20, 1871

Mr. Darwin:

Dear Sir,

Experimenting with *Papilio asterias*, Cramer, I learned to distinguish the sex in the larva state—the female being larger than the male—and this led me to try to control the sex.

My first experiments were a year ago last summer, some three or four hundred miles inland, where I had much better success than I had here last summer near the coast.

The larvae of my first experiment were of the first brood, so that I only had to wait a few days for their final transformation. These larvae fed on two quite dissimilar Umbelliferous plants—the Poison Hemlock (*Conium maculatum*), and the Caraway of the gardens (*Carum Carni*). I could always distinguish the larvae that fed on the Poison Hemlock from those that fed on the Caraway; but with the butterflies there was little or no marked difference in their general appearance.

I noticed that the female larva fed longer than that of the male. So taking several larvae of the same age, I found some specimens were inclined to leave their food several days earlier than others, and these always proved to be males. It then occurred to me to try to induce some of these male larvae to feed longer; so, after they had wandered from their food, and even selected places for their transformations, (of course not *fixed*), if I disturbed them, made them leave their places, and coaxed them with a fresh supply of their favorite food, I could almost invariably induce them to eat from ten days to two weeks longer, when all such ones would be females—If it was a larva that fed on Caraway, I tempted it with the tenderest, and freshest leaves and flowers of the same plant. It was never hungry enough at that stage to be induced to change its food, —though they will change in their earlier stages rather than starve, but evidently they do not like to change even then, and frequently fail to transform when their food is thus changed. They can be induced to change their food to the nearest allied species of plants with less difficulty—

On the other hand, when a larva had become the right size to produce a male, if I cut off its supply of food, even when it was eating greedily, it would wander about perhaps a little longer, as if in search of food, but finally it almost always changed to the chrysalis, and such a chrysalis always produced a male butterfly.

Last summer a fit of sickness prevented my experimenting with the first brood of these butterflies; and with the second brood—although I procured many specimens before the first moult—I was only able to rear about one in ten. An unnamed tiny *Microgaster* was their fatal enemy. But the few larvae that I succeeded with, behaved precisely as those of the previous year. Their food plant was *Archangelica hirsuta*. I have the chrysalids of my last experiment carefully marked, and am fully confident that I know which will produce the different sexes.

Now that I am writing I will give you my observations on *Drosera*, which have escaped the notice of botanists. I had two or three species of these pretty plants growing for window ornaments; and soon saw that *D. longifolia* was a fly—trap of considerable power. The unlucky fly—a common house fly—would

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no sooner be caught by the sticky glands of the leaf, than the blade would at once commence to fold about its victim; it folded from the apex to the stem of the leaf, after the manner of its veneration.

Closer and closer it held the poor fly in its embrace, until it ceased its struggles, when it soon became partly absorbed by the plant —Prof. Gray will give my observations on this plant in his new edition of *"How Plants Grow"*.

I do not know that my experiments can be of use to you, but I thought perhaps they might interest you. A life time of observation and experiments could not repay the debt of gratitude we owe you.

Yours most respectfully, Mrs. Mary Treat.

Footnotes

[f1](#) This letter is not yet published in the edition of The correspondence of Charles Darwin; it will appear in volume 19, due to be published in 2011. The text is being made available here ahead of print publication as part of the 'Darwin and Gender' project funded by The Bonita Trust.

Letter 9005—Darwin to Mary Treat

12 Aug 1873

Down, | Beckenh am, Kent.

Aug. 12. 1873

Dear Madam

I am very much obliged to you for having so kindly sent me an account of *Drosera filiform* is.

Your statements will be very useful to me in my short account of this species. I am familiar with what you state about *D. longifolia* & *rotundifolia*.

I have just lately been working hard for the last 2 months on the latter species; & before long shall draw up an account of their digestive powers & action under various stimulants. If I were in your place I should be afraid to publish the statement about the *Drosera* bending towards flies or meat which they did not touch; unless I had tried the experiment many times, under the most rigorous precautions; for I am convinced that no botanist would believe the statement unless all the precautions taken were described in detail.

With my best thanks, I remain dear Madam | yours faithfully & obliged | Ch. Darwin

Whenever my little book is published I will do myself the pleasure to send you a copy; it will be on the genera *Drosera*, *Dionaea* & *Drosophyllum* with the republication of my paper on Climbing Plants.

Footnotes

^{f1} This letter is not yet published in the edition of *The correspondence of Charles Darwin*; it will appear in volume 21. The text is being made available here ahead of print publication as part of the 'Darwin and Gender' project funded by The Bonita Trust.