

THE CORRESPONDENCE OF CHARLES DARWIN
1875

From Francis Darwin [1875²]¹

Down

My dear Father

I have had two mornings work at *Drosera* but without success. I got into a very good way of doing it but the plants seem sluggish. The first morning 26" was the quickest—counting from the beginning of contact of the drop of meat infusion. The second morning they were more sluggish still; ammonia is not so nice to work with as meat; as with meat one is not afraid of evaporation making ones drop weak if one waits a bit—² The only thing of the slightest interest is that contact for 1' produced movement in just the same time counting from the beginning as contact for 4" did; the tentacles were on the same leaf; but of course one experiment isnt much good— I shall try again, because now I can do it accurately—

My frog preparations are pretty good—³

I am very glad mother & you are keeping well.⁴

Thank you for writing about the pamphlets, we have cut up a lot & sorted some— it will be done when you come home—

Yr affec son | F Darwin

DAR 274.1: 28

¹ The year is conjectured from an archivist's mark on the letter.

² Francis was writing an article on aggregation in the tentacles of *Drosera rotundifolia* (common or round-leaved sundew) for the July 1876 issue of the *Quarterly Journal of Microscopical Science* (F. Darwin 1876b).

³ Francis was working on frogs for his article in the October issue of *Journal of Anatomy and Physiology* (F. Darwin 1875a).

⁴ In 1875, CD and Emma were away from Down from 31 March to 12 April, 3 June to 6 July, 28 August to 11 September, and 10 to 20 December. *Insectivorous plants*, which discussed *Drosera* at length, was published on 2 July. (CD's 'Journal' (Appendix II).)

To F. J. Cohn 1 January 1875

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.

Jan 1. 75

My dear Sir

According to our English fashion "I wish you many happy returns of this day". I write now to ask you whether I might copy two of your drawings of *Aldrovanda*, marked by a red cross on the sketch which you were so kind as to send me. I should

like to add one of the quadrifid trichomes if these have been drawn by you.² I give only woodcuts in my forthcoming work, and I should of course say that they were copied from you. I have not described *Aldrovanda*, & refer my readers to your work; but my few remarks would hardly be intelligible without these drawings.³ I am going to send my other drawings to the woodcut-engraver immediately; & if you grant me permission, & if I receive your work in time, I will have the drawings copied.⁴ It would therefore be a great assistance to me if you could send me a proof of the plate.

I hope that you will excuse my begging this favour and I remain | dear Sir | with much respect | Yours faithfully | Ch. Darwin

LS(A)
DAR 185: 97

¹ Cohn had sent two small sketches of *Aldrovanda* (the waterwheel plant) and a galley proof of his forthcoming article on *Aldrovanda* (Cohn 1875a) with his letter of 4 October 1874 (*Correspondence* vol. 22). CD's annotated proof copy of Cohn 1875a (not including illustrations) is in DAR 58.2: 35–43; the sketches have not been found. In *Insectivorous plants*, p. 323, CD reproduced a woodcut of a whorl of leaves of *Aldrovanda* 'from Prof. Cohn'; it is from fig. 5 in Cohn 1875a. *Aldrovanda* is carnivorous, and has only one extant species, *A. vesiculosa*.
² On the quadrifid processes of *Aldrovanda*, see *Insectivorous plants*, pp. 323–4. There is no illustration of a quadrifid process.
³ CD referred to Cohn 1875a in *Insectivorous plants*, p. 321 n.
⁴ The woodcuts for *Insectivorous plants* were made by James Davis Cooper, from drawings mostly by George Howard and Francis Darwin (*Insectivorous plants*, p. 3 n.).

To Linnean Society 1 January [1875]¹

Jan 1st

Gentlemen,

I hope that you will permit me to republish in a corrected form my paper on Climbing Plants which appears in the 9th vol (1865) of your Journal.² I wish it the paper appear as a second Part to a new work, which I shall soon send to press.—³ If you grant my request, I further hope that you will be so good as to allow me to use the 13 woodblocks illustrating the paper; & in this case I request that they may be sent to M^r Murray of Albermarle St., marked as for my intended, volume,

Gentlemen | Yours obliged & obed servt | C. D.

To the Pres. & Council of | Linn— Soc.

ADraftS
DAR 97: C12

¹ The year is established by the reference to *Climbing plants* (see n. 2, below).
² 'Climbing plants', CD's paper in the *Journal of the Linnean Society (Botany)*, was also published by Longman in 1865 (*Climbing plants*). *Climbing plants* 2d ed. was published in November 1875 (letter from R. F. Cooke, 25 October 1875).

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³ CD originally planned to publish the material on climbing plants as part of his book *Insectivorous plants*, but later decided to publish *Climbing plants* 2d ed. as a separate volume (see letter from John Murray, 9 April [1875]).

From Daniel Oliver 2 January 1875

Royal Gardens Kew
2 Jan. 1875.

My dear Mr Darwin

The generic name *Genlisea* must of course be maintained for the Utricularioid plants with 5-merous calyx of which we have one species from So. Africa & a few from Brazil. Those of which I sent you fragments under that name you keep as *Genlisea*.¹ From your letter this morning I take it you have already seen Warming's paper on *Utricularia* & *Genlisea*²

Ever very sincerely with all best N. Year wishes,— | Yours D. Oliver

DAR 58.1: 115

CD ANNOTATION

Top of letter: '**Merely** to say that fragment sent under name of *Genlisea* must be so called.' *ink*

¹ Oliver had sent CD fragments of *Genlisea* (the corkscrew plant) with his letter to 24 December 1874 (*Correspondence* vol. 22). Oliver had earlier pointed out to CD that *Genlisea* was 'simply *Utricularia* with 5-lobed calyx' (*ibid.*, letter from Daniel Oliver, 19 December 1874). 5-merous: pentamerous, having five parts. Most species of *Utricularia* (bladderwort) have two calyx lobes, while some have four.

² CD's letter to Oliver has not been found. CD cited Eugenius Warming's paper on *Genlisea* and *Utricularia* (Warming 1874) in *Insectivorous plants*, pp. 397 n. and 446 and n. For CD's reply, see the letter to J. D. Hooker, 3 January [1875].

To J. D. Hooker 3 January [1875]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan 3^d Sunday morning

My dear Hooker

I have not heard from Mr Mivart & I do not think that there is now a chance of hearing.—²

I shall be anxious to hear what you finally determine to do, & I will not write till I hear from you.— If you consult Allman, perhaps he will not take so strong a view as you do, influenced, I do not doubt by your kind feelings towards me.³ Whatever anyone else may think, I am convinced that the man is a false hypocrite to the core. All this affair must have cost you much time & what is even worse much annoyance.— As I said in a former note, when I told Huxley & you about it, it never for an instant occurred to me that you would take up the affair in so earnest & sympathetic a manner.⁴ If I had thought so, I ought, perhaps, to have refrained from mentioning it, but I doubt whether I sh^d. have had sufficient self-restraint.

I hope before very long that you may hear about your Assist. Sec?—⁵
Yours affectionately | Ch. Darwin
I have just been reading in Nature the first part of your Royal Address, & I have been particularly glad to learn something about the R. Soc^y: it was all new to me.—⁶
If you can remember, thank Oliver for note received to day about Warming; but I have the pamphlet to which he refers. He sent it to me.—⁷

DAR 95: 363–4

¹ The year is established by the relationship between this letter and the letter from Daniel Oliver, 2 January 1875.
² CD and Hooker had been debating what action to take about an anonymous attack on George Howard Darwin by St George Jackson Mivart in the *Quarterly Review* ([Mivart] 1874b, p. 70). Mivart had acknowledged to Thomas Henry Huxley confidentially that he was the author of the article. See *Correspondence* vol. 22, Appendix V.
³ George James Allman was the president of the Linnean Society of London. Hooker had suggested that Mivart should be removed from his position as secretary of the society (see *Correspondence* vol. 22, letter from J. D. Hooker, 29 December 1874).
⁴ Huxley had learned about the Mivart affair and communicated with Mivart about it in December 1874 (see *Correspondence* vol. 22).
⁵ CD had been helping Hooker in his campaign to have an assistant appointed to him at the Royal Botanic Gardens, Kew (see *Correspondence* vol. 22, letter to J. D. Hooker, 2 December [1874]).
⁶ CD refers to Hooker's presidential address to the Royal Society of London, made on 30 November 1874 (J. D. Hooker 1874c); extracts from it were reprinted in *Nature*, 31 December 1874, pp. 175–8, and 7 January 1875, pp. 196–9, under the heading 'The present condition of the Royal Society'.
⁷ See letter from Daniel Oliver, 2 January 1875. CD refers to Eugenius Warming; there is an annotated copy of Warming 1874 in the Darwin Pamphlet Collection–CUL.

From J. D. Hooker 3 January [1875]¹

Kew
Jany 3/74.

Dear Darwin

I have no intention of consulting Allman—but must Huxley, after his letter.² I have seen the Academy, & do not like it— It is not quite right to make the Review of Haeckel little else but an attack on the Quarterly—³ It is not as if he had brought the Quarterly in incidentally. Further I do not think that it will be quite understood by any outsider.— No doubt it is amazingly able trenchant & *drastic*.
I am writing for your Drosophyllum now it is mild.⁴
Every one (White tells me) is glad of the Address.⁵
Ever yrs aff | J D Hooker

DAR 104: 1

¹ The year is established by the relationship between this letter and the letter from J. D. Hooker, 29 December 1874 (*Correspondence* vol. 22). Hooker wrote '74' in error.
² CD, Hooker, and Thomas Henry Huxley had been debating what action to take about an anonymous attack on George Howard Darwin by St George Jackson Mivart in the *Quarterly Review* ([Mivart] 1874,

January 1875

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p. 70). In his letter of 29 December 1874 (*Correspondence* vol. 22), Hooker had suggested that Mivart should be removed from his post as secretary of the Linnean Society, and that George James Allman, the president, would have to be informed before any steps were taken. Hooker may refer to Huxley's letter to CD of 23 December 1874 (*ibid.*), in which Huxley said that he hoped neither CD nor Hooker would do anything unless Mivart took the initiative; CD had forwarded the letter to Hooker with his letter of 24 December [1874] (*ibid.*).

³ Huxley had lambasted 'the anonymous Reviewer' in a passage of his review of Ernst Haeckel's book *Anthropogenie* (Haeckel 1874) in the *Academy*, 2 January 1875, pp. 16 and 17:

Possessed by a blind animosity against all things Darwinian, the writer of this paper [[Mivart] 1874] outrages decency by insinuations against Mr. George Darwin, well calculated to damage a little-known man with the public, though they sound droll enough to those who are acquainted with my able and excellent friend's somewhat ascetic habits.

...

What is not doubtful is the fact that misrepresentation and falsification are the favourite weapons of Jesuitical Rome.

⁴ Hooker had offered to send CD a specimen of the insectivorous plant *Drosophyllum lusitanicum* (Portuguese sundew or dewy pine) from Edinburgh (*Correspondence* vol. 22, letter from J. D. Hooker, 21 December 1874).

⁵ Hooker's presidential address to the Royal Society of London was delivered on 30 November 1874 (J. D. Hooker 1874c); extracts from it were reprinted in *Nature*, 31 December 1874, pp. 175–8, and 7 January 1875, pp. 196–9, under the heading 'The present condition of the Royal Society'. White: Walter White, assistant secretary and librarian of the society.

To H. E. Litchfield 4 January [1875]¹

Jan 4th

My dear H.

Your letter has led me to think over vivisection² (I wish some new word like Anæ-section could be invented) for some hours, & I will jot down my conclusions, which will appear very unsatisfactory to you.—I have long thought physiology one of the grandest of sciences, sure sooner, or more probably later, greatly to benefit mankind; but judging from all other sciences, the benefits will accrue only indirectly in the search for abstract truth. It is certain that physiology can progress only by experiments on living animals—Therefore the proposal to limit research to points of which we can now see the bearings in regard to health &c, I look at as puerile. I thought at first it w^d be good to limit vivisection to public laboratories; but I have heard only of those in London & Cambridge & I think Oxford; but probably there may be a few others. Therefore only men living in a few great towns could carry on investigation, & this I sh^d consider a great evil. If private men, were permitted to work in their own Houses, & required a license, I do not see who is to determine whether any particular man sh^d. receive one. It is young unknown men who are the most likely to do good work.—I w^d gladly punish severely anyone who operated on an animal not rendered insensible, if the experiment made this possible; but here again I do not see that a magistrate or jury c^d. possibly determine such a point. Therefore I conclude, if (as is likely) some experiments have been tried too often, or anæsthetics have not been used, when they could be, the cure *must* be in the improvement of humanitarian feelings.—

Under this point of view I have rejoiced at the present agitation.³ If stringent laws are passed, & this is likely seeing how unscientific the H. of Commons is & that the gentlemen of England are humane, as long as their sports are not considered, which entail a hundred or thousand fold more suffering than the experiments of physiologists— if such laws are passed, the result will assuredly be that physiology which has been until within the last few years at a stand still in England, will languish or quite cease. It will then be carried on solely on the continent; & there will be so many the fewer workers on this grand subject, & this I sh^d greatly regret.—

By the way F. Balfour, who has worked for 2 or 3 years in the Lab. at Cambridge, declares to George that he has never seen an experiment, except with animals rendered insensible.⁴ No doubt the names of Doctors will have great weight with the H. of Commons, but very many practioners neither know nor care anything about the progress of knowledge.

I cannot at present see my way to sign any petition, without hearing what physiologists thought w^d be its effect & then judging for myself. I certainly could not sign the paper sent me by Miss Cobbe, with its monstrous (as it seems to me) attack on Virchow for experimenting on the Trichinae.—⁵

I am tired & so no more. | Yours affectionately | Ch Darwin

P.S. After what I have said about Balfour I must add that I have this minute heard from Frank, that Klein in the case of frogs does not always use anæsthetics, when he could do so & this is atrocious.⁶

DAR 185: 36

¹ The year is established by the reference to Frances Power Cobbe's memorial against vivisection (see n. 3, below).

² Henrietta's letter has not been found.

³ In December 1874, Frances Power Cobbe had begun to circulate a memorial to be delivered to the Royal Society for the Prevention of Cruelty to Animals asking it to bring a bill before Parliament to restrict vivisection (Cobbe 1904, pp. 628–9). The principal paragraphs of the memorial are reproduced in Cobbe 1904, pp. 633–5. The memorial was presented on 25 January 1875 (Cobbe 1904, p. 635). See also *The Times*, 26 January 1875, p. 7.

⁴ Francis Maitland Balfour and George Howard Darwin were both fellows of Trinity College, Cambridge.

⁵ The memorial mentioned Rudolf Carl Virchow's experiments infecting rabbits with trichiniasis (also called trichinosis), a parasitic disease (Cobbe 1904, p. 634).

⁶ Francis Darwin had become acquainted with Edward Emanuel Klein while he was studying medicine in London (see, for example, *Correspondence* vol. 21, letter from Francis Darwin, [1873]).

To F. B. Goodacre 5 January 1875

Down, Beckenham, Kent.
Jan 5 1875

Dear Sir

I am much obliged for your kind note with the extracts in which you do me much honour.¹ I shall be pleased to have your essay dedicated to me; but I fear that I shall

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not be able to give any assistance towards your excellent scheme as owing to the state of my health I am forced to live a very retired life.²

With my thanks I remain dear Sir | Yours very faithfully | Ch. Darwin

Copy
DAR 221.4: 201

¹ Goodacre's note and extracts have not been found.
² Goodacre dedicated his essay *Hemerozoology* (Goodacre 1875) to CD. There is a lightly annotated copy in the Darwin Pamphlet Collection—CUL; see also letter to F. B. Goodacre, 20 February 1875. In his essay, Goodacre included a plan for establishing a museum of domestic animals; he had written to CD about this in his letter of 7 February 1873 (*Correspondence* vol. 21).

From J. D. Hooker 5 January 1875

Athenæum Club | Pall Mall S.W.
Jany 5/75.

Dear Darwin

Huxley dissuades me so strenuously from writing to Mivart, on the grounds of his being a Fellow of the R.S., & I it's President, that I suppose I must submit. I must confess that I cannot well see why the Secretary may & the President may not, to which the answer is that the Secretary's having done it first,—if right—, renders the action of the President secondary—and if not right for the Secretary, it is still less so for the President.¹

I must confess that I do not at all like the idea of the Presidentship limiting action in such a matter.— My letter is written, & couched in a strain that is widely different from Huxley's, but I hesitate to send it if it would at all compromise me in my official position.² I shall hold my hand till I hear what Bentham says:³ meanwhile I must give Mivart the cold shoulder, if I should happen to meet him.

Ever aff yrs | Jos D Hooker

DAR 104: 2–3

¹ Hooker had wanted to write to St George Jackson Mivart about Mivart's attack on George Howard Darwin's paper on marriage ([Mivart] 1874, p. 70, G. H. Darwin 1873b; see *Correspondence* vol. 22, letter from J. D. Hooker, 21 December 1874). Thomas Henry Huxley was a secretary of the Royal Society of London.
² Huxley had circulated a copy of his letter to Mivart to CD and Hooker; see *Correspondence* vol. 22, letter from T. H. Huxley, 23 December 1874, enclosure.
³ George Bentham, who worked on botany at Kew, had legal training and was a member of the Royal Society (*ODNB*).

To Friedrich Max Müller 5 January 1875

Down, Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan. 5th 1875

My dear Sir

I have just read the few first pages of your article in the Contemporary, & I hope that you will permit me to say that neither I nor my son ever supposed that you were

the author of the Review in the Quarterly.—¹ You are about the last man in England to whom I sh^d have attributed such a review. I know that it was written by M^r Mivart, and the utterly false & base statements contained in it in relation to my son, are worthy of the man.² My son wishes me to add that you have imputed to him a good many criticisms, that are in reality Prof. Whitney's, & is sorry that you sh^d think that he ventured to criticize your writings on his own account.³

I remain | My dear Sir | Yours sincerely | Ch. Darwin

John Wilson (dealer) catalogue 89 (October 2002)

¹ Max Müller had written a paper titled 'My reply to Mr. Darwin' (Max Müller 1875) for the January 1875 issue of the *Contemporary Review*, in answer to George Howard Darwin's paper defending CD's views on language (G. H. Darwin 1874). Müller noted that George's defence had been inspired partly by comments in an anonymous article in the *Quarterly Review* ([Mivart] 1874), and denied that he was the author of that article.
² On St George Jackson Mivart's review and CD's response to it, see *Correspondence* vol. 22, Appendix V.
³ G. H. Darwin 1874 was a discussion of William Dwight Whitney's essay review of Friedrich Max Müller's 'Lectures on Mr. Darwin's philosophy of language' (Whitney 1874). CD had attempted but failed to have Whitney 1874 republished in the *Contemporary Review* (letter from J. T. Knowles, 4 August 1874).

From Joseph Fayrer 6 January 1875

16 Granville Place,
6 Jan'y 1875

Dear M^r Darwin

I have the pleasure of enclosing the rough notes of some experiments recently made by D^r Brunton & myself on the influence of snake poison (cobra) on ciliary action, and on the Valisneria¹ The results are not very definite, but they may interest you. Pray do what you think best with them.²

I hope you are well and have not been inconvenienced by the very inclement weather we have lately had.³

We have just sent in our third and concluding paper on the physiological action of Snake poison to the Royal Society So you will, I hope, soon see it in the proceedings.⁴

Believe me | Your's very truly | J. Fayrer

C. Darwin Esq^r F.R.S

[Enclosure]

The following experiments were made at the suggestion of Mr C Darwin with the object of testing the influence of snake poison on ciliary action, especially in reference to its comparative action on vegetable protoplasm—as will be seen by his remarks.

June 29th 1874

Influence of Cobra poison Ciliary action.

Expert. 1.

Ciliated epithelium from the frog's mouth was treated with the standard watery solution of Cobra poison & examined under the microscope—

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At 1:35 when examined the action of the ciliae was vigorous
 at 1:45 It was much diminished
 at 1:55 It had entirely ceased

Expert 2^d

Ciliated epithelium placed under microscope—one part treated with water—the other with the poisoned solution

at 2:10 ciliary motion vigorous in both perhaps more so in that subjected to the poisoned solution

2:18. Non-poisoned ciliae active

Poisoned ciliae very feeble

2:20. non-poisoned ciliae still active

Poisoned ciliae very feeble

2:24 Non poisoned ciliae active

Poisoned— " very languid

2:30 Non poisoned ciliae still active

2:30 Poisoned have entirely ceased to act

It is evident from this that the poison first stimulates & then destroys the activity of the ciliary action.

Expert 5— 14 Aug

Frog's blood placed in salt solution .75 per cent at 1.25— on warm stage—and then subjected to the action of Cobra poison.

At first the amoeboid movements of white corpuscles went on vigorously—at 2-PM they had ceased or very nearly so in all that appeared in the field

2:30 all movement had entirely ceased.

The red corpuscles seemed more flattened, the nuclei more visible and the edges better defined assuming a pointed & more oval form, than usual.



Aug 25— 1874

Newts blood examined. under $\frac{1}{8}$ th object glass on hot stage white corpuscles moving slowly. C^{ob} poison applied but no perceptible change observed

June 29th. 1874—

Action of Cobra poison on muscle

Expert 1.

A standard solution of cobra poison: .03 gramme to 4.6 cubic centimetres of water was prepared.—

PM 1·25 The gastrocnemius of a frog was separated and immersed in this solution in a watch glass—it immediately contracted considerably

1·30 The muscle contracts with current at 11

1·45 The muscle has lost its irritability does not respond to the strongest current—

Expert 2^d

At the same time, 1·25 the gastrocnemius from the other leg of the same frog immersed in water. Did not immediately contract like that placed in the poisoned solution

1·30 Contracts strongly to current at 15.— more than the poisoned muscle at 11· (at same moment)

1·45. Contracts distinctly at 11— whilst the poisoned muscle has lost all irritability.

From this it is evident that the poison first stimulates the muscular fibre to contract, but rapidly afterwards destroys its irritability.

Expert 2^d

The gastrocnemii of a frog were again treated in the same way as in the previous experiment with precisely the same results

28 June.

I made several experiments with cobra poison on ciliated epithelium of frogs mouth & found that it at first accelerated, then destroyed the action of the ciliae

Nov^r. 1874

Expt. XXX A little cobra poison dissolved in water added to a little water containing some of the cells scraped from the mantle of Fresh Water Mussel. Among these was a large ciliated cell which before the addition of the poison had been moving slowly although the cilia were moving actively. Immediately after the addition of the poison the cell began to spin round on its own axis with extraordinary rapidity. In about 3 minutes its motions began to be languid, the ciliary motion ceased, the cell itself elongated contracted & then slowly resumed its former shape & became perfectly motionless.

E XXXI. A little water from the interior of F Water Mussel & containing two specimens of paramœcium in active motion was examined. They were rotating with great rapidity. A little cobra poison diluted with water was added. Three minutes after the addition one was discovered with both the cilia & cell body perfectly still. The cilia of the other were still but the cell body was contracted. In about half a minute more it expanded to its normal size & then remained perfectly still.

E XXXII A piece taken from the mantle of FW. Mussel was put on a slide & examined at the end of about half an hour. Active ciliary motion could be observed both in the fringe of the mantle itself & in several specimens of Volvox.⁵ A little dilute poison was added. At first the ciliary motion seemed increased but in about 2