

Correspondence Project

Excerpt

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THE CORRESPONDENCE OF CHARLES DARWIN 1882

To G. H. Darwin [1882?]¹

My dear G.

The enclosed has arrived this morning. I will write card to M^r Hill² saying that his letter has been forwarded to you, & that you will send it to the Solicitor who manages the business or the agent.—

But will an agent take any notice of another agent?

Yours | C. D

As M^r Hill has written twice, would it not be fair to give him the Solicitor's address?

DAR 210.1: 116

¹ The date is conjectured from the letter's position in a roughly date-ordered series in the archive.

² Mr Hill has not been identified.

From Fritz Müller 1 January 1882

Blumenau, S^a Catharina, Brazil
January 1st 1882

My dear Sir!

I received last week your kind letter of Novbr. 13, in which you ask me the name of the plant, of which I sent you seeds some months ago. I must confess, that I do not remember well, what seeds they were, but I think they were those of our sensitive Mimosa; if so, you will see it as soon as the first leaves appear.¹

In your "Movements of plants" (pg. 308) you say, that the cotyledons of *Bauhinia* (*grandiflora*) would probably have closed completely at night, if the seedlings had been kept in a warmer place, and to me also this appeared to be most probably.² Now we have presently very hot weather, (about 25°C. at night, 30°C or more at noon), but the cotyledons of some very young seedlings of *Bauhinia grandiflora* do not sleep at all!— In *Bauhinia brasiliensis* I observed lately a curious fact; in bright sunshine the two halves of the leaves rise up more or less, as they do also at night; now I met with a plant, which, after having been exposed for hours to the rays of the sun, had suddenly been overshadowed by a large tree and in this plant the two

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halves of the leaves had descended beneath the horizontal plan, which they use to form during the day, forming with the horizon angles varying from about 15° to nearly 45° .³

I enclosed some fresh seeds of a long-styled plant of *Pontederia crassipes*, which I had legitimately fertilised with pollen from the long stamens of mid-styled plants.⁴

Wishing you a very happy new year I am | dear Sir with the deepest respect | Yours very sincerely | Friz Müller

DAR 106: C19

¹ The seeds were probably of *Mimosa pudica* (shame plant); Müller had sent flower heads with what CD described as 'brown seeds somewhat sculptured on their sides' (*Correspondence* vol. 29, letter to Fritz Müller, 13 November 1881).

² See *Movement in plants*, p. 308; CD had described the plant as a 'Bauhinia from St. Catharina in Brazil'. Müller evidently knew that the species was *Bauhinia grandiflora* (a synonym of *B. aculeata* subsp. *grandiflora*).

³ CD had reported, based on information from Müller (probably contained in a now missing section of the letter from Fritz Müller, 28 February 1881, *Correspondence* vol. 29), that the leaves of *Bauhinia brasiliensis* did not sleep (see *ibid.*, letter to *Nature*, 14 April [1881]). The movement of the leaves upward was an example of movement CD had called paraheliotropism: movement of leaves during the day to reduce intense illumination (*Movement in plants*, p. 419). The downward movement was more typical of sleep (nyctitropic) movement. Müller discussed the movement of leaves in a brief notice in *Kosmos*, May 1882 (F. Müller 1882).

⁴ Müller had written to CD about heterostyly in *Pontederia crassipes* (a synonym of *Eichhornia crassipes*) and sent flowers in a now missing letter of 2 December 1881 (see letter to Fritz Müller, 4 January 1882). CD referred to crosses made using pollen of the same form of flower in dimorphic or trimorphic plant species as illegitimate, and those fertilised by pollen of a different form as legitimate (see 'Three forms of *Lythrum salicaria*', p. 186).

To G. J. Romanes 1 January [1882]¹

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

Jan 1st 1881

My dear Romanes

I send the M.S. but as far as I can judge by just skimming it, it will be of no use to you.— It seems to bear on transitional forms.² I feel sure that I have other & better cases, but I cannot remember where to look to.—

I sh^d. have written to you in a few days on the following case. The Baron de Villa Franca wrote to me from Brazil about 2 years ago, describing new vars. of sugar-cane which he had raised by planting 2 old varieties in apposition.— I believe (but my memory is very faulty) that I wrote that I c^d not believe in such a result & attributed the new varieties to the soil &c.—³ I believe that I did not understand what he meant by apposition. Yesterday a packet of M.S. arrived from the Brazilian Legation, with a letter in French from D^r Glass, Director of the Botanic Garden,⁴ describing fully how he first attempted grafting vars. of Sugar Cane in various ways & always failed, & then split stems of 2 varieties bound them together & planted them, & thus raised some new & very valuable varieties, which like crossed plants

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seem to grow with extra vigour, are constant & apparently partake of the characters of the 2 varieties. The Baron, also, sends me an attested copy from a number of Brazilian cultivators of the success of this plan of raising new varieties.— I am not sure whether the B. Legation wishes me to return the Documents, but if I do not hear in 3 or 4 days that they must be returned, they shall be sent to you, for they seem to me well deserving your consideration.⁵ Perhaps if I had been contented with my hyacinth bulbs being merely bound together without any true adhesion or rather growth together, I sh^d have succeeded like the old Dutch-man.—⁶

There is a deal of superfluous verbiage in the documents, but I have marked with pencil where the important part begins.— The attestations are in duplicate. Now after reading them will you give me your opinion whether the main parts are worthy of publication in *Nature*: I am inclined to think so, & it is good to encourage science in out of the way parts of the world. Keep this note till you receive the documents, or hear from me.— I wonder whether 2 vars. of wheat c^d be similarly treated? no, I suppose not from the want of lateral buds.—

I was *extremely* interested by your abstract on suicide.—⁷

Yours very sincerely | Ch. Darwin

I got the other day the Dec. No^r of the 19th Century with your Article,⁸ but one thing has come so quickly on the back of another that I have not yet got time to read it quietly.—

P.S. I have just had a note from Grant Allen, calling my attention to capital fact about Sexual Selection in Voyage of the Vega Vol. 2 p. 97.⁹

American Philosophical Society (Mss.B.D25.609)

¹ The year is established by the relationship between this letter and the letter to G. J. Romanes, 6 January 1882. CD wrote '1881' in error.

² The manuscript has not been identified.

³ Ignacio Francisco Silveira da Motta, baron de Vila Franca, was a Brazilian politician and farmer. The observations on new varieties of sugar cane produced by 'apposition' (grafting) had been enclosed in the letter from Arthur de Souza Corrêa, 20 October 1880 (*Correspondence* vol. 28). CD's reply to the 1880 communication has not been found.

⁴ 'Dr Glass' was Auguste François Marie Glazieu; his letter has not been found. The packet was enclosed with the letter from Arthur de Souza Corrêa, 28 December 1881 (*Correspondence* vol. 29); however, none of the enclosures have been found.

⁵ Romanes had performed extensive grafting experiments on root vegetables in an effort to produce hybrids; the experiments were designed to test CD's hypothesis of pangenesis (see, for example, *Correspondence* vol. 23, letter from G. J. Romanes, 14 January 1875). For more on the production of sugar cane by graft hybrids, see 'Grafting sugar cane', *Bulletin of Miscellaneous Information (Royal Botanic Gardens, Kew)* 127 (1897): 221–3.

⁶ CD had discussed claims that hyacinths had been grafted by joining two half-bulbs of different colours together, and that the colours sometimes blended, in *Variation* 1: 395. 'Succeeding like the old Dutchman' may refer to a case of hyacinth grafting described in 'an old French Book, published in Amsterdam' (Saint-Simon 1768); see *Correspondence* vol. 11, letter to Thomas Rivers, 7 January [1863]. There are a few undated notes on experiments with feather hyacinth and cauliflower, one of which mentions cutting hyacinth in two, in DAR 206: 17–18.

⁷ Romanes's review of *Suicide: an essay on comparative moral statistics* (Morselli 1881) was published in *Nature*, 29 December 1881, pp. 193–6.

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⁸ Romanes did not publish in the December 1881 issue of *Nineteenth Century*; CD probably means the article ‘The scientific evidence of organic evolution’, which appeared in the December 1881 issue of *Fortnightly Review* (Romanes 1881); a copy is in the Darwin Pamphlet Collection–CUL.

⁹ The letter from Grant Allen has not been found; see, however, the letter to Grant Allen, 2 January 1882. The Vega expedition (1878–80) was a Swedish research expedition that explored the polar sea above Siberia; *The voyage of the Vega round Asia and Europe* (Nordenskiöld 1881, 2: 97) describes a Scotch collie from the expedition that was preferred by a female to other local dogs kept by the Chukchi people along the Behring Strait.

To Grant Allen 2 January 1882

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

Jan 2^d. 1882

My dear Sir

I thank you for sending me the Cornhill, as your article has interested me much.—¹ Many years ago I thought it highly probable that petals were in all cases transformed stamens. I forget (excepting the water-lily) what made me think so; but I am sure that your evolutionary argument never occurred to me, as it is too striking & apparently valid ever to be forgotten.—²

I cannot help doubting about petals being naturally yellow: I speak only from vague memory, but I think that the filaments are generally white or almost white, & surely it is the filament which is developed into the petal.³ I remember some fine purple & bright yellow filaments, but these seemed to me to serve by adding colour to the whole flower. Is it not the pollen alone which renders most stamens yellow at a cursory glance? You may possibly like to hear that I have described cases (& others have been described) when an *excessively* poor soil has rendered a flower double. I can hardly doubt that any great change of conditions (which has so strong a tendency to cause sterility) tends to render a flower double.—⁴ Close interbreeding has a slight tendency in this direction, as has according Gärtner, a hybrid origin.—⁵

With many thanks for the pleasure which your article has given me, I remain | Yours sincerely | Ch. Darwin

I suppose that you know H. Müllers Alpen-Blumen, as it contains much about colour of flowers & orders of visting insects.⁶ I much doubt Wallace’s generalisation about much modified parts being splendidly coloured, except in so far that both have been acted on by the same cause, viz sexual selection.—⁷

That is an excellent case in the Voyage of the Vega, which I am reading, but have not yet got so far.⁸ In former times it w^d have been worth its weight in gold to me.—

Cleveland Health Sciences Library (Robert M. Stecher collection)

¹ The letter from Allen has not been found. He sent his article ‘The daisy’s pedigree’, which appeared in the August 1881 issue of *Cornhill Magazine* (Allen 1881).

² Allen wrote: ‘petals ... are merely specialised stamens, which have given up their original function of forming pollen, in order to adopt the function of attracting insects’ (Allen 1881, p. 175).

³ Allen claimed that the inner florets of the daisy evolved through a flattening and lengthening of the yellow corolla, and that the first rays or petals would also have been yellow (see Allen 1881, p. 180). CD briefly discussed petaloid stamens (or filaments) in *Orchids*, pp. 294 and 303, noting that in the

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Marantaceae, even fertile stamens are sometimes petaloid. See also *Correspondence* vol. 9, letter to J. D. Hooker, 10 November [1861].

- ⁴ In a letter to *Gardeners' Chronicle*, [late August 1843] (*Correspondence* vol. 2; *Shorter publications*, pp. 165–6), CD had discussed cases of double flowers appearing in the poorest soil, noting that the origin of double flowers had often been attributed to excess food: 'Is it, then, too bold a theory to suppose that all double flowers are first rendered by some change in their natural condition, to a certain degree, sterile; and that their vessels being charged with organizable matter in excess, (which would be greatly formed by high cultivation,) it is converted into petals ...?' For more on double flowers, see *Variation* 2: 167–8, 171–2, 200.
- ⁵ Karl Friedrich von Gärtner; CD annotated the discussion of double flowers in his copy of Gärtner 1849, pp. 567–9 (see *Marginalia* 1: 289). By 'close interbreeding', CD meant plants fertilised with their own pollen (see *Variation* 2: 127).
- ⁶ Hermann Müller gave many examples of insects attracted to flowers of different colours in *Alpenblumen, ihre Befruchtung durch Insekten: und ihre Anpassungen an dieselben* (Alpine flowers, their fertilisation through insect agency and adaptations for this; H. Müller 1881, pp. 479–533).
- ⁷ Alfred Russel Wallace had been critical of CD's theory of sexual selection and had presented various alternatives, such as protective mimicry and concealment; in males, he argued, bright colours were a sign of vitality, whereas females were often less conspicuous for the sake of protection (see A. R. Wallace 1878, pp. 217–18, *Correspondence* vol. 15, letter from A. R. Wallace, 26 April [1867], *Correspondence* vol. 25, letter from A. R. Wallace, 23 July 1877).
- ⁸ *The voyage of the Vega round Asia and Europe* (Nordenskiöld 1881, 2: 97). On the case of sexual selection, see the letter to G. J. Romanes, 1 January [1882] and n. 9.

To V. O. Kovalevsky 2 January [1882]¹

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Jan 2^d 1881.

My dear Sir

I thank you for the Photograph & your kind new year wishes, which I very heartily return.² I hope that your [*illeg*] affairs prosper, & I am well assured that you deserve that they sh^d prosper. — As for myself I am fairly well, but feel very old with failing strength.

My dear Sir | Yours sincerely | Ch. Darwin

Postmark: JA 2 | 82

Smithsonian Libraries and Archives (Dibner Library of the History of Science and Technology MSS 405 A.
Gift of the Burndy Library)

¹ CD misdated the letter; the year is established by the postmark.

² Kovalevsky had sent a photograph of Aleksey Ivanovich Butakoff; it has not been found (see *Correspondence* vol. 29, letter from V. O. Kovalevsky, 30 December 1881).

From Arthur de Souza Corrêa¹ 2 January 1882

Brazilian Legation | 2^a. Granville Place. | W. | London.
2 janvier 1882.

Monsieur,

J m'empresse d'accuser réception de votre aimable lettre du 31 Décembre que je ne manquerai pas de transmettre au Baron de Villa Franca.² Les documents que je vous ai remis vous sont destinés en toute propriété, et je serais très heureux de

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voir réalisée votre intention de leur donner publicité dans un journal scientifique Anglais, sous votre haut patronage.³

Veillez agréer, Monsieur, avec mes remerciements reitérés pour toute l'obligeance avec laquelle vous avez bien voulu accueillir les communications que le Baron de Villa Franca vous a adressées par mon intermédiaire, l'assurance de mes sentiments de respect et de haute considération

A. de Souza Corrêa

DAR 160: 284

¹ For a translation of this letter, see Appendix I.

² CD's letter has not been found. It was a reply to the letter from Arthur de Souza Corrêa, 28 December 1881 (*Correspondence* vol. 29), which contained observations by Ignacio Francisco Silveira da Motta, baron de Vila Franca, on a new variety of sugar cane.

³ The documents have not been found; see letter to G. J. Romanes, 1 January [1882].

To G. J. Romanes 3 January [1882]¹

Down Beckenham

Jan 3^d

My dear Romanes

I have heard from the Brazilian Legation that the documents were intended for me. & the Sec^y feels sure that the Baron w^d be gratified by the statements being published.—² Pray, therefore, let me hear what you think about the whole story—

Yours very sincerely | Ch. Darwin

Please return the documents & you can have them hereafter if you think fit.—

American Philosophical Society (Mss.B.D25.610)

¹ The year is established by the relationship between this letter and the letter from Arthur de Souza Corrêa, 2 January 1882.

² See letter from Arthur de Souza Corrêa, 2 January 1882 and n. 2. CD had received documents on new varieties of sugar cane from Ignacio Francisco Silveira da Motta, baron de Vila Franca; they were enclosed in the letter from Arthur de Souza Corrêa, 28 December 1881 (*Correspondence* vol. 29). See also letter to G. J. Romanes, 1 January [1882].

From H. C. Sorby 3 January 1882

Broomfield | Sheffield

Jan 3/82.

My dear Darwin

I very much wish I could give a more satisfactory report but perhaps what I have been able to do may be of some use to you. Besides the cause you suggest the blue colour $\langle mi \rangle ght$ have been due to two other causes.¹ A colouring matter in a dilute acid state might have been already present in that peculiar molecular state into which so many pass when diluted, where they cease to have any colour. Of course I mean quite independent of mere weakening of the solution. If such were the case

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it might turn blue when the acid neutraliz(ed) but at the same time would also turn red when such a strong acid as hydrochloric is added. This is not the case so I do not think any material amount of a coloured substance is present. In the second place a colouring matter might have been formed by oxidization when the acid neutralized, as I reasoned previously. And here I am bothered. When boiled with alcohol some thing is (5 or 6 words)n water which appears to pass so rapidly into a brown substance with curious shade of green that the real change is quite hidden. There may be a red pigment also formed which would be changed to blue by an alkali but the deep brown colour disguises the effect too much to enable me to be certain

On the whole the facts differ a good deal from what I had observed with flowers &c but then I had made very few experiments with colourless stems.² Before being able to give any more confident opinion I should have to work out the whole thing as an independent inquiry. Much as I should like to do this, I cannot well undertake it since I have already promised to do as much as I shall be able to finish before I leave home again. In any case however what I have done will as far as it goes remove some doubts and will make your supposition more probable. Until examined as I have done the question was as I have explained open to several different explanations.

Wishing you a happy new year and trusting that you will be able to throw further light on the interesting facts to which you have called my attention

I remain | Yours very truly | H. C. Sorby

DAR 177: 220

CD ANNOTATIONS

1.8 I do not think ... present. 1.9] *scored red crayon*

1.9 coloured ... present.] *underl red crayon*

1.10 oxidization] 'oxidization' *pencil*

2.1 On the whole ... flowers] *scored red crayon*

¹ No letters from CD to Sorby on this subject have been found. In December 1881, Sorby had replied to a query from CD about colour changes in plants; he had described the changes arising from oxidisation, and from exposure to acidic and alkaline solutions (see *Correspondence* vol. 29, letter from H. C. Sorby, 28 December 1881).

² Sorby had observed plant pigments using a modified microspectroscope that he first developed to examine mineral specimens. His research interests included the optical and chemical properties of chlorophyll, colour changes in autumn leaves, and comparisons between the colouring matter of plants and simple forms of animal life. See Sorby 1871 and Sorby 1873.

From W. E. Darwin 4 January 1882

Bank, Southampton,

Jan 4th 1882

My dear Father,

I send you the account of sale of L.S.W. & purchase of G.W. Stock, the latter is $\frac{1}{2}$ per cent higher, so that I am sorry to say you must send me a cheque for £77. 5[—]¹

Your affect son | W. E. Darwin

Cornford Family Papers (DAR 275: 105)

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¹ CD's Investment book (Down House MS) records the sale of 5000 shares of the London & South Western Railway and the purchase of 5000 shares of the Great Western Railway on 4 January 1882; the sale realised £6925 and the purchase cost £7002 5s., leaving a difference to pay of £77 5s.

To Fritz Müller 4 January 1882

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

Jan. 4th 1882.

My dear Sir

I must write a few lines to thank you for your letter of Dec. 2^d, though I have nothing particular to say.¹ Your appreciation of Balfour's book has pleased me excessively, for though I could not properly judge of it, yet it seemed to me one of the most remarkable books which has been published for some considerable time.—² He is quite a young man & if he keeps his health, will do splendid work. He is the younger brother of a Scotch man A. Balfour M.P. of immense fortune & nephew to a very grand gentleman, the Marquis of Salisbury.³ He has a fair fortune of his own, so that he can give up his whole time to Biology. He is very modest & very pleasant, & often visits here, & we like him very much.

Your *Pontederia* case is very curious: when writing the *Origin*, of *Species* what a fine *instance* it w^d have been of one species beating out another, & under the apparent disadvantage of the mid-styled form alone having been introduced.⁴

As you speak of the seedlings varying I suppose that you feel sure that a suspicion which crossed my mind, of hybrid origin is groundless.— It is also very odd about the seeding & the appearance of the long-styled form. I never saw such oddly-coloured petals which arrived quite brightly coloured.⁵

Your *Janira* seems a very curious & interesting case; & with what exquisite clearness, you have drawn all its exterior organs.⁶

I have been working at the effects of Carbonate of Ammonia on roots, the chief result being that with certain plants the cells of the roots, though not differing from one another at all in appearance in fresh thin slices, yet are found to differ *greatly* in the nature of their contents, if immersed for some hours in a weak solution of C. of Ammonia.⁷

My dear Sir | yours ever sincerely | Charles Darwin

I remember once suggesting to you to write 'a Journal of a naturalist in Brazil' or some such title, & give in it a resume of your endless & most interesting observations; I wish that my suggestion would bear fruit.⁸

P.S.— I have just had to look to Bentham & Hookers *Genera*, & this has reminded me that I do not at all know whether I have completed your set. If you care to have any parts not sent, I beg you to let me hear.— Hooker tells me that they have nearly completed the *Monocotyledons*, & that the *Palms*, *Grasses* & *Orchideae* were fearfully hard work. The *Palms* took 2 years.—⁹

The British Library (Loan MS 10: 58)

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- ¹ Müller's letter of 2 December 1881 has not been found.
- ² CD had received two copies of *A treatise on comparative embryology* (Balfour 1880–1) and arranged with Francis Maitland Balfour to send the spare copy to Müller as a gift from Balfour (see *Correspondence* vol. 29, letter to F. M. Balfour, 6 July 1881, and letter from F. M. Balfour, 10 July 1881).
- ³ Arthur James Balfour and Robert Arthur Talbot Gascoyne-Cecil, third marquess of Salisbury.
- ⁴ In *Origin*, pp. 60–79, CD discussed the natural checks to increase on species and varieties. In his now missing letter of 2 December 1881, Müller had evidently discussed a species of *Pontederia* (pickerel-weed) in which only one form had been introduced to an area (see letter from Fritz Müller, 1 January 1882 and n. 4). The species was probably *P. crassipes* (a synonym of *Eichhornia crassipes*, common water hyacinth); Müller later discussed it in a short paper, 'Einige Eigenthümlichkeiten der *Eichhornia crassipes*' (Some characteristics of *Eichhornia crassipes*; F. Müller 1883).
- ⁵ Müller had probably sent seeds of crosses between long- and mid-styled plants of *Pontederia crassipes* with his letter of 2 December 1881 (see letter from Fritz Müller, 1 January 1882).
- ⁶ Müller's description of a species he identified as belonging to the isopod genus *Janira* was evidently in his now missing letter of 2 December 1881. Müller later described and figured the species in his paper 'Descrição da *Janira exul*, crustaceo isopode do estado de Santa Catharina' (Description of *Janira exul*, an isopod crustacean from the state of Santa Catharina; F. Müller 1892). *Janira* was a genus with only marine species, but Müller's species was found in fresh water and was notable for its distinctive antennae.
- ⁷ The results of CD's research were published in 'Action of carbonate of ammonia on roots'.
- ⁸ In his letter of 22 April [1867] (*Correspondence* vol. 15), CD had suggested that Müller should write a book of 'miscellaneous observations on all branches of natural history', noting that such books were very popular in England. CD repeated the suggestion in a letter of [9 February 1876] (*Correspondence* vol. 24).
- ⁹ See *Correspondence* vol. 29, letter from J. D. Hooker, 27 October 1881. *Genera plantarum* (Bentham and Hooker 1862–83) was a systematic work undertaken by Joseph Dalton Hooker and George Bentham in 1860 (see Stearn 1956). Monocotyledones was the heading of the final part of *Genera plantarum* (Bentham and Hooker 1862–83, 3 (2): 448). The Orchideae (a synonym of Orchidaceae, orchids) was a large section completed by Bentham in August 1880, after which he worked on the Cyperaceae (sedges), finished in October 1880; the Gramineae (a synonym of Poaceae, grasses) formed the last section, which Bentham finished in late 1881 (Stearn 1956, p. 130; Bentham 1881). Hooker was working on palms (Palmae, a synonym of Areaceae); see Bentham and Hooker 1862–83, 3 (2): 870–948. CD had sent earlier parts to Müller (see *Correspondence* vol. 14, letter to Fritz Müller, [late December 1866 and] 1 January 1867).

To Theodor Eimer 6 January [1882]¹

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

Jan. 6th 1881.

Dear Sir

I am much obliged to you for your kindness in having sent me your work on the variation of the wall-lizard and for another paper.—²

Please accept my thanks and believe me | dear Sir | yours very faithfully | signed:
Charles Darwin

Copy

CUL: Library Correspondence 1953: ref. 1273

¹ The transcription of this letter is from a handwritten copy made in 1953. A note on the copy says that the letter was addressed to 'Prof. D: Th. Eimer' and postmarked 1882. CD misdated the letter 1881.

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- ² The paper on the wall lizard (*Lacerta muralis*, a synonym of *Podarcis muralis*) is Eimer 1881a. The other paper is probably Eimer 1881b; a copy is in the Darwin Pamphlet Collection–CUL. Eimer had sent a previous work on lizards in 1874 (Eimer 1873–4; see *Correspondence* vol. 22, letter to Theodor Eimer, 12 December 1874).

To Hyacinth Hooker 6 January [1882]¹

Down Beckenham

Jan— 6th

Dear Lady Hooker

I have much pleasure in sending 5—5—0 in aid of your subscription for poor M^{rs} Fitch..— If you want more, I beg you to apply to me again; for it would require a great many letters before I could “treat you as a troublesome person”.²

Believe me dear Lady Hooker | Yours truly obliged | Charles Darwin

Royal Botanic Gardens, Kew (JDH/2/2/1 f. 313)

- ¹ The year is established by the relationship between this letter and the letter from Hyacinth Hooker, 7 January 1882.
- ² Hannah Fitch was the wife of Walter Hood Fitch, a botanical artist at the Royal Botanic Gardens, Kew, who illustrated several of Joseph Dalton Hooker’s works. A cheque for £5 5s. to ‘L. Hooker to M^{rs} Fitch Charity’ is recorded in CD’s Account books—cash account (Down House MS) on 6 January 1882. No previous letter from Hyacinth Hooker on this matter has been found.

To G. J. Romanes 6 January 1882

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

Jan 6th 1882

My dear Romanes

I had no intention to trouble you about preparing the paper, but you seem to be quite untirable & I am glad to shirk any extra labour. It is shabby of me, but I gladly accept your offer to prepare a paper for Linn. Soc. if you think fit, & an abstract for Nature. I can thus send copies to the Baron & D^r. Glass.— By the way I cannot remember which of the two started the plan so this must be left in the dark.— As it w^d appear so odd the sending of a document signed & stamped without some explanation, I think it is quite necessary that the paper sh^d be presented with *some such* statement as I have written down.¹

As it can do no harm I have scribbled down the headings of the sort of paper which I sh^d have made, had I not shabbily allowed you to undertake the task.²

I quite agree about the Microscope & Grant Allen³

Yours very sincerely | Ch. Darwin

[Enclosure]

M^r. Darwin received, as he informs me, about two years ago a letter from the Baron de Villa Franca in Brazil, stating that he had raised new varieties of the Sugar-Cane