

THE CORRESPONDENCE OF CHARLES DARWIN
1880

To Horace Darwin 1 January 1880

My dear Horace—

My memory is so bad that I am not sure whether I wrote to the Bank, about your payments. I agreed to pay you the same allowance as before viz £300 (Aunt Catherine's money does not come into consideration) & as £5000 was transferred to you, & this produces 200£ per annum, I ought to pay you 100£ viz 50 on Jan 1 & on July 1— Is this all right?¹ Does not the £5000 produce £200 less income tax?— Please get your banking book & see whether 5.0£ has been placed to your account. & let me hear.— My overplus shall be divided in 10 or 14 days.—²

Many thanks for charming little compasses—³ May I trust you to make memorandum that I owe you for them, so that I c^d pay whenever you come here.—

What a nice few days we had at Christmas.—

Your affectionate Father | C. Darwin

Jan 1.— 1880—

DAR 185: 6

¹ CD placed £5000 of stock in trust for Horace Darwin and Ida Farrer before their marriage (see CD's Investment Book (Down House MS) and *Correspondence* vol. 27, letter to W. M. Hacon, 8 November [1879], and letter from W. M. Hacon, 31 December 1879). In 1880, he paid Horace £50 on 1 January and 1 July (CD's Classed account books (Down House MS)). Catherine Langton had left money in trust to CD that was intended for distribution to his children (see *Correspondence* vol. 14, letter to W. E. Darwin, 8 November [1866]).

² CD had decided to distribute the surplus income from his investments to his children in 1879 (see *Correspondence* vol. 27, letter to the Darwin children, 21 February 1879).

³ Horace may have built some of the compasses that CD used in his observations for *Movement in plants*.

From C. H. Tindal 1 January 1880

The Manor House | Aylesbury.

Jan: 1st 1880.

Dear Sir—

I have been through the whole of the Clive Correspondence & have made extracts of all the allusions to D^r Darwin— and I have also selected three letters in

which mention is made of your father—¹ Pray keep the extracts if they are of any interest to you—and kindly return the three letters—

The M^r. Gifford referred to, was the Rev^d. Richard Gifford. M.A. of Balliol College Oxford. Rector of North Ockington Co: Essex & Vicar of Duffield Co. Derby

He was a person of great literary attainments, & a fine Hebrew & Classical Scholar— The article in the gentlemans Magazine which appeared on his death will give you a fair description of him & his writings generally.² I cannot say how he became acquainted with D^r. Darwin— The correspondence between him & M^r. Clive began in 1760. and they were then both acquainted with D^r. D. then.

The M^r. Clive referred to is the Ven^{ble}. Archdeacon Robert Clive Rector of Moreton, Prebendary of Westminster & Archdeacon of Salop. He was son of the Rev^d. Benjamin Clive of Duffield & cousin of the celebrated L^d. Clive whose sister he married.³

Duffield is about 5 miles from Derby. M^r. Richard Gifford is buried there, & his daughter & only child Euphemia Gifford likewise— She lived to be 89 & died in the year 1854.⁴

It may interest you to know that I have here a picture cut out in black Paper of Archdeacon Clive, & his brother George Clive a banker in London playing Chess—⁵ The picture was cut out by Miss Wedgwood daughter of Josiah Wedgwood who married D^r. Robert Darwin of Shrewsbury.⁶ It was left together with a print of the 1st. Lord Clive to M^r. Gifford by the Archdeacon.

I have a long correspondence between M^r. Gifford & his wife,⁷ & daughter which may throw some light on the commencement of the acquaintance with D^r. Darwin. I will look through this tomorrow & let you know the result.

I must apologise for troubling you with so long a letter— containing I am afraid much that is irrelevant, & subscribe myself, Y^{rs}. very faithfully. | Charles H. Tindal

[Enclosure 1]

July 4th. 1768

The Ven: R. Clive to Rev^d R Gifford.

I thank you, for the account you sent me of M^r. Rotton,⁸ & was very glad to find that you were of opinion he mended very fast. I have since seen D^r. Darwin, who seems to think he may get much better with care & proper management, but he can by no means approve of y^c. Bark as he apprehends his case to be dropsical. He happened to have in his pocket a little treatise which he had just written upon y^c. use of y^c. Bark, & which he was going to send up to y^c. College of Physicians, to be printed amongst their next annual publications— he gave me y^c. perusal of it, & leave to transcribe a part of it, which I will send you, & I dare say you will think it very judicious as I do.⁹ “The effect of ye Peruvian Bark & other Bitters seems much to depend on their decreasing ye Irritability of the nervous system; as is evinced by their daily successful use in Fever & other diseases from Irritation— And as y^c. palsies & dropsies described below were owing to a decrease of this Irritability, the Bark seems, whenever it was given to have produced or increased these diseases,

or to have destroyed y^c patient— The words Relaxation and Bracing cannot be opposed to this idea of the effect of Bitter medicines, as they can with no propriety be ascribed to ye nervous system of animal bodies; but are mechanical terms that belong alone to dead matter, & not to y^c laws of life, & have misled many of y^c faculty to ye great detriment of their patients— One observation I shall add, which has appeared to me invariably true viz: that y^c violent coughs, & y^c febrile symptoms that are attendant on obstructed Livers, after proper Evacuations by bleeding with repeated vomiting or purging, are cured with certainty by the Bark, either alone or with the addition of a slight chalybeate¹⁰ or anodyne. But I believe from the following case that, where no symptoms of Irritation are attendant on obstructed Livers, ye Bark & chalybeates have induced palsies & dropsies to g^t destruction of Thousands”— Thus far y^c ingenious Doctor, whose observations stand upon facts which have happened in his own practice, & which he produces in his treatise. His sentiments may be considered as a key to y^c use of the Bark & I am sure they will make me more cautious in recommending this medicine Send me word whether you will be at Moreton this day fortnight in y^c Evening or, coming by Lichfield, & calling on D^r. Darwin give me y^c meeting at D^r. Adams' at Counde on Tuesday by dinner¹¹

[Enclosure 2]

Moreton
Nov: 6th 1768.

R. Clive to R. Gifford

I am glad to find by your last papers to y^c Doctor & his letter to you, that matters are likely to be adjusted very amicably betwixt you even without the help of a moderator. I think you were a little off your guard in your definition of a Living Substance when you said you meant no more by it than 'a substance well fitted to convey impressions to y^c mind & to execute her orders— *Living* here signifies *apt for y^c business of Life.*' If this be a true definition of a *Living substance* will it not follow that an axe, a saw, or an hammer, or any other material instrument that we make use of for y^c business of life is as much a *living substance* fitted to convey impressions to y^c mind, & to execute her orders as y^c body itself is—

From R. Clive to R. Gifford

Moreton
Dec: 12. 1768.

'Dear Sir—

I am desired by Doctor Darwin to let you know, that his pig & M^r. Whitleys¹² company of comedians will be ready for our Establishment at Lichfield in y^c 2nd week of January next. I propose being there on Tuesday ye 10th & hope nothing will prevent you giving me y^c meeting, & pray bring your strictures on Berkeleys book upon y^c material world—for y^c Doctor tells me he has been writing a chapter to prove a material world, which he hopes will please you, as it plucks up the root

of that kind of infidelity called scepticism.—¹³ We must try spend two or three days with y^c Doctor, which I think we shall pass very agreeably—. Pray bring Berkeleys book with you as ye Doctor may not have it.’—

R. Clive to R Gifford

[Enclosure 3]

Moreton

Sept: 12th (no year)

R. Clive to R. Gifford—

I read over your remarks with D^r. Darwin, who took all in good part, said he was very much obliged to you & would write soon— by the observations he made it appeared you had misunderstood some few passages in his book—& I think he will respond to your objections very well. I mentioned to him our desire of attending him sometime when he opened an human body— to which he said that he could give us all the satisfaction we desired from an inspection of y^c parts of a pig, & invited us to spend two or three days with him at Xmas, when he should kill a fat pig— I propose much pleasure from this meeting & hope nothing will happen to prevent it

[Enclosure 4]

Stych.

Jan: 10th 1771.

R. Clive to R. Gifford.

I thank you for your letter & your proposal to meet me at Lichfield, which perhaps I may call upon you to fulfil before it be long, as D^r. Darwin has often pressed me to come and spend a week with him—& two new philosophical friends who have lately settled in Lichfield, induced to it by that cheerfulness and benevolence of disposition, with which you & I & all who know y^c good Doctor are so much charmed.

R. Clive to R. Gifford

Lichfield

May 30. 1771.

I wish you could have spent tomorrow with D^r. Darwin & his two ingenious friends Edgeworth & Day¹⁴

R. Clive to Richard Gifford

Moreton

Oct: 25th 1772.

‘I have been much out of order for these last 5 days with a fever of the bilious kind—luckily D^r. Darwin was in y^c neighbourhood & prescribed for me from my written account of myself though he had not time to see— he has lain at home but one night these five weeks so great is his fame & usefulness

[Enclosure 5]

Moreton.
July 1st 1773.

R. Clive to R. Gifford

I dare say you had a very sincere pleasure in Lord Clive's victory over his enemies in parliament which was very complete & very honorable—¹⁵ D^r. Darwin wrote me a good blackguard letter on the occasion which will Entertain you better than anything else I can add to this hasty scrawl. I will therefore transcribe it "I had a great mind to have written a letter in y^e newspapers on y^e following plan. To all y^e Blackguards in Great Britain— Loving friends & Cousins— To it again—at him— We shall conquer this Lion at last I warrant y^e: Never mind a pinch or two— Burgoyne will stave, & Meredith tail—¹⁶ At him again. We'll first demolish Clive & then Chatham,¹⁷ and there shall not be a man of virtue left in the Kingdom if we can help it— Now's the time, My loving Cousins in Parliament, proscribe & confiscate all that are against the ministers at present; and when a new ministry prevails, then all the present proscribers shall be proscribed in their turn. till Temple bar is hung round with Calves heads like a Butcher's shop— This is the true levelling principle!¹⁸ Rare times for old England! At him again, my lads next session— Never yelp & howl so for one defeat— Give me another pot of porter— Oh! d— your virtue— it has saved your country"—.

[Enclosure 6]

Lichfield.
Jan: 25th. (no year).

R. Clive to R. Gifford.

When I came here yesterday I found D^r. Darwin & M^r. Boothby busily engaged in translating the Genera Plantarum of Linnæus into English, in which if they succeed to their Satisfaction M^r. Boothby is to publish it.¹⁹ It will probably be a work of time, as it will be of some nicety—in which I told them I thought you could be useful as you have y^e: book. I also told them you had been trading on metaphysical ground— They both wished much to see your M.S. M^r. B. said he would give you his free sentiments ou bien ou mal. He is going to publish a work which Rousseau entrusted to him about 4 years ago with a request not to do it till after his death.²⁰

R. Clive to R. Gifford.

Lichfield.
Jan: 20th. (no year).I will send you some verses I found upon D^r. Darwin's table.

On the death of Brindley the great perfector of Inland Navigation.²¹
Leek, Cheadle, Cheddleton, Delf, Burslem, Woor,
Stoke, Turnhurst, Ipstones, Draycot in y^e: Moor,
All strive for Brindley's birth, but strive in vain.²²

For Brindley sprung immortal from the main—
 On the much lamented death of M^r. G—r—ck.²³
 Ambubiarum collegia pharmacopolæ.²⁴
 Pimps, prompters, poets, painters, rhymers, riddlers,
 Beaux, taylors, link boys, fruit girls, singers, fiddlers,
 Scene shifters, tumblers, fairies, goblins, witches,
 Backs, bullies, gamblers, demireps, & bitches,
 All mourn for Garrick dead with wild distraction
 A crowded playhouse was their scene of action—

R. Clive to R. Gifford

Shrewsbury.

Sept 14th. (no year)

I received D^r. Darwins response which I think very ingenious & solid. I should be glad of your sentiments as soon as you have perused it. I am now sitting in Company with the Hero of D^r. Darwins book, who is in better health & spirits than I ever remember him—

DAR 227.7: 11–13, 16, 18, 25, 128

CD ANNOTATION

Top of letter: 'I must make note of obligation to M^r Tindal' *pencil*

- ¹ CD had asked Tindal for details about Richard Gifford and Robert Clive (1722/3–92), friends of his grandfather Erasmus Darwin (see *Correspondence* vol. 27, letter to C. H. Tindal, 29 December 1879. CD's father was Robert Waring Darwin.
- ² Gifford's obituary appeared in *Gentleman's Magazine* 77 (May 1807): 477–8.
- ³ 'Lord Clive' was Robert Clive (1725–74); his sister was Rebecca Clive.
- ⁴ Gifford's daughter was Euphemia Gifford.
- ⁵ George Clive. The plate on p. 12 shows a similar silhouette.
- ⁶ Susannah Darwin was the daughter of Josiah Wedgwood I, and the wife of Robert Waring Darwin, CD's father.
- ⁷ Elizabeth Gifford.
- ⁸ John Rotton.
- ⁹ 'Peruvian bark' from cinchona trees was a common source of quinine. Erasmus Darwin prescribed it as a stimulus for treating fevers and other conditions (see E. Darwin 1794–6, 1: 83–4, 100 and 2: *passim*). Its medical properties are praised in *The botanic garden*, pt 2, *The loves of plants* (E. Darwin 1799, pp. 103–5); however, no separate publication on Peruvian bark has been found.
- ¹⁰ Chalybeate: water or other liquid containing iron (*OED*; see also E. Darwin 1794–6, 2: 175–6).
- ¹¹ William Adams was rector of Counde, Shropshire (*ODNB*).
- ¹² James Whitley.
- ¹³ George Berkeley's controversial views on perception and the material world were published in *An essay towards a new theory of vision* (Berkeley 1709). Erasmus Darwin discussed visual perception in *Zoonomia* (E. Darwin 1794–6, 1: 14–29).
- ¹⁴ Richard Lovell Edgeworth and Thomas Day; both were members of the Lunar Society of Birmingham (see King Hele 1999, pp. 79–80).
- ¹⁵ In 1772 and 1773, several motions were brought against Robert Clive (1725–74) in the House of Commons, in connection with criticism of the East India Company's administration in Bengal. In May 1773, Clive successfully defended himself against accusations of appropriating money while serving as governor of Bengal. (*ODNB*)

- ¹⁶ John Burgoyne was an army officer, MP, and outspoken critic of Robert Clive (1725–74) and the East India Company. William Meredith served on Burgoyne’s committee to investigate the East India Company and seconded his motion to censure Clive for corruption. (*ODNB*).
- ¹⁷ William Pitt (Pitt the elder), first earl of Chatham.
- ¹⁸ Temple Bar, the historic gateway to London, was used to display the severed heads of traitors until 1746. The Levellers was a name given to supporters of a democratic republic during the period of the English Civil War and Commonwealth (*EB*).
- ¹⁹ Brooke Boothby. The translation of Carl von Linné’s *Genera plantarum* was credited to ‘a botanical society at Lichfield’ (Linnaeus 1787; see King-Hele 1999, pp. 217–18).
- ²⁰ *Ou bien ou mal*: whether well or ill (French). Boothby edited *Rousseau juge de Jean Jacques: dialogue* (Rousseau 1780). On the friendship between Boothby and Jean Jacques Rousseau, see Zonneveld [2003].
- ²¹ James Brindley was principal engineer on a number of canals across the Midlands; he died in 1772 (*ODNB*).
- ²² Leek, Chedale, Cheddleton, Delph, Burslem, Woore, Stoke-on-Trent, Ipstones, and Draycott are villages or towns in Staffordshire. Brindley was born in Wormhill, Derbyshire, and lived at Turnhurst Hall in Staffordshire after his marriage in 1765 (*ODNB*).
- ²³ The actor David Garrick died in 1779 (*ODNB*).
- ²⁴ ‘Ambubiarum collegia pharmacopolae’ (correctly, ‘Ambubaiarum collegia, pharmacopolae’): ‘A band of flute girls, quack doctors ...’. The source is Horace, *Satires* 1.2.1.

From Samuel Butler 2 January 1880

15, Clifford’s Inn | Fleet Street E.C.
Jan 2. 1880

Charles Darwin Esq^r | F.R.S. &—

Dear Sir

Will you kindly refer me to the edition of ‘Kosmos’ wh: contains the text of D^r Krause’s article on D^r Erasmus Darwin, as translated by M^r W. S. Dallas?

I have before me the last February number of Kosmos, which appears by your preface to be the one from wh: M^r Dallas has translated; but his translation contains long and important passages which are not in the February number of Kosmos, while many passages in the original article are omitted in the translation.¹

Among the passages introduced are the last six pages of the English article, which seem to condemn by anticipation the position I have taken as regards D^r Erasmus Darwin in my book *Evolution old & New*, and which I believe I was the first to take.² The concluding and therefore, perhaps, most prominent sentence of the translation you have given to the public stands thus;—

“Erasmus Darwin’s system was in itself a most significant first step in the path of knowledge which his grandson has opened up for us, but the wish to revive it at the present day as has actually been seriously attempted shows a weakness of thought and a mental anachronism which no man can envy.”³

The Kosmos which has been sent me from Germany contains no such passage—

As you have stated in your preface that my book *Evolution old & New* appeared subsequently to D^r Krause’s article, and as no intimation is given that the article has been altered and added to since its original appearance, while the accuracy of the translation as though from the February number of Kosmos, is as you expressly

say guaranteed by M^r Dallas's "scientific reputation together with his knowledge of German",⁴ your readers will naturally suppose that all they read in the translation appeared in February last, and therefore before *Evolution old and New* was written, and therefore independently of, and necessarily without reference to that book.

I do not doubt that this was actually the case, but have failed to obtain the edition which contains the passage above referred to, and several others which appear in the translation.

I have a personal interest in this matter and venture therefore to ask for the explanation which I do not doubt you will readily give me

I am | y^r. faithfully | S. Butler—⁵

DAR 92: B65–6

¹ Ernst Krause had published an article on Erasmus Darwin in *Kosmos* (Krause 1879a). William Sweetland Dallas agreed to translate this article from the German for *Erasmus Darwin* (see *Correspondence* vol. 27, letter from W. S. Dallas, 14 March 1879). However, Krause sent a revised and much enlarged version of the article in manuscript, and it was this revised text, not the original *Kosmos* article, that Dallas translated (see *Correspondence* vol. 27, letter from Ernst Krause, 5 May 1879, and letter from W. S. Dallas, 7 May 1879). In the preface to *Erasmus Darwin*, p. iii, CD mentioned only Krause's original article, having decided to omit much of the revised essay from the published English version. The additions were reinstated in Krause 1880.

² Butler's *Evolution old and new* (Butler 1879) was published in May 1879 and contained lengthy discussions of Erasmus Darwin, Jean Baptiste de Lamarck, and other descent theorists. CD sent Krause a copy of Butler's book (*Correspondence* vol. 27, letter to Ernst Krause, 13 May 1879). Both CD and Krause were critical of Butler's views (see *ibid.*, letter to Ernst Krause, 14 May 1879, and letters from Ernst Krause, 23 May 1879, 7 June 1879, and 13 June 1879); however, CD remarked: 'I hope that you will not expend much powder & shot on M^r Butler, for he really is not worthy of it' (letter to Ernst Krause, 9 June [1879]).

³ The quotation is taken from the final sentence of Krause's essay in *Erasmus Darwin*, p. 216.

⁴ *Erasmus Darwin*, p. iii n.

⁵ Emma Darwin wrote at the head of the letter, "To be returned as it means war we think".

From Joseph Prestwich 2 January 1880

21 Park Crescent | Portland Place
2 Jan^y. 1880

My dear M^r Darwin,

May I trouble you with two lines in explanation to set me right if I am wrong in my statement. In reviewing the history of the "Parallel Roads" I refer to your views, and relying on the opinion expressed by later writers, whom I suppose to have been in communication with you, I have stated that you have abandoned the "marine theory"—therefore finally committing myself to this statement I should be glad to hear that I am quite correct in doing so, or if I am to put it in any other form.¹

With the best wishes of the season I am | My dear M^r. Darwin | Very truly your's
| Joseph Prestwich

DAR 174: 66

¹ In his 1839 paper ‘Parallel roads of Glen Roy’, CD argued that the three horizontal terraces across the steep sides of the valley in Lochaber were the remains of beaches formed by the sea as the landmass of Scotland rose in graduated steps. In an article on the parallel roads, Prestwich remarked: ‘the marine theory was brought forward with his usual power of illustration, but subsequently abandoned, by Darwin’ (Prestwich 1879, p. 665). CD had abandoned his theory when an alternative explanation involving glacial lakes was advanced by Thomas Francis Jamieson (see *Correspondence* vol. 9, letter to T. F. Jamieson, 6 September [1861], and Rudwick 1974). CD discussed his reasons for giving up the marine theory with Charles Lyell and John Tyndall, both of whom published on the geology of Glen Roy (see *Correspondence* vol. 10, letter to Charles Lyell, 14 October [1862], and *Correspondence* vol. 24, letter to John Tyndall, 5 June [1876]; see also Lyell 1873, p. 307, and Tyndall 1876, p. 238).

From B. J. Sullivan 2 January [1880]¹

Bournemouth

Jan^y. 2./79

My dear Darwin

Many happy new years to you & M^{rs}. Darwin and all your party. I think you will be amused with a few extracts from Bishop Stirlings youngest daughter’s letter, who has gone out with him this time; as it gives an amusing account of semi civilised Fuegians.²

“We heard from M^r. W. a story of our name sake “Stirling”. M^r. Bridges was pitching into them all rather strongly one day in his sermon, and Stirling stood up and said “I came here to worship God, and not to be lectured by you”—³

“One or two at the Station have quite large pieces of land, which they are converting into gardens, Stirling is far ahead of any, and takes great pride in his. He has now 12 cows, including calves, and two pigs, so he is quite a rich man, I like him very much; his wife is “Louisa”. and one of his little children “Gertrude”. He said he was pleased to see “Mees” as he called me, as he wanted to see the girl his “little Gerty” was called after”—⁴

“A few outsiders came over yesterday in their Canoes, among them the leader of the Mapacie long ago”—⁵

“There are several nice men living here, and the boys who have grown up here look nice manly lads.”

“They are very fond of biscuits. Eleanor—“Joe’s” wife—has not been well and papa asked her if he took her any of the biscuits he had given him. He looked quite reproachful as he answered that “he always took it home to her”; and we found that when he had six given him he only eat one, and took five to her”—⁶

“This morning, one family brought a bucket of milk for sale; and Stirling some butter which he got Louisa to make, because papa asked him if they had any”. “The orphanage children look very jolly”. “James Sullivan⁷ is a splendid little chap, the jolliest boy here”.

“The Eldest orphan girl is to be married tomorrow, she is about sixteen, she is said to be fond of dressing up in M^{rs}. What’s clothes.⁸ Once one of the girls found a stocking, so they took it in turns to wear the one stocking. They always choose who shall sit at the head of their table at meals”—

“We went to the Sunday school which Mr. Laurence⁹ was taking, sometimes one or two of the married men come to it if the room is not too crowded. They sang hymns in English very nicely, and repeated texts, and then translated them into their own language”.

“Papa wanted to get two boys for his boat, but found it difficult, as all the nice ones had wives; one wanted to come very much, but as he had a wife papa did not like to take him. Presently he came running & said. “I quick find man not bad, take care of wife”, which meant that Stirling would let her live with him & his wife while he was away: on that “Barry Loring” and his wife would do the same;¹⁰ and his wife told him he might go”—so he is coming”—

“The other boy is from Lennox Island, a nice small little fellow who does not speak English except “yes”, “Sailor”.— “One of the men who is to be married tomorrow said “he was ashamed to go to church because he had not got any “*pants*” to go in “using that expression”.

“This morning we went to the wedding. The bride looked very nice, she was dressed in a pretty blue and white print. The bridegroom too looked very nice. They were both shy, as there were about 78 as a congregation to see the performance.”—

“This morning we watched Willie,¹¹ and James Sullivan, sawing wood they did it splendidly”— “The Indians are very sarcastic if displeased at any thing; before leaving Ushuwia the Bridges¹² gave away many old clothes, and flour, and one man was discontented because he thought he ought to have more. They had given him less because he was not so deserving, so he shouldered his bag of flour and trudged off saying it was really so heavy, and so large that he could hardly carry it”. When they were off a “splendid” glacier on their way to Sandy point—which from their position I think must be one near Mount Darwin she describes hearing the noise several times of avalanches, sounding like peals of thunder.

The Bishop was preparing to hop from Sandy Point to Falklands in a boat sent out from England taking her with him. The boat is decked but only 30 feet long. He had her built for work at Falklands, and sent out by steamer to meet him in Straits: I urged him to send her direct to Falklands as there was great risk if she had heavy weather crossing. I think he goes in her instead of A. Gardiner Yawl¹³ because he would not send men in her without showing his confidence by going himself with them. Of course she might go often safely, if carefully handled, but she is too small for very heavy weather

I am sorry to say I have had a sad account from Mellersh of his health. He went to live at Brighton for his daughter’s health, and was soon very ill with bad liver and an abscess in one lung, and suffering in one leg from sciatica.¹⁴ He had been near death for some time, but has been better lately.

Mr. Bridges has had to return from Ushuwia through breaking down entirely in health. with our united kind regards to Mr. Darwin and your family | Believe me dear Darwin | yours very sincerely | B. J. Sullivan

I am reading with great interest your G. Fathers life which Mrs. Langton has lent me.—¹⁵

DAR 177: 308

- ¹ The year is established by the relationship between this letter and the letter to B. J. Sullivan, 3 January 1880. Sullivan wrote '79' in error.
- ² Waite Hockin Stirling was bishop of the Falkland islands; his youngest daughter was Gertrude Louisa Stirling.
- ³ 'Mr. W' was Robert Whaits, a blacksmith and assistant to Thomas Bridges in Ushuaia, Tierra del Fuego (Hazlewood 2000, pp. 326, 347). Stirling's namesake has not been identified.
- ⁴ The wife and daughter were evidently named after Louisa Jane Stirling and Gertrude Louisa Stirling; they have not been further identified.
- ⁵ The Mapuche are an indigenous people occupying parts of Chile and Argentina, including Patagonia.
- ⁶ Joe and Eleanor have not been identified.
- ⁷ Cooshaipunjiz (renamed James Button Sullivan).
- ⁸ Mary Ann Whaits. The orphan girl was Wilooshwahwilis; she was renamed Orphie Loftus (*South American Missionary Magazine*, 2 January 1882, p. 15).
- ⁹ John Lawrence was a gardener who settled in Ushuaia (Hazlewood 2000, p. 326).
- ¹⁰ Barry Loring and his wife have not been identified.
- ¹¹ Pucananlacitanjiz (renamed William Beckenham Button).
- ¹² Thomas Bridges had settled in Ushuaia with his wife, Mary Ann Bridges, and daughter, Mary Ann Varder Bridges (*ODNB*).
- ¹³ The *Allen Gardiner* was a two-masted vessel owned by the South American Missionary Society (Macdonald 1929); it was named after the missionary Allen Francis Gardiner.
- ¹⁴ Arthur Mellersh and Henrietta Alice Mellersh.
- ¹⁵ Sullivan borrowed the copy of *Erasmus Darwin* from Emily Caroline Langton, CD's niece by marriage.

To Samuel Butler 3 January 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan 3. 1880

My dear Sir,

D^r Krause soon after the appearance of his article in *Kosmos* told me that he intended to publish it separately and to alter it considerably and the altered M.S. was sent to M^r Dallas for translation.¹ This is so common a practice that it never occurred to me to state that the article had been modified; but now I much regret that I did not do so. The original will soon appear in German, and I believe will be a much larger book than the English one; for with D^r Krause's consent many long extracts from Miss Seward were omitted (as well as much other matter) from being in my opinion superfluous for the English reader. I believe that the omitted parts will appear as notes in the German edition.² Should there be a reprint of the English life I will state that the original as it appeared in *Kosmos* was modified by D^r Krause before it was translated.³ I may add that I had obtained D^r Krause consent for a translation and had arranged with M^r Dallas, before your book was announced, I remember this because M^r Dallas wrote to tell me of the advertisement.⁴

I remain, | Yours faithfully | Charles Darwin

LS

British Library (Add MS 34486 D ff. 84-5)



ERASMUS DARWIN (left) and his Father Dr ERASMUS DARWIN (right) at Chess.
From a silhouette at Claverdon in the possession of Mr Wheeler Galton.

Wellcome Collection (CC BY 4.0).

- ¹ A revised version of Ernst Krause's article on Erasmus Darwin, originally published in *Kosmos* (Krause 1879a), was translated from the German by William Sweetland Dallas for *Erasmus Darwin* (see letter from Samuel Butler, 2 January 1880 and n. 1).
- ² CD decided to cut parts of Krause's manuscript, including biographical details taken from Anne Seward's *Memoirs of the life of Erasmus Darwin* (Seward 1804; see *Correspondence* vol. 27, letter to Ernst Krause, 13 August 1879). Krause placed most of this material in lengthy notes to the German edition (Krause 1880, pp. 183ff.).
- ³ The following note was added to *Erasmus Darwin* 2d ed., p. v: 'Mr. Darwin accidentally omitted to mention that Dr. Krause revised, and made certain additions to, his Essay before it was translated. Among these additions is an allusion to Mr. Butler's book, "Evolution, Old and New."'
- ⁴ CD asked for permission to translate the article in his letter to Ernst Krause, 9 March 1879 (*Correspondence* vol. 27); Dallas mentioned Butler's book in his letter of 9 May 1879 (*ibid.*).

From J.-H. Fabre¹ 3 January 1880

Monsieur

Permettez-moi de vous offrir, comme à un Maître de la Science, un exemplaire de mes *Souvenirs entomologiques*, traitant expérimentalement de l'Instinct chez les Insectes.²

Quelques unes de mes recherches vous sont déjà connues, mais dans l'opuscule act(uel) elles sont complétées par de nouvelles observations; d'autres, en plus grand nombre, paraissent pour la première fois.

Comme le sujet est de nature, à intéresser de nombreux lecteurs, j'ai cru devoir abandonner la forme académique, trop sévère, et laisser courir un peu plus librement ma plume; toutefois, telle qu'elle est, la forme n'enlève rien à la rigoureuse exactitude des faits.

Je serais heureux, Monsieur, si, du fond de mon village, je pouvais vous procurer une lecture digne de votre attentions.

Peut-être ne serons-nous pas toujours d'accord sur les conclusions à tirer des faits observés; mais ce qui sera au parfaite harmonie chez nous, c'est la profonde admiration pour l'industrie de l'Insecte.

Vous poursuivez avec une noble ardeur la vérité dans les plus hautes sphères, je la poursuis aussi dans mon humble domaine et je serais assez récompensé de mes efforts si j'ajoutais quelques épis à la moisson.

Veillez agréer, Monsieur, l'expression de ma profonde estime et de mes sentiments les plus respectueux | J. H. Fabre

Sérignan (par Orange) Vaucluse

3 Janvier 1880

DAR 164: 1

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

² Fabre 1879; CD's annotated copy is in the Darwin Library–CUL.

To Joseph Prestwich 3 January 1880

Down
Jan 3. 1880

My dear Dear Prof Prestwich

You are perfectly right. As soon as I read M^r Jamieson's article on the parallel roads, I gave up the ghost with more sighs and groans than on almost any other occasion in my life¹

Believe me, yours very sincerely | Charles Darwin

Copy
DAR 147: 253

¹ See letter from Joseph Prestwich, 2 January 1880 and n. 1. Thomas Francis Jamieson proposed that the 'parallel roads' of Glen Roy marked the shores of glacial lakes in a paper presented to the Geological Society of London (Jamieson 1863). In 1861, CD received a report of Jamieson's initial findings and expressed support for Jamieson's theory, referring to his own paper on the subject as 'one long gigantic blunder' (see *Correspondence* vol. 9, letter from T. F. Jamieson, 3 September 1861, and letter to T. F. Jamieson, 6 September [1861]).

To B. J. Sullivan 3 January 1880

Down | Beckenham. Kent.
Jan. 3. 1880.

My dear Sulivan.

I return you your Xmas good wishes with all my heart, and may you live for many years to do good to others and to make all around you happy. Your extracts about the Fuegians are extremely curious & have interested me much.¹ I have often said that the progress of Japan was the greatest wonder in the world but I declare that the progress of Fuegia is almost equally wonderful—²

I am very sorry to hear about poor Mellersh; his case seems hopeless. He told me some time ago how much he suffered from Sciatica and said he could not wish his worst enemy to suffer nearly so much.—³ I have nothing to tell you about myself as my days are uniformly spent in working on the Physiology of Plants. I am pleased to hear that you like my little life of D^r Eras⁴

With all good wishes, believe me | My dear Sulivan. | Yours very sincerely. | Charles Darwin.

Copy
DAR 147: 515

¹ See letter from B. J. Sullivan, 2 January [1880].

² CD had remarked on the progress of Japan in his letter to E. S. Morse, 21 October 1879 (*Correspondence* vol. 27).

³ Arthur Mellersh had mentioned his severe sciatica in his letter of 13 February 1879 (*Correspondence* vol. 27).

⁴ *Erasmus Darwin*.

From Wallis Nash 4 January 1880

Corvallis, Oregon

4th. Jan'y 1880.

Charles Darwin Esq^{re}

My dear Sir

I have been for a long while promising myself the pleasure of writing to you—I would not have imposed a long letter on you had I not known that you had more than one secretary at hand to read it to you: and certainly not with any idea that you should be bound to answer it in person— But I wish, & hope, that you may devolve the answering it on your son Frank,¹ from whom I shall look for a letter. And now, where to begin? Why, with the ancient formula, that I hope this will find you well, as it leaves us at this present. For we are all well, in spite of the violent variations of temperature for the last week or two. Observing people here say that extremes of temperature here are getting more marked year by year, the country losing its repute among the early settlers for equability, & this though the Japan or Pacific current has not changed its course so far as mariners observe, & still strikes our shores, & there has been no activity among the volcanic summits of the Cascades. I believe the observation to be correct, & only wonder if the gradual thinning out of the forests accounts for it. Two days before Christmas the frost set in suddenly: on Christmas Eve the thermometer registered 26 degrees of frost, & so stood each night for nearly a week, & then the cold left us as suddenly as it came on & the three inches of snow which covered the fields vanished— The sudden cold struck the wild geese & ducks, which swarm round us, almost but not quite by surprize. They seemed to have about two days notice, for they took flight Southwards in long Vs by hundreds at a time: we noticed their loss without putting it down to the proper cause— They came back, to gladden our sportsmen's eyes as soon as the frost went. The small birds became as tame as they do in England in frost, & we had a large congregation to gather up the crumbs under our windows— It was curious to see the American cousins of the finches and thrushes so familiar at home.

We find from a good many questions and observations in our home letters that friends are expecting us to be beginning to be tired of our new life; to be homesick; to be planning our return; & they seem surprized that we should continue to be happy here. For happy we are— We are contented to get our *Spectator*, & *Pall Mall Budget*, & *Punch*, and *Illustrated News*,² and *Field* three weeks old: and having so many people about, & each with so many correspondents we seem to be always in communication with home. But the sound of the commotions & excitements of public affairs in England seems dulled and distant, & the impression is that of patients who have struggled through some epidemic & see and know of their friends still fighting and fevered. Politics here we have none of. Our inflexible rule is non-interference—& hitherto we have resisted every seduction to declare ourselves Democrats or Republicans, or Greenbackers, or Know-nothings,³ or anything— Local interests we have in plenty, and do what we can for school, &

college, and church, and temperance Society. Our neighbours have been very kind: they have not shewn any signs of jealousy, whilst we are very much on our guard against exclusiveness, which is the one thing they would resent. You would smile, or M^{rs} Darwin would, to see M^{rs} Nash⁴ pay a call at her Grocer's or linendraper's house. She is warmly welcomed by the lady of the house, who well nigh pushes her into a (rocking) chair, & seizes her cloak & bonnet, saying "Sit *right* down, & take off your things"—"How is your health?" "How is M^r Nash: how is his health?" How do you like this "country?" And so on through a long string of stereotyped questions, now as familiar as the Catechism. And then the contrast between the dowdy, dressing gowny style of costume in their homes, and the spick & span, kid glove and new bonnet appearance with which they come here.

Some of the men, notably one or two of the college professors are very intelligent; & one or two of the doctors round about have read & still think a good deal. It is a surprize to me to find that socially the lawyer is considered to stand several degrees above the doctor. I can find *no* justification for it. Hitherto we have lived in our own well furnished, comfortable house only a mile from the town—with no lack of shops, and only too many visitors— But as soon as the season will permit us we shall move out for the summer to our "ranch" in the hills, 25 miles off, & between this & the sea. It is a valley, about 3000 acres in extent; quite among the mountains; with one main stream or "creek" running its whole length, & several smaller lateral brooks. Plenty of beaver live there now, & seem to have done so for many years past. Their roads between one stream & the next, & their dams are models of Engineering— Having had a good sized log house built (of cedar), on the banks of the stream, on a rising ground looking South West down the Valley, & having fenced & planted our orchard & garden the time of the masculines will have to be spent in bush clearing, & fencing, & grass seed sowing, & riding round after the cattle: the feminines will have the care of the house & children, assisted by our pet Chinaman, Charlie. I expect that all, children in particular, will enjoy the life to the full. We shall be surrounded by wild flowers & shrubs: the valley is rich with vegetation. The wild animals make it now their favorite home, the deer tracks cross & recross the hill sides everywhere like the sheep tracks one sees on the South Downs. A curious experiment is being worked out in our neighbourhood now. The fern is the herdsman's great enemy; the brake grows on the hill sides & tops four & five feet high, & in the dells eight & nine feet high. It has beaten all the grasses that have been tried, until they have got the "mesquit"⁵ from Mexico & Southern California during the last two years. The folk burn the fern in January or February & then sow the mesquit on the ashes. The young grass & the fern start together & the grass ousts & overcomes the fern— I have seen many acres now covered with a thick grass carpet or sod, free from fern, & where the grass is now pushing its way out beyond, into the unburnt fern. It grows a thick, succulent, blue green, herbage on which the cattle thrive & which gives a good sweet hay.

My paper is full, but my topics only begun: I should dearly like a long talk, for I know you would ask many questions of us, and there are, oh so many things, on which I would be so glad to get your replies— If one were but a botanist, & a zoologist, and

a geologist! M^{rs}. Nash means to write M^{rs}. Darwin a long letter very soon in reply to one which she was very glad indeed to get. She sends her love to you all.

And I am My dear Sir | Yours most truly | Wallis Nash

Charles Darwin Esq^{re}:

DAR 172: 4

¹ Francis Darwin.

² The *Spectator*, *Pall Mall Gazette*, *Punch*, *Illustrated London News*, and *Field* were all weekly magazines.

³ The Greenback Party was a US political party founded in 1874 in opposition to gold-backed currency; the Know Nothing party was a political party founded in the 1850s in opposition to immigration (*EB* 27: 703, 721).

⁴ Louisa A'hmuty Nash.

⁵ Mesquite grass is *Hilaria belangeri*.

To Ercole Ricotti 4 January 1880

Down, | Beckenham, Kent.

Jan 4. 1880

Sir,

I beg leave to acknowledge the receipt of your letter of Dec 29th in which you are so good as to inform me that the Royale Academy of Sciences of Turin has awarded me the great Bressa prize.¹ I hope that you will express to your Academy how deeply I feel this honour, which I believe to be the greatest which can be conferred on any scientific man. The remembrance of this award will stimulate me to do whatever little more I can do in Science during the few remaining years of my life

With great respect & gratitude | I have the honour to remain, | Sir, | Your obedient & obliged servant | Charles Darwin

P.S. | I have sent to Mess^{rs} Vincent Teja through the Union Bank of London an order signed by me to receive the 12,000 lir²

LS

www.ornithomedia.com/magazine/art_mag441_alberto_masi.pdf (accessed 1 December 2011)

¹ See *Correspondence* vol. 27, letter from Ercole Ricotti, 29 December 1879. The prize was founded by Cesare Alessandro Bressa.

² CD recorded the receipt of £418 18s. 10d. under the heading 'Bressa prize' on 17 January 1880 (CD's Account books—banking account (Down House MS)). Vincent, Teja & Co. were bankers in Turin (*Banking almanac*).

To Adolf Ernst 5 January 1880

Down, | Beckenham, Kent.

Jan 5. 1880

Dear Sir,

I have read your paper in 'Nature' with great interest & you have added a new family with heterostyled plants.¹ Your plan of trying the pollen grains in abs: alcohol

seems to me very good.² But I have taken the liberty of writing to urge you to ascertain whether the illegitimate seedlings when legitimately fertilised are less fertile than the legitimate seedlings legitimately fertilised. I believe that you possess my 'Forms of Flowers' (if you do not I should be happy to send you a copy) and I would suggest your reading my experiments on the fertility of the illegitimate seedlings of *Lythrum* & *Primula*.³ The establishment of such infertility in other cases seems to me important as bearing on the sterility of hybrids. This opinion is not confined to me, for some years ago Fritz Müller in S. Brazil told me that he intended to repeat my experiments, but I suppose that other investigations interfered.⁴

Wishing you success in all your future researches, | I remain, dear Sir, | Yours faithfully | Charles Darwin

LS

State Darwin Museum, Moscow (GDM KP OF 8972)

¹ Ernst's note 'On the heterostylism of "*Melochia Parvifolia*"' was published in *Nature*, 1 January 1880, p. 217. *Melochia parvifolia* is a species in the mallow family. In *Forms of flowers* 2d ed., p. v., CD remarked that Ernst had added the Byttneraceae to the ranks of families with heterostyled members. The Byttneraceae is now the neotropical subfamily Byttnerioideae of the Malvaceae (mallow family).

² Ernst found that pollen-grains of the short-styled form of *Melochia parvifolia* shrank more in alcohol, from which he concluded that the protoplasm of that form contained a larger percentage of water than that of the long-styled form.

³ Ernst compared the number of seeds produced from crossed and self-fertilised plants, using charts like those in *Forms of flowers* for legitimate and illegitimate offspring of *Lythrum* and *Primula*.

⁴ No letter from Fritz Müller on this subject has been found.

To Ernst Krause 5 January 1880

Down, | Beckenham, Kent.

Jan 5./80

My dear Sir,

There have been a good many reviews of the *Life*, & all without exception favourable. Some chiefly about your part, some about mine & some equally about both parts.¹ I enclose one rather good review. The others, excepting the Pall Mall which was sent you, did not seem to me worth sending. I assume that you saw the review in the *Times* which was chiefly about your part. There is an article in the *Journal of Science* just published which I have not yet read.² I am surprised that a new edition has not been called for. I hope the German edition will soon appear.³

My dear Sir, | Yours sincerely | Ch. Darwin

LS(A)

The Huntington Library (HM 36198)

¹ *Erasmus Darwin* contained a lengthy biographical notice by CD and an essay by Krause on Erasmus Darwin's scientific work.

- ² The enclosure was a review of *Erasmus Darwin* from *Popular Science Review* n.s. 4 (1880): 69–71 (see letter from Ernst Krause, 16 January 1880). Reviews of *Erasmus Darwin* were published in the *Pall Mall Gazette*, 12 December 1879, p. 12, and *The Times*, 22 November 1879, p. 6; an article-length review, ‘The history of evolutionism’, was published in the January 1880 issue of *Journal of Science*, pp. 1–8. Other reviews appeared in the *Academy*, 6 December 1879, p. 411, the *Athenaeum*, 6 December 1879, pp. 723–4, and the *Derby Mercury*, 3 December 1879, p. 6. See *Correspondence* vol. 27, Appendix V.
- ³ *Erasmus Darwin* 2d ed. was published in 1887. The German edition was Krause 1880.

To C. H. Tindal 5 January 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan 5. 1880

Dear Sir,

Your kindness has been extreme in having taken so much trouble in searching through the letters and copying the extracts.¹ I value them much, and they interest me strangely by bringing before my eyes old passed scenes. I never knew how my father (D^r R. Darwin of Shrewsbury)² became so intimate with all the Clives. I dimly remember staying when a child at Stych & seeing a very old M^{rs} Clive, no doubt the widow of the Archdeacon.³ The extract about the eagerness of the two learned divines to see a pig’s body opened is very amusing; & that about my grandfather’s character is of much value to me.⁴ I now hope that there may be a second edition of the *Life*, that I may use a few extracts from the materials which you have so kindly given me.⁵ The information about M^r Gifford & the Archdeacon is now quite sufficient. I was glad to read the P.S. about my father & about the outline made by my mother.⁶

With the most cordial thanks, I remain, | dear Sir, | Yours faithfully | Charles Darwin

LS

DAR 185: 118

¹ Tindal had sent extracts from the correspondence between Richard Gifford and Robert Clive (1722/3–92), friends of CD’s grandfather Erasmus Darwin (see letter from C. H. Tindal, 1 January 1880).

² Robert Waring Darwin.

³ Styche Hall in Shropshire was the seat of the Clive family; Rebecca Clive was the wife of Robert Clive, archdeacon of Shropshire.

⁴ See letter from C. H. Tindal, 1 January 1880, enclosures 2 and 3 (about the dissection of a pig), and enclosure 4 (about Erasmus Darwin’s character).

⁵ *Erasmus Darwin* 2d ed. was published after CD’s death, but no new material was added.

⁶ The postscripts must have been in one of the letters that CD returned to Tindal; they have not been found. In his letter of 1 January 1880, Tindal mentioned a picture cut out in black paper by CD’s mother, Susannah Darwin.

To Vincent, Teja & Co 5 January 1880

Gentlemen,

I sh^d be much obliged if you would be so good as to apply to the R. Academy of Sciences of Turin, for the sum of 12,000 lire which have been awarded to me.¹ Also

January 1880

I hereby authorise you to receive this sum for me, which please to & to transmit to the Union Bank of London on my account—²

Gentlemen | your obed & obliged servt | Charles Darwin

To M^{ssrs}. — | Turin, | Jan. 5. 1880

ADraftS

DAR 202: 68

¹ CD had been awarded the Bressa prize by the Royal Academy of Sciences of Turin (see letter to Ercole Ricotti, 4 January 1880).

² £418 18s. 10d. was deposited into CD's banking account on 17 January 1880 (CD's Account books—banking account (Down House MS)).

To A. R. Wallace 5 January 1880

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

Jan 5. 1880

My dear Wallace,

As this note requires no sort of answer, you must allow me to express my lively admiration of your paper in the *Nineteenth Cen*.¹ You certainly are a master in the difficult art of clear exposition. It is impossible to urge too often that the selection from a single varying individual or of a single varying organ will not suffice. You have worked in capitally Allen's admirable researches.² As usual you delight to honour me more than I deserve. When I have written about the extreme slowness of natural selection (in which I hope I may be wrong) I have chiefly had in my mind the effects of intercrossing.³ I subscribe to almost everything you say excepting the last short sentence.⁴

And now let me add how grieved I was to hear that the City of London did not elect you for the Epping office;⁵ but I suppose it was too much to hope that such a body of men should make a good selection. I wish you could obtain some quiet post & thus have leisure for moderate scientific work. I have nothing to tell you about myself; I see few persons, for conversation fatigues me much; but I daily do some work in experiments on plants, & hope thus to continue to the end of my days.

With all good wishes. | Believe me yours very sincerely | Charles Darwin

P.S. | Have you seen M^r Farrer's article in the last *Fortnightly* it reminded me of an article on bequests by you some years ago which interested & almost converted me.⁶

LS

British Library (Add MS 46434 ff. 286–8)

¹ Wallace's article, 'On the origin of species and genera', was published in the January 1880 issue of *Nineteenth Century* (Wallace 1880b).

² Wallace 1880b, pp. 98–102, discussed the wide range of variations between individuals of the same species, with examples from Joel Asaph Allen's monograph, 'On the mammals and winter birds of East Florida' (Allen 1871).

³ Wallace argued that if the conditions of existence changed rapidly, then natural selection could act more quickly than CD had supposed (Wallace 1880b, p. 104).

⁴ Wallace 1880b, p. 106:

I have also attempted to show that the causes which have produced species of one *genus*, of one *family*, or perhaps of one *order* from a common ancestor, are not necessarily the same as those which have produced the separate *orders*, *classes*, and *sub-kingdoms* from more remote ancestors. That all have been alike produced by “descent with modification” from a few primitive types the whole body of evidence clearly indicates; but while individual variation with natural selection is proved to be adequate for the production of the former, we have no proof and hardly any evidence that it is adequate to initiate those important divergences of type which characterise the latter.

⁵ CD had supported Wallace’s application to become superintendent of Epping Forest (see *Correspondence* vol. 26, letter from A. R. Wallace, 14 September 1878 and letter to A. R. Wallace, 16 September 1878). On his failure to obtain the post, see *Correspondence* vol. 27, letter from A. B. Buckley, 16 December 1879.

⁶ CD refers to Wallace’s essay on the injustice of trust-deeds, settlements, and wills that allowed the interests of the deceased to control the use of land and other property by the living (Wallace 1873). Thomas Henry Farrer made a similar argument in his article ‘Freedom of land’, which appeared in *Fortnightly Review*, 1 January 1880 (Farrer 1880).

From A. S. Wilson 5 January 1880

North Kinnmundy, | Summerhill, | by Aberdeen.
5 Jany. 1880.

Charles Darwin, Esq. F.R.S.
Down | Beckenham | Kent.

My Dear Sir,

I duly received your letter of the 30th and was sorry to hear that the wheats had gone amissing.¹ Perhaps they may yet turn up.

I have now had another year’s experiments with the Russian wheats, but as my report is about finished and will probably appear in the *Gards’ Chron.* I need say little on the matter at present.² I have had some letters from Dr. Asher combating the view I took last year. I reply to some of his contentions in my report. He mentions that you had supposed that Saxonka seeds or plants may have remained in the land from one season to another.³ I think myself that this is highly probable, and is a cause working in the same direction as the superior fertility of one of the wheats. I have had barleys and wheats frequently, some of the culms of which ripened seed, while others on the same stool, coming up but a few inches, stood through the winter and ripened next season. And I understand that it is not uncommon in Russia for the seeding to consist merely of what is shed during winds and harvesting. I have seen cases of this kind even here, where strong gales before harvest shook out so much as to produce what was the first stage of a good thick crop.

I have several times been going to inform you that in my first experiments with *Aegilops*, I had been misled as to the species, having used *ventricosa* in place of *ovata*.⁴ Some of the seeds of *ventricosa* I sent to you. But some years ago I got ten species from Vilmorin⁵ and think I am now clear of some errors. I mentioned a curious fact to you regarding the non-germination of the outer seeds of what I called *ovata*, but which was *ventricosa*, or *cylindrica*; and I thought that the pressure of the glumes

killed the outer embryos.⁶ But nature is not found out so soon as one would wish. I had planted a number of spikelets, uncorticated, of *ventricosa*, each containing a central and two lateral seeds. When I was going to pull up the ripe plants, consisting of from 6 to 10 culms, in autumn, what was my surprise to find in many cases the two lateral seeds which I had supposed were killed, just beginning to germinate, throwing o(ut) short plumules and roots. There they had rested during the time the central seeds had produced a crop, and were now ready to go on and produce another crop, which they did; for I transplanted a good many of them, and they stood out the winter producing ripe crops in due time. I made absolutely sure that the young plants were not tillers from the central seeds; taking out a good many of those side seeds from the glumes just beginning to germinate and planting them by themselves.

I enclose an ear of *AEgilops speltaeformis*, which is understood to be the species used by Faber. You will see that there is almost no difference between it and some wheats, except that the outer glume has an awn.⁷

Though a believer in Evolution myself, I have always doubted the rapid transformations accepted by some.

Trusting you will excuse my writing at such length | I am | yours very sincerely,
| A. Stephen Wilson.

DAR 181: 115

¹ See *Correspondence* vol. 27, letter to A. S. Wilson, 30 December [1879]; CD was expecting a collection of wheat varieties from Turkestan (see *ibid.*, letter from K. P. von Kaufman, 9 May 1879, and letter to A. S. Wilson, 4 [June] 1879).

² Wilson's experiments were designed to test whether Kubanka wheat transformed into a variety with shorter, rounder grains (Saxonka wheat), when grown in infertile soil; his report was published in *Gardeners' Chronicle*, 24 January and 7 February 1880 (Wilson 1880).

³ Georg Michael Asher had written to CD, initially through John Murray (1808–92), about the possibility of transmutation between the wheat varieties (see *Correspondence* vol. 25, letter from G. M. Asher to John Murray, 1 November 1877, and letter from G. M. Asher, 7 November 1877). CD's letters to Asher have not been found.

⁴ Wilson was studying variation in *Aegilops* (goatgrass; see *Correspondence* vol. 26, letter from A. S. Wilson, 28 February 1878). *Aegilops ovata* is a synonym of *A. geniculata*, ovate goatgrass; *A. ventricosa* is barbed goatgrass.

⁵ Vilmorin-Andriex was a Paris seed company; the head of the firm was Henry de Vilmorin (Heuzé 1899).

⁶ See *Correspondence* vol. 26, letter from A. S. Wilson, 14 March 1878. *Aegilops cylindrica* is jointgrass.

⁷ *Aegilops speltaeformis* (a synonym of \times *Aegilotriticum triticoides*) was found in Agde, France, by Esprit Fabre and named by Alexis Jordan (see Jordan 1855, p. 313, and Slageren 1994, pp. 40–1). In grasses, the glume is the lowermost bract surrounding a spikelet in the flower cluster; the awn is a hair-like appendage that in wheat typically extends from the middle bract or lemma of the floret, while in goatgrass the awn is often on the glume.

To William Cole 6 January 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan 6. 1880

Dear Sir,

I belong to so many Societies that I am not willing to join any others.

But I beg permission to enclose a donation of £1-1 in aid of your preliminary expenses,¹ & remain | dear Sir | Yours faithfully | Ch. Darwin

W. Cole Esq

LS

Imperial College of Science, Medicine and Technology Archives (Essex Naturalists Field Club MLDA/9)

¹ Cole was secretary of the Epping Forest and County of Essex Naturalists' Field Club, which was founded in 1880; the president was Raphael Meldola (*Transactions of the Epping Forest and County of Essex Naturalists' Field Club* 1 (1880): 1). A payment to Cole of £1 1s. was recorded in CD's Account books—banking account (Down House MS). CD's donation was mentioned in the *Transactions of the Epping Forest and County of Essex Naturalists' Field Club (Journal of Proceedings)* 1 (1880): lxxi.

From W. E. Darwin 6 January [1880]¹

Basset
Jan 6th

My dear Father.

We are delighted to hear they have done you so great an honour at Turin, and I entirely agree with what was I fancy Horace's notion that you should hand the proceeds over to the Laboratory at Naples.² It would be a fine opportunity of doing the Laboratory a good turn & paying scientific Italy a compliment. Hen: & George³ also would like to see this done, at all events with a portion of the sum.

I think it would be very well worth while, and interesting as a case of heridity, if you would write a Memorandum to be inserted in your Autobiography shewing how little you think Old Erasmus's work influenced you.⁴

I send you another box of "B" pens,⁵ which I hear have been in stock for 4 years, & so I hope they may be better. I tried to get at the makers

I got Bessy's card⁶ I am afraid Uncle Jos. must be near the end.⁷

Sara⁸ sends her love she is glad to get home to rest after London

Your affect son | W. E Darwin

Cornford Family Papers (DAR 275: 79)

¹ The year is established by the relationship between this letter and the letter to Ercole Ricotti, 4 January 1880 (see n. 2, below).

² CD had been awarded the Bressa prize by the Royal Academy of Sciences of Turin (see letter to Ercole Ricotti, 4 January 1880). Horace Darwin evidently suggested that CD give the prize money to the Zoological Station at Naples.

³ Henrietta Emma Litchfield and George Howard Darwin, William's sister and brother.

⁴ CD had written 'Recollections of the development of my mind and character' in 1876; handwritten copies were made for his children. Samuel Butler had recently compared CD's theory of evolution unfavourably with the theories of earlier writers, including Erasmus Darwin, hinting that CD had given insufficient credit to his predecessors (Butler 1879, pp. 196–7). For CD's remarks on his grandfather's evolutionary theory, see 'Recollections', p. 371; for his comments on Butler, see *ibid.*, pp. 419–20.

⁵ 'B pen': broad-nibbed steel pen.

⁶ Elizabeth Darwin's note has not been found.

⁷ Josiah Wedgwood III died on 11 March 1880 (Emma Darwin's diary (DAR 242)).

⁸ Sara Darwin, William's wife.

To J.-H. Fabre 6 January 1880

Down, | Beckenham Kent.

Jan 6. 80

Dear Sir,

It has gratified me much that you should have thought of sending me a copy of your 'Souvenirs Entomologiques &c'.¹ In one sense I am worthy of the gift, for I do not believe that any one in Europe has more truly admired your investigations than I have done.²

I look forward with much pleasure to reading your volume, & remain | With great respect | Yours faithfully | Charles Darwin

Mons. Fabre

LS

Muséum national d'histoire naturelle, Bibliothèque centrale, Paris (Ms FAB 32)

¹ CD's annotated copy of Fabre 1879 is in the Darwin Library-CUL; see letter from J.-H. Fabre, 3 January 1880.

² CD described Fabre as an 'inimitable observer' in *Descent* 1: 364. See also *Origin* 6th ed., pp. 216, 394.

To J. T. Murray 6 January 1880

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

Jan 6. 80

Dear Sir,

I am much obliged to you for your kindness in having taken the trouble to send me the specimens of *Drosera*, with which I am well acquainted. It is *D. binata* vel *dichotoma*¹

Dear Sir | Yours faithfully | Charles Darwin

LS

Mitchell Library, Sydney (MLMSS 3069)

¹ No letter from Murray has been found. *Drosera dichotoma* is a synonym of *D. binata*, the forked-leaf sundew, which is native to Australia and New Zealand. CD had obtained a specimen from Dorothy Fanny Nevill (see *Insectivorous plants*, pp. 281-4).

From F. B. Goodacre 7 January 1880

Wilby Rectory | Attlebro' | Norfolk

Jan 7/80

Dear Sir,

I ought to have written to thank you for the copy of "Nature" you so kindly sent me before this, allow me to do so now; I am greatly interested in your article although I remain of my old opinion:¹ There are two little facts in connexion with domestic birds I can answer for which perhaps you may like to know; some years

ago a blue mongrel pigeon of mine changed the white band over tail for a pale blue one such as many blue pigeons have; & a wild drake I bought young in the spring of 1864 is still flourishing & was the father of some 40 or so young ones which I reared last year for the table, I do not know whether there is anything remarkable in his age, having never read anything about the natural length of life of such birds

With kind regards | Believe me | yrs truly | F B Goodacre

DAR 165: 70

¹ Using specimens supplied by Goodacre, CD had successfully crossed hybrids of the common and Chinese goose, reporting his results in the letter to *Nature*, 15 December [1879] (*Correspondence* vol. 27). Goodacre believed that the Chinese goose was only a variety of the common goose, and that true hybrids could not produce fertile offspring (Goodacre 1879; see also *Correspondence* vol. 26, letter from F. B. Goodacre, 2 September 1878). The Chinese goose is a domestic variety of the wild swan goose (*Anser cygnoides*); the common European domestic goose is a variety of the wild greylag goose (*Anser anser*).

From Ernst Krause¹ 7 January 1880

Berlin N.O. Friedenstrasse 10. II.
den 7.1.1880.

Hochverehrter Herr!

Ihre gütige Mittheilung, dass die englischen Zeitschriften das kleine Buch über Erasmus Darwin freundlich beurtheilen, macht mir grosse Freude und danke ich Ihnen herzlichst dafür.² Ich muss gestehen, dass ich in Bezug auf meinen Theil nicht ganz ohne Besorgnisse hinsichtlich der Aufnahme bei englischen Kritikern war und dass es mir höchst schmerzlich gewesen sein würde, wenn man die Uebersetzung als nicht der Mühe werth erklärt, und Ihnen dadurch einen Verdruss bereitet hätte.

Der Druck der deutschen Ausgabe schreitet nur langsam vor, doch hoffe ich, dass sie jedenfalls vor Ostern fertig werden wird, da ich in diesen Tagen bereits die Correctur des 9 Bogens erhalten habe.³ Herr Reinwald hat mir nicht geschrieben, ob er von den mit Bezug auf die deutschen Verhältnisse hinzugefügten Anmerkungen Gebrauch machen will und ich werde sie ihm deshalb nicht zuschicken.⁴ Vielleicht will er, um in seinen Entschliessungen ganz frei zu sein, das Erscheinen der deutschen Ausgabe abwarten, und gegen diese Vorsicht lässt sich nichts sagen.

Mit den besten und innigsten Wünschen für das Neubegonnene Decennium zeichne ich, hochverehrter Herr | Ihr dankbar ergebenster | Ernst Krause

DAR 92: B51

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

² CD had remarked on the favourable reviews of *Erasmus Darwin* (see letter to Ernst Krause, 5 January 1880 and n. 2).

³ The German edition of *Erasmus Darwin* was published in April 1880 (Krause 1880; see letter from Ernst Krause, 19 April 1880).

⁴ CD had corresponded with Charles-Ferdinand Reinwald about a French translation of *Erasmus Darwin* (see *Correspondence* vol. 27, letter to C.-F. Reinwald, 29 October 1879); however, no French edition was published.

To Antonio Mendola 8 January [1880]¹

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

Jan^y. 8th. 1879

Dear Sir

I am obliged to you for your kindness in writing to me & for the specimens which you are so good as to offer to [send]. Your statements are so remarkable that I can assure you that no naturalist, without *repeated* & most (care)ful observations with all the many chances of error avoided, would think them worthy of any consideration.—² I am myself so much engaged on other work, that I cannot undertake the investigation, & therefore will not trouble you to send the horn.

I beg leave to remain | Dear Sir | Yours faithfully & obliged | Charles Darwin

Christie's (dealers) (7 June 2010)

¹ The year is established by the relationship between this letter and the letter from Antonio Mendola, 29 December 1879. CD wrote '1879' in error.

² Mendola claimed that roots had grown from a calf's horn buried in earth, and that hairs from the tails of donkeys, mules, and horses had come alive when placed in water (see *Correspondence* vol. 27, letter from Antonio Mendola, 29 December 1879).

To A. S. Wilson 8 January 1880

Down

Jan. 8. 1880

My dear Sir

I am much obliged for your interesting letter. I am glad that you have solved the puzzle of the outer seeds.—¹ I am very unwilling to believe in Nature blundering, and I imagined that the rootlets, which so clearly manage to escape from between the glumes, perhaps afterwards entered, consumed and digested these seeds. I therefore looked twice at ears which had been in the ground for several months, but could not detect the entrance of any radicles. I was, however, astonished at the whiteness of these seeds when cut into two, yet never thought about their being alive and capable of subsequent germination!

I quite agree with you about the great improbability of sudden transformations,
| My dear Sir, | Yours very sincerely | Ch. Darwin.

P.S. I have ceased to take in *Gardeners Chronicle*, from want of time to read it. Will you therefore send me a post card when you know number of *Journal* containing your Report.²

Copy

DAR 148: 369

¹ See letter from A. S. Wilson, 5 January 1880. Wilson found that the outer seeds in a glume of *Aegilops* (goatgrass) that had failed to sprout when planted were starting to germinate the following season.

² Wilson's report was published in *Gardeners' Chronicle*, 24 January and 7 February 1880 (Wilson 1880).

From A. R. Wallace 9 January 1880

Waldron Edge, Duppas Hill. | Croydon.
Jan^y. 9th. 1880

My dear Darwin

It is a great pleasure to receive a letter from you sometimes—especially when we do not differ very much.¹ I am of course much pleased & gratified that you like my article.² I wrote it chiefly because I thought there was something a little fresh still to say on the subject, & also because I wished to define precisely my present position which people continually misunderstand. The main part of the article forms part of a chapter of a book I have now almost finished on my favourite subject of “Geographical Distribution”. It will form a sort of supplement to my former work & will I trust be more readable & popular—³ I go pretty fully into the laws of variation & dispersal—the exact character of specific & generic areas, & their causes,—the growth dispersal & extinction of species & groups, illustrated by maps &c.—Changes of geography & of climate as affecting dispersal with a *full discussion* of the Glacial theory adopting Croll’s views (part of this has been published as a separate article in *Quarterly Rev.* of last July, & has been highly approved by Croll & Geikie)⁴—a discussion of the theory of *permanent continents & oceans*, which I see you were the first to adopt, but which geologists I am sorry to say quite ignore—⁵ All this is preliminary—Then follows a series of chapters on the different kinds of **Islands**—Continental & Oceanic, with a pretty full discussion of the character, affinities, & origin of their fauna & flora in typical cases. Among these I am myself quite pleased with my chapters on New Zealand, as I believe I have fully explained & accounted for *all* the main peculiarities of the New Zealand & Australian Floras. I call the book **Island Life**: &c. &c. & I think it will be interesting.

Thanks for your regrets & kind wishes anent Epping.⁶ It was a disappointment, as I had good friends in the Committee & therefore had too much hope. I may just mention that I am thinking of making some application through friends for some post in the new *Josiah Mason College of Science* at Birmingham,⁷ as *Registrar* or *Curator & Librarian* &c. The Trustees have advertised for Professors to begin next October. Should you happen to know any of the Trustees or have any influential friends in Birmingham perhaps you could help me.

I think this book will be my last as I have pretty well said all I have to say in it, and I have never taken to experiment as you have. But I want some easy occupation for my declining years with not too much confinement or desk-work which I cannot stand. You see I had some reason for writing to you; but do not you trouble to write again unless you have something to communicate.

With best wishes | Yours very faithfully | Alfred R. Wallace

I have not seen “Fortnightly” yet but will do so.⁸

Charles Darwin Esq.

DAR 106: B142-3

- ¹ See letter to A. R. Wallace, 5 January 1880.
² ‘The origin of species and genera’ (Wallace 1880b).
³ *Island life* (Wallace 1880a); his previous work on geographical distribution was *The geographical distribution of animals* (Wallace 1876).
⁴ Wallace’s review of works by James Croll, James Geikie, and others was published in the July 1879 issue of *Quarterly Review* ([Wallace] 1879). Croll had argued that glacial epochs occurred during prolonged periods of high eccentricity of the earth’s orbit (see *Correspondence* vol. 16, letter from James Croll, [2 December 1868]; see also Croll 1875 and [Wallace] 1879, pp. 233–44).
⁵ In *Island life* (Wallace 1880a, pp. 97–8), Wallace discussed the permanence of continents and oceans, citing CD’s remarks in *Origin* 6th ed., p. 288.
⁶ Wallace had failed to be appointed superintendent of Epping Forest (see letter to A. R. Wallace, 5 January 1880 and n. 5).
⁷ Mason College in Birmingham was founded in 1880 by the manufacturer Josiah Mason (*ODNB* s.v. Mason, Josiah).
⁸ See letter to A. R. Wallace, 5 January 1880 and n. 6.

To the Darwin children 10 January 1880

Excess of my income to be distributed & ordered on Jan 12th to be paid into each account.¹ N.B I invested £800 early in year otherwise the distribution wd have been larger.

	£	
William	360	600 to Williams
Bessy	240	Bank
George	360	
Frank	360	
Leonard	360	
Horace	360	
Henrietta	240. ²	
	£2280	

Each child must put initial to show he has seen it.

C. Darwin

Everybody has seen this. | C.D

Jan 10th. 1880.—

To *William*

Do not be in such a hurry in investing Bessy’s money, both she & her mother have been howling all summer as if I had starved her death. The only way to feel rich is to have a good balance in the Bank.

C. D

DAR 210.6: 155

- ¹ CD had informed his children of his plan to distribute the surplus income from his investments in 1879; each daughter was to receive two-thirds of the amount allotted to each son (see *Correspondence* vol. 27, letter to the Darwin children, 21 February 1879).
- ² William Erasmus Darwin, Elizabeth Darwin, George Howard Darwin, Francis Darwin, Leonard Darwin, Horace Darwin, and Henrietta Emma Litchfield. '600 to Williams Bank': this included the sum allotted to Bessy, which William was to deposit or invest. William was a partner in the Southampton and Hampshire Bank.

From E. A. Wheler 10 January 1880

3 Bertie Terrace
10 Jan^y 1880.

My dear Cousin,

I will not delay longer to thank you very much for so kindly sending me a copy of my Grandfather's life last November.¹ I have read it several times over, with the *greatest interest*, & all his descendants must feel much indebted to you for writing it, & undoing Miss Seward's calumnies.² I am surprised though at your dwelling so much on my Grandfather being so pitted with small pox.³ It must have quite worn off in later life, for I have heard my Mother⁴ over & over again say "his complexion was beautiful, like a child's", & she also used to remark upon "the sweetness of expression in his mouth"— He had lost all his teeth, & I have the scoop he always used when eating apple &c. My sister Adele Bunbury & M^{rs} Nixon who have been here both remember with me, that my Mother & Aunt Gisborne spoke of his complexion being so good & clear in his later life.⁵ I think the Photograph of him at the beginning of the book very good, far better than one I have, taken from Rollason's picture. Will you kindly give me the name & address of the Photographer, as I should much like to order one or two?⁶

I must not conclude without congratulating you on your son's recent marriage⁷ & with kind remembrances to M^{rs} Darwin & your Daughter⁸ Believe me | Yours very truly, | Elizth. A Wheler.

I am sure you will be sorry to hear we have just received the news of Violetta Darwin's death, which took place yesterday.⁹ She has long been suffering from dropsy.

DAR 99: 197–8

- ¹ Wheler's name is on the presentation list for *Erasmus Darwin* (see *Correspondence* vol. 27, Appendix IV, and letter from E. S. Galton, 12 November 1879).
- ² CD was highly critical of remarks by Anna Seward in her biography of Erasmus Darwin (see Seward 1804, pp. 64–8 and 406, and *Erasmus Darwin*, pp. 70–80).
- ³ *Erasmus Darwin*, p. 54.
- ⁴ Violetta Galton.
- ⁵ Millicent Adele Bunbury, Emma Nixon, and Millicent Gisborne.
- ⁶ The Autotype Company produced the frontispiece of *Erasmus Darwin*, a photograph of an engraving of a painting by Joseph Wright (see *Correspondence* vol. 27, letter to John Murray, 4 September 1879). James Rawlinson painted two portraits of Erasmus Darwin, one of which was purchased for Wheler by Emma Nixon (see Keynes 1994, p. 79).
- ⁷ Horace Darwin and Ida Farrer were married on 3 January 1880 (Emma Darwin's diary (DAR 242)).

⁸ Elizabeth Darwin.

⁹ Violetta Harriot Darwin died on 9 January 1880 (*Darwin pedigree*).

To Horace Darwin 11 January [1880]¹

Down
Jan 11th

My dear Horace

Everybody has seen the enclosed except William so please forward it to him immediately.—²

I suppose that you & Ida are too high in the sky to care for base money.³

Your affect. | C. Darwin

DAR 185: 7

¹ The year is established by the relationship between this letter and the letter to the Darwin children, 10 January 1880.

² William Erasmus Darwin. The enclosure was the letter to the Darwin children, 10 January 1880.

³ Horace had married Ida Farrer on 3 January 1880 (Emma Darwin's diary (DAR 242)).

To C. E. Ferguson 12 January 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan 12/80

Dear Sir

If you will read Häckels "Evolution of Man"—if this Translation has appeared in America, or his Schöpfungsgesichter—and my Descent of Man, I think that you will find reference to everything important.—¹

Dear Sir | Yours faithfully | Ch. Darwin

Indiana University, The Lilly Library (Ferguson MSS.)

¹ Ferguson had asked for a list of books on 'the theory of evolution as relating to man' (see *Correspondence* vol. 27, letter from C. E. Ferguson, 27 December 1879). The US edition of Ernst Haeckel's *Natürliche Schöpfungsgeschichte* was published by D. Appleton & Company in 1880 (Haeckel 1880). *Descent* 2d US ed. was also published by Appleton.

From Asa Gray 12 January 1880

Herbarium of Harvard University, | Botanic Garden, Cambridge, Mass.
Jan 12 1880.

My Dear Darwin

So you want some cotton-seeds!¹ I sent south for them, & here will be all you need, but you can have more. Two-kinds; those in the copious white wool probably best. That is 'Green-seed' from Alabama. The other is sea-island.²

The Man who sent the seed has heard of "*Vine Cotton*", of which he once had a few seeds "sent from England" and he wants more—to grow.³ Now you ask Hooker⁴

if he knows of such a thing, and can get hold of some for this southern planter,—& will send by post hither.

Ever Yours | Asa Gray

DAR 165: 201

¹ CD had requested seeds of ‘common cotton’ in his letter to Asa Gray, 16 December 1879 (*Correspondence* vol. 27).

² Alabama cotton is *Gossypium hirsutum* (upland or short-staple cotton); sea-island cotton is *G. barbadense* (pima or extra-long-staple cotton).

³ The man who sent the seed has not been identified. A variety known as vine cotton was grown in British Guiana and Jamaica; it was believed to have originated from sea-island cotton (*Gossypium barbadense*; see Duff 1866, p. 182).

⁴ Joseph Dalton Hooker.

To Alfred Krakauer 12 January 1880

I am much obliged for your note. I have heard of other analogous cases, but there remains the doubt whether they may not be accidental coincidences, for such cases certainly occur in non-Jewish families.—¹

Yours faithfully & obliged | Ch. Darwin

Down, Beckenham | Jan 12.—1880.

ApcS (photocopy)

British Library Surrogate (RP 4481/3)

¹ Krakauer’s note has not been found and the cases referred to have not been identified. The recipient is given in the J. S. Stargardt catalogue for an auction on 4 and 5 October 1989 (Catalogue 645); he was an ear specialist.

From Lawson Tait 12 January 1880

7. Great Charles St | Birmingham.
Jan 12/80

My Dear Sir,

This day month is your birth day and we propose here to have a “Darwin Festival” on a small scale probably, but as hearty and as large as we can secure. May I ask if you would be willing to accept some little expression of our gratitude for your work—say an illuminated address or something of the kind? Also can you refer me to any biographical notes beyond that of “Men of the Time” & Dublin University Magazine?¹ The date in question is the completion of your 70th year is it not?—or rather the day before is—& that your 71st birth day.²

Our Philosophical Society³ meets that day, therefore arises the opportunity.

Yours truly, | Lawson Tait

DAR 178: 41

¹ Biographical articles on CD appeared in *Men and women of the time* (1865) and *University Magazine* 92 (August 1878): 154–63 ([Lubbock] 1878); *Dublin University Magazine* was renamed *University Magazine* in 1878.

² CD was 71 on 12 February 1880.

³ Tait was a council member of the Birmingham Philosophical Society (*Proceedings of the Birmingham Philosophical Society* 2 (1879–81)).

From J. I. Rogers 13 January 1880

1 Ravenna R^d. Putney S.W.
13th January 1880.

Dear Sir,

With reference to your article in last weeks “Nature” it appears to me, that in some cases the *forewings* of our British Butterflies, are either conspicuously marked underneath, or are transparent, so that the bright colour of the upper surface shows through when the insects are flying.¹

The undersurface of the *hinder* wings, is generally very soberly coloured in these instances, and (if my impression be correct) I think that when the insect is settled, the bright portion of the undersurface of the forewing, is hidden, or covered, by the dull part of the hind wing

If this be so, the insects when flying would be aided in finding each other, by being able to see bright colours both from above and from below: while this would not prevent complete protection when the butterfly was at rest.

Cases in point, are, the Red Admiral; the peacock, (to a less degree); the painted Lady; the large & small Tortoiseshell; the small Copper; the clouded Yellow; the pale clouded Yellow; the small Heath; the Grayling; the Meadow Brown; and the Orange Tip.²

It seems strange that some of our brilliant Butterflies, like the Red Admiral, should be so conspicuous when flying, & when therefore most exposed to the attacks of birds; and so dully coloured when they are at rest and so little exposed— If the bright colours were warnings that the species was uneatable, one would expect the undersurfaces to be similarly coloured.

It may be that the butterflies are on the wing chiefly at times when birds are not so. Or that the bright colours are warnings to certain foes, while the dull ones are protections against others. Or again, the ready recognition of the butterflies by each other, may be so advantageous for the continuation of the species, as to outweigh the risk of capture by birds.

Some butterflies—the metallic looking fritillaries for instance—appear most attractive when the wings are closed. possibly in such cases the sexes seek each other when one or the other is at rest.

The brighter colours of the White Admiral & the Swallow tail³ are transparent, so that the Insects are readily seen both from above & below when flying.

Believe me to be, dear Sir | yours obediently | J Innes Rogers.

Ch. Darwin Esq F.R.S.

DAR 176: 200

- ¹ CD's letter to *Nature*, 16 December 1879 (*Correspondence* vol. 27) was published in the 8 January 1880 issue of the journal. CD discussed cases in which the wings of male butterflies were iridescent on the upper surface only from a particular point of view, and so noticeable in general to approaching females, while the dull under surface was visible when the wings were closed and thus served a protective function when the butterflies were at rest.
- ² The red admiral is *Vanessa atalanta*; the peacock is *Aglais io*; the painted lady is *Vanessa cardui*; the large tortoiseshell is *Nymphalis polychloros*; the small tortoiseshell is *Aglais urticae*; the small copper is *Lycena phlaeas*; the clouded yellow is *Colias croceus*; the pale clouded yellow is *Colias hyale*; the small heath is *Coenonympha pamphilus*; the grayling is *Hipparchia semele*; the meadow brown is *Maniola jurtina*; the orange tip is *Anthocharis cardamines*.
- ³ The white admiral is *Limenitis camilla*; the swallow-tail is *Papilio machaon*.

To Lawson Tait 13 January 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Jan 13. 1880

My dear Sir

The honour which you propose to do me is a great one.¹ But would it not be better to wait until I am in my grave? Nevertheless if you & friends remain of the same opinion, I can express only my gratitude & the wish that I were more worthy of the honour. I know of no other biographical notices published in England so full as the two to which you allude. A good sketch by Prof. Preyer appeared in the last Feb^y. no^r. of *Kosmos*, as a sort of commemoration of my Birth-day.² I could lend you the no^r, if you sh^d. think it worth while to read it in German, which to almost all Englishmen is a great trouble & sorrow.

I was born on Feb. 12th 1809, so shall be 71 next Feb.^y

Pray reflect on what I have said about my grave, & till then, pray believe me |
Yours very faithfully | Charles Darwin

Shrewsbury School, Taylor Library

¹ See letter from Lawson Tait, 12 January 1880.

² The biographical sketch by William Preyer appeared in the special issue of *Kosmos* commemorating CD's 70th birthday (Preyer 1879).

From W. C. Williamson 13 January 1880

Fallowfield | Manchester
Jan 13th 1880

My Dear Darwin

Though you are working now at very different objects, I thought you might be interested to see the enclosed seedling of *Drosera Capensis*:¹ I sowed the seed gathered from my own plant about midsummer or rather later and i(t) (has) come up very freely () I sowed upon Sphag(num) packed *tightly* into a pot and then cut very close— I also scattered some upon the peat of the pot in which one of my *Odontoglossums*² is growing— Both have come up—but the sphagnum pot is the best—I have to keep the moss down by Clipping or it would soon smother the *Drosera*—

You will see that like my seedling plants of *Spathulata* (which are flourishing splendidly) it is in the shape of *rotundifolia*³

Have you noticed how large & fleshy the roots of *Capensis* are? and also that the roots of *dichotoma* are also very large & numerous—unless the *latter* are subterranean rhizomes seeing that they give off foliar buds so freely.—⁴

I am ever yours | W C Williamson

DAR 181: 107

¹ *Drosera capensis* is Cape sundew.

² *Odontoglossum* is a genus of orchids.

³ Williamson had sent specimens of *Drosera spatulata* that resembled *D. rotundifolia* (see *Correspondence* vol. 26, letter from W. C. Williamson, 20 September 1878). *Drosera spatulata* is the spoon-leaved sundew (*spatulata* is a common misspelling); *D. rotundifolia* is the common or round-leaved sundew.

⁴ *Drosera dichotoma* is a synonym of *D. binata*, the forked-leaf sundew. This plant reproduces both by seed and vegetatively by means of underground rhizomes.

To Jabez Hogg 14 January 1880

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

Jan 14/80

My dear Sir,

I am glad that you are calling attention to the arsenic question.¹ I cannot answer your question positively, but have hardly any doubt that it was my grandfather Erasmus, as my father never published on medical subjects.²

Dear Sir | Yours faithfully | Ch. Darwin

LS

Maggs Brothers (dealers) (catalogue 1453, 2011)

¹ Hogg had recently published a pamphlet on poisoning from arsenic pigments in wallpaper (Hogg 1879). For more on environmental arsenic poisoning in Victorian Britain, see Bartrip 1994.

² No letter from Hogg has been found. Erasmus Darwin discussed the medical uses of arsenic in *Zoonomia* (E. Darwin 1794–6, 2: 726–28); no publications by him on the dangers of arsenic have been found. CD's father was Robert Waring Darwin.

From Daniel Mackintosh 15 January 1880

36 Whitford Road | Tranmere, | Birkenhead,

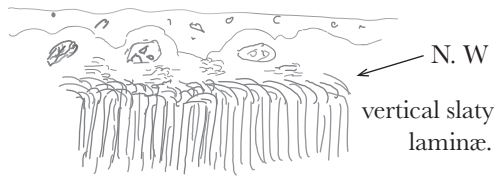
15th Jan. 1880

Dear Sir,

I send a syllabus of a paper I am about forwarding to the Geological Society.¹ I can easily understand how the retreat of the sea may have left the terraces of Patagonia,² while in this neighbourhood the sinking of the land was the cause of the submergence. But I have not made any positive assertions on the subject.

I find from your paper in the Phil. Magazine that you were the first to notice the proofs of the violent stranding of floating ice, which I cannot but regard as the

most remarkable of all the Moel Tryfan phenomena.³ I have made use of the term “violent impact” &c in my paper.



Clay above all—parcels of clay rolled up in sand,—& parcels of slate chips rolled up in sand.

With many thanks for the letters with which you have honoured me,⁴ | I am,
Dear Sir, | Yours very truly, | D Mackintosh

DAR 171: 9

- ¹ The syllabus has not been found. Mackintosh’s paper on the Moel Tryfan deposits in Wales was published in the *Quarterly Journal of the Geological Society of London* (Mackintosh 1881).
- ² In ‘Distribution of the erratic boulders’, CD discussed the submarine origin of terraces in the valley of Santa Cruz, Patagonia.
- ³ CD’s paper, ‘Ancient glaciers of Caernarvonshire’, was published in the *Philosophical Magazine* in 1842; it described deposits on Moel Tryfan (a mountain in Wales) as shattered and rounded by icebergs grating over the surface (*ibid.*, p. 144).
- ⁴ CD had praised Mackintosh’s paper on erratic boulders (Mackintosh 1879; see *Correspondence* vol. 27, letters to Daniel Mackintosh, 9 October 1879 and 16 October 1879).

To Lawson Tait 15 January [1880]¹

From Mr. C. Darwin, Down, Beckenham.

Kosmos despatched by this Post.—² Kindly return it me when done with, as I have no duplicate copy.— When my son³ returns home, I will see about some letter from Erasmus D., as I do not doubt I can spare one.—⁴

C.D.

Jan. 15th

ApcS (photocopy)
DAR 221.5: 41

- ¹ The year is established by the relationship between this letter and the letter to Lawson Tait, 13 January 1880.
- ² CD sent the issue of *Kosmos* containing a biographical sketch by William Preyer (Preyer 1879; see letter to Lawson Tait, 13 January 1880).
- ³ Francis Darwin.
- ⁴ Tait’s request for a letter from Erasmus Darwin has not been found.

To W. T. Thiselton-Dyer 15 January 1880

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

Jan 15—80

My dear Dyer:

It was very very good of you to have taken such great trouble about the cotton-seeds. Some of those from Naples show this day their noses above ground, & I have no doubt will serve my purpose excellently.¹ I hope my work will some day end, but new points are continually turning up.

I am now observing the germination of the Cucurbitaceous genus *Megarrhiza*, & it is a very curious case, for the petioles of the 2 cotyledons (which never break out of the seed-coats) unite & form a hollow tube which penetrates deeply into the ground, & becomes functionally (& structurally as far as surface is concerned) a root; the plumule bursts the tube at a depth of 2–3 inches beneath the ground & then rises to the surface.—²

Ever yours sincerely | Ch. Darwin.

You are an unfortunate man: writing about *Megarrhiza* has made me think that I ought to look again at the germinating seeds of *Trichosanthes anguina* or I daresay any *Trichosanthes*, for I record in my notes that “cotyledons fleshy almost like hypogean ones”. Therefore the hypocotyledonous stem ought not to be provided with that wonderful peg or heel by which the seed-coats of most other Cucurbitaceæ are torn apart beneath the ground, & which I was delighted to find quite absent in *Megarrhiza*.³ Am I not a superb bore?!

Can you give me seeds of *Trichosanthes*; but do not write.—

Royal Botanic Gardens, Kew (Darwin: Letters to Thiselton-Dyer, 1873–81: ff. 199–200)

¹ CD had requested cotton seeds in his letter to W. T. Thiselton-Dyer, 20 November 1879 (*Correspondence* vol. 27). CD studied sleep in the cotyledons of ‘Naples’ cotton; his notes from 21 January to 17 February 1880 are in DAR 209.9: 16.

² CD studied germination in *Megarrhiza californica*, a synonym of *Marah fabacea*, California manroot; Cucurbitaceae is the family of gourds and squashes. He observed the plant between 14 and 30 January 1880 (his notes are in DAR 209.6: 105–15; his observations of the plumule, the stem immediately above the cotyledons, are in DAR 209.6: 107 and DAR 209.6: 109). See also *Movement in plants*, pp. 81–3.

³ *Trichosanthes anguina* is a synonym of *T. cucumerina*, snake gourd. CD had noted its ‘thick & fleshy’ cotyledons in April 1877 (DAR 209.9: 99); he made further observations of the peg or heel of the radicle later in January 1880 (DAR 209.6: 165). See also *Movement in plants*, pp. 102–6.

From Ernst Krause¹ 16 January 1880

Berlin N.O. Friedenstrasse 10. II.

den 16.1.80.

Hochverehrter Herr!

Zunächst meinen ergebensten Dank für die freundliche Uebersendung der Blaetter aus der Popular-Science-Review abstattend,² muss ich leider hinzufügen, dass ich mit meinen Plackereien immer noch nicht am Ende bin. Ich hoffe aber, es

wird die letzte Auskunft sein, um die ich Sie, in Sachen des kleinen Buches bemühen muss, nämlich mit der Bitte, mir freundlichst sagen zu wollen, was das Amt des auf Seite 1 erwähnten *yeoman of the armoury* für Funktionen in sich schloss?³ Ich habe das Wort im Text unübersetzt gelassen, fühle mich aber verpflichtet, in den Anmerkungen ein Wort darüber zu sagen und kann in deutschen Lexicis keine passende Uebersetzung dazu finden. War es, was man jetzt Director oder Vorsteher nennen würde, oder mehr ein subalternes Amt, wie Aufseher, Schlieser und dergl.?

Ferner möchte ich Sie noch um gütige Auskunft bitten, ob der S. 34 erwähnte "alte Hooker", mit Richard Hooker (X 16) identisch ist?⁴

Verzeihen Sie, diese wiederholten Attentate auf Ihre kostbare Zeit, hochverehrter Herr | Ihrem | Dankbar ergebenen | Ernst Krause

DAR 92: B52

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

² A review of *Erasmus Darwin* was published in *Popular Science Review* 19 (1880): 69–71. See letter to Ernst Krause, 5 January 1880.

³ In *Erasmus Darwin*, p. 1, William Darwin (of Cleatham, 1573²–1644) is described as a 'yeoman of the armoury of Greenwich to James I and Charles I'. A yeoman, in this sense, was a servant or attendant in a royal household, usually of a superior grade (*OED*).

⁴ 'X 16' is a footnote marker; this was changed to n. 18 in Krause 1880, p. 20.

From the Darwin children 17 January 1880

Jan 17. 1880

Dear Father,

We hope that you will let us give you the accompanying fur-coat. We know that you will not often wear it and that you will think it too magnificent, but we cannot bear to think that when you *do* travel in winter you should suffer from cold; and so we hope you will forgive this little indulgence of the feelings of your affectionate children

William. | Sara. | Henrietta. | George. | Bessy. | Frank. | Leonard | Horace |
(per HEL by his request)¹

DAR 99: 208

¹ William Erasmus Darwin, Sara Darwin, Henrietta Emma Litchfield, George Howard Darwin, Elizabeth Darwin, Francis Darwin, Leonard Darwin, and Horace Darwin.

To the Darwin children 17 [January 1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Saturday evening 17th

My dear Children.

I have just found on my table your present of the magnificent fur-coat.² If I have to travel in the winter, it will be a wonderful comfort, for the last time I went to

London I did not get over the cold for 2 or 3 days.³ The coat, however, will never warm my body so much as your dear affection has warmed my heart.

My good dear children | Your affectionate Father | Charles Darwin

N.B. I should not be myself if I did not protest that you have all been shamefully extravagant to spend so much money over your old father, however deeply you may have pleased him.

To Henrietta—please send this on to William, who can send it to Lenny. No man knows where Horace is.—Bessy will be with you.—⁴

DAR 211: 1

¹ The month and year are established by the relationship between this letter and the letter from the Darwin children, 17 January 1880.

² See letter from the Darwin children, 17 January 1880.

³ CD was last in London from 3 to 11 December 1879 (Emma Darwin's diary (DAR 242)).

⁴ Henrietta Emma Litchfield, William Erasmus Darwin, Leonard Darwin, Horace Darwin, and Elizabeth Darwin. Horace and Ida Darwin were on honeymoon in Penzance (letter from Emma Darwin to W. E. Darwin, 19 January 1880 (DAR 219.1: 131)).

From Lawson Tait 18 January [1880]¹

7, Great Charles St | Birmingham
Jan 18

My Dear Sir,

I have pleasure in informing you that the Council of our Philosophical Society has received the proposal to commemorate your birthday with enthusiasm, that we propose to elect you our first honorary member and to vote you an address which shall be proposed by our President, Dr. T. P. Heslop² our leading Physician and seconded by, yours truly.

I am entrusted with the drafting of the address and if not bothering you too much I shall send you a rough copy of what I propose, after it has been submitted to my colleagues, in order that you may express any desire for alteration which may occur to you and that you may respond as you may think fit. It will of course be properly engraved before you receive it formally.

We desire to do this, not only for the honouring of a man whom we regard as the head of the scientific world, but to give a fillip to the removal of that singular terror which the word "Darwin" excites in the minds of religionists and other uneducated people—that is the bulk of the community

Yours faithfully | Lawson Tait

DAR 99: 213–214

¹ The year is established by the relationship between this letter and the letter to Lawson Tait, 13 January 1880.

² Thomas Pretious Heslop was president of the Birmingham Philosophical Society for 1879–80 (*Proceedings of the Birmingham Philosophical Society* 2 (1879–81)).

To W. T. Thiselton-Dyer 18 January [1880]¹

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

Jan 18th

My dear Dyer

The sight of the seeds of *Trichosanthes* delighted me.—²

In last *Nature* there is a Review of Erasmus Darwin, which I think much the best which has appeared & has pleased me greatly.—³

I can give no sort of reason, but whilst reading it, I kept on thinking that you were the author. If you are not, which is of course the most probable view, pray forgive me for writing; but if you are I am sure that you will not grudge me the pleasure of thanking you heartily for so appreciative & good a review. If you are the author do please tell me; if you are not I shall so understand by your silence.

Yours very sincerley | Ch. Darwin

Royal Botanic Gardens, Kew (Darwin: Letters to Thiselton-Dyer, 1873–81: ff. 201–2)

¹ The year is established by the relationship between this letter and the letter to W. T. Thiselton-Dyer, 15 January 1880.

² CD had requested seeds of *Trichosanthes anguina* (a synonym of *T. cucumerina*, snake gourd), or any *Trichosanthes*, in his letter to W. T. Thiselton-Dyer, 15 January 1880.

³ The anonymous review of *Erasmus Darwin* was published in *Nature*, 15 January 1880, pp. 245–7.

From Hermann Welcker¹ 18 January 1880

Halle a d. Saale,

18. Januar 1880.

Hochverehrter Herr!

Gestatten Sie, dass ich Ihnen, hochverehrter Herr, dessen lichtbringende Gedanken auf jeder Etappe meiner Studien mir begegnen, dessen befruchtende Anregung mich bei meinen Forschungen so (vie)lfach gefördert hat, einige meiner bescheidenen Publ(ica)tionen vorlege.²

Die Anordnung des *ligamentum te(res) femoris*, () Sonderbare, (dass) dieses B(and) die Hüftkapsel frei und umgreifbar durchzieht, sein Fehlen beim (O)rang u(nd) so manches Andere veranlas(sten) mic(h), dieses Band bei verschiedenen Thieren naeher zu untersuchen. Ich fand (wie dies die Zeichnung pag. 232 der (mit) III bezeichneten Abhandlung³ andeut(et)) beim Tapir-Embryo das lig. teres (femoris der) ganzen Laenge nach, pilasterart(ig) an die Wandung der Kapsel ang(elegt,) beim erwachsenen Tapir frei und u(m)greifbar, in's Innere des Gelenk(es) (ein)gerückt; bei Phoca⁴ aber in (verschiedenen) (Le)bensaltern denselben Zustand, (wie beim) Tapir-Embryo, so dass ($\frac{1}{2}$ line) Entwicklung des lig. teres beim Menschen der Reihe nach dieselben Zustaende transitorisch aufweist, die in der Thierreihe bei bestimmten Thieren die definitiven Zielpunkte der Entwicklung sind.— Interessant schien mir ferner, dass ich bei zweien sonst ganz normalen menschlichen *Schultergelenken* eine strangförmige Partie der Fasern des Kapselbandes ganz aehnlich columnenartig

in das Innere des Gelenkes eingerückt fand, wie dies nach obigen Untersuchungen bei der Entwicklung des lig. teres femoris die Regel ist. Jene Schultergelenke zeigten eine fovea capitis *humeri* und ganz denselben Zustand, wie das Hüftgelenk des Seehundes und des menschlichen Embryo (conf. die mit I bezeichnete Abhandlung, pag. 76)⁵.

Ges(ta)tten Sie, dass ich Ihre Aufmerksamkeit auf ein anderes eigenthümliches Vorkommniss, den freien Durchgang der *Sehne des musculus biceps brachii* durch die Schulterkapsel, lenke. Beim Pferde, Tapir, Maulwurf, fand ich den in Abh. IV, pag. 22, 23, und pag. 25 bei 1 abgebildeten Zustand: die Sehne liegt ganz ausserhalb des Gelenkes, freivorbeistreifend.⁶ Bei *Vespertilio*, etc, s(ow)ie beim 2 monatlichen menschlichen Embryo fand ich den auf pag. 25 bei 2 und 3, sowie pag. 27, angedeuteten Zustand; die Sehne hatte sich in die Wandung der Kapsel eingedraengt ja durchgedraengt, die Sÿnovialmembran mesenteriumartig hinter sich nachschleppend.⁷ So namentlich bei *Phoca* (pag. 29), bis schliesslich nach Zerreissung des Mesotenontium die Sehne, wie beim erwachsenen Menschen, frei durch das Gelenk (du)rchzieht. Man sollte die gesammte (Sac)u(get)hierreihe auf dieses Structurverhaelt(nis) (unter)suchen.

Von Interesse für die Descendenzlehre scheint mir ferner der in Abh. II, pag. 181, beim *Baeren* nachgewiesene Zustand, wo das Muskelbündel z, welches von dem m. coracobrachialis aus eine Brücke nach dem glenoradialis zu schlagen beginnt, die phylogenetische Entwicklung des biceps erkennen laesst.⁸

Ich möchte ferner Ihre Aufmerksamkeit auf in Abh. V, Taf. 1 abgebildete, von einigen Forschern für ein Amnion gehaltene, von mir als *Epitrichium* bezeichnete Haut lenken.⁹ Sollte in der Entwicklung dieser Haut, welche meinen Untersuchungen zufolge die sich abhebende oberste Schicht des Epidermoid(al)blattes ist, nicht ein Vorgang anerkannt werden müssen, der sonst bei höheren Thieren feh(lt:) eine die Entwicklung begleitende *Häutu(ng?)*

Einige Untersuchungen, welche ich ü(ber) die künstlich verkrüppelten Füsse der (Chi)nesinnen angestellt habe, führten mich (auf) die Frage, ob diese Fusstoilette nicht irgend w(el)chen erblichen Einfluss auf die Groesse un(d) Entwicklung der Füsse dieses Volks ausge(übt) habe? Wirklich scheint es so (Arch. f. Anthrop. V, 1872, p. 149); die Füsse auch der nicht g(e)bundenen Chinesinnen scheinen sehr klein (zu) sein: mittlere Fusslaenge bei 8 Javanesisinnen 248 Mm.; bei 3 Tahitierinnen 243; bei 8 Sundan(esinnen) 242 Mm.; bei 3 Chinesinnen 232 Mm.¹⁰

Noch erlaube ich mir, Ihnen zu gü(tiger) Aufnahme ein Heft des Brockhauschen (Con)versationslexicons beizulegen, in welche(m) (ich) den Artikel "*Darwinismus*" verfasst (habe;) eine Arbeit freilich, die mir heute ke(ines)wegs genügt und welche damals ei(lig) (in) wenig Tagen, vollendet werden (musste) Auf pag. 480 habe ich durch (1 or 2 words) Strich die Stelle bezeichnet (1 or 2 words) (per)sönlich für mich—das ist ja vielfach individuell!—die entscheidensten Gründe für Ihre grosse Lehre ausgesprochen sind.¹¹

Seit zwei Jahren beschaeftigt mich eine Untersuchung über die phylogenetische Entwicklung der Wirbelsaeule; eine vorlaeufige Mittheilung, welche einige Punkte

dieser Untersuchungen berührt, habe ich in der mit VI bezeichneten Beilage veröffentlicht.¹² Das Buch selbst, dessen Abschluss nahe bevorsteht, wird den Titel führen:

“*Die Wirbelsäule der Säugethiere
und des Menschen,
insbesondere der Bradypoden und Anthropomorphen,
des Europäers und der niederen Menschenrassen.*”¹³

Es würde mir eine grosse Ehre und Freude sein, wenn Sie, hochverehrter Herr, gestatten wollten, dass ich den Tribut des Dankes und der Bewunderung, welchen wir Alle Ihren bahnbrechenden Forschungen und Lehren zollen, für meinen Theil durch Dedication des genannten Buches an Sie öffentlich darbringen und mein bescheidenes Werk durch Voranstellung Ihres berühmten Namens schmücken dürfte.

Darf ich Sie um Ihre Erlaubniss hierzu bitten?

Hochachtungsvoll und ergebenst | Dr Hermann Welcker, | Professor der Anatomie in Halle.

⟨ ⟩ (Darw)in.

DAR 181: 88

CD ANNOTATIONS

2.4 III] *underl red crayon*

2.17 I] *underl red crayon*

3.4 IV] *underl red crayon*

4.1 II] *underl red crayon*

5.1 V] *underl red crayon*

8.3 VI] *underl red crayon*

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

² Six papers by Welcker are in the Darwin Pamphlet Collection–CUL; five of them are numbered in red in Roman numerals as follows: I. ‘Ueber das Hüftgelenk’ (On the hip joint; Welcker 1875a); II. ‘Beiträge zur Myologie’ (Contributions to myology; Welcker 1875b); III. ‘Zur Anatomie des ligamentum teres femoris’ (On the anatomy of the foveal ligament; Welcker 1876); IV. ‘Die Einwanderung der Bicepssehne in das Schultergelenk’ (The migration of the biceps tendon in the shoulder joint; Welcker 1878a); V. ‘Ueber die Entwicklung und den Bau der Haut und der Haare bei Bradypus’ (On the development and structure of the skin and hair in *Bradypus*; Welcker 1864). The sixth, unnumbered paper is ‘Ueber die künstliche Verkrüppelung der Füße der Chinesinnen’ (On the artificial crippling of the feet of Chinese women; Welcker 1870). It may have been sent separately, but no other letter from Welcker mentioning it has been found.

³ See Welcker 1876, p. 232; the illustration referred to shows two cross-sections of a hip capsule, one typical of a tapir embryo or a seal from embryo to adult, and the other typical of humans and other mammals, in which the comparative position of the ligamentum teres femoris (foveal ligament) is shown.

⁴ *Phoca* is a genus of earless seals.

⁵ The fovea capitis humeri is the depression or pit at the head of the femur; see Welcker 1875a, p. 76.

⁶ Welcker 1878, pp. 22, 23, and 25; the illustration on p. 25 (fig. 3.1) shows the location of the tendon directly over the joint.

⁷ *Vespertilio* is a genus of bats. See Welcker 1878, p. 25, figs. 3.2 and 3.3. The mesenterium is a fan-shaped fold of peritoneum (lining of the abdominal cavity) encircling most of the small intestines. The mesotendon is a fold of the synovial membrane (a layer of connective tissue that lines the cavities of joints) connecting a tendon to its synovial sheath.

- ⁸ See Welcker 1875b, p. 181; in the illustration of the upper right limb of an embryo of *Ursus maritimus* (polar bear), the highlighted muscle bundle, identified as a rudimentary coracoradialis muscle, is shown branching from the coracobrachialis towards the glenoradialis.
- ⁹ See Welcker 1864, plate I, figs. 1 and 2, illustrating an fetus of *Bradypus tridactylus* (pale-throated sloth) and showing the epitrichium, or outer layer of epidermis beneath which hair develops, at an early stage of development and just before birth. Welcker coined the term epitrichium in this paper.
- ¹⁰ Welcker's first paper on the binding of Chinese women's feet, Welcker 1870, is in the Darwin Pamphlet Collection–CUL; his second paper, Welcker 1872a, has not been found in the Darwin Archive–CUL. For the measurements of foot length, see Welcker 1872a, p. 150 n. 2.
- ¹¹ CD's annotated copy of [Welcker] 1872b is in the Darwin Pamphlet Collection–CUL.
- ¹² Welcker's paper, 'Zur Lehre von Bau und Entwicklung der Wirbelsäule' (On the theory of the structure and development of the spine; Welcker 1878b) has not been found in the Darwin Archive–CUL.
- ¹³ The projected title translates as: 'The spine in mammals and humans, in particular in Bradypodidae and anthropoid apes, in Europeans and lower human races.' No book with this or a similar title was published.

From Charles Dixon 19 January 1880

Albert Road | Heeley | near Sheffield
 Jan'y 19. 80.

Sir

Some few days ago I happened to be dissecting a Common Heron, shot on one of the streams near this place. Upon examining its plumage I found the inclosed seed firmly fastened among the breast feathers. I consider this another instance as to how seeds are conveyed from one place to another. Doubtless when the bird preened its plumage the seed would have been cast forth, to germinate or not according to circumstances.¹

There are many other seeds which depend upon animals and birds, too, for dispersal, by sticking firmly to the fur or feathers of a bird or animal walking amongst the plants.

I trust, sir, that you will excuse me troubling you on the matter, and let my enthusiastic desire for assisting in working out Nature's problems, be my apology for bringing before your notice what seems such a trivial circumstance.

Believe me, Sir, | Very respecty yours | Charles Dixon

Charles Darwin Esq

DAR 205.2: 228

CD ANNOTATIONS

Top of letter: 'Means of Distribution—' blue crayon

Verso of p. 1: 'The external [interl] glumes serrated by which I suppose had adhered, but the 2 enclosed sets of inner glumes contained no seeds.'² ink

¹ The common heron is the grey heron (*Ardea cinerea*). CD had discussed cases in which seeds had been dispersed by birds in *Origin*, pp. 361–3.

² Glumes are the lowermost bracts of a grass inflorescence, usually found in pairs. A packet containing a seed is pinned to the letter.

To Asa Gray 19 January 1880

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

Jan 19. 80

My dear Gray,

I have been greatly interested with the *Megarhiza* seeds which you so kindly sent me.¹ You have been misinformed about their germination, for I think you cannot have watched the whole process—Some were placed by me on, and others half an inch beneath the surface, and others deeper—, but none of the cotyledons were lifted up.² One seed on the surface was a little tilted by the root not penetrating the ground, but this often occurs with all kinds of seeds. The petioles of my specimens were not stiff enough to bear the weight of the seed. What takes place is that the radicle bends down & penetrates the ground, but grows only to a length of about half an inch or less (length rather doubtful as I did not wish to kill specimens by making sections). When of this length its growth is arrested, and the lower ends of the tubular petioles grow quickly & penetrate the ground just like a root to a depth of nearly $2\frac{1}{2}$ inches; then their growth ceases, and now the radicle takes up the game & grows very quickly. In every case the base of the radicle lay $2\frac{1}{2}$ inches beneath the surface. You probably know that if ordinary seedlings are placed in solution of permanganate of potassium the radicle is coloured brown whilst the hypocotyl & cotyledons is left uncoloured. Now when a seedling *Megarhiza* with the plumule just reaching the surface was thus treated, the whole radicle (+ hypocotyl) & the whole of the tubular petioles (densely covered with root hairs) became brown whilst the plumule was quite uncoloured. Therefore I think it certain that the tubular petioles act functionally like a root and that the cotyledons are hypogæan. The sole use of this wonderful manner of growth which occurs to me is to hide the enlarged root, at least at first, beneath $2\frac{1}{2}$ inches of soil as a protection against enemies.³ When my plants are two or three weeks old I will cut a slice from the root, and taste it & test it for starch. Now can you tell me whether the plant is an annual or perennial? When the root has become huge does it come to the surface, and is it then hard, and is it then bitter? I wonder whether it is attacked by beasts birds insects or slugs in California?

It is has been a great grief to me that not one of the seeds of *Ipomæa leptophylla* has germinated: my gardener opened some & found them rotten.⁴ I hope I haven't wearied you much

Ever, my dear Gray, | Yours sincerely | Charles Darwin

LS

Archives of the Gray Herbarium, Harvard University (126)

¹ CD had received seeds of *Megarhiza californica* (a synonym of *Marah fabacea*, California manroot) from Gray in December 1879 (see *Correspondence* vol. 27, letter to Asa Gray, 16 December 1879). His notes on the plant, recorded between 10 and 30 January 1880, are in DAR 209.6: 106–11; other undated notes are in DAR 185: 135. See also letter to W. T. Thiselton-Dyer, 15 January 1880 and n. 2.

² Gray had described the germination of *Megarhiza californica* as a process in which the cotyledons were raised out of the ground (see A. Gray 1877 and A. Gray 1879, pp. 20–1). CD annotated the illustration from A. Gray 1877, p. 23, to show the level of the ground (DAR 209.6: 114 and 175).

- ³ CD discussed the germination of *Megarrhiza californica* and the protective function of its root growth in *Movement in plants*, pp. 81–4. His notes on the application of potassium permanganate or permanganate of potash (KMnO₄), dated 9 January 1880, are in DAR 209.6: 105.
- ⁴ CD began sowing seeds of *Ipomoea leptophylla* (bush morning-glory) in December 1879 (see letter to Asa Gray, 16 December 1879). His new gardener was William Duguid.

To Ernst Krause 19 January 1880

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

Jan 19. 80

My dear Sir,

Pray do not apologise for it is no trouble to me to answer your questions as far as I can.¹ The Hooker alluded to is Richard Hooker.

With respect to Yeomen of the Armoury, it is doubtful whether any man in England could tell you what the duties were. Probably there were no duties and it was a sinecure in the gift of the king. I would advise you to use the English term. There are at the present day what are called Yeomen of the Guard who are gentlemen who attend the Queen on state occasions, & I believe they serve merely for parade.²

My dear Sir | Yours very sincerely | Ch. Darwin

LS

The Huntington Library (HM 36199)

¹ See letter from Ernst Krause, 16 January 1880.

² See letter from Ernst Krause, 16 January 1880 and n. 3. During the Tudor period, yeoman of the armoury was a special office of the Yeomen of the Guard, serving within the royal household (Hewerdine 2012, p. 53).

To Asa Gray 20 January [1880]¹

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

Jan 20

My dear Gray

This is a P.S. to yesterday's letter to say that the drawing at p. 21 of your text Book w.^d represent perfectly all that I have seen, supposing that a line were drawn, representing the surface of the soil, $\frac{1}{2}$ inch above the seed, assuming that the seed had been sown at $\frac{1}{2}$ inch depth.—² You may perhaps like to hear that the first true leaves break out through a split at base of confluent petioles of *Delphinium nudicaule* precisely as in *Megarrhiza*.³ The swelling of the plumules bursts the tube, & then the bowing downwards of the tip of the plumules of *Megarrhiza*, forces it laterally out of the tube. The tip is at first straight.—

This bowing down of the tip is a very common or rather universal movement with seedlings, but here it plays a new part.

Ever yours | C. Darwin

Archives of the Gray Herbarium, Harvard University (127)

- ¹ The year is established by the relationship between this letter and the letter to Asa Gray, 19 January 1880.
- ² See letter to Asa Gray, 19 January 1880 and n. 2. The drawing of *Megarrhiza californica* is in Gray's *Botanical text-book* (A. Gray 1879, p. 21).
- ³ CD's notes on the germination of *Delphinium nudicaule* (red larkspur), with diagrams, dated 19 and 20 January 1880, are in DAR 209.6: 66–78. *Megarrhiza californica* is a synonym of *Marah fabacea*, California manroot. CD discussed the two species in *Movement in plants*, pp. 80–4.

From Samuel Butler 21 January 1880

15. Clifford's Inn | E.C.
Jan 21. 1880

Charles Darwin Esq^r FR.S &c.

Dear Sir

I have to acknowledge the receipt of your letter of the 3^d inst. which I should have done sooner had I not been in great doubt what course to adopt in regard to it—¹

I have now decided on laying the matter before the public and have accordingly written to the Athenæum stating the facts²

I am | y^r. faithfully | S. Butler—

DAR 92: B67

- ¹ See letter to Samuel Butler, 3 January 1880; see also letter from Samuel Butler, 2 January 1880 and nn. 1 and 2.
- ² Butler's letter to the *Athenæum* appeared in the issue for 31 January 1880 (see letter to H. E. Litchfield, 1 February [1880], enclosure 1).

To Ernst Haeckel 21 January 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan 21st 1880

My dear Häckel

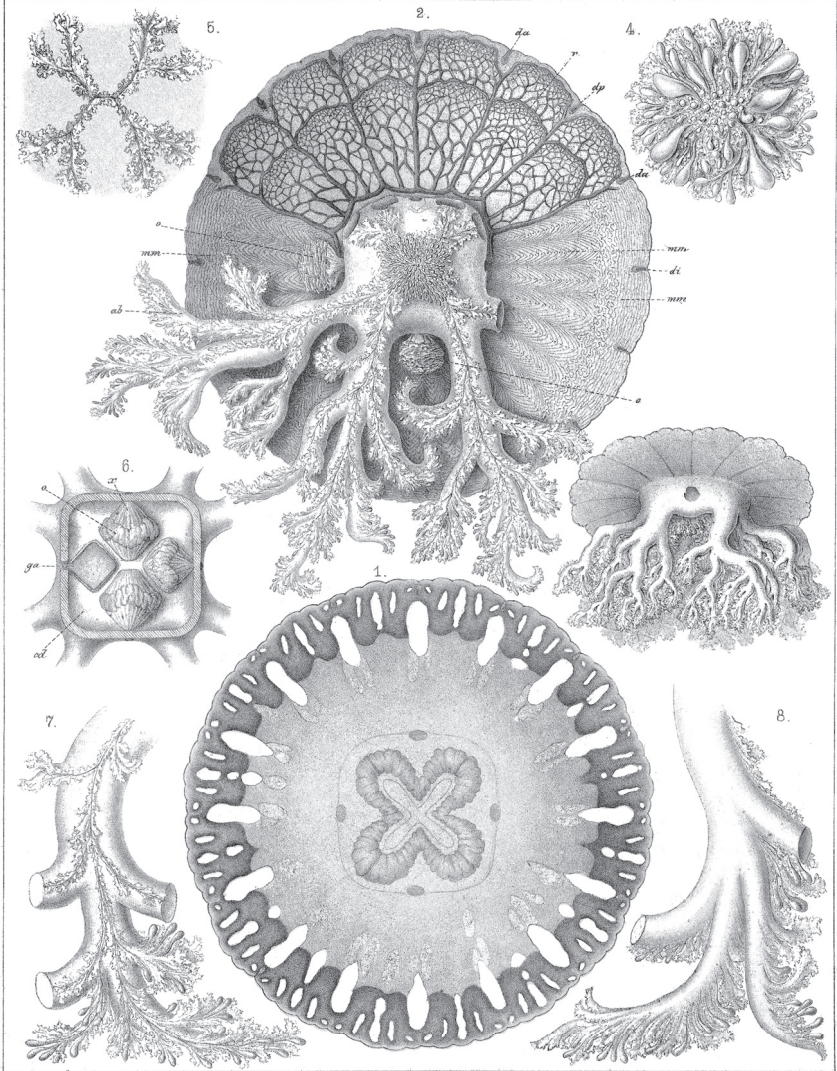
I have just received your grand present & thank you heartily. The book is magnificent.— I have just looked over all the Plates & they are wonderful. An artist gone mad could never have imagined such diversified and & curious structures.¹

I congratulate you on the completion of so gigantic a work. You are a wonderful man to do so much.

Ever my dear Häckel | Yours very sincerely | Charles Darwin

Ernst-Haeckel-Haus (Bestand A-Abt. 1:1-52/50 [A 9904])

- ¹ Haeckel's *Das System der Medusen* (the first volume of Haeckel 1879–81) was published in 1879; it included an atlas with coloured plates based on Haeckel's illustrations. See plate on p. 46. CD's copy is in the Darwin Library–Down.



F. Haeckel del.

Verf. v. Gustav Fischer Jena

Lith. v. A. C. Göttsche, Jena.

BRYOCLONIA.

Bryoclonia (Discomedusae).

Haeckel 1879–81, 1: pl. 37.

By permission of the Syndics of Cambridge University Library.

To W. T. Thiselton-Dyer 27 January [1880]¹

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

Jan 27th

My dear Dyer.

Asa Gray has sent me cotton-seeds (so that I now have a superfluity) & he asks me to ask Hooker whether you know a var. of Cotton called the "Vine Cotton".² A planter³ in the S. wants some seed of this var, & Gray wants to send him some.— Can you oblige them by sending such seeds to Gray?

The Planter says he formerly received the seeds of this var. from England.—

Ever yours sincerely | Ch. Darwin

If you will let me hear sometime I will write to Gray or if you are writing to him you can just say whether you know the var.

Royal Botanic Gardens, Kew (Darwin: Letters to Thiselton-Dyer, 1873–81: ff. 203–4)

¹ The year is established by the relationship between this letter and the letter from Asa Gray, 12 January 1880.

² Gray had sent seeds of *Gossypium hirsutum* (upland or short-staple cotton) and *G. barbadense* (pima or extra-long-staple cotton; see letter from Asa Gray, 12 January 1880). He had asked CD to write to Joseph Dalton Hooker for seeds of 'Vine cotton'.

³ A marginal note on the letter reads: 'Col [or 'W'] Clarke written to'; the planter has not been further identified.

From Grant Allen to G. J. Romanes¹ 28 January 1880

9 Boulevard des Iles d'Or. | Hyères. Var. France.

Jan. 28.80.

My dear Romanes,

I must just drop you a line or two to let you know how we are getting on; though we are so isolated here, and get so little news from England, that I am afraid I must fill my letter by nothing more than our own personal affairs. I send Robertson a short bulletin from time to time, for the information of all friends who have kindly come forward to help us through the winter:² but he tells me he seldom sees you, so I had better let you know occasionally how we prosper, direct. I am glad to say that I am still steadily, though very slowly, gaining health and strength: and I hope when the spring sets in—which it will do here next month—that I shall take a complete turn for the better. Even now, I can walk five miles without the least fatigue, and I have almost entirely lost my cough. I don't doubt that by next spring I shall be quite well enough to resume work in England, if any should offer. The winter here has been for the most part simply delicious. We had a week's frost in December, and three days again last week: but with these two exceptions there have been few days when one could not have picnicked in the open air with perfect comfort. For three weeks together in the beginning of January we had perfectly cloudless skies and warm sunlight day after day without intermission: and though

the weather is now a little dubious again, we were able yesterday to take out baby,³ and lunch by the seaside in weather like an English July. As a rule, people who come here suffering as I do, do not *get better* during December and January, but begin to mend in February: in other words, they simply stand still during the coldest months, and improve as soon as it gets warm again. I therefore look upon the fact that I have been steadily picking up ever since I came here as an indication that Hyères suits me particularly well. The surroundings are extremely pretty, and we find the walks among the hills grow upon one continually, so that we like the place much better the longer we stop in it, especially as we can now take longer walks and climb higher hills than when we first came. Morison⁴ passed through on his way to and from Italy, and visited us each time; so you will probably have heard all about Hyères and its surroundings from him already. Mrs. Allen,⁵ I am glad to say, is extremely well, and baby is doing capitally: both of which facts are the more gratifying as the whole family was more or less constantly invalided all last winter, so that I counted upon the change for their sakes almost if not quite as much as for my own. I suppose you are now in the midst of the gaieties of the London season, which I hope you are both thoroughly enjoying. If you can find time to write some day, I shall be glad to hear whether you have been doing any fresh scientific work. For myself, I have been completely lazy ever since Christmas, taking an entire holiday, and enjoying my idleness most thoroughly, after at least three years of high-pressure work. I cannot tell you what a relief it has been to me to have this winter free from the pressing anxieties which have weighed upon me so long, and to feel that I could go out and enjoy myself in the bright sunshine of Provence, without being perpetually harassed as to immediate wants. *That* has done me more good than the climate or anything else; though the air here is certainly delicious, and the winter the most pleasant I have ever felt anywhere.— We got cards of Miss Hertz's wedding,⁶ but have heard nothing else about it. Of course you and Mrs. Romanes⁷ went.—

Mrs. Allen joins me in very kindest regards to you both; and with best remembrances to your mother and sister,⁸ believe me, | Yours most sincerely, | Grant Allen.

DAR 159: A46

¹ Romanes forwarded this letter to CD (see letter to G. J. Romanes, 3 February 1880 and n. 2, below).

² George Croom Robertson had worked with Romanes to raise a subscription for Allen and his family (P. Morton 2005, p. 55). CD had contributed £25 to the fund (see *Correspondence* vol. 27, letter to G. J. Romanes, 23 July 1879 and n. 1).

³ Jerrard Grant Allen.

⁴ Probably James Augustus Cotter Morison, an acquaintance of Romanes (see E. D. Romanes 1896, pp. 142, 160, 184).

⁵ Ellen Allen.

⁶ Probably Helen Augusta Hertz, who was married on 10 January 1880.

⁷ Ethel Romanes.

⁸ Isabella Gair Rose Romanes and Charlotte Elizabeth Romanes.

From W. E. Darwin [28 January 1880]¹

Basset
Wednesday

My dear Father,

I have just received advice of the £600, & am placing £240 to Bessy & £360 to my account.² It is a splendid addition to one's income, and I had no idea it would be so large; it makes me thankful to think you can trust us all with so large a sum:

Sara³ sends her best love and thanks, she thinks you really must be pinching yourself to be able to make us so grand a surplus division.

I returned Butler's letter; there was something of the viper in the tone of the letter, I fancy he wants a grievance to hang an article upon.⁴

Goodbye Dear Father | Ever your affect son | W. E. Darwin

PS. | I wo'nt invest Bessy's balance till she is so rich that I can say on my conscience though a Banker that she has an indecently large balance.⁵

Cornford Family Papers (DAR 275: 80)

¹ The date is established by the relationship between this letter and the letter from Samuel Butler, 21 January 1880; in 1880, the Wednesday following 21 January was 28 January.

² CD had arranged for the surplus income from his investments to be distributed annually to his children; Elizabeth Darwin's share was to be deposited in the bank by William (see letter to the Darwin children, 10 January 1880).

³ Sara Darwin, William's wife.

⁴ See letter from Samuel Butler, 21 January 1880, and letter to H. E. Litchfield, 1 February [1880], enclosure 1.

⁵ CD had remarked, 'the only way to feel rich is to have a good balance in the bank' (see letter to the Darwin children, 10 January 1880).

To G. H. Darwin 29 January [1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan. 29th

My dear George

We have both been very sorry to hear of your illness. You must have had a miserable time. I always thought that the journey in your state of health was a rash one. We are particularly glad to hear that you intend seeing a doctor & do follow his advice. We gather from your note this morning that the worst of the attack is over.—² Now that you have got so far I hope that you may get on so as to see M^cLennan;³ but **I pray** you not to stay long there. You have perfect reason in your state of health.—

By the way if M^cL. sh^d. say, or you sh^d hear from M^{rs} L. anything about his book, & if he leaves his materials in a form which could be worked up by anyone, tell him from me, *supposing that he knows no better man*, that Wallace I think w^d do the work well & conscientiously, but he would require liberal payment as he is poor.—⁴ He writes admirable English & knows a good deal about anthropology & I think w^d work conscientiously.— Pray give my kindest sympathy to him & say how grieved I have been for the sake of science as well his own sake at his long & terrible illness.—

Poor Frank⁵ is in bed with rheumatism & very bad cold & has to give up his Lecture on Saturday.

My poor dear old George | Your affect Father | C. Darwin

DAR 210.1: 90

- ¹ The year is established by the mention of George's plan to visit John Ferguson McLennan (see n. 3, below).
- ² No letter from George has been found. He had left for Switzerland on 20 January and had fallen ill in Zurich (letter from Emma Darwin to H. E. Litchfield, 19 January 1880 (DAR 219.9: 218), and letter from Emma Darwin to Sara Darwin, 31 January 1880 (DAR 219.1: 132)).
- ³ George planned to visit John Ferguson McLennan in Davos, Switzerland; McLennan was very ill with consumption (letter from Emma Darwin to Sara Darwin, 31 January 1880 (DAR 219.1: 132)).
- ⁴ Eleonora Anne McLennan was J. F. McLennan's wife. McLennan died in 1881; a collection of his writings on the origin of patriarchal family structures was published posthumously (McLennan 1885). CD had been asked to find employment for Alfred Russel Wallace in 1879 (see *Correspondence* vol. 27, letter from A. B. Buckley, 16 December 1879).
- ⁵ Francis Darwin.

From Oskar von Giesl¹ 29 January 1880

Hochgeehrter Herr!

Im vorigen Jahre war der Verfasser des in meinem Blatte "Reunion" erschienen Aufsatzes: "Die Darwinische Theorie und die Landwirtschaft" so frei Ihnen die betreffenden Nummern zu senden und Sie hochgeehrter Herr hatten auch die Güte dieselben anzunehmen und zu lesen.² Ihr werthes Schreiben sowie Urtheile über die Abhandlung die an mich von Büchner, Haeckl und Settegast³ gelangt sind war ich so frei in mein(e)m Blatte seinerzeit zu veröffentlichen. Die Sache machte damals in Kreisen von Landwirthen ziemliches Aufsehen und pro und contra's wurden laut; nicht allein von den Anhängern sondern auch von Gegnern erklärten einige dass hochgeehrter Herr die Abhandlung allerdings angenommen aber darüber kein Urtheil gefällt hätten. Ich lege auf Ihr Urtheil hochgeehrter Herr, der ich Ihre Lehre ungemein hoch schätze und auch ferner zur Verbreitung und Verfechtung derselben mit aller Kraft eintreten will, das grösste Gewicht. Ich bitte Sie demnach vielmals mir zu schreiben: ob Sie mit dem Aufsatz beziehungsweise mit der Interpretation und Erklärung Ihrer Lehre einverstanden sind. Ihre Zuschrift werde ich dann gleich einer Religion aufbewahren und sie wird mir stets ein Sporen sein für die grosse Idee einzutreten.

Ich verstehe wol englisch kann aber nur schlecht schreiben bitte mir deshalb den Brief nur in englischer Sprache zu senden; zur Vereinfachung lege ich ein Couvert mit meiner Adresse bei und bitte Sie hochgeehrter Herr vielmals meine Zeilen freundlich aufzu(n)ehmen.

Ergebenst | Oskar von Giesl | Redacteur und Herausgeber der Reunion.

Sassin, pr. Hohenau in Ungarn am 29. Jänner 1880.

DAR 165: 41

- ¹ For a translation of this letter, see Appendix I.
- ² CD probably received the issues of the journal in April 1879 (see *Correspondence* vol. 27, letter to ?, 21 April 1879). The issues have not been found in the Darwin Archive–CUL; the author of the essay and the magazine *Reunion* have not been identified.
- ³ CD's letter has not been found. Ludwig Büchner and Ernst Haeckel had been involved in controversies over materialism and anti-clericalism in Germany (see Gregory 1977); Hermann Settegast was an agronomist who had published on animal breeding.

To Hermann Welcker 30 January 1880

Down, | Beckenham, Kent.
Jan 30th 1880

Dear Sir

I thank you sincerely for your very courteous letter of the 18th, received only yesterday, & for your kind present of a whole series of Essays.¹ Many of the points which you have discussed are very interesting & bear directly on the descent-theory.— I am particularly glad that you have investigated the 'ligamentum teres' for its absence in the anthropoid apes has long appeared to me a most perplexing circumstance.² Broca has advanced, & perhaps justly, this case as one in which natural selection could not have come into play.³ I hope before long to read your essays. I am also much interested by what you say about the feet of the Chinese; but we should remember that people always wish to exaggerate any character which they *already* possess to an unusual degree.⁴

If on further reflexion you still wish to dedicate your forthcoming book to me, I shall certainly esteem it a very great honour.⁵

With all good wishes I remain, Dear Sir | Yours very faithfully | Charles Darwin

Martin-Luther-Universität Halle-Wittenberg, Universitätsarchiv (Rep. 29, Nr. 715)

- ¹ See letter from Hermann Welcker, 18 January 1880 and n. 2.
- ² Welcker 1876 discussed the position and developmental stages of the ligamentum teres femoris (foveal ligament) in different mammals (see letter from Hermann Welcker, 18 January 1880 and n. 3). CD had been alerted to the absence of the ligament in orang-utans and a few other mammals (see *Correspondence* vol. 5, letter from Edward Blyth, [1–8 October 1855], and *Correspondence* vol. 13, supplement, letter from W. C. L. Martin, [1859–61]).
- ³ Paul Broca; see Broca 1870, p. 556.
- ⁴ Welcker had sent CD two papers on Chinese foot-binding and its possible hereditary effects (Welcker 1870 and Welcker 1872a; see letter from Hermann Welcker, 18 January 1880 and n. 10).
- ⁵ Welcker's projected book on the spine was never published; see letter from Hermann Welcker, 18 January 1880 and n. 13.

To J.-H. Fabre 31 January 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Jan 31. 1880

My dear Sir,

I hope that you will permit me to have the satisfaction of thanking you cordially for the lively pleasure which I have derived from reading your book¹ Never have the

wonderful habits of insects been more vividly described, and it is almost as good to read about them as to see them. I feel sure that you would not be unjust to even an insect much less to a man— Now you have been misled by some translator, for my grandfather. Erasmus Darwin states (*Zoonomia*— Vol I p. 183.— 1794) that it was a wasp (guêpe) which he saw cutting off the wings of a large fly I have no doubt that you are right in saying that the wings are generally cut off instinctively, but in the case described by my grandfather; the wasp after cutting off the two ends of the body rose in the air and was turned round by the wind; he then alighted and cut off the wings.² I must believe with Pierre Huber that insects have “une petite dose de raison”³ In the next edition of your book—I hope that you will alter *part* of what you say about my grandfather.

I am sorry that you are so strongly opposed to the Descent Theory; I have found the searching for the history of each structure, or instinct an excellent aid to observation; and wonderful observer as you are, it would suggest new points to you.— If I were to write on the evolution of instincts, I could make good use of some of the facts which you give. Permit me to add that when I read the last sentence in your book, I sympathise deeply with you—⁴

With the most sincere respect | I remain dear Sir. | Yours faithfully. | Charles Darwin.

P.S. | Allow me to make a suggestion in relation to your wonderful account of insects finding their way home.⁵ I formerly wished to try it with pigeons. Namely to carry the insects in their paper ‘cornets’ about a hundred paces in the opposite direction to that which you ultimately intended to carry them; but before turning round to return to put the insect in a circular box with an axle which could be made to revolve very rapidly first in one direction & then in another, so as to destroy for a time all sense of direction in the insects. I have sometimes *imagined* that animals may feel in which direction they were at the first start carried. If this plan failed, I had intended placing the pigeons within an induction coil, so as to disturb any magnetic or dia-magnetic sensibility which it seems just possible that they may possess.—

C.D.

Copy and LS incomplete⁶

DAR 144: 14; Muséum national d’histoire naturelle, Bibliothèque centrale, Paris (Ms FAB 32)

¹ CD had received a copy of *Souvenirs entomologiques* (Fabre 1879; see letter from J.-H. Fabre, 3 January 1880).

² In *Zoonomia*, Erasmus Darwin had described a common wasp (probably *Vespa vulgaris*) removing the wings of a fly (E. Darwin 1794–6, 1: 183). Fabre had quoted a French account of Erasmus Darwin’s description in *Introduction à l’entomologie* (Lacordaire 1834–8, 2: 460–1), in which the wasp was misnamed *Sphex*, a genus of digger wasps that paralyse prey (see Fabre 1879, p. 124).

³ Jean Pierre Huber; the correct quotation is ‘une petite dose de jugement’ (Huber 1836, p. 157). CD referred to this passage in *Origin*, p. 208 (see also *Natural selection*, p. 469).

⁴ The final page of Fabre’s book (Fabre 1879, p. 323) contained a dedication to his son, Jules, who had died in 1877:

Cher enfant, ravi si jeune à ton amour passionné des fleurs et des insectes, tu étais mon collaborateur, rien n’échappait à ton regard clairvoyant; pour toi, je devais écrire ce livre, dont les récits faisaient ta joie; et tu devais toi-même le continuer un jour. Hélas! tu es parti pour une meilleure demeure, ne connaissant encore du livre que les premières lignes! Que ton nom du moins y figure, porté par quelques-uns de ces industriels et beaux Hyménoptères que tu aimais tant.

(Dear child, torn so young from your passionate love of flowers and insects, you were my collaborator, nothing escaped your perceptive gaze; for you, I had to write this book, whose stories made you happy; and you were going to continue it one day. Alas! You have gone to a better place, still knowing nothing of the book but the first lines! May your name at least appear here, carried by some of the industrious and beautiful hymenopterans that you loved so much.)

Fabre named the wasps *Cerceris julii*, *Bembex julii*, and *Ammophila julii* after his son (Fabre 1879, pp. 320–3).

⁵ See Fabre 1879, pp. 261–74. For CD's interest in homing instincts, see *Correspondence* vol. 21, letters to *Nature*, [before 13 March 1873] and [before 3 April 1873]. A manuscript by CD on instinct, containing an extended discussion of migration, was eventually published as an appendix to G. J. Romanes 1883, pp. 355–84.

⁶ The postscript has been transcribed from a photocopy of the original in Paris.

To H. E. Litchfield 1 February [1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Feb. 1

My dear Henrietta

Will you & Litchfield read article in *Athenæum* & my answer.—² I have resolved to send one, as I can say something in defence of my negligence.— I wish my letter to appear in next number & I sh^d like to see proof, so if you do not object to anything *greatly* please post it on Monday addressed to Editor of *Athenæum with my note to Editor*, & return the *Athenæum* to me.³ If you or L. object very strongly to anything please return my letter here that I may post it on Tuesday. The *Athenæum* is published on Friday evening.— There is one sentence of which Frank suggested the insertion; but I am doubtful & so is your Mother— It is on separate paper, & if inserted please gum it in by cutting p. 3 into two pieces before the last paragraph, beginning with words “As Mr Butler evidently does not believe &c”⁴

It is very disagreeable being accused of duplicity & falsehoods—

All here approve of letter⁵

Since the above was written I have by advice of Frank & Leonard rewritten my letter & shortened it.⁶ I hope that you & L. may approve of it. If you do not, I cannot promise to follow your advice,, but it shall be well considered.

I am sure that neither of you will grudge the bother of considering the case.— Mr Butler's letter is very artful: he throughout makes it appear as if I had written Dr Krause's part.—⁷

Yours affectionately | C. Darwin

[Enclosure 1]

‘EVOLUTION OLD AND NEW.’⁸

I beg leave to lay before you the following facts:—

On February 22nd, 1879, my book, ‘Evolution Old and New,’ was announced. It was published May 3rd, 1879. It contained a comparison of the theory of evolution as propounded by Dr. Erasmus Darwin with that of his grandson, Mr. Charles Darwin, the preference being decidedly given to the earlier writer. It also contained other matter which I could not omit, but which I am afraid may have given some offence to Mr. Darwin and his friends.

In November, 1879, Mr. Charles Darwin's 'Life of Erasmus Darwin' appeared.⁹ It is to the line which Mr. Darwin has taken in connexion with this volume that I wish to call attention.

Mr. Darwin states in his preface that he is giving to the public a translation of an article by Dr. Krause, which appeared "in the February number of a well-known German scientific journal, Kosmos," then just entered on its second year. He adds in a note that the translator's "scientific reputation, together with his knowledge of German, is a guarantee for its accuracy."¹⁰ This is equivalent, I imagine, to guaranteeing the accuracy himself.

In a second note, upon the following page, he says that my work, 'Evolution Old and New,' "has appeared since the publication of Dr. Krause's article."¹¹ He thus distinctly precludes his readers from supposing that any passage they may meet with could have been written by the light of, or with reference to, my book.

On reading the English translation I found in it one point which appeared to have been taken from 'Evolution Old and New,' and another which clearly and indisputably was so; I also found more than one paragraph, but especially the last—and perhaps most prominent in the book, as making the impression it was most desired the reader should carry away with him—which it was hard to believe was not written at myself; but I found no acknowledgment of what seemed taken from 'Evolution Old and New' nor any express reference to it.

In the face of the English translation itself, it was incredible that the writer had written without my work before him; in the face of the preface it was no less incredible that Mr. Darwin should have distinctly told his readers that he was giving them one article, when he must have perfectly well known that he was giving them another and very different one.

I therefore sent for the February number of Kosmos and compared the original with what purported to be the translation. I found many passages of the German omitted, and many in the English which were wholly wanting in the German. Among these latter were the passages I had conceived to have been taken from me and the ones which were most adverse to me.

Dr. Krause's article begins on p. 131 of Mr. Darwin's book. There is new matter on pp. 132, 133, 134, 135, 136, 137, 138, 139, while almost the whole of pp. 147–152 inclusive and all the last six pages are not to be found in the supposed original.

I then wrote to Mr. Darwin, putting the facts before him as they appeared to myself, and asking an explanation; I received answer that Dr. Krause's article had been altered since publication, and that the altered MS. had been sent for translation. "This is so common a practice," writes Mr. Darwin, with that "happy simplicity" of which the Pall Mall Gazette (Dec. 12th, 1879) declares him "to be a master," "that it never occurred to me to state that the article had been modified; but now I much regret that I did not do so." Mr. Darwin further says that, should there be a reprint of the English life of Dr. Darwin, he will state that the original as it appeared in Kosmos was modified by Dr. Krause. He does not, however, either deny or admit that the modification of the article was made by the light of, and with a view to, my book.¹²

It is doubtless a common practice for writers to take an opportunity of revising their works, but it is not common when a covert condemnation of an opponent has been interpolated into a revised edition, the revision of which has been concealed, to declare with every circumstance of distinctness that the condemnation was written prior to the book which might appear to have called it forth, and thus lead readers to suppose that it must be an unbiassed opinion.

S. Butler.

P.S.—A reviewer in the *Pall Mall Gazette* (Dec. 12th, 1879) quotes the last sentence of the spurious matter, apparently believing it to be genuine.¹³ He writes:—“Altogether the facts established by Dr. Krause’s essay thoroughly justify its concluding words:— ‘Erasmus Darwin’s system was in itself a most significant first step in the path of knowledge which his grandson has opened up for us, but the wish to revive it at the present day, as has actually been seriously attempted, shows a weakness of thought and a mental anachronism which no one can envy.’” On this (which has no place in the original article, and is clearly an interpolation aimed covertly at myself) the reviewer muses forth a general gnome that “the confidence of writers who deal in semi-scientific paradoxes is commonly in inverse proportion to their grasp of the subject.” When sentences have been misdated, the less they contain about anachronisms the better, and reviewers who do not carefully verify Mr. Darwin’s statements should not be too confident that they have grasped their subject.

I have seen also a review of Mr. Darwin’s book in the *Popular Science Review* for this current month, and observe that it does “occur to” the writer to state (p. 69), in flat contradiction to the assertions made in the preface of the book he is reviewing, that only part of Dr. Krause’s original essay is being given by Mr. Darwin. It is plain that this reviewer had seen both *Kosmos* and Mr. Darwin’s book.¹⁴

The writer of the review of ‘*Evolution Old and New*’—which immediately follows the one referred to in the preceeding paragraph—quotes the passage above given as quoted in the *Pall Mall Gazette*. I see it does “occur to” him, too—again in flat contradiction to Mr. Darwin’s preface—to add that “this anachronism has been committed by Mr. Samuel Butler, in a . . . little volume now before us, and it is doubtless to this, which appeared while his own work was in progress (*italics mine*), that Dr. Krause alludes in the above passage.”¹⁵

Considering that the editor of the *Popular Science Review* and the translator of Dr. Krause’s article for Mr. Darwin are one and the same person, it is likely that the *Popular Science Review* has surmised correctly that Dr. Krause was writing at ‘*Evolution Old and New*’: yet he seems to have found it very sufficiently useful to him.

[Enclosure 2]

First letter disapproved by everybody¹⁶

To the Editor of the Athenæum

Sir.

M^r Butler in his letter in your last number seems to think me guilty of intentional duplicity in not having stated in the preface to my notice of the life of Erasmus Darwin, that D^r Krause had considerably altered the article in *Kosmos* before he sent it to M^r Dallas for translation. In my private letter to M^r Butler I said that it was so common a practice for an author to alter an article before its re-publication, that it never occurred to me to state that this had been done in the present case.¹⁷ Afterwards a dim recollection crossed my mind that I had written something on the subject, & I looked at the first proof received from Mess^{rs} Clowes, & found in it the following passage, here copied verbatim

(To the Compositor, be so good as to insert inverted commas to the whole of this extract)¹⁸

“Dr. Krause has taken great pains, and has added largely to his essay as it appeared in ‘Kosmos;’ and my preliminary notice, having been written before I had seen the additions, unfortunately contains much repetition of what Dr. Krause has said. In fact the present volume contains two distinct biographies, of which I have no doubt that by Dr. Krause is much the best. I have left it almost wholly to him to treat of what Dr. Darwin has done in science, more especially in regard to evolution.”

This proof sheet was sent to D^r Krause, with a letter in which I said that on further reflection it seemed to me absurd to publish two accounts of the life of the same man in the same volume; & that as my Notice was drawn up chiefly from unpublished documents, it appeared to me best that my account alone of the life should appear in England, with his account of the scientific works of Erasmus Darwin; but that he could of course publish the extracts from Miss Seward &c in the German edition. D^r Krause, with the liberality & kindness which has characterized all his conduct towards me, agreed instantly to my suggestion; but added that he thought it better that the text of the German edition should correspond with the English one, & that he would add the extracts &c in a supplement or in foot-notes. He then expressly asked me to strike out the passage above quoted, which I did; & having done so, it did not occur to me to add, as I ought to have done, that the retained parts of D^r Krause’s article had been much modified.¹⁹ It seems to me that any one, on comparing the article in *Kosmos* with the translation, & on finding many passages at the beginning omitted, & many towards the end added, might have inferred that the author had enlarged & improved it, without suspecting a deep scheme of duplicity. Finally I may state, as I did in my letter to M^r Butler, that I obtained D^r Krause’s permission for a translation of his article to appear in England, & M^r Dallas agreed to translate it, before I heard of any announcement of M^r Butler’s last book—²⁰

A??

As M^r Butler evidently does not believe my deliberate assertion that the omission of any statement that D^r Krause had altered his article before sending it for translation, was unintentional or accidental, I think that I shall be justified in declining to answer any future attack which M^r Butler may make on me.

Sir | your obedient servant— | Charles Darwin

Down Beckenham | Jan 24th. 1880.²¹

A (a sentence that [most] objected to)²²

He is mistaken in supposing that I was offended by this book, for I looked only at the part about the life of Erasmus Darwin; I did not even look at the part about evolution; for I had found in his former work that I could not make his views harmonise with what I knew.²³ I was indeed told that this part contained some bitter sarcasms against me; but this determined me all the more not to read it.

[Enclosure 3]

(Second letter) ultimately rejected—²⁴

Evolution Old and New

Sir,

In regard to the letter from M^r Butler which appeared in your columns last week under the above heading, I wish to state that the omission of any mention of the alterations made by D^r Krause in his article before it was re-published had no connection whatever with M^r Butler. I find in the first proofs received from Mess^{rs} Clowes, the words “D^r Krause has added largely to his essay as it appeared in *Kosmos*”.²⁵ These words were afterwards accidentally omitted, & when I wrote privately to M^r Butler I had forgotten that they had ever been written. (I could explain distinctly how the accident arose, but the explanation does not seem to me worth giving.) This omission, as I have already said, I much regret. It is a mere illusion on the part of M^r Butler to suppose that it could make any difference to me, whether or not the public knew that D^r Krause’s article had been added to or altered before being translated. The additions were made quite independently of any suggestion or wish on my part. (As M^r Butler evidently does not believe my deliberate assertion that the above omission was unintentional, I must decline any further discussion with him.)

Sir, | Your obedient servant | Charles Darwin

To the Editor of the *Athenæum*
Down, Beckenham, Kent. Feb. 1. 1880

DAR 92: B98–101, B102, B121; DAR 185: 40

CD ANNOTATIONS

- 1.7 There ... believe &c” 1.10] *del blue crayon*
3.1 All ... letter] *del blue crayon*

- ¹ The year is established by the reference to Samuel Butler’s letter to the *Athenæum* (see n. 2, below).
- ² For Butler’s letter in the *Athenæum* of 31 January 1880, see enclosure 1. Litchfield: Richard Buckley Litchfield, Henrietta’s husband.
- ³ The Monday following 1 February 1880 was 2 February. CD sent this letter, a copy of the *Athenæum*, and the second or both of his two draft letters to the *Athenæum* (enclosures 2 and 3) with his coachman, John Skinner (letter from H. E. Litchfield, [1 February 1880], enclosure 2 and n. 10).
- ⁴ CD’s first draft letter to the *Athenæum* (enclosure 2) consisted of three large manuscript pages plus an additional partial sheet with the suggested addition. A capital ‘A’ on the third manuscript page indicated where the addition should be inserted before the sentence mentioned in this letter (see n. 22, below). The section of the paragraph that follows ‘Friday evening’ was crossed out in blue crayon, probably at a later date.
- ⁵ This sentence was crossed out in blue crayon, probably at a later date.
- ⁶ For the second draft letter to the *Athenæum*, written on the advice of Francis Darwin and Leonard Darwin, see enclosure 3.
- ⁷ Ernst Krause had written the second section of *Erasmus Darwin*, titled ‘The scientific works of Erasmus Darwin’. CD’s ‘Preliminary notice’, a biographical sketch of Erasmus Darwin, focused on Erasmus’s character and work as a physician, and also aimed to correct inaccuracies in earlier accounts. The comments that Butler objected to were in Krause’s part, although he also objected to the fact that CD had failed to mention that Krause’s part had been modified in the light of Butler’s book, *Evolution, old and new* (Butler 1879).

- ⁸ Samuel Butler's letter regarding *Evolution, old and new* (Butler 1879) appeared in the *Athenaeum*, 31 January 1880, p. 155.
- ⁹ *Erasmus Darwin* was published in early November 1879 (*Correspondence* vol. 27, letter from Reginald Darwin, 12 November 1879). The title page refers to Krause as the main author, with CD's contribution described as a 'preliminary notice'.
- ¹⁰ See *Erasmus Darwin*, p. iii. William Sweetland Dallas translated Krause's essay (Krause 1879a) for *Erasmus Darwin*.
- ¹¹ *Erasmus Darwin*, p. iv.
- ¹² See letter from Samuel Butler, 2 January 1880, and letter to Samuel Butler, 3 January 1880. For Butler's brief reply to CD, see the letter from Samuel Butler, 21 January 1880.
- ¹³ The unsigned review that appeared in the *Pall Mall Gazette* was written by Frederick Pollock ([Pollock] 1879b; for the attribution, see the letter from R. B. Litchfield, 1 February 1880).
- ¹⁴ The unsigned review in the *Popular Science Review*, January 1880, pp. 69–71, was probably written by Dallas, who was the editor of the journal.
- ¹⁵ The unsigned review of *Evolution, old and new* in the *Popular Science Review*, January 1880, pp. 72–3, was also probably written by Dallas. Square brackets in original.
- ¹⁶ This sentence was added in pencil in CD's hand, probably at a later date; he added the words 'First letter' at the top of each subsequent page of enclosure 2, except for added paragraph 'A'. The body of the enclosure is in Emma Darwin's hand.
- ¹⁷ See letter to Samuel Butler, 3 January 1880.
- ¹⁸ The following paragraph is from a printed proof-sheet pasted onto the first manuscript page, with quotation marks added by hand; the instructions to the compositor are written in CD's hand next to the pasted extract.
- ¹⁹ See *Correspondence* vol. 27, letter to Ernst Krause, 13 August 1879, and letter from Ernst Krause, 6 August 1879. Miss Seward: Anna Seward.
- ²⁰ See *Correspondence* vol. 27, letter from Ernst Krause, 12 March 1879, and letter from W. S. Dallas, 14 March 1879. Butler 1879 was published on 3 May 1879 (see enclosure 1, above).
- ²¹ If this date is correct, CD wrote most of this draft (except the section labelled 'A') before reading Butler's letter in the *Athenaeum* of 31 January 1880, being already aware of its probable contents (see letter from Samuel Butler, 21 January 1880).
- ²² The following section was intended to precede the last paragraph of the body of the letter (see also n. 4, above). 'a sentence that [most] object to' was added in pencil by CD at a later date.
- ²³ See *Correspondence* vol. 27, letter to Ernst Krause, 14 May 1879. Butler's former work was *Life and habit* (Butler 1878), in which Butler presented his own view on evolution, which relied heavily on the theory of Jean Baptiste de Lamarck.
- ²⁴ This sentence was added in pencil by CD at a later date. The body of the enclosure is in the hand of an amanuensis.
- ²⁵ Krause read *Evolution old and new* (Butler 1879) before finishing his own essay, but did not cite it in *Erasmus Darwin*. For his criticisms of Butler 1879, see *Correspondence* vol. 27, letter from Ernst Krause, 7 June 1879, and Krause 1879b. William Clowes & Sons were printers to John Murray, CD's publisher.

From H. E. Litchfield [1 February 1880]¹

4, Bryanston Street, | Portman Square. W.

My dear Father—

R was very late coming in so that we had rather a hurried consultation over the letter & I did not thank you for caring to consult us—which I do most heartily whatever you do with our advice— You will see by my first letter which was written before I got yours how sure I felt that you w^d not think of answering Butler.²

I foresee one result of your letter that Butler will say you have been guilty of another quibble— first you say to him that it never occurred to you to state that

Krause had altered his article & then that you actually had it in the proof sheets & as you say accidentally omitted to publish it— Now, Butler, will say which of these two statements are true—& so it gives him scope for a whole set of fresh insults—& with his clever pen he can make something very disagreeable, out of this— The world will only know or at any rate remember that you & Butler had a controversy in which he will have the last word— If they understand it at all they'll see that its nothing whatever against you—but if they merely know there have been letters backwards & forwards they may think there is some ground for Butlers accusation agst you of jealousy of yr grandfather—³

If you leave the letter alone the facts are all there for those who care to read them, & it remains that Butler said some nasty spiteful things which you didn't care to answer

So Goodbye dear Father— You get enough advice from us in quantity—
your most affec. | H E L

[Enclosure 1]

4, Bryanston Street, | Portman Square. W.
1 Feb 1880

Dear Mr. Darwin,

Henrietta asks me to write my ideas on the Butler letter & your proposed answer.⁴

When I read Butler's statement at the Club⁵ yest^y I was much relieved to find that it was of a kind which, as I thought made *any answer* absolutely unnecessary. Neither in form, nor in substance, is it such as to suggest that a reply is expected. You will observe that it does not, as is common with newspaper attacks, ask for any further information or explanation, or touch any point of fact on which either the assailant, or a reader, could require such— In short, I never was clearer about anything than that, if it were my case, I should say nothing.

I tried, a second time, to read the Statement, as if I were an outsider who knew nothing of the quarrel, & felt entirely sure this is the right conclusion.

Not one reader in a thousand will make head or tail of the grievance. It's all muddled up with complaints agst divers reviewers—⁶ This alone practically neutralizes any effect it might have had otherwise. Then if an attentive reader *does* care to look back & see what the complaint was he will also see (though in a brief form) your substantial reply: and this is on the face of it, sufficient for the purpose. All the rest of B.'s insinuations read to an outsider as merely the annoyance & venom of a man out of temper & hitting wildly about him.

If you answer him you bring about *exactly* the result he most wants, w^h is to fill people's heads with the notion that y^r. b^k is in some way a reply or rejoinder to his: in fact you make it a "Darwin-Butler affaire" as the French w^d. say—and this is what will delight him.

As it stands there is nothing w^h: any friend of y^r^s. or any absolutely indifferent person c^d want explained or answered, and the *tone* of Butler is of itself quite enough to deprive him of any shadow of *claim* to an answer w^h: a loyal and friendly correspondent might have.

What I am trying to convey in this letter is that I have thought the thing over as a *cold outsider*. & that it is in this character that I am against any reply to B.

I agree however wholly with all that H. says as to y^r draft reply.

Y^{rs} affec | R B. L.

[Enclosure 2]

4, Bryanston Street, | Portman Square. W.

To Father⁷

But in any case omit interpolated sentence about you— Could give explanation of how omission came to be made⁸— People will easily guess that such an accident is possible & this weakens the effect— Also & this is much more important omit last sentence which shows that Butler has stung you—⁹ of course there is no question of a controversy between you & the letter shd be done with a perfectly dry cold manner. I shd like you to treat Butler like a man does a woman who hits him— it isn't pleasant, but its impossible to meet it—

Your character for perfect fairness & magnanimity is known to everybody who knows anything of science & to think that anybody will heed what Butler says is absurd— I sh^d be very sorry that this last sentence sh^d go forth—

R. wants to say some more but hasn't time before John's train—so please don't make up yr mind—till you hear from us by post—¹⁰

We have copied out your letter to *Athenæum* so as to make any corrections

John going

DAR 92: B72-4; B91-4

¹ The date is established by the date of the first enclosure. This packet of letters was sent by hand to CD in response to the letter to H. E. Litchfield, 1 February [1880]. The letters are presented in the probable sequence in which they were written.

² See letter to H. E. Litchfield, 1 February [1880]. The 'first letter' has not been found. Henrietta refers to Samuel Butler.

³ Henrietta Emma Litchfield had been sent one or two draft letters from CD to the *Athenæum*, along with a copy of the *Athenæum* containing the letter from Samuel Butler (see letter to H. E. Litchfield, 1 February [1880] and three enclosures). In his first draft response, CD had stated that text had inadvertently been left out of the preface to *Erasmus Darwin* explaining that Ernst Krause's original essay (Krause 1879a) had been revised (see letter to H. E. Litchfield, 1 February [1880], enclosure 2). Erasmus Darwin was CD's grandfather.

⁴ Richard Buckley Litchfield appears to have read Henrietta's letter before writing his own view on the matter.

⁵ The club was probably the Oxford and Cambridge Club (H. E. Litchfield 1910, p. 245).

⁶ See letter to H. E. Litchfield, 1 February [1880], enclosure 1, and nn. 13–15.

⁷ This note was evidently written after further discussion between Henrietta and her husband.

⁸ The interpolated sentence in CD's second draft letter begins, 'I could explain ...' It is written between the lines (see letter to H. E. Litchfield, 1 February [1880], enclosure 3). This sentence is enclosed in pencil square brackets. In his first draft letter, CD gave a more detailed description of how a section of the preface to *Erasmus Darwin* had accidentally been omitted (see n. 3, above).

⁹ The last sentence in CD's first draft letter to the *Athenæum* was shortened and slightly modified in his second draft, where it was enclosed with square brackets (letter to H. E. Litchfield, 1 February [1880], enclosures 2 and 3).

¹⁰ John Skinner was CD's coachman. In a letter dated 4 February 1880 (DAR 219.1: 134), Emma Darwin wrote to her son William Erasmus Darwin, 'F. got so anxious to get his answer to Butler's attack off his mind that he sent John up w. it to R. & Hen. for their approval. They sent it down again with 2 very sensible letters from R. & Hen warmly dissuading him from taking any notice.'

From R. B. Litchfield 1 February 1880

4, Bryanston Street, | Portman Square. W.

1 Feb 1880

Dear Mr. Darwin,

Since sending off our last packet I bethought myself I shd. like to ask the first *bystander* I could meet how Butler's letter struck him as regards the need of an answer, and so I went in & shewed the Ath^m. to Pollock in this street.¹ As an accustomed literary man & man of the world I wanted to see whether he w^d. agree w. me. But of course I did not tell him anything before he read the article. I merely said when you have read it I want to ask y^r. opinion on a certain point and when he had read it my question was does that in y^r. opinion need any answer? His reply was 1st. that he *might* be prejudiced as he knew something of B.—but he was strong that it *didn't* want answering. (Of course I didnt lead him to suppose it was any more than a question *from me* personally.)

He had himself written the Pall Mall Article as it happened—also an Art. in Sat. Rev on Butlers Book.² B., he tells me, is known to be getting up a grand reply to *all* his critics & he is making a point of getting their names. He wrote to the Sat^y w. this enq^y but the Sat. put him off with a formal refusal.³ All w^h. helps to shew that he is a virulent Salamander of a man⁴ who will fight to the end, and as P. said, his greatest joy w^d. be to get into a public dispute w. a man of eminence.

P. however, tho' aware of his character, was by no means prejudiced agst his b^{ks}. (he thinks them nonsense but very clever nonsense) and his opinion on the question of a reply was I have no doubt a dry opinion.

I have since looked with a critical eye at y^r. draft & I am thereby only confirmed in my impression for I do not find that it, in substance, contains anything w^h. is not already in the sentence quoted by B. from y^r. note.⁵ But while to the *Substance* of y^r. explanⁿ it adds nothing it gives B. the most admirable chance for another nasty letter inasmuch as it gives him *new facts*. At present he knows, & need know, nothing of the mere mechanical detail of the accident w^h. caused the omission. These you in part give him. As he is now he *cannot* say anything more: he has made the worst of all he knows. But to a wretched unscrupulous word-fencer as he is y^r. letter opens material for a wholly new attack, and if the Ath^m. likes to put it in, he can easily make it appear that there's something very suspicious & mysterious in y^r. proceedings.

Given only that a man that is a blackguard and there is no end to the stuff he mighn't write on such a theme. For illustration's sake I have put down a few sentences, as they came into my head, such as he might string together.⁶

The fact is that such a story as that of the alteration of the proofs in this case *cannot* be made satisfactory unless it is told in full: and of course to tell it in full w^d

be ridiculous. The main topic is itself a merely microscopic point, and to go *into* the business w^d be too intolerable.

But over & above all special considerations is the one that a reply in such a case is *necessarily* an apologetic process, and that you have *nothing to apologize for*.

I dare say much of this repeats Henrietta.⁷ In what she has read to me I wholly agree.

[Enclosure]

Sketch of imaginary reply by Butler

Sir.

When I wrote &^c last week I thought I knew all that was likely to be known ab^t M^r Darwin's extraord^y treatment of my book, but his letter to you makes some most remarkable additions to the strange story. M^r D. had told me that it "never occurred to him" to state &^c— Never occurred to him!! When now it seems that it not only occurred to him, but that he *did* state &^c &^c Stated it in a printed preface, and afterwards, in some mysterious way, this statement disappeared from the proof! Perhaps M^r D. will complete the story &^c &^c. Sentences do not vanish out of a printed page by accident, *only* &^c &^c. He goes on to tell us that "it is an illusion to suppose it c^d make any diff^{er} &^c &^c" It might have been an illusion due to my ignorance but the details kindly given by M^r D. now shew it to be a fact that it *did* make a diff^{er}. If no diff^{er} why was the sentence expunged?

If the excision was an accident it is of course needless for M^r D. to tell us that it had nothing to do with M^r Butler— Accidents do not usually need to be thus explained &^c &^c. Nor is a great Naturalist the man we sh^d think likely wholly to forget(!) the act of cancelling his own deliberate statements

Then M^r D. tells us that the add^{ns} were made independently &^c— Strange that an author of distinction sh^d be so delightfully pliable in the hands of somebody else. Who this somebody else might be, whether D^r K.⁸ or &^c &^c we are not told.

And lastly M^r D. is obl^{is} eno^t to say that I do not believe his delib. assertion &^c. I have not to my knowledge adopted this severe estimate of M^r D.'s veracity but certainly if M^r D. wanted to create the incredulity w^h he is so polite as to attribute to me the best means of achieving that result w^d be to supply us with more of the explanations of w^h he has given a sample in y^t columns of Sat^y last.

DAR 92: B75–8, 81

¹ See letter from H. E. Litchfield, [1 February 1880]. For Samuel Butler's letter, published in the *Athenaeum*, 31 January 1880, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1. Frederick Pollock lived at 24 Bryanston Street (*Post Office London directory* 1880).

² Pollock had written an unsigned review of *Erasmus Darwin* in the *Pall Mall Gazette*, ([Pollock] 1879b); also an unsigned review of *Evolution, old and new* (Butler 1879) in the *Saturday Review* ([Pollock] 1879a).

³ When the second edition of *Evolution, old and new* was published, Butler added an appendix in which he discussed the reviews of the first edition (Butler 1882, pp. 385–94); he noted that the *Saturday Review* had attacked his book almost savagely (*ibid.*, p. 389).

- ⁴ Litchfield alludes to the mythical ability of salamanders to be able to endure fire.
- ⁵ In his letter to the *Athenæum*, Butler had quoted from CD's letter to him of 3 January 1880 as follows: 'it never occurred to me to state that the article had been modified; but now I much regret that I did not do so' (see letter to H. E. Litchfield, 1 February [1880], enclosure 1; for CD's draft replies, see enclosures 2 and 3).
- ⁶ See the enclosure to this letter.
- ⁷ See letter from H. E. Litchfield, [1 February 1880].
- ⁸ Ernst Krause was the co-author of *Erasmus Darwin*.

To Wallis Nash 1 February 1880

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

Feb 1. 80

My dear M^r: Nash.

I thank you cordially for your long & very interesting letter.¹ Your life sounds very prosperous & I am delighted to hear that you are all well & happy. We heard some time ago with much alarm of your illness, but I trust it was not as bad as it sounded.² I can well understand your enjoying your new life, for in old days I well remember thinking that a colonist's lot, with children, was a happy one. I remember especially this in regard to Tasmania.³

Frank will tell you what little news there is to be told about this quiet place.⁴ But I must send my own kindest remembrances to M^{rs}: Nash. You will both ever be a heavy loss here.⁵

Believe me my dear M^r: Nash

Yours very sincerely | Ch. Darwin

F. Louise Nash Barton (private collection)

- ¹ See letter from Wallis Nash, 4 January 1880.
- ² Nash and his family had emigrated to Oregon in 1879 (Smith and Dimick 1976, pp. 78–9). In his book *Two years in Oregon* (Nash 1882, p. 100), Wallis Nash refers to 'a sharp attack of illness' in the autumn of 1879. The Darwins received a false report in October 1879 that Nash had died (see *Correspondence* vol. 27, letter to ?, 23 October 1879).
- ³ While on the HMS *Beagle* voyage, CD had written from Van Diemen's Land (Tasmania) praising it as 'a most admirable place of emigration' (*Correspondence* vol. 1, letter to Catherine Darwin, 14 February 1836).
- ⁴ Nash had written that he hoped CD's son Francis Darwin would reply to his letter of 4 January 1880.
- ⁵ When Wallis and Louisa Ahmuty Nash moved from Down to Beckenham in 1878, CD had called it 'an irreparable loss to our village' (*Correspondence* vol. 26, letter to Wallis Nash, 27 March 1878).

To T. H. Huxley 2 February 1880

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

Feb 2. 1880

My dear Huxley,

I am going to ask you to me a great kindness. M^r Butler has attacked me bitterly, in fact accusing me of lying, duplicity, & God knows what, because I unintentionally omitted to state that Krause had enlarged his *Kosmos* article before sending it

for translation.¹ I have written the enclosed letter to the *Athenæum* in reply; but Litchfield is strongly opposed to my making any answer, and I enclose his letter if you can find time to read it.² Of the other members of my family some are for & some against answering. I should rather like to show that I had intended to state that Krause had enlarged his article. On the other hand a clever & unscrupulous man like M^r Butler would be sure to twist whatever I may say against me; & the longer the controversy lasts the more degrading it is to me. If my letter is printed both the Litchfields want me to omit the two sentences now marked by pencil brackets; but I see no reason for their omission.³

Now will you do me the lasting kindness to read carefully the attack & my answer; & as I have unbounded confidence in your judgement, whatever you advise that I will do: whether you advise me to make no answer, or to send the enclosed letter as it stands or to strike out the sentences between brackets

Ever yours sincerely | Charles Darwin

P.S. Since writing the above I have received another letter from Litchfield, with a splendid imaginary letter from Butler, showing how he would probably travestie my answer. He tells me that he took the *Athenæum* to M^r Pollock, & asked him, (without giving any hint of his own opinion) whether Butler's attack ought to be answered & he said "no".⁴ But I wait in anxiety for your answer *as this will decide me*.

LS(A)

Imperial College of Science, Technology and Medicine Archives (Huxley 5; 336)

¹ For Samuel Butler's letter to the *Athenæum*, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1. Ernst Krause had revised his essay on Erasmus Darwin (Krause 1879a) for publication in *Erasmus Darwin*. Although Krause had read Butler's *Evolution, old and new* (Butler 1879), he did not cite it in his own essay. In his letter to the *Athenæum*, Butler accused CD and Krause of responding to Butler 1879 while maintaining that they had written their book before Butler 1879 was published.

² CD evidently enclosed a copy of the *Athenæum* containing Butler's letter; his second draft letter to the *Athenæum*, and the first enclosure to the first letter from H. E. Litchfield, [1 February 1880] (see letter to H. E. Litchfield, 1 February [1880], enclosures 1 and 3; see also letter to R. B. Litchfield, 2 February 1880). His son-in-law, Richard Buckley Litchfield, had advised against responding to Butler, arguing that no reply was necessary (letter from H. E. Litchfield, [1 February 1880]).

³ For the positions of CD's family members on whether or not to respond to Butler's attack, see the letter to R. B. Litchfield, 2 February 1880. The sentence beginning 'I could explain ...' and the final sentence of CD's second draft letter to the *Athenæum* are enclosed in pencil brackets (see letter to H. E. Litchfield, 1 February [1880], enclosure 3).

⁴ See second letter from R. B. Litchfield, 1 February 1880 and enclosure. Litchfield had consulted his neighbour Frederick Pollock, who had written reviews of both Butler 1879 and *Erasmus Darwin* ([Pollock] 1879a and [Pollock] 1879b).

To R. B. Litchfield 2 February 1880

Down,
Feb 2. 1880

My dear Litchfield

I have only a moment or two to thank you & Henrietta *most warmly* for all the trouble which you have taken.¹ Your first letter I think about the most sensible one

I ever read.² Your imaginary answer for B is splendid. I am almost converted not to answer & I did not think I could be. Indeed I am converted.— So almost is Mother— Leonard partially.— F still maintains that if it were his case he would answer.³ We had thought of Huxley & I shall despatch by this post the *Athenæum* & my answer to him & I will enclose (for I think you could not object) your first letter. I will not enclose 2nd letter, merely not to trouble H with reading so much.—⁴ I hope to god Huxley will say no. We do not agree about the 2 sentences to be cut out, if my answer is to be printed.⁵ You have both been very very kind to me. The affair has me to a silly extent⁶

yours affectionately | Ch. Darwin

Copy

DAR 146: 56

¹ CD had asked the Litchfields for advice on whether to respond to Samuel Butler's letter to the *Athenæum* complaining of the treatment of his own work in *Erasmus Darwin* (see letter to H. E. Litchfield, 1 February [1880]).

² Letter from H.E. Litchfield, [1 February 1880], enclosure 1.

³ Litchfield had composed an imaginary reply by Butler to CD's draft letter to the *Athenæum*; see letter from R. B. Litchfield, 1 February 1880 and enclosure. Emma Darwin, Leonard Darwin, and Francis Darwin had also read CD's draft reply to Butler.

⁴ CD sent Thomas Henry Huxley a copy of the *Athenæum* containing Butler's letter, his second draft letter to the *Athenæum*, and the first enclosure to the letter from H.E. Litchfield, [1 February 1880] (see letter to H. E. Litchfield, 1 February [1880], enclosures 1 and 3).

⁵ The sentence beginning 'I could explain ...' and the final sentence of CD's second draft letter to the *Athenæum* were enclosed in pencil brackets to indicate that they should possibly be omitted (see letter to H. E. Litchfield, 1 February [1880], enclosure 3).

⁶ The copyist left a gap, presumably because a word was illegible.

From W. Z. Seddon 2 February 1880

21 Portugal St | Bolton
February 2nd 1880

C. Darwin Esq. F.R.S. D.C.L. &c.

Sir

My schoolmaster the other day in Conversation Lesson (That is a lesson every Wednesday from 11–15 am to 12 when we ask him anything we like) Had this question asked. "What causes the different shades of colour in the inhabitants of the earth?". He could not exactly tell the answer but he thought the different kinds of climate might cause it: I am just turned 12 years of age and attend the British School. Mawdsley St. Bolton.¹ If I am not troubling you too much, may I ask if you could kindly tell me where I shall find an answer to this question. If in a few words you could give me any idea of the reasons, I should be greatly obliged

I am Sir | your obedient Servant | William Zaccheus Seddon

DAR 177: 126

¹ British schools were non-denominational elementary schools of a type founded by the British and Foreign School Society (formed in 1814) and run according to the educationalist Joseph Lancaster's system of using older pupils to help teach younger children (*OED*).

From Asa Gray 3 February 1880

Herbarium of Harvard University, | Botanic Garden, Cambridge, Mass.
Feb. 3 1880.

My Dear Darwin

Your letter of the 19th ult, made me open my eyes.¹ I am just off on a little journey, and have only a moment to say that Mr. Watson, Prof. Goodale, my artist who drew the figures & others can make affidavit to the *facts*.² Two or three plants—on one, I think fully 2 inches of the seeming radicle was out of ground³

There are 3 or 4 species. The second lot I sent you was probably different from the first, or of 2 species.⁴

If your plants were weak, even of the same species, it might account for their not lifting the weight of the seed.

About nomenclature, following the current nomenclature I called the hypocotyledonous internode *radicle*—properly caulicle.⁵

Please call all below it *root*, so as to avoid confusion, "*Perennial*" I should think so! The root is said to be sometimes of the size of a *barrel*. Those I have seen in California were from the size of a carrot to that of the biggest ruta-baga.: the tip near the surface of the ground,—but under it, where there was no wash.⁶

In haste | Ever Yours | Asa Gray

DAR 209.6: 201

CD ANNOTATIONS

2.1 If your ... the seed. 2.2] *scored red crayon*

Top of letter: 'In my case petioles geotropic— In Asa Gray apogeotropic!!'⁷ *ink*

¹ See letter to Asa Gray, 19 January 1880; see also letter to Asa Gray, 20 January 1880.

² Sereno Watson was the curator of the herbarium at Harvard (*ANB*); George Lincoln Goodale was professor of botany at Harvard. The figure in *Gray's botanical textbook* (A. Gray 1879, p. 21) illustrating the development of a seedling of *Megarrhiza californica* (a synonym of *Marah fabacea*, California man-root) did not show which part of the seedling was below ground; no artist is credited for the illustrations in the textbook. In his article on the germination of the genus *Megarrhiza* (A. Gray 1877, p. 23), Gray had reported that the body of the seed was raised well outside the soil on what seemed to be a well-developed radicle.

³ In *Movement in plants*, p. 82, in his copy of Gray's figure of the *Megarrhiza* seedling, CD added a dotted line to indicate that most of the illustrated seedling was below the ground.

⁴ CD had first requested seeds of *Megarrhiza* from Gray in October 1879 (see *Correspondence* vol. 27, letter to Asa Gray, 24 October 1879); he received some in December (*ibid.*, letter to J. D. Hooker, 19 December [1879]), but later reported that some of a second batch of seeds failed to germinate (letter to Asa Gray, 17 February 1880). For the species of *Megarrhiza* recognised at this time, see A. Gray 1877; see also Stocking 1955).

⁵ In the glossary of his textbook, Gray defined the caulicle as the initial stem in an embryo; the radicle of the embryo was defined as the hypocotyledonary and primary internode (A. Gray 1879, pp. 401,

429–30). In *Movement in plants*, p. 5, CD defined the stem supporting the cotyledons as the hypocotyl; it was distinguished from the radicle only by the presence of root-hairs in the latter.

⁶ CD had asked whether *Megarhiza californica* was an annual or perennial (letter to Asa Gray, 19 January 1880). The rutabaga or swede (*Brassica napus* var. *napobrassica*) is a cross between a turnip (*B. rapa*) and a cabbage (*B. oleracea*).

⁷ CD described the petioles of *Megarhiza californica* as completely confluent, forming a tube; he noted that as soon as they protruded from the seed, they were strongly geotropic and penetrated the ground unless they met with an obstacle; in that case, the cotyledons were lifted above the ground (*Movement in plants*, p. 81).

From T. H. Huxley 3 February 1880

Science and Art Department | South Kensington
Feb'y. 3. 1880

My dear Darwin

I read Butler's letter & your draft and Litchfield's letter last night; slept over them, and after lecturing about Dogfish & Chimæra (subjects which have a distinct appropriateness to Butler) I have read them again—and I say, without the least hesitation, burn your draft & take no notice whatever of Mr Butler until the next edition of your book comes out—when the briefest possible note explanatory of the circumstances—will be all that is necessary¹

Litchfield ought hereafter to be called 'the judicious' as Hooker was (I don't mean Sir Joe but the divine)—² To my mind nothing can be sounder than his advice and "I am a man of (sor)rows and acquainted with (coming to) grief"³

I am astounded at Butler—who I thought was a gentleman though his last book appeared to me to be supremely foolish—Has Mivart bitten him & given him Darwinophobia?⁴ It is a horrid disease & I would kill every son of a



I found running loose with it—without mercy—

But dont you worry as to these things Recollect what old Goethe said about his Butlers & Mivarts

"Hat doch der Wallfisch seine Laus Muss auch die meine haben."⁵

We are as jolly as people can be who have been living in the dark for a week & I hope you are all flourishing

Ever Yours | T H Huxley

DAR 92: B82–3

¹ See letter to T. H. Huxley, 2 February 1880. CD enclosed a copy of the *Athenæum* containing Samuel Butler's letter; CD's second draft letter to the *Athenæum* (see letter to H. E. Litchfield, 1 February [1880], enclosures 1 and 3), as well as the first enclosure of the letter from H. E. Litchfield, [1 February 1880]. Dogfish sharks and chimaeras are types of cartilaginous fishes; Huxley grouped sharks and chimaeras into a single class, Chondrichthyes (T. H. Huxley 1880b, p. 660). Huxley also alludes to the Chimaera of Greek mythology, since the term, used figuratively, refers to a wild fancy or unfounded conception (*OED*).

- ² Richard Buckley Litchfield had advised against responding to Butler, arguing that no reply was needed (letter from H. E. Litchfield, [1 February 1880], enclosure 1). Joseph Dalton Hooker was a close friend of both CD and Huxley. The theologian Richard Hooker had been characterised as ‘judicious’ by many different religious groups (*ODNB*).
- ³ Huxley alludes to the biblical verse, ‘He was despised and rejected of men; a man of sorrows, and acquainted with grief’ (Isaiah 53:3).
- ⁴ Butler’s book was *Evolution, old and new* (Butler 1879). CD had had a similar falling out with St George Jackson Mivart after Mivart suggested in an anonymous essay review in the *Quarterly Review* that, in an article on marriage, George Howard Darwin spoke in an approving strain of the encouragement of vice to check population growth ([Mivart] 1874, p. 70, G. H. Darwin 1873; see *Correspondence* vols. 22 and 23).
- ⁵ After all, the whale has its louse, so I must also have mine (German; Johann Wolfgang von Goethe, in ‘Pseudo-Wanderer’; see, for example, Goethe 1845–6, 1: 138). The poem refers to Johann Friedrich Wilhelm Pustkuchen. On Pustkuchen’s critique of Goethe and Goethe’s response, see Bahr 1998, pp. 7–8, 52–5.

From R. B. Litchfield 3 February 1880

4 Bryanston Street | Portman Square. W.
3. Feb 1880

Dear Mr. Darwin,

I think Huxley’s judgm^t will be a safe one on the question of reply^g to Butler—unless it be perhaps that he is himself horribly pugnacious & w^d *naturally* be for fighting¹

I still cannot frame to myself *any* answer w^h w^d be of the slightest use, or logically sound, except it confine itself to a mere reiterⁿ of what you’ve already told B. If such a reply seems any good it might be in such form as I have put down on back of this. I *do* think it of the most supreme importance *not* to allude to B.’s pretending to think you untrustworthy—and that any reply sh^d be *absolutely* without feeling.²

Always Yrs affec. | R. B. L.

Evolⁿ. Old & New³

Sir,

I have read the statement by M^r. S. Butler w^h appeared in y^r columns of Sat^v last under the above heading, as to my having omitted to mention, in the preface to the lately issued translⁿ of D^r. Krause’s Essay on Erasmus Darwin, that his paper had been somewhat altered before being thus republished.⁴ As Mr. Butler quotes my letter to him in w^h I informed him that this omission was accidental, & that it sh^d be corrected in case of the little book reaching a Second Edition, I do not see that I need trouble y^r. readers with any further observations on the matter.⁵

DAR 92: B79–80

¹ In his letter of 2 February 1880, CD told Litchfield that he planned to ask Thomas Henry Huxley’s opinion on whether he should respond to accusations made by Samuel Butler in a letter to the *Athenaeum* (see letter to T. H. Huxley, 2 February 1880). For Butler’s letter to the *Athenaeum*, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1.

- ² CD had sent one or two versions of his draft letter to the *Athenaeum* to Henrietta Emma Litchfield (letter to H. E. Litchfield, 1 February [1880], enclosures 2 and 3). Both the Litchfields advised CD not to respond to Butler, but also advised CD to delete some sentences from his draft if he did decide to reply (see letter from H. E. Litchfield, [1 February 1880], and letter from R. B. Litchfield, 1 February 1880).
- ³ This draft is Litchfield's suggestion for a letter from CD to the *Athenaeum* in response to Butler.
- ⁴ The translation of Ernst Krause's essay (Krause 1879a) was the second part of *Erasmus Darwin*.
- ⁵ See letter to Samuel Butler, 3 January 1880. A second edition of *Erasmus Darwin* was published in 1887, after CD's death (*Erasmus Darwin* 2d ed.). The following note was added to the preface: '[Mr. Darwin accidentally omitted to mention that Dr. Krause revised, and made certain additions to, his Essay before it was translated. Among these additions is an allusion to Mr. Butler's book, 'Evolution, Old and New.'],' (*ibid.*, p. iv).

To G. J. Romanes 3 February 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Feb 3. 1880

My dear Romanes,

I will keep your diagram for a few days, but I find it very difficult now to think over new subjects, so that it is not likely that I shall be able to send any criticisms; but you may rely on it that I will do my best.¹

I am glad you like Guthrie's book.² The two inclosed letters from Mr Moulton on the subject are I think worth your reading, but you must consider them private & please return them. He doesn't think much of Guthrie as a physicist.³

If you care to read a little book on pure instinct get Fabre *Souvenirs Entomologiques* 1879. It is really admirable, and very good on the sense of direction in insects I have sent him some suggestions such as rotating the insects but I do not know whether he will try them.⁴

I have been very glad to read Grant Allen's letter; we have all done a good work.⁵

Yours very sincerely | Charles Darwin

LS

American Philosophical Society (Mss.B.D25.570)

- ¹ Romanes's letter has not been found; the diagram was evidently an early version of a fold-out one opposite the title page of *Mental evolution in animals* (G. J. Romanes 1883); the published diagram detailed the evolution of emotions and intellect across the animal world over time, as well as showing the development of these faculties in humans from birth to age 15 months.
- ² Malcolm Guthrie had written a critique of Herbert Spencer's views on CD's theory of natural selection, *On Mr. Spencer's formula of evolution* (Guthrie 1879). CD had discussed the book with family members and friends in December 1879 (see *Correspondence* vol. 27).
- ³ John Fletcher Moulton had read Guthrie 1879 after discussing it with CD, and sent two letters giving his opinion of the book (see *Correspondence* vol. 27, letters from J. F. Moulton, 10 December 1879 and 13 December 1879). In his letter of 10 December 1879, Moulton had criticised Guthrie's use of physical terminology without accurate knowledge of what the terms meant, and referred to the critique of Spencer as 'a pseudo-scientific criticism of a pseudo-scientific work'.
- ⁴ Jean-Henri Fabre had sent CD a copy of *Souvenirs entomologiques* (Fabre 1879). For CD's comments on the book, including his suggestions for experiments on the sense of direction of insects, see the letter to J.-H. Fabre, 31 January 1880.

- ⁵ Romanes evidently sent a letter he received from Grant Allen with his now missing letter (letter from Grant Allen to G. J. Romanes, 28 January 1880). CD had contributed to a subscription to send Allen and his family to the French Riviera for the winter of 1879–80 (see *Correspondence* vol. 27, letter to G. J. Romanes, 23 July 1879 and n. 1).

To T. H. Huxley 4 February [1880]¹

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

Febr 4th

My dear Huxley

Oh Lord what a relief your letter has been to me. I feel like a man condemned to be hung who has just got a reprieve. I saw in the future no end of trouble, but I feared that I was bound in honour to answer.² If you were here I c^d show you exactly how the omission arose.—³

Your letter when read aloud made us all shake with laughter.— You have indeed done me a lasting kindness

Yours affectionately | Ch. Darwin

The affair has [annoyed/ & pained me to a silly extent; but it w^d be disagreeable to any one to be publickly called in fact a liar.

He seems to hint that I interpolated sentences in Krause's M.S, but he could hardly have really thought so. Until quite recently he expressed great friendship for me & said he had learnt all he knew about Evolution from my books, & I have no idea what has made him so bitter against me.⁴ You have done me a real kindness.—

Litchfield will be infinitely pleased at your letter. Emma is copying it to send him.—⁵

Imperial College of Science, Technology and Medicine Archives (Huxley 5; 338)

- ¹ The year is established by the relationship between this letter and the letter from T. H. Huxley, 3 February 1880.
- ² Huxley had advised CD not to respond to Samuel Butler's letter to the *Athenæum* (see letter to H. E. Litchfield, 1 February [1880], enclosure 1), in which Butler suggested that CD intentionally failed to mention that Ernst Krause's original essay (Krause 1879a) had been reworked for *Erasmus Darwin* (see letter from T. H. Huxley, 3 February 1880).
- ³ CD had explained the accidental omission of a note about the revision of Krause's essay in his first draft letter to the *Athenæum*, but had not sent that draft to Huxley (see letter to H. E. Litchfield, 1 February [1880], enclosure 2).
- ⁴ Butler's first letter to CD had expressed his enjoyment of *Origin*; he later sent CD drawings by Arthur Dampier May, some of which were published in *Expression* (see *Correspondence* vol. 13, letter from Samuel Butler, 1 October 1865, and *Correspondence* vol. 20, letter from Samuel Butler to Francis Darwin, [before 30 May 1872]).
- ⁵ Huxley had praised the advice CD received from his son-in-law Richard Buckley Litchfield. A copy of the letter from T. H. Huxley, 3 February 1880, in Emma Darwin's hand, is in DAR 92: B95–6; in the copy, the words 'every son of a' were substituted with 'everyone', and Huxley's drawing was omitted.

To Ernst Krause 4 February 1880

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

Feb 4 1880

My dear Sir,

I enclose a page from the *Athenæum* with a fierce attack by M^r Butler on both of us, especially on me.¹ No doubt I committed a great error in not having stated that you had largely altered the article in *Kosmos*; but I now find that there was a sentence to this effect in the first proof-sheet, which was afterwards accidentally omitted.²

I have consulted three men well capable of judging and they unanimously think M^r Butler's letter so ungentlemanlike as not to deserve an answer from me.³ He seems to insinuate that I suggested to you or persuaded you to add passages attacking his book, or that I myself interpolated such passages. As far as I can remember the sole suggestion which I made to you was to take no notice of M^r Butler's book.⁴ You will be able to judge better than I can whether it is incumbent on you to answer M^r Butler's letter.

I am very sorry that you should be in any way troubled in this affair.

My dear Sir | Yours sincerely | Charles Darwin

P.S. | The obscure expression 'writing at' in the last sentence in the *Athenæum* which I failed to understand at first seems to mean attacking

LS

The Huntington Library (HM 36200)

¹ For Samuel Butler's letter to the *Athenæum*, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1.

² See letter to H. E. Litchfield, 1 February [1880], enclosure 2. CD had pasted in the section of the first printed proof of *Erasmus Darwin* that made it clear that Krause's original essay (Krause 1879a) had been revised.

³ Richard Buckley Litchfield, Frederick Pollock, and Thomas Henry Huxley had all advised that CD should not respond to Butler's letter (see letter from R. B. Litchfield, 1 February 1880, and letter from T. H. Huxley, 3 February 1880).

⁴ See *Correspondence* vol. 27, letter to Ernst Krause, 9 June [1879]. CD had written, 'I hope that you will not expend much powder & shot on M^r Butler, for he really is not worthy of it. His work is merely ephemeral.'

To W. Z. Seddon 4 February 1880

Down. Beckenham. Kent.

Feb^y. 4. 1880.

Dear Sir.

I wish that you or anybody else could account for the colours of the different races of man.¹

Few persons believe that differences of climate suffice.—

I have offered an explanation in my *Descent of Man* which hardly anybody has accepted.²

Since then other views equally or more improbable have been published, & this is all that I can say—

Dear Sir. | Yours faithfully | Ch. Darwin.

Copy

DAR 147: 461

¹ See letter from W. Z. Seddon, 2 February 1880. Seddon's schoolmaster had suggested that climate might be a contributing factor in determining skin colour.

² In *Descent* 2d ed., pp. 195–6, CD added information on the susceptibility of lighter skin to sunburn and suggested the possibility that darker skin might be gradually acquired by humans through natural selection in some places.

From W. S. Dallas 5 February 1880

Geological Society, | Burlington House, W.

5 Feb^y. 1880

My dear M^r. Darwin

I have no doubt you are quite right in abstaining from replying to Butler, & as you who are so much more aggrieved than I am let him alone, I think I may do so too.—¹ If Krause would reply to him it would be the most satisfactory thing, for although by a judicious use of the pronoun “he” in the last paragraph of his letter M^r. Butler manages to leave his readers in doubt whether he charges Krause or me with having made unacknowledged use of his work, the reference must properly be to Krause.—²

The latter gentleman no doubt is acquainted with Butler's work, which, indeed, he criticized in some of the MS. parts translated by me but not printed.—³

If you will send me the MS. of M^r. Francis Darwin's lecture I will look through it to see whether it will suit me for Pop. Sci. Review but I am afraid I shall be unable to put it with the next (April) number.—⁴ I am very much obliged to you & to him for the kind offer of it.—

Believe me | Yours very truly | W. S. Dallas

DAR 99: 133–134

¹ CD had recently decided not to respond to allegations concerning *Erasmus Darwin* made by Samuel Butler in a letter to the *Athenaeum* (see letter to T. H. Huxley, 4 February [1880]; for Butler's letter, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1). CD's letter to Dallas has not been found.

² In his letter to the *Athenaeum*, Butler compared the original essay by Ernst Krause (Krause 1879a) with the version in *Erasmus Darwin*, and suggested that some of the new material made critical reference to Butler 1879 without acknowledging its target. Butler also criticised the reviews of Butler 1879 and *Erasmus Darwin* in *Popular Science Review*, the journal edited by Dallas.

³ A substantial amount of Krause's revised essay for *Erasmus Darwin* was deleted because it covered subjects already discussed by CD in his preliminary notice (see *Correspondence* vol 27, letter to Ernst Krause, 13 August 1879).

⁴ Dallas published Francis Darwin's lecture on climbing plants in the July 1880 issue of *Popular Science Review* (F. Darwin 1880c). The lecture was given by Francis to the Sunday Lecture Society at St George's Hall, London, on 25 January 1880 (Emma Darwin's diary (DAR 242); for more on the Sunday Lecture Society, see Barton 2014, pp. 199–206).

To G. J. Romanes 5 February 1880

Down, | Beckenham, Kent. | Railway Station | Orpington.S.E.R.
Feb 5. 80

My dear Romanes

As I feared, I cannot be of the least use to you. I couldn't venture to say anything about babies without reading my expression book, & paper on infants; or about animals without reading the Descent of man & referring to my notes; & it is a great wrench to my mind to change from one subject to another.¹

I will however hazard one or two remarks. Firstly I should have thought that the word 'love' (not sexual passion) as shown very low in scale to offspring & apparently to comrades, ought to have come in more prominently in your table than appears to be the case.²

Secondly if you give any instance of the appreciation of different stimulants by plants, there is a much better case than that given by you. Namely that of the glands of *Drosera* which can be touched roughly 2 or 3 times & do not transmit any effect, but do so if pressed by a weight of $\frac{1}{78,000}$ grain (*Insectiv Plants* 263). On the other hand the filament of *Dionæa* may be quietly loaded with a much greater weight with no effect, while a touch by a hair causes the lobes to close instantly.³ This has always seemed to me a marvellous fact.

Thirdly I have been accustomed to look at the coming in of the sense of pleasure & pain as one of the most important steps in the development of mind; & I should think ought to be prominent in your table.⁴ The sort of progress which I have imagined is that a stimulus produced some effect at the point affected; & that the effect radiated at first in all directions, & then that certain definite advantageous, lines of transmission were acquired, inducing definite reaction in certain lines. Such transmission afterwards became associated in some unknown way with pleasure or pain. These sensations led at first to all sorts of violent action such as the wriggling of a worm, which was of some use. All the organs of sense would be at the same time excited. Afterwards definite lines of action would be found to be the most useful & would be practiced. But it is of no use my giving you my crude notions

Yours very sincerely | Ch. Darwin

The diagram is returned as *may* wish to show it some one else

LS

American Philosophical Society (Mss.B.D25.571)

¹ Romanes's letter, to which this letter is the second response, has not been found, but Romanes evidently asked CD to comment on a diagram showing the evolution of emotions and intellect in the animal world; the diagram was later published in *Mental evolution in animals* (G. J. Romanes 1883, facing the title page). For CD's initial response, see the letter to G. J. Romanes, 3 February 1880 and n. 1. There is a section in the published diagram in G. J. Romanes 1883 that correlates the development of instincts and intellect in different groups of animals with the age at which these faculties develop in a human infant up to the age of 15 months. CD had discussed these topics in *Expression*, 'Biographical sketch of an infant', and *Descent*.

² In the published diagram, under the heading 'Products of emotional development', Romanes included 'parental affection' and 'social feelings' at the same level as 'sexual selection'; the term 'sexual passion' does not appear on the chart.

- ³ Romanes did not include any mention of plants in the diagram. CD had noted that the glands of *Drosera rotundifolia* (common or round-leaved sundew) did not bend, even when touched with considerable force, if touched momentarily, but bent to the slightest prolonged pressure, while a filament of *Dionaea muscipula* (Venus fly trap) was highly sensitive to momentary touch but less so to prolonged pressure (*Insectivorous plants*, pp. 289–90). Romanes discussed the different types of sensitivity displayed by these plants in G. J. Romanes 1883, pp. 49–51.
- ⁴ Romanes included a category ‘pleasure and pains’ as a relatively early acquisition under the heading ‘Products of intellectual development’ in his published diagram.

From Ernst Krause¹ 6 February 1880

Berlin N.O. Friedenstrasse 10. II.
den 6.2.1880.

Hochverehrter Herr!

So eben empfang ich Ihr freundliches Schreiben vom 4¹. i., mit dem höchst boshaften Artikel des Mr. Butler.² Auch mir will es scheinen, als ob Sie nicht nöthig hätten, auch nur eine Zeile auf diese lächerliche Reclamation zu erwidern. Das Einzige wäre vielleicht, dass *ich* meinerseits die beiden versteckten Insinuationen zurückwies, als hätte sich mein Artikel auf Grund seiner Arbeit verbessert und vermehrt, oder als hätte ich auf Ihre Anstiften einen Angriff gegen ihn eingefügt. Ich werde mir erlauben, einige diesbezüglichen Zeilen auf der folgenden Seite beizufügen und zwar in deutscher Sprache, weil ich nicht genug Übung habe, mich geläufig englisch auszudrücken.³ Sollten Sie den vollständigen oder gekürzten Abdruck für irgendwie nützlich halten, so würde vielleicht Herr Dallas die Güte haben, eine Uebersetzung davon, sei es im Athenäum oder in seinem eigenen Journal zu publiciren.⁴ Es wäre mir nicht im Traume beigefallen, dass die Weglassung des allerdings in dem ersten Entwurfe Ihrer Vorrede enthaltenen Hinweises auf die geschehenen Veränderungen, derartige Verdächtigungen ermöglichen konnte, deren ganze Stärke darin beruht, dem Leser unbegründete Vorstellungen zu erwecken.⁵

Der Angriff charakterisirt die Wuth, in welche der Herr durch die Publikation des Buches versetzt worden ist und insofern könnte er mich amüsiren, wenn ich dabei nur hoffen könnte, dass Sie, hochverehrter Herr, ihn ebenfalls von der humoristischen Seite auffassen möchten.

Was die deutsche Ausgabe betrifft, so hoffe ich Ihnen dieselbe im Laufe dieses Monats vorlegen zu können.⁶

Mit dem herzlichsten Wunsche, dass diese Zeilen Sie in erwünschten Wohlsein antreffen mögen, zeichne ich, hochverehrter Herr | Ihr | aufrichtig ergebener | Ernst Krause

DAR 92: B53

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

² See letter to Ernst Krause, 4 February 1880. For Samuel Butler’s letter to the *Athenaeum*, see letter to H. E. Litchfield, 1 February [1880], enclosure 1.

³ The enclosure has not been found, but was evidently returned to Krause with CD’s letter to him of 9 February 1880.

⁴ William Sweetland Dallas had translated Krause’s revised essay for *Erasmus Darwin*; he was the editor of *Popular Science Review*.

⁵ See letter to H. E. Litchfield, 1 February [1880], enclosure 2).

⁶ The German edition of *Erasmus Darwin* (Krause 1880) was published in April 1880 (letter from Ernst Krause, 19 April 1880).

From G. J. Romanes 6 February 1880

February 6, 1880.

I have to thank you very much for your two letters, and also for the enclosures from [Moulton], which I now return.¹ The latter convey exactly the criticism that I should have expected from [Moulton], for while writing my essay on Theism I had several conversations with him upon the subject of Spencer's writings, and so know exactly what he thinks of them.² But in none of these conversations could I get at anything more definite than is conveyed by the returned letters. In no point of any importance did he make it clear to me that Spencer was wrong, and the only result of our conversation was to show me that in [Moulton's] opinion it was only my ignorance of mathematics that prevented me from seeing that Mr. Spencer is merely a 'word philosopher.' Upon which opinion I reflected, and still reflect, that the mathematicians must be a singularly happy race, seeing that they alone of men are competent to think about the facts of the cosmos. And this reflection becomes still more startling when supplemented by another, viz. that although one may not know any mathematics, everybody knows what mathematics are: they are the sciences of number and measurement, and as such, one is at a loss to perceive why they should be so essentially necessary to enable a man to think fairly and well upon other subjects. But it is, as you once said, that when a man is to be killed by the sword mathematical, he must not have the satisfaction of even knowing how he is killed. Of course, in a general way I quite understand and agree with [Moulton] that Spencer has done but little service to science. But I believe that he has done great service to thinking, and all the mathematicians in the world would not convince me to the contrary, even though they should all deliver their judgment with the magnificent authority of a [Moulton].

Coming now to the diagram, I am much obliged to you for your suggestions. The 'Descent of Man,' with all its references upon the subject, and also your paper on the 'Baby,' were read, and the results embodied in the diagram, so I am very glad you did not take the needless trouble of consulting these works.³ By 'Love' I intend to denote the complex emotion (dependent on the representative faculties) which, having been so lately smitten myself, I am perhaps inclined to place in too exalted a position. But you did not observe that I placed 'Parental Affection' and 'Social Feeling' very much lower down.⁴

In my essay I carefully explain the two cases of *Drosera* and *Dionæa* as being the best hitherto observed for my purpose in establishing the principle of discrimination among stimuli, as a principle displayed by non-nervous tissues.⁵

E. D. Romanes 1896, p. 95

- ¹ See letters to G. J. Romanes, 3 February 1880 and 5 February 1880. With the first of these letters, CD had enclosed letters to him from John Fletcher Moulton of 10 December 1879 and 13 December 1879 (*Correspondence* vol. 27). Moulton's letters contained his comments on Malcolm Guthrie's critique of Herbert Spencer's views on CD's theory of natural selection in *On Mr. Spencer's formula of evolution* (Guthrie 1879). In the printed source of this letter, Moulton's name has been replaced by dashes.
- ² In [G. J. Romanes] 1878, Romanes had discussed ideas presented by Spencer in *First principles* (Spencer 1860–2), notably Spencer's concept of 'persistence of force' ([G. J. Romanes] 1878, p. 88 and passim). See letter to G. J. Romanes, 3 February 1880 and n. 3. Moulton's criticism of both Spencer and Guthrie centred on their apparent failure to understand many of the scientific terms and concepts they employed in their writing.
- ³ In his letter of 5 February 1880, CD had told Romanes that he would need to consult *Expression*, 'Biographical sketch of an infant', and *Descent* before venturing to say anything on babies or animals.
- ⁴ Commenting on a diagram that later appeared in *Mental evolution in animals* (G. J. Romanes 1883, facing title page), CD expressed surprise that Romanes had not put the development of 'love' at an earlier stage (see letter to G. J. Romanes, 5 February 1880 and n. 2). Romanes alludes to his marriage in February 1879 to Ethel Duncan.
- ⁵ See G. J. Romanes 1883, pp. 49–51. Romanes had evidently mentioned a different case illustrating plant sensitivity in a now missing letter to CD; CD suggested that the types of sensitivity shown by *Drosera* (sundew) and *Dionaea* (Venus fly trap) were better examples (see letter to G. J. Romanes, 5 February 1880 and n. 3).

From Ernst Krause¹ 7 February 1880

Hochverehrter Herr!

Es fällt mir ein, dass Sie den Text des malitiösen Angriffs vielleicht noch zur Vergleichung nöthig haben könnten, und ich beeile mich deshalb, Ihnen denselben zurückzusenden.² Ich hatte es gestern vergessen.³

In herzlicher Ergebenheit | Ihr | Ernst Krause

Berlin 7.2.1880.

DAR 92: B54

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

² See letter to Ernst Krause, 4 February 1880. CD had sent Krause a copy of Samuel Butler's letter to the *Athenaeum* (for the text of Butler's letter, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1).

³ See letter from Ernst Krause, 6 February 1880.

From Ernst Haeckel¹ 9 February 1880

Jena
9/2 1880

Hochverehrter theurer Freund!

Zu der Wiederkehr Ihres Geburtstages bringe ich Ihnen von ganzem Herzen die aufrichtigsten Glückwünsche dar.² Mögen Sie noch viele Jahre mit ungeschwächter Geisteskraft und körperlicher Rüstigkeit zum Heile der Wissenschaft und zum Wohle der Menschheit wirken, und sich der beispiellosen Erfolge erfreuen, welche Ihre bahnbrechenden Theorien auf allen Gebieten menschlichen Wissens hervorgerufen haben.

Unter allen Ihren zahllosen Verehrern und Schülern kann keiner Ihnen aufrichtiger dankbar sein und an Ihrem kommenden Jahrestage herzlicher Ihrer gedenken, als ich selbst. Verdanke ich Ihnen doch für mein ganzes Leben die zielbewusste Richtung meiner wissenschaftlichen Thätigkeit, und jeder Erfolg, den ich dabei erringe, ist im Grunde durch die von Ihnen zuerst gegebene phylogenetische Anregung bestimmt.

Obleich die reichen Früchte des “Darwinismus” im Gebiete der Physiologie, der Oekologie und Chorologie, wie auf allen Gebieten der Lebensforschung, jährlich glänzender hervortreten, so glaube ich doch, dass das Gebiet meiner eigenen Thätigkeit, die Morphologie, darin keinem anderen Fache nachsteht.³ Ich hoffe demnächst in der “Morphologie der Medusen” (II. Theil der Monographie) zu zeigen, wie herrlich sich hier Alles phylogenetisch erklären lässt.⁴

Mit lebhafter Freude denke ich noch oft an meinen letzten, dritten Besuch in Down bei Ihnen zurück, und wie wohl und frisch ich Sie wieder gefunden habe.⁵ Mögen Sie immer so bleiben, das wünscht von ganzem Herzen Ihr treulichst ergebener | Ernst Haeckel

P.S. Meine freundlichsten Grüsse an Ihre Frau Gemahlin und Ihre Kinder!

DAR 166: 76

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

² CD's birthday was on 12 February; in 1880 he was 71.

³ In his work *Generelle Morphologie der Organismen* (Haeckel 1866, 2: 286–9), Haeckel had described ecology and chorology as neglected disciplines within physiology. Ecology was defined as the study of the relation of organisms to the outside world, both organic and inorganic, or their ‘conditions for existence’, while chorology was the study of the spatial dispersal of organisms, their geographical and topographical spread over the earth.

⁴ The second volume of the *Monographie der Medusen* (Haeckel 1879–81), titled *Die Tiefsee-Medusen der Challenger-Reise und der Organismus der Medusen* (The deep-sea medusae of the *Challenger* voyage and the organism of the medusa), was published in 1881.

⁵ Haeckel's visit took place on 5 September 1879; his earlier visits were on 21 October 1866 and 26 September 1876 (Emma Darwin's diary (DAR 242)).

To Ernst Krause 9 February 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Feb 9. 80

My dear Sir,

I am very much obliged for your letter, which I have considered attentively.¹ In my opinion it would be best for you not to write to the Athenæum, because M^r Butler is quite unscrupulous & he would in answer pick out some passages in your essay & say that they were borrowed from his book.² You could only deny this, & no one would take the trouble to compare the two. Everybody will see in what a bad temper Butler has written & will disregard his charges against you. M^r Dallas means to take no notice of him.³ Nevertheless if you decide to publish your letter I will get it translated; but in this case will you be so good as to strike out the passage about the first proof-sheets, for which reason I return your letter.⁴ Having utterly

forgotten these proofs I wrote to Butler that it had *never* occurred to me to insert anything on the subject; & after reading the above passage in your letter he would not hesitate to say that I had intentionally told a falsehood⁵

I am sorry that you should have had the trouble of returning the Athenæum.⁶ It has surprised me much, seeing what the reviews have said about our book, & knowing that many strangers have greatly liked it, that the sale has not been greater, for the 1,000 copies have not yet all been disposed of. M^r Murray makes up his annual accounts in the middle of the summer, but I fear that there will be then but a small profit to transmit to you.⁷ It is good news that the German edit will soon be out.⁸

My dear Sir, | Yours sincerely. | Charles Darwin

LS

The Huntington Library (HM 36201)

¹ See letter from Ernst Krause, 6 February 1880.

² In his letter to CD of 6 February 1880, Krause had enclosed a draft letter to the *Athenæum*, responding to Samuel Butler's letter to the *Athenæum* (for Butler's letter, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1). Krause's draft has not been found.

³ See letter from W. S. Dallas, 5 February 1880; William Sweetland Dallas had translated Krause's revised essay for *Erasmus Darwin*.

⁴ See n. 2, above. In the first proof-sheets of *Erasmus Darwin*, CD had included a passage mentioning that Krause added largely to his essay; he deleted it at Krause's request, because the sentence also mentioned that much of Krause's essay was later omitted since it duplicated material in CD's section of the book (see letter to H. E. Litchfield, 1 February [1880], enclosure 2).

⁵ See letter to Samuel Butler, 3 January 1880.

⁶ See letter from Ernst Krause, 7 February 1880.

⁷ In the event, CD had not heard as late as the end of July 1880 that any profit had been made (letter to Ernst Krause, 29 July 1880).

⁸ The German edition of *Erasmus Darwin* was published in April 1880 (letter from Ernst Krause, 19 April 1880).

From Wilhelm Breitenbach¹ 10 February 1880

Verehrtester Herr Darwin!

In der vorletzten Woche hat sich unter einer Anzahl Studirender der Naturwissenschaft an hiesiger Universität ein naturwissenschaftlicher Verein gegründet, welcher den Zweck hat seine Mitglieder im Studium der Naturwissenschaft zu fördern und in ihnen ein lebhafteres Interesse an der Natur wachzurufen.² Der junge Verein erfreut sich des ganz besonderen Wohlwollens der Herrn Haeckel, Strasburger und Preyer,³ welche dies bei der Eröffnungsfeier ausdrücklich ausgesprochen haben. Getreu dem Wahlspruch seines hochverehrten Lehrers Ernst Haeckel: "Impavidi progrediamur"⁴ wird der junge Verein stets bestrebt sein, auf den von Ihnen und Herrn Haeckel bezeichneten Wegen zu wandeln, und so hoffen wir denn, dass aus unseren Kreise noch mancher tüchtige Forscher hervorgehen möge. Es würde uns eine grosse Freude sein, wenn auch Sie diesen Wunsch mit uns theilten; mit um so grösserer Lust und Liebe würden wir alle dann arbeiten.

Unser Verein hat mich beauftragt, Ihnen, hochverehrter Herr Darwin, zu Ihrem heutigen Geburtstage seine innigsten Glückwünsche auszusprechen.⁵ Wir Alle wünschen aus vollem Herzen, dass es Ihnen vergönnt sein möge, noch recht lange unter den Lebenden zu weilen und dass Sie den Riesenbau immer schöner und vollendeter werden sehen, den Ihre Hand zu meisterhaft errichtet hat

Indem ich Sie bitte, diese Zeilen nur als den schlichten Ausdruck unseres aufrichtigen Wunsches zu nehmen, bin ich | mit vorzüglichster Hochachtung | Ihr ganz ergebenster | Wilhelm Breitenbach

Jena. 10.II.1880.

DAR 160: 294

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

² Breitenbach was a student of natural sciences at the University of Jena; the society, Naturwissenschaftlicher Verein Studirender der Universität Jena (Natural Sciences Society of Students of the University of Jena), had its first meeting on 22 January 1880 (Nöthlich 2009, pp. 24–5).

³ Ernst Haeckel, Eduard Strasburger, and William Preyer were among the professors who supported the society.

⁴ *Impavidi progrediamur*: let us proceed fearlessly (Latin). Haeckel had adopted the motto in his book *Freie Wissenschaft und freie Lehre* (Haeckel 1878, p. 93). For more on the context of Haeckel's motto, see Di Gregorio 2005, pp. 360–3.

⁵ CD's birthday was on 12 February.

From W. E. Darwin 10 February [1880]¹

Basset
Feb 10th

My dear Father,

We got back v. comfortably & S. is quite fresh today comparatively speaking,² dia in composition means through, across, as diabainō to go across—diaballō to throw across.³

I send the small Greek dictionary by rail.

My love to mother— I hope she is better.

Your affect Son | W. E. Darwin

The next time you have the Huxleys give us a chance of meeting them—⁴ S. is very anxious to meet him

DAR 209.7: 159

¹ The year is established by the reference to returning home (see n. 2, below).

² William and Sara Darwin had visited Down from 7 to 9 February 1880 (Emma Darwin's diary (DAR 242)).

³ *Diabainō*: transliteration of Greek διαβαίνω (cross, pass through); *diaballō*: transliteration of Greek διαβάλλω (throw across). CD used the term 'diaheliotropism' to refer to a movement more or less transverse to the light and induced by it (*Movement in plants*, p. 5). The term was probably chosen by CD to replace the German term 'Transversal-Heliotropismus', which had been introduced by Albert Bernhard Frank (see Frank 1870, p. 77).

⁴ Thomas Henry Huxley and Henrietta Anne Huxley.

From Ernst Krause¹ 10 February 1880

Berlin N.O. Friedenstrasse 10. II.
den 10. Februar 1880.

Hochverehrter Herr!

Unter den vielen Glückwünschen, die Sie übermorgen aus allen Theilen der Welt erhalten werden, hoffe ich, werden sich viele befinden, die ebenso herzlich und aufrichtig wünschen mögen, dass Ihnen noch recht häufige Wiederholungen dieses Tages in ungetrübtester Gesundheit beschieden sein möchten, wie ich dies thue, aber wenige werden dies mit innigeren Dankesgefühlen können. Wenn ich denke, wie viele Freude mir die im vorigen Jahre zu dieser Veranlassung verfasste kleine Skizze nachher bereitet hat, so fällt mir um so schwerer auf's Herz, dass Ihnen der Aerger über den malitiösen Butler'schen Angriff doch vielleicht einige böse Stunden macht.² Je mehr ich darüber nachdenke, um so perfider erscheint mir der auf ein Nichts begründete Angriff, und es thut mir fast leid, die Ihnen übersandte Antwort nicht noch etwas schärfer abgefasst zu haben.

Verzeihen Sie, dass ich in einem Gratulations-Briefe von so unliebsamen Dingen zu sprechen beginne, und gestatten Sie mir gütigst noch eine Frage. Der 10 April in der Grabschrift ist doch ein Druckfehler und das erstere Datum das richtigere?³ Ich sehe eben, dass ich den Fehler in der deutschen Uebersetzung kopirt habe.³

Ich zeichne, hochverehrter Herr, | mit den herzlichsten Glückwünschen | Ihr | aufrichtig ergebener | Ernst Krause

DAR 92: B55

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

² CD's birthday was on 12 February. Krause's essay on Erasmus Darwin (Krause 1879a) was published in February 1879, in a special number of the journal *Kosmos* celebrating CD's 70th birthday. CD asked permission to have Krause's essay translated (see *Correspondence* vol. 27, letter to Ernst Krause, 9 March 1879); the revised essay, together with a biographical sketch of his grandfather by CD, was published in November 1879 (*Erasmus Darwin*; *Correspondence* vol. 27, letter from Reginald Darwin, 12 November 1879). On Samuel Butler's attack, see the letter to Ernst Krause, 4 February 1880.

³ See *Erasmus Darwin*, pp. 124 and 127; the error was copied in the German edition (Krause 1880, p. 72). Erasmus Darwin died on 18 April 1802 (*ODNB*).

From Anton Dohrn 11 February 1880

Stazione Zoologica | di | Napoli
February 11th: 1880.

Dear Mr. Darwin!

It has become a privilege of the Zool. Station to congratulate you on your birthday.¹ Last year I was unable to do it by letter,—at least I was unwilling to do it, because I wished to tell something definitively about the position of the Zool. Station, and could not do it on account of the unfinished state of my negotiations with the German Government.²

Today I am able to add to my own and the congratulation of all the Naturalists assembled in the Station the good news, that a new subvention of £.1500 is added

to our regular income, and that I have good reason to retain it as an annual grant, though the formal and legal assurance of it, is still to be got. Parliament and Federal Council have both consented to place that sum on the Budget of the German Foreign Office, and I shall have to go to Berlin next month, to get it put down on the regular expenses of that Budget,—an achievement which will get the Zool. Station completely safe, and guarantee our considerable annual expenses.

The Zool. Station will now be able to concentrate all its energies to the direct scientific work, to the development of all sorts of technical methods of fishing, preserving, etc. and to the best ways of rendering the considerable amount of scientific material accessible to the best hands for working at it.

It is almost ten years, when I had for the first and only time the honour of talking to you.³ I daresay I did not quite anticipate the difficulties of the enterprise, of which I told you at that time. You did, and you did still more, some years after in helping me over the deadlock,—I may say the *first* deadlock.⁴

I have always remembered your exceedingly kind letter, which you wrote to me at that time, when my purse and my nerves were equally exhausted.⁵ It is therefore my great satisfaction to be able to give the above news, and to add to it once more my heartfelt thanks for the sympathy and material help, which you and your sons at that most critical moment bestowed upon me.

At present we are again twenty Naturalists in the Station, and there is a new Government added to the list of the supporters, Belgium.⁶ All our technical and scientific apparatus has been largely developed in the past year, scientific diving has been practised with great success, and almost twenty larger Monographs on the Ctenophorae, Fierasfer, Pycnogonidae, Planariae, Nemertinae, Actiniae, Balanoglossus, Sipunculoidae Caprellidae, Capitellidae, Echinodermata and several families of Algae are in course of preparation, two will soon be published forming the beginning of a large periodical publication “Fauna & Flora of the Gulf of Naples and neighbouring seas.”⁷

If all fits in,—if we are especially not drowned in a great European war,⁸—then I hope to give soon proof of a very active scientific life, and I wish to be able to present for many coming years the congratulations of the Zoological Station together with the results of its action to your birthday,—and I hope you will kindly allow of this liberty as hitherto so also in future.

With my kindest compliments to Mrs. Darwin and to Mr. Frank and George Darwin | believe me | Yours most respectfully | Anton Dohrn

To | Charles Darwin Esq. | Down.

DAR 162: 218

¹ CD's birthday was on 12 February. See *Correspondence* vol. 22, telegram from Anton Dohrn, 12 February 1874, *Correspondence* vol. 23, letter from Anton Dohrn, 7 February 1875, and *Correspondence* vol. 27, telegram from the Naples Zoological Station, 12 February 1879.

² Dohrn was in negotiations with the German Empire to include the Zoological Station at Naples in the regular budget of the government; this goal was not reached until 1889. For an account of Dohrn's negotiations, see Heuss 1991, pp. 194–9.

- ³ Dohrn visited CD at Down on 26 September 1870 (Heuss 1991, pp. 108–9).
- ⁴ On the early financial difficulties of the Zoological Station at Naples, see *Correspondence* vol. 21, letter from F. M. Balfour, 11 November 1873, and letter to G. H. Darwin, 15 November [1873].
- ⁵ See *Correspondence* vol. 22, letter to Anton Dohrn, 7 March 1874. CD had contributed £100 to a subscription to raise funds for Dohrn; Francis Darwin and George Howard Darwin had also contributed £10 each to the subscription.
- ⁶ Countries or institutions, but not individuals, could rent tables at the Zoological Station; rental periods varied from one to five years and renewal was not always assured (see Heuss 1991, pp. 237–40).
- ⁷ The monograph series, *Fauna und Flora des Golfes von Neapel und der angrenzenden Meeres-Abschnitte*, began publication in 1880 with the works *Die Ctenophoren des Golfes von Neapel und der angrenzenden Meeres-Abschnitte* (Ctenophorae of the Gulf of Naples and neighbouring seas; Chun 1880), and *Le Specie del genere Fierasfer del golfo di Napoli e regioni limitrofe* (Species of the genus *Fierasfer* of the Gulf of Naples and neighbouring seas; Emery 1880). Ctenophora is the phylum of comb jellies. *Fierasfer* is a synonym of *Carapus*, a genus in the family Carapidae (pearlfishes). Pycnogonidae is a family of sea spiders. Planariae is a synonym of Planariidae, the family of freshwater planarian worms. Nemertinae is a synonym of Nemertea, the phylum of ribbon-worms. Actiniae is a synonym of Actiniidae, a family of sea anemones. *Balanoglossus* is a genus of acorn worms. Sipunculoidae, a synonym of Sipunculidae is a family of peanut worms. Caprellidae is a family of skeleton shrimps. Capitellidae is a family of polychaete worms. Echinodermata is the phylum of sea urchins, sand dollars, and sea stars (starfish). Algae are mostly aquatic photosynthetic organisms formerly classed as plants; some forms are now included in other kingdoms such as Chromista and Bacteria.
- ⁸ The latest European crisis had centred in the Balkans, culminating in the Russo-Turkish war (1877–8), which was finally settled to the satisfaction of the major European states by the Treaty of Berlin (see Mackenzie 1993; see also *Correspondence* vol. 26).

To Ernst Haeckel 12 February 1880

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

Feb. 12th 1880

My dear Häckel

I thank you cordially for your letter,—one of the kindest which I have received in my life-time.¹ As I have much to do will write no more.—

Your ever true friend | Charles Darwin

Ernst-Haeckel-Haus (Bestand A-Abt. 1:1-52/51 [A 9905])

¹ See letter from Ernst Haeckel, 9 February 1880; Haeckel sent birthday wishes to CD.

From James Torbitt 12 February 1880

58. North Street. | Belfast.

12. Feb^y. 1880.

Charles Darwin Esq^{re} | Down.

My dear Sir.

I duly received your much esteemed letter of 27th Dec^r last and now write to suggest might it be possible to *borrow* £500 from the Government for the purpose of carrying on the work; giving the new varieties of the potato as security for repayment of the loan and of your advance—²

A single variety of the plant “The Champion” which is now spreading all over the Kingdom has been worth many thousands of pounds to the country within the last few years, and doubtless it will be worth a great many more before it disappears so that the security ought to be good—

The “Champion” was grown from a seed sown in the Spring of 1863 and latterly it has become more and more susceptible to the Disease and less and less able to produce its fruit—³

Watching it for the last two seasons I found all the Stamens abnormally twisted and all the flowers dropped off.— Last year as I am informed it did not produce any fruit anywhere and unquestionably new varieties should be coming forward to replace it—⁴

In the present state of affairs with the whole kingdom “hungry” for potatoes such as we had fifty years ago, it will be too bad if £500 stops the way. and yet I cannot devote another shilling to the work, although I thought I could afford to spend a thousand a year on it when I commenced—⁵

If no better may be I will offer these new varieties to all the Governments in Europe, and if they be allowed to pass out of the Country. I think it will be a mistake—but better that, than that they should perish.

I am my dear Sir. | most respectfully | and faithfully. | James Torbitt.

Copy

DAR 144: 486

¹ See *Correspondence* vol. 27, letter to James Torbitt, 27 December 1879.

² CD had been trying to secure government aid for Torbitt in his work on breeding blight-resistant potatoes. He had written to his friend Thomas Henry Farrer, who tried to interest Dudley Francis Stuart Ryder, minister for the Board of Trade, where Farrer was permanent secretary (see *Correspondence* vol. 27, letter to James Torbitt, 17 November 1879).

³ The Champion potato was bred in 1863 by John Nicoll in Forfar, Scotland; in 1879 it was the only variety to yield a substantial harvest in Ireland during a widespread outbreak of blight (Salaman 1985, p. 168).

⁴ The Champion variety was, in fact, grown in greater quantities in Ireland until the end of the century (Salaman 1985, p. 168).

⁵ Torbitt alludes to the great Irish potato famine in the 1840s, which led to mass emigration to America (for more on the famine and its consequences, see Donnelly 2001).

To Wilhelm Breitenbach 13 February 1880

Down, | Beckenham Kent. (&c).

Feb 13./80.

Dear Sir.

I thank you sincerely for your kind & courteous letter of the 10th.¹ I hope that you will be so good as to return to the Society of Students at Jena my cordial acknowledgments of their kind congratulations on my birthday—² I wish them every kind of success in their studies & throughout life— He who heartily sympathises

with the work done by other men shows that he works for the sake of discovering the truth & not for gaining distinction; & this is the best of all auguries for success in the pursuit of Science.

Believe me, My dear Sir. | Yours very faithfully. | Charles Darwin.

Copy

DAR 143: 143

¹ Letter from Wilhelm Breitenbach, 10 February 1880.

² The student society was the Naturwissenschaftlicher Verein Studirender der Universität Jena (Natural Sciences Society of Students of the University of Jena; see letter from Wilhelm Breitenbach, 10 February 1880 and n. 2).

To Ernst Krause 13 February 1880

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

Feb 13./80

My dear Sir

I thank you heartily for your *most* kind letter.¹ I write now merely to say that April 10th on the Epitaph is a dreadful mistake, made by the copyist, & I cannot think how I overlooked it, but I very often overlook errata.²

Yours very sincerely | Ch. Darwin

The Huntington Library (HM 36202)

¹ Letter from Ernst Krause, 10 February 1880.

² CD had not noticed the error in *Erasmus Darwin*, p. 127 (see letter from Ernst Krause, 10 February 1880 and n. 3).

To Lawson Tait 13 February 1880

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

Feb. 13th. 1880.

My dear Sir

Although you tell me not to write I must thank you for your congratulations, your two notes, the articles in the Daily Post & the copy of the Address.—¹ Nothing can be more honourable to me than the article & the Address, & according to my judgment no one could possibly have written them in better taste. I fear that you have expended much time & trouble in this whole affair.

Accept my sincere thanks & believe me | Yours sincerely | Ch. Darwin

Of course whenever I receive the Address I will send a formal answer.²

Shrewsbury School, Taylor Library

¹ The letter and notes from Lawson Tait have not been found. Two cuttings of articles from the *Birmingham Daily Post* are in DAR 226.2: 44 (1 and 2). The first article is headed 'Charles Darwin: a birthday note' (*ibid.*, 12 February 1880, p. 4); the second is headed 'Birmingham Philosophical Society. Address to Dr. Darwin' (*ibid.*, 13 February 1880, p. 5).

- ² The address from the Birmingham Philosophical Society was presented by Tait when he visited CD in London on 4 March 1880 (Emma Darwin's diary (DAR 242); LL 3: 227).

To A. S. Wilson 13 February 1880

Down
Feb: 13. 1880

My dear Sir

It was very kind of you to send me the two numbers of the Gard. Chron. with your two articles, which I have read with much interest. You have quite convinced me, whatever M: Asher may say to the contrary.¹ I want to ask you a question on the bare chance of your being able to answer it; but if you cannot please do not take the trouble to write. The lateral branches of the silver fir often grow out into knobs through the action of a fungus *Æcidium*; and from these knobs shoots grow vertically instead of horizontally like all the other twigs on the same branch.² Now the roots of Cruciferæ³ and probably other plants are said to become knobbed through the action of a fungus; now do these knobs give rise to rootlets and if so do they grow in a new or abnormal direction?

Believe me, my dear Sir | Yours sincerely | Charles Darwin

Copy
DAR 148: 370

- ¹ Wilson's letter and the copies of *Gardener's Chronicle* have not been found. Wilson sent the issues for 24 January and 7 February 1880, containing his two-part article, 'Kubanka and Saxonka wheat' (Wilson 1880); Wilson had published an earlier article with his preliminary findings on the wheat varieties (Wilson 1879). Georg Michael Asher had arranged for samples of the wheat to be sent from Russia in 1878 (see *Correspondence* vol. 26, letter from G. M. Asher, 14 February 1878). Asher had disputed Wilson's view that the apparent transformation of Kubanka into Saxonka was merely an artefact of the greater productivity of the latter variety (Wilson 1879, p. 654, and Wilson 1880, p. 173; see letter from A. S. Wilson, 5 January 1880).
- ² In his letter of [after 2 June 1879] (*Correspondence* vol. 27), Francis Darwin had provided CD with information on the abnormal shoots of *Abies pectinata* (a synonym of *A. alba*, silver fir), caused by *Accidium elatinum* (a synonym of *Melampsorella caryophyllacearum*, fir broom rust).
- ³ Cruciferae is a synonym of Brassicaceae, the mustard and cabbage family.

From William Cole 14 February 1880

Laurel Cottage, | Buckhurst Hill. | Essex.
Feby 14th 1880.

Dear Sir,

I have the honour to inform you that at the Inaugural Meeting of "The Epping Forest & County of Essex Naturalists' Field Club" held on January 10th 1880, you were unanimously elected an Honorary Member of the Club, in humble recognition of the great services rendered by you to Natural Science, and in appreciation of kindly aid afforded to the Club during its formation.¹ I trust you will see fit to ratify your election; your consent would be very gratifying to the members of the Club.

I now beg to enclose for your information a copy of the Rules, and list of Officers for 1880.²

I am | Yours faithfully | W^m Cole. | (Sec^y)

Charles Darwin Esq M.A. F.RS &c

DAR 202: 19

¹ CD had made a donation of a guinea (£1 1s.) to the club when he turned down an invitation to become an ordinary member (see letter to William Cole, 6 January 1880).

² CD's copy of the rules and list of officers has not been found; they were published in the *Transactions of the Epping Forest and County of Essex Naturalists' Field Club* 1 (1880-1): 1-7 and iii.

To T. H. Farrer 14 February 1880

Down. | Beckenham. Kent &c

Feb: 14. 80.

My dear Farrer.

If you are not utterly weary of the subject will you read this letter—¹ It seems that Mr T is too poor to go on without aid, and it will be a grievous shame, in my opinion, if he is not aided & there has been so much delay on the part of Lord Sandon, that there is now but little time to spare—² There is, I suppose, no hope, only political squabbles are cared for: would you be so kind as to let me hear as soon as you feel convinced that Government will give no aid that I may inform Mr Torbitt that he must give up his experiments & dispose of his stock—

Ever yours sincerely | Ch. Darwin.

PS. I would subscribe £50 but I have not strength or time to go begging for the remainder; and it is too horrid work for anyone whatever to undertake—

Therefore if anything is to be done it must I think, be done by Government—though whether the G. *can* or will do such a thing is quite beyond my knowledge—

Copy

DAR 144: 100

¹ CD enclosed the letter from James Torbitt, 12 February 1880.

² See letter from James Torbitt, 12 February 1880 and n. 2. Farrer had asked Dudley Francis Stuart Ryder, Viscount Sandon, to seek government aid for James Torbitt.

To Lawson Tait 14 February 1880

Down.

Feb. 14. 1880.

My dear Sir.

I have received the Newspaper which you kindly sent me, with the account of the meeting and this has deeply gratified me.¹ The whole affair is a quite unprecedented honour— If you think fit to come here to give me the Address I shall be extremely glad to see you, but is it not a pity that you should spend your valuable time in

travelling here? Had you not better send it by post? I would offer to come to London to receive it, but even a short journey is apt to fatigue me much, and I am always very unwilling to undertake one. Should you decide to come to Down March 21st: would not be quite convenient on two or three accounts; nevertheless I could manage it, but should prefer Wednesday, March 3rd: if equally convenient to you.²

With my sincere thanks for all the extraordinary interest which you have shown in this affair. | I remain, my dear Sir. | Yours sincerely | Charles Darwin.

Copy

DAR 147: 535

¹ See letter to Lawson Tait, 13 February 1880 and n. 1; Tait evidently sent a copy of the *Birmingham Daily Post*, 13 February 1880, containing an account of a meeting of the Birmingham Philosophical Society at which an address to CD was read (*ibid.*, p. 5). CD had been made the first honorary member of the Birmingham Philosophical Society (see letter from Lawson Tait, 18 January [1880]).

² The address from the Birmingham Philosophical Society was presented by Tait when he visited CD in London on 4 March 1880 (Emma Darwin's diary (DAR 242); *LL* 3: 227).

From George Bentham 15 February 1880

25, Wilton Place. | S.W.
Feb^y 15/80

My dearest Darwin

If you do not object would you kindly sign the enclosed and return it to me?¹

I have been for the last four or five months at work at *Orchideæ for Genera Plantarum* and have been wonderfully assisted by your book on the fertilisation of Orchids.² I had no idea that Lindley and Reichenbach together could have brought the order into such a state of confusion both as to generic limitation and terminology for both showed great ability and intimate knowledge of species.³ I am very glad that you have suppressed the term *caudicle* which is indiscriminately applied to three very different organs—in *Neottieæ* &c a true caudicle or tail-like end of the pollen mass—in *Epidendreæ* a portion of the pollen not consolidated into the pollen masses but connecting them—in *Vandææ* what you much more correctly have styled the *pedicel*, being as you have shown a production of the rostellum I should however prefer the term *stipes* for *pedicel* is I believe as generally restricted to the stalk of a single flower as *petiole* is to the stalk of a leaf whilst *stipes* is used more generally as the support of any organ.⁴

I do not like the term *gland* generally used for what you have called *disk* but there is some inconvenience in using the latter term on account of its being more generally applied sometimes to an expansion of the torus sometimes to the centre of the upper surface of a petal etc.—the appearance of the part in question of the pollinarium is not always that which we associate with the word *gland*, and only very rarely like that of a *quoit* but I cannot at present devise a better term than the usually adopted one of *gland*, which in botany has not any very definite meaning being applied in different instances to very different organs.⁵

Your labours have done so much to the elucidation of the very curious modifications of the fertilising apparatus in the Order that I trust you will excuse my troubling you with these observations in the hopes that if any improved terminology occurs to you you would kindly communicate it— I have still I have no doubt six months work before I can get through the Order. Hooker is working up the Palms⁶

Yours very sincerely | George Bentham

DAR 160: 171

- ¹ Bentham had sent CD the certificate proposing William Turner Thiselton-Dyer for election as a fellow of the Royal Society of London (letter to George Bentham, 16 February 1880). The certificate was read to the society on 19 February 1880 and Thiselton-Dyer was elected 3 June 1880 (Royal Society archives, GB 117 EC/1880/07).
- ² The section on the order Orchideae (a synonym of the family Orchidaceae) appeared in volume 3 part 2 of *Genera plantarum* published in 1883 (Bentham and Hooker 1862–83, 3: 460–636); Bentham published a summary of his classification in 1881 (Bentham 1881). *Orchids* was first published in 1862; the revised edition appeared in 1877 (*Orchids* 2d ed.).
- ³ John Lindley's *The genera and species of orchidaceous plants* (Lindley 1830–40) was one of the earliest attempts to systematise orchids; Bentham significantly reduced the number of orchid genera recognised by Lindley as well as subsuming some of Lindley's tribes within others and introducing subtribes (see Bentham 1881, pp. 285–7). Heinrich Gustav Reichenbach never published his own classification of orchids, but did publish a critical essay on the topic in 1885 (Reichenbach 1885).
- ⁴ Neottieae, Epidendreae, and Vandaeae are orchid tribes recognised by both Lindley and Bentham (Lindley 1830–40, p. xvii; Bentham 1881, pp. 284–5). CD had differentiated the caudicle, an extension of the pollen mass, from the pedicel, an extension of the rostellum (a modified stigma; see *Orchids*, pp. 6–7). Modern orchid terminology, following Bentham, has referred to the supporting organ derived from tissue of the rostellum as the stipe (Dressler 1981, p. 315).
- ⁵ The torus or receptacle is the thickened stem tissue from which the floral organs grow. Some botanists had referred to the fleshy or glandular part of the receptacle as the disk (see, for example, MacGillivray 1840, p. 114). CD had called the portion of the rostellum which was attached to and removed with the pollen masses the 'viscid disc' (*Orchids*, p. 7). In modern orchid terminology, this is called the viscidium or viscid disk (Dressler 1981, p. 316).
- ⁶ Joseph Dalton Hooker's section on the order Palmae (a synonym of the family Areaceae) appeared in Bentham and Hooker 1862–83, 3: 870–948.

To Anton Dohrn 15 February 1880

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

Feb. 15th 1880

Dear D^r Dohrn

I thank you sincerely for your kind congratulations, & beg you to return my best acknowledgments to all the naturalists at the Station, who have joined you sending their kind message.—¹ I rejoice most truly to hear of the 1500[£] subvention, & trust it may be permanent.² This will remove the manifold anxieties from which you have suffered; whilst from the first you deserved the highest sympathy, & encouragement.

I was thinking of writing to you on a small point. Perhaps you saw in the papers that the Turin Soc^y honoured me to an extraordinary degree by awarding me the Bressa prize.³ Now it occurs to me that if your Station wanted some piece of

apparatus of about the value of 100£ I sh^d. very much like to be allowed to pay for it. Will you be so kind as to keep this in mind, & if any want should occur to you, I w^d send you a cheque at any time.—⁴

With all good wishes for your own success & happiness & for the prosperity of the Station, believe me | My Dear D^r Dohrn | Yours very sincerely | Charles Darwin

Bayerische Staatsbibliothek München (Ana 525. Ba 704)

¹ See letter from Anton Dohrn, 11 February 1880; Dohrn had sent birthday greetings to CD.

² The Zoological Station at Naples, of which Dohrn was director, had received the grant from the Imperial German government; the grant was not made part of the regular government budget until later (see letter from Anton Dohrn, 11 February 1880 and n. 2).

³ CD was awarded the Bressa Prize in late 1879 (see *Correspondence* vol. 27, letter from Michele Lessona, 28 December 1879). CD recorded the receipt of £418 18s. 10d. under the heading ‘Bressa prize’ on 17 January 1880 (CD’s Account books—banking account (Down House MS)).

⁴ See letter from Anton Dohrn, 21 February 1880.

To George Bentham 16 February 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Febr 16th 1880

My dear M^r Bentham

I have had real pleasure in signing Dyer’s certificate.—¹ It was very kind in you to write to me about the Orchideæ, for it has pleased me to an extreme degree that I could have been of the **least** use to you about the nature of the parts.² They are wonderful creations these orchids, & I sometimes think with a glow of pleasure, when I remember making out some little point in their method of fertilisation.—

With respect to terms no doubt you will be able to improve them greatly, for I knew nothing about the terms as used in other groups of plants.— Could you not invent some quite new term from gland, implying viscosity? or append some word to gland,— I used for Cirripeds “cement-gland”.—³

Your present work must be frightfully difficult: I looked at a few dried flowers & could make neither heads nor tails of them; & I well remember wondering what you would do with them when you come to the group in the *Genera Plantarum*.—⁴

I heartily wish you safe through your work, & I remain in a very conceited frame of mind, My dear M^r Bentham, Yours very sincerely | Charles Darwin

Royal Botanic Gardens, Kew (Bentham Correspondence, Vol. 3, Daintree–Dyer, 1830–1884, GEB/1/3: f. 722)

¹ CD had signed the certificate proposing William Turner Thiselton-Dyer for election as a fellow of the Royal Society of London (see letter from George Bentham, 15 February 1880 and n. 1).

² Bentham was preparing the section on orchids for *Genera plantarum* (Bentham and Hooker 1862–83, 3: 460–636). He had praised *Orchids*, and in particular, CD’s clarification of some terms related to orchid morphology (see letter from George Bentham, 15 February 1880 and nn. 2, 4, and 5).

³ See letter from George Bentham, 15 February 1880 and n. 5. In *Living Cirripedia* (1851), pp. 33–4, CD described following the ducts that carried the substance by which barnacles affix themselves to a substrate back to two small organs, which he called cement-glands.

⁴ Taxonomists often relied on dried herbarium specimens alone to describe plants. CD found working from dried flowers very difficult and once told Asa Gray, 'It is dreadful work making out anything about dried flowers; I never look at one without feeling profound pity for all botanists, but I suppose you are used to it like eels to be skinned alive' (*Correspondence* vol. 25, letter to Asa Gray, 8 March 1877).

From Henry Faulds 16 February 1880

Tsukiji Hospital, Tokio | Japan
Feb'y 16th 1880

Sir,

As an ardent student of your writings I trust I may venture to address you on a subject of interest. I allude to the rugae or furrows on the palmar surface of the human hand. These form singular and intricate patterns which vary in detail with each individual but may be classed according to their leading lines without much difficulty— Now, I have been led to study these in prehistoric pottery & I am engaged in what proves a somewhat difficult task—collecting data from all quarters of such ancient impressions & comparing them with similar impressions of living men of all races— Already I see some glimpses of light but *facts* are hard to get in sufficient numbers.¹ The few monkeys &c which I have got show similar but *somewhat* different markings & if man's origin has been from organised 'dust' perhaps a comparative study of lemuroids &c may yield results of real value— I hope for this & have bethought myself of your powerful aid— A word or two would set observers working everywhere.

I enclose a filled up form & can send a number to any one who wishes them.

The practical value,—and Englishmen will look for that,—is in the work of Identification in medico legal studies, thus.

1. Copies of palmar impressions of convicted criminals—as photographs now are used— the latter become unlike the original, the rugae, never

2. In cases like that of 'Tichborne?' Are his rugae of the Tichborne or Orton type?—for *heredity rules here* marvellously.²

3. In cases where mutilated remains are found & various people are missing The rugae again may be compared with that of parents &c

4. Where impressions exist of bloody fingers by a murderer, or prints on fresh paint or drinking glasses, windows &c by a robber—

&c

The Chinese, I find since entering on this enquiry, take impressions of this kind from criminals, as our servant girls used to seal their letters with their *thumbs*. (The Japanese as the ancient Egyptians, use their *finger nail*.)

Hoping you may find this to be of interest

I remain | Your obliged reader | Henry Faulds | Surgeon Supt. of Hospital

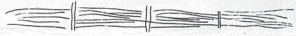
[Enclosure]³

UNIVERSITY COLLEGE
GALTON
PAPERS
MOUNT LOUIS

COUNTRY Japan SEX M

RACE of Father Jap.
Mother Do.

COLOUR of Eyes dark Brown

Specimen of HAIR to be attached here 

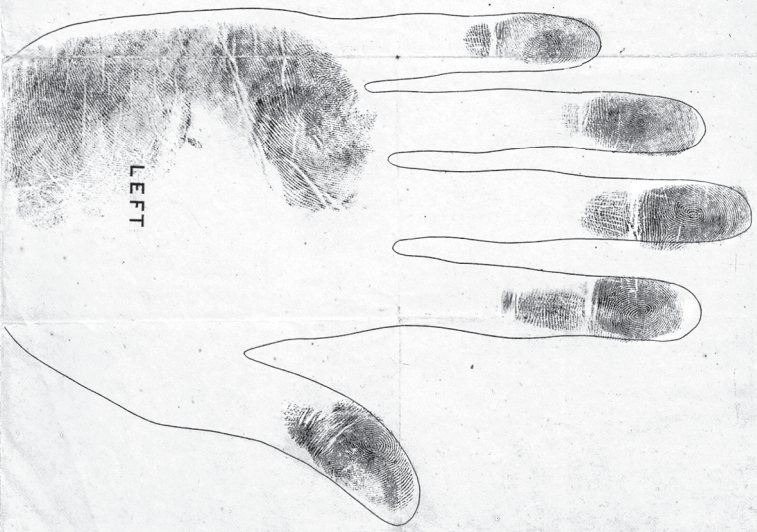
Remarks

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UCL Library Services, Special Collections (GALTON/3/2/1/22, 23)

CD ANNOTATION

Top of letter: 'Not to be returned' ink

¹ The pottery examined by Faulds came from a collection uncovered by Edward Sylvester Morse at the Ōmori archaeological site in Tokyo (Rubinger 2007, p. 138). Faulds published a more detailed letter on skin-furrows of the hand in *Nature*, 28 October 1880, p. 605. For more on Faulds's experiments with fingerprinting, see McDermid 2014, pp. 118–19.

- ² The Tichborne claimant, in a series of dramatic legal cases, claimed to be the rightful heir to the Tichborne estates; he was identified by some as Arthur Orton (b. 1834), although most of the Orton family denied he was their relative. (*ODNB* s.v. Tichborne claimant.)
- ³ The images are reproduced at 55 per cent of their original size.

From W. H. Flower to Francis Darwin 16 February 1880

Royal College of Surgeons of England. | Lincoln's Inn Field, | (W.C.)
16th day of Feb. 1880

My dear Darwin

I send by this post addressed to your father a copy of the Catalogue of Human osteology of which I was speaking, which he might like just to spend a few minutes in looking at, but not more¹—and Hunter's Memoranda on Vegetation, which may interest you, although I find that the experiments on the growth of beans during rotation are not recorded there, but in the 5th volume of the Catalogue of the 'Physiological Series of Comparative Anatomy' p. 10—of which I have not a copy to spare, but you will find it in most of the libraries.² They are also to be found in Hunter's work "on the Blood".³

We enjoyed our visit to Down exceedingly, and I was particularly glad to find your father so well.⁴

Please give our united kind regards to him and to your mother and many thanks for the pleasure they have given us

Believe me | Yours very truly | W. H. Flower

DAR 164: 142

- ¹ CD's copy of the first part of Flower's *Catalogue of the specimens illustrating the osteology and dentition of vertebrated animals, recent and extinct, contained in the museum of the Royal College of Surgeons of England* (Flower 1879), on human osteology, is in the Darwin Library—Down.
- ² Flower sent the 1860 edition of John Hunter's *Memoranda on vegetation* (Hunter 1860), which had been copied from a manuscript notebook of Hunter's loaned to the Royal College of Surgeons (*ibid.*, p. iii). The copy in the Darwin Library—CUL has annotations by both CD and Francis Darwin. The fifth volume of the *Descriptive and illustrated catalogue of the physiological series of comparative anatomy contained in the museum of the Royal College of Surgeons in London* (Royal College of Surgeons 1833–40) was subtitled 'Products of generation' and began with a section on plants. Hunter's experiments with beans are described in *ibid.* 5: 11–12.
- ³ The description of the bean experiments in Hunter's *Treatise on the blood, inflammation, and gun-shot wounds* (Hunter 1794, pp. 237–8) is the same as that in Royal College of Surgeons 1833–40 (see n. 2, above).
- ⁴ According to Emma Darwin's diary (DAR 242), Flower and his wife, Georgiana Rosetta Flower, visited Down from 14 to 16 February 1880.

From Ernst Krause¹ 16 February 1880

Berlin N.O. Friedenstrase 10. II.
den 16.2.1880.

Hochverehrter Herr!

Ich schreibe heut nur ein Paar Zeilen, um Ihnen für Ihre freundlichen Mittheilungen herzlichst zu danken und Ihnen zu sagen, dass ich Ihrem Rathe

folgen und auf jegliche Antwort an Herrn Butler verzichten werde.² Es ist gewiss das Richtige, Angreifern, die eine solche mala fides³ verrathen, wie die Benutzung Ihres Briefes, und seine verschiedenen Verdächtigungen enthalten, gar nicht zu antworten. Ihm selbst ist das gewiss viel unangenehmer, als wenn er irgend eine Antwort erh(iel)te, die ihm erlaubte, den Streit fortzuspinnen.

In herzlicher Ergebenheit, hochgeehrter Herr, | Ihr | dankbarer | Ernst Krause

DAR 92: B56

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

² See letters to Ernst Krause, 9 February 1880 and 13 February 1880. Krause had considered responding to Samuel Butler's letter to the *Athenaeum* (see letter from Ernst Krause, 6 February 1880; for Butler's letter, see the letter to H. E. Litchfield, 1 February [1880], enclosure 1).

³ *Mala fides*: bad faith (Latin).

From S. B. J. Skertchly 16 February [1880]

Handed in at the Edingburgh Office at 5.26 .M., Received here at 6.35 .M.

From | SBJ Skertchly | Edingburgh | *To* | Charles Darwin | Down | Bromley Kent

will you Kindly Telegraph testimonial as to my Cientific attainments especially in natural History am Candidate for chair of Biology Inotage and things have to be settled forthwith you can judge by my books of my fitness will pay all expences please be sure and reply¹

Telegram

Date stamp: FE 16 80

DAR 202: 121

¹ The institution to which Skertchly applied was probably the University of Otago in New Zealand, where the chair in biology was filled in early 1880 (Morrell 1969, p. 61). He was an assistant geologist with the British Geological Survey at this time, but retired from that post in 1881 (Flett 1937, p. 258).

To [Otago University] [16 February 1880]¹

From my knowledge of M^r Skertchleys works, I consider him a very able observer, & well fitted to fill any post such as he now seeks.² I have been much str by the ability shown in his books.

ADraft

DAR 202: 43

¹ The date is established by and the addressee conjectured from the relationship between this letter and the telegram from S. B. J. Skertchly, 16 February [1880].

² See telegram from S. B. J. Skertchly, 16 February [1880] and n. 1.

To W. H. Flower [after 16 February 1880]¹

I am much obliged for the books. I see that they are marked from the Council & if it is proper will you be [so kind/] return my sincere thanks to the C for its gift.—² I shall be glad to read about the skulls, & I daresay you will convert me from my previous state of heresy.—³ Frank & I will both be glad to read [about/] the [curiosities/] Hunter says on plants.—⁴ We thoroughly enjoyed your visit here.—⁵

Yours very sincerely | C. D.

ADraft

DAR 202: 19v

¹ The date is established by the relationship between this letter and the letter from W. H. Flower to Francis Darwin, 16 February 1880.

² Flower had sent Flower 1879 to CD and Hunter 1860 to both Francis and CD (see letter from W. H. Flower to Francis Darwin, 16 February 1880 and nn. 1 and 2). The council was that of the Royal College of Surgeons, where Flower was professor of comparative anatomy and curator of the Hunterian Museum.

³ CD refers to Flower's work on human osteology (Flower 1879) and probably alludes to some remarks he made on the subject while Flower was visiting (see n. 5, below).

⁴ John Hunter's *Memoranda on vegetation* (Hunter 1860) was a collection of notes made by Hunter that were later transcribed from a manuscript loaned to the Royal College of Surgeons (see letter from W. H. Flower to Francis Darwin, 16 February 1880 and n. 2).

⁵ Flower and his wife, Georgiana Rosetta Flower, visited Down from 14 to 16 February 1880 (Emma Darwin's diary (DAR 242)).

To William Cole 17 February 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Feb 17/80

Dear Sir,

I am much obliged for your courteous letter in which you inform me that the members of the Epping Field Club have elected me an honorary member.¹

I request that you will be so good as to express to the club my sincere thanks for this honour; & with every wish for its success & long life, I remain, | dear Sir, | Yours faithfully | Charles Darwin

LS

Imperial College of Science, Medicine and Technology Archives (Essex Naturalists Field Club MLDA/10)

¹ See letter from William Cole, 14 February 1880.

To Asa Gray 17 February 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Feb 17/80

My dear Gray,

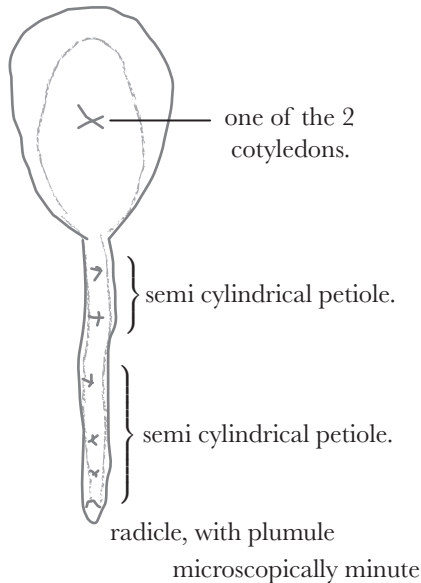
If my letter opened your eyes, yours has opened mine much wider. It is very strange that plants, if they belong to the same species, should behave so differently.¹

The seeds were laid on the surface or buried in a mixture of peat sand & common soil, & this may have yielded more easily than your soil. From the extraordinary intermission in the growth of the true radicle, from the root-hairs & from the petiole staining brown with permanganate of potash I must believe that the normal function is to bury itself.²

My plants are growing very vigorously. Should they flower, I will send some dried with leaves, for the chance of your being able to name them.— I am astounded at the whole case.— I suppose when the petioles grow in the air they are stiffer than when hypogæan, for mine could not support the weight of the cotyledons.³ One seed germinated abnormally; *one* alone of the 2 cotyledons emitted its petiole, which was a hollow $\frac{1}{2}$ cylinder, as in sketch with mere rudiment of true radicle, with a minute plumule.

Ever my dear Gray | Yours very sincerely | Ch. Darwin

P.S. Some of the seeds received last were a little flattened & evidently different; they were sown separately, but not one germinated.⁴



Archives of the Gray Herbarium, Harvard University (128)

¹ See letter from Asa Gray, 3 February 1880 and n. 1. CD's description in his letter to Gray of 19 January 1880 of the germination of seeds of an unnamed species of *Megarhiza* had surprised Gray.

² Gray had reported that at least two inches of the seeming radicle was above ground after germination (see letter from Asa Gray, 3 February 1880 and n. 2). CD had described his observations on the intermission of growth in the true radicle and argued that the tubular fused petioles acted functionally like a root (letter to Asa Gray, 19 January 1880).

³ CD presumably meant that when he removed the petioles from the ground they could not support the weight of the cotyledons (seed leaves).

⁴ Gray had sent seeds of *Megarrhiza* in December 1879 (see *Correspondence* vol. 27, letter to J. D. Hooker, 19 December [1879]).

From A. S. Wilson 17 February 1880

North Kinnmundy, | Summerhill, | by Aberdeen.

17 Feby. 1880.

Charles Darwin Esq. F.R.S. | Down | Beckenham | Kent.

My Dear Sir,

I received your letter and feel very much gratified that you think my conclusions warranted in regard to the Russian wheats. I intend to give them another trial.¹

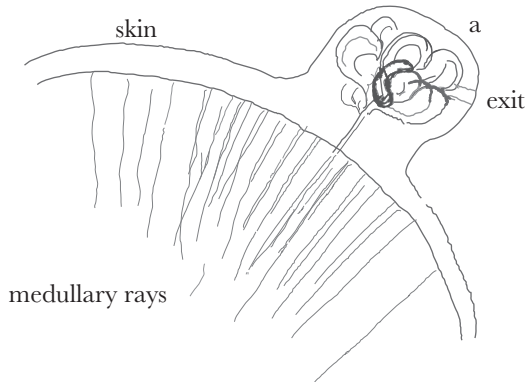
In regard to the question you ask, I have never seen the silver fir affected with fungus in the way you describe; but I have examined to some extent the knobs on the roots of several of the Crucifers—turnips, swedes, mustard, cabbage, charlock &c. I have a paper in hand on the *Plasmodioph(ora) brassicae* or cl() fungus lately discovered by Woronin, and in preparing for this, I have found that on the turnip root for example, there are knobs or clubs arising proximately from three causes.²

First there are round knobs which Woronin holds to be caused by his fungus, but in which I can find none. These nodes are in reality tuberculated buds. They are of this form:—



And they are not only capable of throwing out leaves but also a stem, and ripening seed, as I have found during last season, in the case of swedes. A stem may arise at a node *a* and reach a height of two or three feet. These tubercles have a certain analogy to the potato. The fibrous roots which they throw out have nothing peculiar (abo)ut them—

⟨Se⟩cond there are the nodes or galls produced by one or more beetles for the nursing of their larvae.³ They arise thus:—

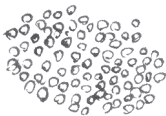


And these are also tuberculated buds. A section shows that in some way, not known to me, the piercing of the skin of the turnip and the deposit of an egg, sets free one or more medullary rays which push out the skin by the growth of a true bud the fleshy leaves of which are very obvious and sometimes even green. It is upon the bud that the young larva feeds— These nodes, I have not observed to throw out any roots—

Thirdly we have the true Club-root or Finger-and-Toe knobs or clubs, ⟨ ⟩ have many specimens of these o⟨ ⟩ of one hundred(th) of an inch th⟨ick⟩ and upwards. These take oblong and swelling curves thus:—



These are not buds; these alone, as I think are produced by the clubroot fungus; the main feature of which is the formation from a granular plasma of enormous

shoals of globular spores thus:  the diameter of which I make

.0044 mm. but which Woronin makes only about one third as large— These clubs do not throw out roots; they are upon the roots already thrown out.

Now here is a point for your consideration. Did this fungus and these beetles come by experience to know that cruciferous roots had a natural tendency to that tuberculation which ()ould be advantageous to their propa(gatio)n?

I am | yours very sincerely | A Stephen Wilson.

DAR 181: 116

- ¹ See letter to A. S. Wilson, 13 February 1880 and n. 1. Wilson viewed the apparent transformation of one wheat variety into another as an artefact of the different productivity of the two varieties.
- ² Mikhail Stepanovich Voronin had identified the club-root fungus in his paper 'Plasmodiophora brassicae организм, причиняющий капустным растениям болезнь, известную под названием "кила"' (*Plasmodiophora brassicae* the organism responsible for the sickness in the cabbage plant, known by the name 'club-root'; Voronin 1877). Turnip is *Brassica rapa* subsp. *rapa*; swede is *B. napus* var. *napobrassica*. Various species of the genera *Brassica* and *Sinapis* are referred to as mustard; charlock is *S. arvensis*. Cabbage is *Brassica oleracea*.
- ³ The larvae of some flea-beetles (genus *Phyllotreta*) cause root damage in cabbage.

From J.-H. Fabre¹ 18 February 1880

Monsieur

Votre lettre a été pour moi tout à la fois une source de satisfaction et une source de regret.²

De Satisfaction, car elle m'a appris qu'au milieu du l'obscurité où m'avait plongé le texte erroné dont je disposais, la rigueur de la logique et la précision de l'expérience m'avaient néanmoins conduit juste au tracés de petit drame observé par le savant Erasme Darwin.³

De regret, car elle m'a montré en quelle erreur j'étais en suivant l'auteur que je cite, l'entomologiste Lacordaire, qui a designé par le mot Sphex, la Guêpe réellement observée.⁴ La faute remonte plus haut que votre humble serviteur, aussi j'espère que vous voudriez bien excuser mon inadvertance.

On me parle d'une traduction Anglaise. Si cette traduction se fait, le passage erroné sera modifié en conséquence.⁵ Toutefois la conclusion n'en restera pas moins la même. Il m'est impossible de voir le moindre indice de raison dans le fait d'un insecte qui détourné du travail instinctif qu'il fait pour préparer le manger de ses larves, effrayé peut-être, dérangé en son oeuvre, remet pied à terre pour achever le travail interrompu.⁶

Mon impartialité pour l'homme est encore plus vive que pour l'insecte, je négligerai donc rien, soyez en persuadé, pour réparer l'erreur commise.

Vous vous étonnez de mon peu de goût pour les théories si réduisantes qu'elles soient. Ce travers d'esprit, si c'en est un, tient un peu à mes longues études mathématiques qui m'ont habitué à ne reconnaître la vérité qu'à la lueur d'un irrisistible faisceau de lumière.

Ne jurant pas aucun maître, libre d'idées préconçues, pas enclin aux séductions des théories, je cherche avec passion la vérité, prêt à admettre quelle qu'elle soit et de quelque part qu'elle vienne. Et comme moyen de recherche, je ne connais qu'une chose: l'expérience.

J'ai déjà préparé pour le mois de Mai prochain les matériaux pour l'expérimentation que vous me proposer au sujet du Insectes retournant à leurs nids. L'idée de la boîte qui tourne rapidement dans un sens puis dans l'autre, pour désorienter les insectes, me paraît fort ingénieuse, et je ne saurais trop vous remercier de me l'avoir suggérée. En temps propice, au mois de Mai prochain, je la mettrai en application; et si vous voulez bien me le permettre, je vous tiendrai au courant de mon résultats. Si je peux me procurer les appareils nécessaires, j'essaierai aussi les courants d'induction.⁷

Parmi la population de paysans de mon village, l'habitude est de faire tourner dans un sac le chat que l'on se propose de porter ailleurs, et dont on voit empêcher le retour. J'ignore si cette pratique obtient du succès. Je l'expérimenterai à mon tour. J'ai été très frappé de voir si bien concorder l'opération que vous me proposez au sujet des insectes et la pratique des gens de la campagne dans mes environs. Cette pratique a-t-elle aussi cours en Angleterre, au sujet du chat bien entendu? Je serais bien désireux de le savoir

Je suis avec le plus profond respect | Monsieur | Votre très humble serviteur
| J. H. Fabre

Sérignan (par Orange) | Vaucluse | 18 Fev. 1880.

DAR 164: 2

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

² See letter to J.-H. Fabre, 31 January 1880.

³ In his letter to Fabre of 31 January 1880, CD had pointed out an error in Fabre's work, *Souvenirs entomologiques* (Fabre 1879, p. 124), that stemmed from a mistranslation of a name in Erasmus Darwin's *Zoonomia*; Darwin had described a wasp removing the wings of a fly (E. Darwin 1794–6, 1: 183; see also n. 4, below).

⁴ *Sphex* is the genus of digger wasps; these wasps, unlike the common wasp that Erasmus Darwin observed (probably *Vespa vulgaris*), paralyse prey but leave them alive in the nest for larvae to feed on. Common wasps, on the other hand, kill prey and have powerful mandibles for chewing (malaxation); larvae are fed the masticated prey. Jean Théodore Lacordaire had discussed the case in his *Introduction à l'entomologie* (Lacordaire 1834–8, 2: 460–1).

⁵ No translation of Fabre 1879 was made during CD's lifetime; a translation appeared in 1913 (Fabre 1913). The original text was translated, but the error about the wasp was explained in a note (*ibid.*, p. 123).

⁶ See letter to J.-H. Fabre, 31 January 1880 and n. 2.

⁷ See letter to J.-H. Fabre, 31 January 1880 and n. 5.

To J.-H. Fabre 20 February 1880

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

Feb 20/80

Dear Sir,

I thank you for your kind letter and am delighted that you will try the experiment of rotation.¹ It is a very curious that such a belief should be held about cats in your country. I never heard of anything of the kind in England.² I was led, as I believe, to think of the experiment from having read in Wrangel's Travels in Siberia of the wonderful power which the Samoyedes possess of keeping their direction in a fog

whilst travelling in a tortuous line through broken ice.³ With respect to the cats, I have seen an account that in Belgium there is a society which gives prizes to the cat which can soonest find its way home, & for this purpose they are carried to distant parts of the city—⁴ Here would be a capital opportunity for trying rotation.

I am extremely glad to hear that your book will probably be translated into English.⁵

With much respect | Dear Sir | Yours faithfully | Charles Darwin

P.S. | I shall be much pleased to hear the result of your experiments

LS

Muséum national d'histoire naturelle, Bibliothèque centrale, Paris (Ms FAB 32)

¹ See letter from J.-H. Fabre, 18 February 1880. The experiment was designed to test the homing instinct of insects.

² Fabre had described a practice common among local people of turning a cat round in a bag to disorient it so it would not return to a previous location (letter from J.-H. Fabre, 18 February 1880).

³ Ferdinand Petrovich Wrangel had written an account of his exploration of Polar regions (Wrangel 1840) that included several accounts of the dense fog and the difficulties of navigating in it. Samoyedes (now more commonly Samoyedic people) are closely related ethnic groups of traditionally nomadic people of northern Siberia.

⁴ The Belgian practice of cat racing was described in an article in the *Pictorial Times*, 16 June 1860; the article was reprinted in a book on cats (Weir 1889, p. 218). According to the article the prizes were a ham and a silver spoon to the owner whose cat found its way home soonest.

⁵ In the event, an English translation of Fabre's *Souvenirs entomologiques* (Fabre 1879) appeared in 1913 (Fabre 1913).

To A. S. Wilson 20 February 1880

Down
Feb: 20, 1880

My dear Sir

I thank you for your letter which has interested me much; though alas! it does not aid me about roots growing in an abnormal direction when growing from a part abnormally developed.¹ Your investigation seems to have been made with great care

I can throw no light on your problem.² In such cases I have imagined some such steps as the following. That the parent species laid its eggs in plants belonging to various families, perhaps allied ones, at least with tissues of the proper texture, not poisonous &c,— that a fluid accompanied the deposition of the eggs and that this caused a slight abnormal growth of the tissues in certain families alone, and that such growth was advantageous to the larvæ,— that these larvæ inherited the taste of their parents which led the latter to select the particular plant in question,— and lastly that the fluid accompanying the eggs was increased in quantity or in intensity from being beneficial to the insect, until regular galls were formed. But all this is mere idle speculation.

I remain, with many thanks | Yours faithfully | Ch. Darwin

Copy

DAR 148: 371

- ¹ See letter from A. S. Wilson, 17 February 1880. Wilson had written in response to CD's query about whether growths on the roots of Cruciferae caused by a fungus produced rootlets, and if so, in which direction they grew (letter to A. S. Wilson, 13 February 1880). CD had observed that shoots from growths on branches of silver fir grew vertically, in contrast to horizontally growing shoots on normal branches.
- ² Wilson had described tubercles on turnips caused by fungi and beetles, and speculated whether the fungi and beetles learned by experience that cruciferous roots had a tendency to tuberculation.

To G. H. Darwin 21 [February 1880]

From Mr. C. Darwin, Down, Beckenham

See 'Nature' p. 382 about the microphone & earthquakes & tremblings in Japan.—¹

C. D.

21st—

ApcS

Postmark: FE () 80

DAR 210.1: 91

- ¹ *Nature*, 19 February 1880, pp. 382–3, reported on a method for detecting seismic trembling in Japan through the use of microphones buried in pits; the method was described by its inventor, John Milne, a professor of mining and geology at the Imperial College of Engineering, Tokyo.

From Anton Dohrn 21 February 1880

Stazione Zoologica | di | Napoli

21. February 1880.

Dear Mr. Darwin

I hasten to thank you most cordially for your generous offer.¹ It is at once the greatest honour that possibly could happen to the Zoological Station, and it will be of very great service to the very ends, the Station has to promote.

In accepting your great gift I venture to propose a little change in its employment. Of course there are many, many things still wanting in the Laboratory, Library etc.; but as time goes on, and if I succeed in getting the Subvention of the German Empire a lasting one (an event, which I anticipate to see soon accomplished) I may be in 2 or 3 years quite able to endow the Zoolog. Station with a small chemical and physiological Laboratory, to promote physiological Research on marine Animals, and bring Physiology and Morphology into closer material and personal union.²

But there is another serious want, which might in part be filled up by your gift. The English Naturalists, when coming to work in the Zool. Station do not enjoy the same privileges, which most of the other Naturalists enjoy: to be assisted by either a government—or another travelling fund for paying travelling and other expenses. Of course not all do want such assistance, but I know some Naturalists would have

asked for either the Cambridge-Table or the one taken by the Brit. Association, if they had had some assistance in the said way.³

Now I believe if you would consent to grant a sum for founding such a fund, it might easily lead to similar gifts; I know even it would, and thus English Naturalists might be enabled to come more frequently to Naples than has hitherto been the case,—an event which would give me the greatest satisfaction, the Zoological Station and I myself owing so much to English help, sympathy and encouragement. Besides there is always one danger to be taken in view: the Zoolog. Station becoming more and more exclusively German;—I wish to do all in my power to prevent that, and it would be of great help towards maintaining the International character of the Institution, if England continued to have a greater share in its working.

I venture to submit this proposition to your consideration and approval, adding once more my sincerest thanks for your generosity and asking your permission to speak of your grant publicly, as it will show and prove your acknowledgement of the scientific usefulness of the Zool. Station.—

This letter remained a day on my desk. Meanwhile Prof. Allen Thomson has arrived to visit Naples and to see the Station.⁴ I hope he is satisfied with its general arrangement, he will take the trouble of inspecting it still more; and told me, that it wanted to be made much more known in England.

Another good notice I am able to add. Prof. Du Bois-Reymond writes to me about a conversation which he had with one of the chief men in the Foreign Office at Berlin, who told him, that the German Government is ready to ask already now the transfer of the new Subvention upon the ordinary budget of the Empire; thus I shall soon hear about the definite result.⁵

The Naturalists at the Zool. Station join me in my thanks for your gift, and want me to express their renewed wishes for your good health and welfare.

With my own sincerest regards and kindest wishes | I remain | dear M^r. Darwin
| Yours most truly | Anton Dohrn

DAR 162: 219

¹ See letter to Anton Dohrn, 15 February 1880. CD had offered to donate £100 for equipment for the Naples Zoological Station.

² On Dohrn's attempts to get a regular grant from the Imperial German government, see the letter from Anton Dohrn, 11 February 1880 and n. 2. The next addition to the station was a building housing individual laboratories and a department for collecting and preserving organisms, completed in 1886; a laboratory for comparative physiology was added in 1906 (Edwards 1910, p. 213).

³ In 1879, the British Association for the Advancement of Science had rejected a petition to give money to English scientists for travelling to Naples, due to lack of funds (Heuss 1991, p. 211). The University of Cambridge and the British Association rented tables at the Zoological Station (see letter from Anton Dohrn, 11 February 1880 and n. 6).

⁴ Allen Thomson was a retired professor of anatomy, known for his anatomical and embryological research (*ODNB*). He was a former president (1877) and member of the council of the British Association for the Advancement of Science.

⁵ Emil du Bois-Reymond had signed a petition to the German parliament in March 1879, asking for a subsidy for the Zoological Station; although the government donated money, it did not make the payment a regular part of the budget (Heuss 1991, pp. 196–7).

To A. Gapitche 24 February 1880

Down,
Feb. 24th, 1880.

I suppose that no one can prove that death is inevitable, but the evidence in favour of this belief is overwhelmingly strong from the evidence of all other living creatures.¹ I do not believe that it is by any means invariably true that the higher organisms always live longer than the lower ones. Elephants, parrots, ravens, tortoises and some fish live longer than man. As evolution depends on a long succession of generations, which implies death, it seems to me in the highest degree improbable that man should cease to follow the general law of evolution, and this would follow if he were to be immortal.

This is all that I can say.

Incomplete

ML 2: 444–5

¹ According to Francis Darwin (*ML* 2: 444), this letter was written to the author of a pamphlet, *Quelques mots sur l'éternité du corps humaine* (A few words on the immortality of the human body; Gapitche 1880). The text was provided by Benjamin Vetter, the editor of *Kōsmos* from October 1882. Francis wrote that the author's name was a pseudonym; no further information on the identity of the author has been found. When the letter was published in German translation in *Kōsmos* 15 (1884): 80, the author was identified as A. Panchin of Kiev, who has also not been identified. CD's copy of the pamphlet is in the Darwin Pamphlet Collection–CUL.

To J. W. C. Fegan [before 25 February 1880]¹

Dear Mr. Fegan,

You ought not to have to write to me for permission to use the Reading Room.² You have far more right to it than we have, for your services have done more for the village in a few months than all our efforts for many years. We have never been able to reclaim a drunkard, but through your services I do not know that there is a drunkard left in the village.

Now may I have the pleasure of handing the Reading Room over to you? Perhaps, if we should want it some night for a special purpose, you will be good enough to let us use it.

Yours sincerely, | Charles Darwin.

Fullerton 1930, p. 30

¹ The date is established by the relationship between this letter and letters from Emma Darwin to H. E. Litchfield, 25 February 1880 (DAR 219.9: 222) and [8 March 1880] (DAR 219.9: 225). In the first of these Emma Darwin wrote, 'The maids went to hear Mr Fegan last night & were delighted with him— The room quite full— There are to be regular meetings every Tuesday interspersed with tea drinkings.' In the second letter, Emma noted, 'Tuesday is the day of Mr Fegan's lecture at the R. room'.

² The letter from Fegan has not been found. The Reading Room had been opened in 1875 (*Correspondence* vol. 23, letter from Emma Darwin to J. B. Innes, 24 December [1875]).

From E. A. Darwin [26? February 1880]¹

Of course Mr. Salt² knows where the money is to go, but I confess I don't
EAD.

[Enclosure]

Shrewsbury
February 25th 1880.

My dear Sir—

I have this morning received a letter saying that the inclosed Road bonds will be paid off on *Saturday* next.— I shall be much obliged if you will sign the receipt *on each* across the stamp where your initials are, & then forward them by the next post to M^r: Charles Darwin that he may sign & return them to me by the following post.—³ The notice they have given me is so short that there is only just time to do it.— Please forward this letter with them to M^r: Charles Darwin as it will explain the reason for the hurry.— I enclose a stamped envelope for transmission of the papers to him, & another in which he can return them to me.—

I am, respectfully, | Your obliged & faithful serv^t | Geo. M. Salt.

E. A. Darwin Esq^{re}

P.S. The composition is better than I expected, most of the roads paying much less.—

DAR 177: 9

¹ The date is conjectured from the date of the enclosure.

² George Moultrie Salt was the Darwin family's solicitor in Shrewsbury.

³ The enclosures have not been found. No record of road bonds has been identified in CD's Investment book (Down House MS). 'Saturday next' was 28 February 1880.

To Anton Dohrn 27 February 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Feb 27 1880

My dear D^r Dohrn,

Your very kind letter has pleased me much.¹ You are so incomparably a better judge how best to use my small gift of £100 that I should greatly prefer leaving it entirely to your decision.² As the sum is small it appears to me that it would be prudent in you not to speak of the use to which you propose to put it until you are

assured of receiving considerable additions You are at perfect liberty to use my name on this subject as you may think fit, and I hope that your generous wish to assist poor English naturalists may be successful. I enclose a cheque but should there be any difficulty in cashing it I could find some other means of sending the sum to you & I repeat that I beg you to do whatever you think best with it.³

I remain, with all good wishes | Yours very sincerely | Charles Darwin

LS

Bayerische Staatsbibliothek München (Ana 525. Ba 705)

¹ See letter from Anton Dohrn, 21 February 1880.

² CD had suggested using the money to buy a piece of equipment (see letter to Anton Dohrn, 15 February 1880). In his reply Dohrn suggested using the money for travelling expenses for English naturalists (see letter from Anton Dohrn, 21 February 1880 and n. 3).

³ CD recorded a payment of £100 under the heading 'Dohrn' on 27 February 1880 in his Account books—banking account (Down House MS).

From H. W. Crosskey 28 February 1880

Birmingham Philosophical Society. | 28 George Road | Birmingham.
Feb^{ry} 28/1880

My dear Sir,

I have been instructed to forward to you, on behalf of the President, Council, & members of the Birmingham Philosophical Society, an Address, respectfully offering to you the first Honorary membership which the Society has conferred; & congratulating you on your 71st Birthday.¹ This address was adopted at a meeting of the members held on February 12th; & has since been in the hands of an Engrosser. It is now reported however to be duly prepared—; & in the course of next week will I trust reach your hands.²

The formal record of the Proceedings runs as follows— At a meeting of the members of this Society, held on February 12th 1880, the President (D^r T. P. Heslop) in the chair It was moved by the Rev. H. W. Watson³ m.a (vicar of Berkswell) Seconded by the Rev. H. W. Crosskey V.G.S. (Minister of the Church of the Messiah Birmingham) and unanimously resolved That D^r Charles Darwin m.a. F.R.S. &c. be elected an Honorary member of this Society.

It was further unanimously resolved, on the motion of the President (D^r T. P. Heslop) seconded by Mr. Lawson Tait

That the address to D^r. Darwin, submitted by the Council, be adopted by the meeting.

As soon as you have received the address (—which is now daily expected from the Engrosser—) I shall hope to hear that you will comply with the request of the Society—a request made, I am bound to add, with a unanimity & heartiness which c^d not be exceeded.

Believe me | very respectfully | Henry W. Crosskey | Sec.

To | Charles Darwin Esq | m.a. L.L.D. F.R.S. | &c.

[Enclosure]

Birmingham Philosophical Society.

*Address to Dr. Darwin; submitted by the | Council for the consideration of the | Meeting,
Feb. 12th, 1880*

TO CHARLES DARWIN, M.A., LL.D., F.R.S., &C.

The President, Council, and Members of the Birmingham Philosophical Society, assembled on this, the 12th day of February, 1880, your 71st birthday, respectfully and unanimously offer to you the first Honorary Membership which the Society has conferred.

They desire also to present their hearty congratulations, and to express their earnest hope that you may long be spared to continue those researches which have so widely extended the boundaries of human knowledge, and so profoundly influenced modern scientific thought.

Few men are permitted to form by their works Epochs in the history of the world; but the appearance of the "Origin of Species," followed as it has been by numerous other publications illustrating its doctrines and extending their application, has constituted an Epoch as important as any that has yet marked the intellectual development of our race.

Those who may refuse their assent to the philosophical principles enunciated in your works, must admit, equally with those who accept them, that there are few realms of thought into which their influence has not travelled; while there is no branch of Natural History, and hardly a problem connected with the position of man himself upon the earth, which has not had new light cast upon it, by the investigations called forth by your genius.

The Members of the Society are conscious that, in offering you this Honorary Membership, they are asking you to confer a distinction upon them; but they feel that such a tribute of respect as they now desire to pay may not inappropriately come from the Town which is the centre of that district with which your family have so long, and with so much honour been associated.

Signed on behalf of the Philosophical Society of Birmingham

*President.
Vice-Presidents.
Treasurer.
Secretaries.*

OTHER MEMBERS OF COUNCIL:

This address is being engrossed & will be duly forwarded with the signatures attached

DAR 160: 187

CD ANNOTATION

Back of second page: 'P.S I beg leave to thank you sincerely for your courteous letter of Feb. 28th ink

¹ CD had already received the text of the address with additional comments by the president that appeared in the *Birmingham Daily Post*, 13 February 1880, p. 5 (see letter to Lawson Tait, 13 February 1880). The president of the society was Thomas Pretious Heslop.

² An engrosser copied documents in large fair characters or in legal style (*OED*). Tait delivered the address to CD in person on 4 March 1880 (Emma Darwin's diary (DAR 242); letter to H. W. Crosskey, [4 March 1880]). The presentation copy of the address has not been found in the Darwin Archive-CUL.

³ Henry William Watson.

⁴ CD's annotation was probably a note for his reply of [4 March 1880], but it was not included in the printed source that is the only existing version of that letter.

From Adolf Ernst 29 February 1880

Caracas,
february 29th 1880

Dear Sir,

Many thanks for your highly encouraging letters.¹ The first I did not answer hitherto, because I expected to find an opportunity to visit once more our plains, where I intended to pay a close attention to the point of your inquiry.² However this opportunity is not yet come: my professional work at the University keeps me month after month in town, and when holidays come, there are examinations, and special commissions, and I do not know what not. When I first saw the plains (in February 1874), it was at the end of the dry season; and though there are undoubtedly a good many plants with glaucous leaves (especially Cyperaceæ and Eriocaulaceæ),³ I do not recollect that the general appearance of the flora struck me as glaucous. I think it would be best to make observations in the middle of the rainy season, where any difference ought to be more striking.

I am very glad that my observation on *Melochia* was of any interest for you, and shall attend to your indications in further experiment.⁴ I think heterostyled flowers are far more numerous than is generally believed. I lately noticed that *Turnera ulmifolia* is in the same case, though there is scarcely any difference in the shape and size of the pollen grains.⁵ I am preparing to make a series of experiments with this species, and shall later report on them.

There is in this *Turnera* another point, which I think is sufficiently interesting to be studied carefully; unfortunately I am not able to do it. The two glands at the base of the leaves are very remarkable organs. Morphologically I believe them to be either modified teeth of the margin, or atrophied lateral leaflets. A vascular bundle enters each of them. The produce a sugary secretion, which appears on their surface as a clear drop, where it is eagerly sucked by ants. It is next to impossible to find one single plant without half a dozen or more of these insects fairly stationed among the rather densely crowded leaves on the upper end of the stems. Besides this secretion of sugar, there are *a great many sphaeraphides of oxalate of lime* in the tissue of the gland. This circumstance appears to me of interest, and suggestive of the question whether there can be a certain relation between both substances. But this point I must leave to persons more versed in organic chemistry than I am. I may be allowed to add, that I never saw an ant, nor indeed, any other insect, in the corollas of *Turnera*.⁶

Just when I was examining the glands of *Turnera*, I received Bonnier's memoir "On Nectaries", publ. in the *Annales de Sciences naturelles*. I must confess that I cannot agree with his conclusion that the physiological work of any kind of nectary is "*en*

*relation directe avec la vie de la plante.*⁷ If the nectaries produce *always* substances *directly* useful for the plant, how is it, I ask, that generally so much, if not the whole of these substances flows away, being thus exposed, as really happens, to be stolen by insects? Every physiological work has of course a certain purpose, but this may be either direct or indirect, and if the first is neither apparent nor demonstrable, it is but natural to look for the second.

I have made observations on two plants, where the *direct* usefulness of similar productions appears to me out of any question. We have a species of Marcgravia, it is probably the *M. acuminata* Miq.⁸ The flowers (about 30) occupy an inch more or less of the branch, whilst the 6 to 8 pitcher-like bracts grow all from the very end of the branch. These pitchers are somewhat of the shape of a Phrygian cap, and about two inches long, their peduncle included (see the sketch). They hang of course perpendicular, whilst the flowers are so placed that nearly all of them are in the same horizontal plane. Now, there is honey in the pitchers up to the day when the floral envelopes drop off, and fragments of them are still adhering. But as soon as these too are falling away, fecundation must be realized, the stamens wither very soon, and the pitchers are honey-less, dry up, and get black, but remain for some time hanging on the branch. Their honey-making function ends therefore with the opening of the flowers and the probably immediate fecundation of the same, so that there remains nothing for further reabsorption.



Another case is that of *Passiflora biflora*.⁹ There is in the middle of each leaflobe, on its underface, a conspicuous gland, which produces a considerable quantity of sweet honey. The secretion is so energetic, that plants which had been one night kept in my *vasculum*, had on all their leaves drops of this substance. It is sought after by hymenopterous insects, as I had several times occasion to see. Of what *direct* use can this secretion be to the plant? The leaves certainly would grow without, nor is the secretion as such useful for the plant, because it must lose by it a large quantity of sugar. It is on the contrary very easy to observe, how the insects in their search for the drops of honey, scramble over flowers on every part of the little creeper, and assist thus in fecundating the stigmata. I have planted some specimens in my little garden, and will try to find out whether there is any fecundation without insects, and whether the plants will grow, when the places of the leaves with the glands are punched out.

Is it known that the fierce little ants, which inhabit the hollow stems and branches of *Triplaris americana*, keep *scale-insects* in their lodgings?¹⁰ They are very abundant, and even in the thinnest branches it is easy to discover them among the brown felt-like substance which covers the inside of the wood.

Abutilon striatum Dicks. introduced here from Perú, as people say, thrives not any longer, and never set fruit.¹¹ I have seen humming-birds attacking the flowers from outside, perforating the calyx, and thus robbing the honey. I have been told that about 40 years ago, Señor Antonio L. Guzman, father of our actual President,¹²

brought *one* plant from Peru, and that from this by cuttings all the other specimens were derived. In the last years all have small leaves with many yellow spots, and flower very poorly. The species is evidently dying out with us.

Some years ago a friend of mine discovered in the mountains towards the south of Caracas the very handsome *Ionidium anomalum* HBK (Nov. Gen. et Sp. V, pl. 500), and brought seeds, which were sown in several gardens. The plants grew everywhere, but none produced ripe seeds. It is evident that insect help was wanted, as might have been expected, the large snow white labellum of the flower looking just like a beautiful landing-place for welcome visitors.¹³ I have lately obtained good seeds by artificial fecundation, but only from two flowers.—

I apologize for this long letter, but hope that you will excuse me, as I am desirous to communicate to you some facts which perhaps are not entirely void of interest. Unfortunately this year I am bound to teach mineralogy, a study which is somewhat out of my line, so that it keeps up nearly all my time. We have here the queer old system of a Professorship of Natural History, so that I must lecture one year on botany, one year on Zoology, and another year on Mineralogy: rather hard work in our times! And this the more so, as with respect to Geology, there is no poorer place on God's earth than the valley of Carácas, entirely built of metamorphic rocks, and showing nothing else but interesting examples of gigantic denudation. Field-work, with me the best thing I use to do with my students, is therefore not very productive. In this connection I take the liberty to make you a request. I have nearly every line you have published, but not your works on Coral Reefs and on the volcanic islands and parts of South America.¹⁴ Would you honour me with a copy of each of them? It is needless to say that I would prize both as tokens of your kindness towards an humble, but sincere, fellow-worker in the extensive fields of your scientific researches.

Allow me to add that Venezuela belongs now to the postal Union, so that simple lettres pay no more postage than 2½ pence, and printed matter a halfpenny for every 50 grammes.¹⁵ I think you had a shilling stamp on your last letter.

I am, dear Sir, very truly yours | A Ernst

DAR 163: 21

CD ANNOTATIONS

1.7 and though ... glaucous. 1.9] *double scored pencil*

2.3 I lately ... grains. 2.5] *double scored pencil*

¹ See letters to Adolf Ernst, 16 January 1878 (*Correspondence* vol. 26), and 5 January 1880.

² CD had asked whether plants growing on interior dry plains were more likely to be glaucous, that is, protected by a waxy or powdery secretion (see *Correspondence* vol. 26, letter to Adolf Ernst, 16 January 1878 and n. 2).

³ Cyperaceae is the family of sedges; Eriocaulaceae is the family of pipewort. These families are widely represented in the Venezuelan llanos (plains) and tepuis (flat-topped mountain summits).

⁴ CD had suggested that Ernst try experiments on the fertility of illegitimate seedlings of *Melochia parvifolia*, a neotropical species of the subfamily Byttnerioideae of the Malvaceae or mallow family (see letter to Adolf Ernst, 5 January 1880 and n. 2). CD mentioned Ernst's discovery of heterostyly in this species in *Forms of flowers* 2d ed., p. v.



Terpinc del. et diract.

JONIDIUM anomalum.

De Empiriarie de Langlois.

Jonidium anomalum.

Humboldt *et al.* 1818–25, 5: pl. 500.

By permission of the Syndics of Cambridge University Library.

- ⁵ *Turnera ulmifolia* is ramgoat dashalong or sage-rose, a member of the Passifloraceae (passion-flower family). Most varieties of this species are heterostyled.
- ⁶ *Turnera ulmifolia* bears foliar nectaries on the petiole of the leaf. Oxalate of lime is now usually referred to as calcium oxalate (CaC₂O₄, the calcium salt of oxalic acid); sphaeraphides are globular clusters of raphides, needle-shaped crystals of calcium oxalate.
- ⁷ Gaston Bonnier's doctoral thesis 'Les nectaires. Étude critique, anatomique et physiologique' (Nectaries. A critical anatomical and physiological study; Bonnier 1879b) was published in *Annales des sciences naturelles (Botanique)* in 1879. For Bonnier's conclusion that the work of the nectary is directly related to the life of the plant ('*en relation direct avec la vie de la plante*'), see *ibid.*, p. 206.
- ⁸ *Marcgravia acuminata* is a synonym of *M. coriacea*.
- ⁹ *Passiflora biflora* is twoflower passion-flower.
- ¹⁰ *Triplaris americana* is a species of knotweed (family Polygonaceae) sometimes known as ant-tree. It has a mutualistic relationship with ants, particularly the species *Pseudomyrmex triplarinus*. Ants of this genus raise scale insects (subfamily Coccoidea) in excavated cavities (domatia) in the stem and branches.
- ¹¹ *Abutilon striatum* (a synonym of *A. pictum*) is painted Indian mallow.
- ¹² Antonio Leocadio Guzmán was a founder of the Liberal party of Venezuela; his son Antonio Guzmán Blanco was president of the country.
- ¹³ *Ionidium anomalum* is a synonym of *Hybanthus prunifolius* (family Violaceae); Ernst refers to the illustration in Humboldt *et al.* 1818–25, 5: pl. 500. See plate on p. 111. Its anterior petal (the term labellum is now usually restricted to orchids) is several times larger than its other petals and is used as a landing platform by some insects. For more on its breeding system, see Augspurger 1980.
- ¹⁴ In his letter to Ernst of 5 January 1880, CD had offered to send a copy of *Forms of flowers*. CD later sent *Coral reefs* 2d ed. and *Geological observations* 2d ed. (letter to Adolf Ernst, 4 April 1880).
- ¹⁵ Venezuela entered the Universal Postal Union on 1 January 1880 (Universal Postal Union, <http://www.upu.int/en/the-upu/member-countries/americas/venezuela.html> (accessed 22 October 2018)). The postal union was set up in 1874; the charges for letters and printed matter were covered in articles 3 and 4 of the treaty (*Treaty on postal union*, pp. 4–5).

To J. L. Chester 2 March 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R
March 2^d 1880

Dear Sir

Your letter & pedigree arrived here on Sunday & the M.S. yesterday.¹ I have read it with the greatest interest, & am completely astonished at your success & at the wonderful amount of labour which you have bestowed on the subject. I am sure we have all much cause to be grateful to you. It is surprising that you sh^d have been able to find out so much.

I well remember my Father² saying that no one knew or probably c^d ever know anything about W. Darwin of Cleatham.—³ There is a strange interest in reading the old Wills, & this has been increased manyfold by your remarks. My son Leonard has not seen the M.S. as he was forced to return to [Chatham] early on Monday morning.⁴ I will this day write & tell George in Cambridge.⁵ Both of them will feel the keenest interest in reading your lucid history of the family.

With my sincere thanks, I remain | Dear Sir | Yours very faithfully |
Charles R. Darwin—

James Innes (private collection) (Sold at Bonhams, 13 March 2002)

- ¹ Chester's letter has not been found; his history and the pedigree of the Darwin family in Lincolnshire, drawn from sixteenth and seventeenth-century wills, many of which are transcribed in full, are in DAR 210.14: 40.
- ² Robert Waring Darwin.
- ³ In 1879, CD had considered employing Chester to research the first William Darwin of Cleatham, Lincolnshire, and his ancestors; see *Correspondence* vol. 27, letter from J. L. Chester, 11 July 1879.
- ⁴ Leonard Darwin was an instructor in chemistry and photography at the School of Military Engineering, Chatham (*ODNB*).
- ⁵ There are extensive notes by George Howard Darwin in DAR 210.14: 41, dated April 1880, clarifying various points in Chester's history.

From J. L. Chester 3 March 1880

124, Southwark Park Road, | London, | S.E.
3 March 1880

My Dear Sir

If anything had been needed to convince me that I have not been wasting my time and energies in devoting myself to the sometimes decried branch of literature known as Family History, your very kind letter of yesterday would have supplied the want.¹ The pleasure it gives me is twofold: it is most gratifying to know that my labours are appreciated, but infinitely more so to know that I have been the means of affording you a pleasure which you had despaired of ever enjoying.

When I entered upon this pursuit, twenty years ago, in order that I might have some occupation, I started with the theory that the history of individuals is the history of the nation. It is not a nation which makes history, but rather the individuals who compose that nation. I have never yet pursued the history of any family, or even of any person, without finding something of interest or importance in it. Sometimes a single fact in the life of one person has been the turning point of events that have influenced the whole Kingdom, & even the world.

I venture to send you, as one of the results of my labours, a volume which occupied me upwards of ten years, and which is my legacy to the nation which has treated me so kindly during the twenty one years it has accepted me as a voluntary resident. It is not, of course, a book to sit down to & read through, but you may perhaps deem it worthy a place in your library, & occasionally find it convenient to consult.²

Believe me, with the highest respect & sincerest regard | Yours faithfully |
Jos: L. Chester

DAR 161: 140

- ¹ See letter to J. L. Chester, 2 March 1880.
- ² Chester sent CD *The marriage, baptismal, and burial registers of the collegiate church or abbey of St. Peter, Westminster* (Chester 1876), which had taken him ten years to complete; a copy is in the Darwin Library–Down. Chester was an American who had lived in London since 1858 (*ODNB*).

From R. F. Cooke 3 March 1880

50A, Albemarle Street, London. W.
March 3 1880

My dear Sir

As you are aware we printed 1000 copies of the *Life of Erasmus Darwin*.¹ By sales & presentations about 700 copies have been disposed of & we have the residue in our Warehouse & I am sorry to there is little or no demand for it, so that the prospect of a reprint is dismal. There is another matter I wish to mention.

We have still the type standing of your volume on *Forms of Plants & Flowers & Mess^{rs}*. Clowes write to ask if they make distribute it, as they want some of the type.²

We have about 25 copies remaining.

Would you like to send any corrections, have them made & then print off 250 copies & set the type loose?

I trust you have been keeping well this winter.

Yours faithfully | Rob! Cooke

Cha^s Darwin Esqr

DAR 171: 503

¹ *Erasmus Darwin* was published in November 1879 (*Correspondence* vol. 27, letter from Reginald Darwin, 12 November 1879). CD had asked for 1000 copies to be printed but had hoped that more would be required (see *ibid.*, letter to R. F. Cooke, 18 November [1879]).

² *Forms of flowers* was published in July 1877 (Freeman 1977). CD had asked for the printers William Clowes & Sons to keep the type up so that he could make corrections (see *Correspondence* vol. 25, letter to R. F. Cooke, 24 November 1877). A second edition of 1250 copies was published in July 1880, with a few errors corrected in the main text and new material added in a preface (Freeman 1977; *Forms of flowers* 2d ed., pp. v–xii).

From Anton Dohrn 3 March 1880

Stazione Zoologica | di | Napoli
March 3rd 1880.

Dear Mr. Darwin!

I got yesterday your kind letter and the enclosed cheque of £.100. I thank you once more most sincerely for your generous present and for the permission to use it in the way I ventured to propose.¹

It was urged at the last Meeting of the Brit. Association, that such a fund was necessary, but no money was granted for it. I hope this time it may have a better success, and I have confidentially told about your gift to the Secretary of the Committee for the Table of the Brit. Ass, so that he may report upon it at the next meeting.²

With my kindest regards | I remain | Yours respectfully | and most sincerely |
Anton Dohrn

DAR 162: 220



Anton Dohrn as a young man in his laboratory.
©Stazione Zoologica Anton Dohrn – Archivio Storico, Lb.2.13.

- ¹ See letter to Anton Dohrn, 27 February 1880 and n. 2.
- ² In his letter of 21 February 1880, Dohrn had suggested that he could use CD's gift to start a fund for English naturalists visiting the Zoological Station at Naples. The British Association for the Advancement of Science rented a laboratory table at the station; the secretary of the committee for the table was Albert George Dew-Smith. For the report requesting additional funds for travel, see the *Report of the British Association for the Advancement of Science* (1879): 165–71. For the subsequent report detailing Dohrn's new fund for the travelling expenses of English naturalists and requesting additional contributions from the British Association, see *ibid.* (1880): 161–9.

From James Torbitt [3 March 1880]

Handed in at the Waring St Belfast Office at .M. Received here at 4.50 .M.

From James Torbitt | Belfast

To Charles Darwin | Down | Beckenham Kent

The potatoes are now growing and will be lost millions worth of them unless I have Immediate authority to proceede¹

Telegram

Date stamp: MR 3 80

DAR 178: 160

- ¹ Torbitt had asked CD for help with securing a loan from the Government to continue developing his varieties of blight-resistant potato; see letter from James Torbitt, 12 February 1880, and letter to T. H. Farrer, 14 February 1880.

To H. W. Crosskey [4 March 1880]¹

Down, Beckenham, Kent.

Dear sir,

I have this day received, through Mr. Lawson Tait, the address from the Birmingham Philosophical Society congratulating me on my birthday, and communicating to me the fact of my election as honorary member.² The society has thus conferred on me an honour which I believe to be unprecedented.³ Both the address and my election have gratified me deeply, more especially as coming from Birmingham, the birthplace or residence of so many distinguished men, and where the famous Lunar Club, which included my grandfather as one of its members, used to meet.⁴ At my age I cannot expect to do much more scientific work, but the society may be assured that so great an honour as it has conferred on me will encourage me to further exertion.—

I beg leave to remain, dear sir, yours faithfully and obliged, CHARLES DARWIN.

Birmingham Daily Post, 21 April 1882, p. 4

- ¹ The date is established by the reference to Lawson Tait's visit to CD in London to deliver a presentation copy of an address (see n. 2, below).
- ² See letter from H. W. Crosskey, 28 February 1880. The Darwins were in London from 4 to 8 March 1880; Tait visited on 4 March (Emma Darwin's diary (DAR 242)). The presentation copy of the address has not been found in the Darwin Archive–CUL; CD had already received the text in an article from

the *Birmingham Daily Post*, 13 February 1880, p. 5 (see letter to Lawson Tait, 13 February 1880), and Crosskey enclosed a copy in his letter of 28 February 1880.

³ CD was the society's first honorary member; see the *Birmingham Daily Post*, 13 February 1880, p. 5.

⁴ CD's grandfather Erasmus Darwin was a member of the Lunar Society of Birmingham, a small club of pioneering natural philosophers, doctors, and manufacturers (for more on the Lunar Society, see Schofield 1963 and Uglow 2002).

From G. H. Darwin 4 March 1880

Trin. Coll Camb.

Mar 4. 80

My dear Father,

If the M.S be sent *registered* to me I will begin the copying at once as I can borrow a machine here.¹ I have just finished the paper at wh. I am at work & shall have time & ought to get it done in a fortnight or so.

Leo. will have told you that we paid him £50 for which he agreed to exhaust London resources, but he has been outside his contract & has got the wills at Lincoln searched.² Do you think of giving more; I suspect he w^d. accept if it were done delicately. What a lot of work he must have spent.

I have been having a bad cold in my head & somewhat on my chest but have got thro' so far rather better than usual as I have only been languid & stupid & have continued doing a little work. I fear however the later stages more than the active nose-blowing.

As I said above I have just finished off my paper & shall send it to the R. S in the day or two. I am glad to say it will be short eno' for the Proceedings³

We are getting on with the pendulum but have been delayed by finding the stone pillar is not isolated from the floor & so have been having the paving up & have got bricklayers & carpenters at work⁴ I hope we shall get to work again in a day or two.

Last Sunday I observed a little & thought the thing wobbled when cabs drove along a street 60 yds away— this proves its sensitiveness but does'nt look very hopeful for our doing anything in a town.

I can't remember whether I told you that Sackville Cecil & I propose going down to Glasgow together after Easter. I'm sorry to say he's got another railway place & is not going to come here after all.⁵

I enclose a pleasant letter of Mc'Lennan's which gives a better account of things at Davos. I hope they will adhere to the scheme of coming to Hayes.⁶

I'm sorry to hear that you are knocked up & I hope going to London will do you good⁷

I find these incessant colds very disheartening, it is now many months that I have had more or less of a break down every 10 days & it never gives one time to get into good order between whiles.

I shall be tremendously interested with Col. Chester's papers.

Yours affectionately | G H Darwin

DAR 210.2: 82

- ¹ CD was planning to send Joseph Lemuel Chester's manuscript of research on William Darwin of Cleatham and his ancestors; see letter to J. L. Chester, 2 March 1880. George's extensive notes on Chester's family history are in DAR 14: 41; the 'machine' was probably a letter copying press (for examples, see https://www.officemuseum.com/copy_machines.htm (accessed 6 June 2019)).
- ² Leonard Darwin took an interest in the Darwin family history; see letter to J. L. Chester, 2 March 1880. The payment was to Chester for his work on the Darwin ancestry; see letter to G. H. Darwin, 5 March [1880] and n. 4. The wills at Lincoln relate to the Darwin family's ownership of the Cleatham estate in Lincolnshire.
- ³ George's paper 'On the analytical expressions which give the history of a fluid planet of small viscosity, attended by a single satellite' (G. H. Darwin 1880) was received by the Royal Society of London on 6 March 1880 and published in their *Proceedings*.
- ⁴ George and Horace Darwin were constructing a pendulum to measure the lunar disturbance of gravity (G. H. Darwin 1907–16, 5: 1).
- ⁵ Sackville Arthur Cecil became general manager of the Metropolitan District Railway in 1880. George was probably planning to visit William Thomson in Glasgow, as he had done in 1879; see *Correspondence* vol. 27, letter from G. H. Darwin, 10 May 1879.
- ⁶ The enclosure has not been found. John Ferguson McLennan was very ill with consumption; George had planned to visit him in Davos, Switzerland, in January (see letter to G. H. Darwin, 29 January [1880]). McLennan died at Hayes Common, Kent, in 1881 (*ODNB*).
- ⁷ CD was in London from 4 to 8 March 1880 (CD's 'Journal' (Appendix II)).

To James Torbitt 4 March 1880

Down,
Thursday, | March 4, 1880

Private.

My dear Sir

Telegram received last night.¹ I am starting for London immediately, and will see Mr Farrer this afternoon. Mr. F. was here the other day and told me he could not get Ld. Sandon to do anything.² He always promises that he will *at once* attend to the subject and then does nothing! Mr. Farrer spoke of subscription and discussing the subject with Mr. Caird.³ I will now tell Farrer that something must be done at once if ever. As soon as ever I can get anything settled definitely, you shall hear. I suppose you would not plant for a month. I have told Farrer I would subscribe £50.⁴ Possibly I could influence Ld. Derby.⁵ But I have not strength for much exertion. Unless Farrer & Caird will take up the subject in earnest the whole affair will be a failure and in my opinion a disgrace to the country. Farrer *suspects* that Ld. Sandon has lost all my long letters on the subject!⁶ My address, if you have anything to communicate (it is no use telegraphing) will be till early on next Monday morning "6 Queen Anne St. Cavendish Sq.

In Haste | Yours sincerely | Ch. Darwin.

Copy

DAR 148: 111

¹ See telegram from James Torbitt, [3 March 1880].

² CD stayed at his brother Erasmus Alvey Darwin's house at 6 Queen Anne Street, London, from 4 to 8 March 1880; he called on Thomas Henry Farrer on 4 March (Emma Darwin's diary (DAR 242)). An earlier meeting with Farrer is not recorded. Farrer had been communicating with Dudley Francis Stuart Ryder (Viscount Sandon), minister for the Board of Trade, about securing government aid for

Torbitt to continue his experiments to breed blight-resistant potatoes (see *Correspondence* vol. 27, letter to James Torbitt, 17 November 1879, and this volume, letter to T. H. Farrer, 14 February 1880).

³ James Caird.

⁴ See letter to T. H. Farrer, 14 February 1880.

⁵ Edward Henry Stanley.

⁶ CD had written several drafts of a long letter of support for Torbitt (see *Correspondence* vol. 26, letter to T. H. Farrer, 7 March 1878, enclosure. Further materials were communicated to Viscount Sandon through Farrer in November and December 1879; see *Correspondence* vol. 27, letter from T. H. Farrer, 2 November 1879, and letter to James Torbitt, 17 November 1879).

To G. H. Darwin 5 March [1880]¹

6. Q. Anne St.
March 5th—

My dear George.

I am glad that I wrote to the Colonel, for the enclosed shows that he is pleased.—² I will write to Frank to send the M.S. to you.—³ If you settle to give him more money, I will subscribe or give alone, but you must manage it.—⁴ I am very sorry my poor dear old fellow to hear so bad an account of you.—⁵ Letter this morning shows that Jos, as we think, is dying.⁶

MacLennan must be a good deal better. He seems & all of them much attached to you.—⁷

Your affect Father | C. Darwin

I am going to send Life of Erasmus Darwin⁸ to the Colonel

DAR 210.1.92

¹ The year is established by the relationship between this letter and the letter from G. H. Darwin, 4 March 1880.

² See letter to J. L. Chester, 2 March 1880. CD enclosed Joseph Lemuel Chester's letter of 3 March 1880.

³ Francis Darwin. Chester had sent CD a manuscript containing his research on William Darwin of Cleatham, one of Erasmus Darwin's ancestors; see letter to J. L. Chester, 2 March 1880 and n. 1. Erasmus Darwin was CD's grandfather.

⁴ In his letter of 4 March 1880, George had suggested that they pay Chester more for his family research. CD recorded a payment of £25 under the heading 'Col Chester pedigree' on 27 March 1880 (CD's Account books—banking account (Down House MS)).

⁵ George had been suffering from a bad cold; see letter from G. H. Darwin, 4 March 1880.

⁶ Josiah Wedgwood III was gravely ill; see letter from W. E. Darwin, 6 January [1880]. The latest extant report on his health was quoted in a letter from Emma Darwin to H. E. Litchfield, 2 March 1880 (DAR 219.9: 224): 'I will only send a line to say since Sat. there have been no more of those suffering attacks, but he is weaker & more ill, tho' he has no pain'.

⁷ With his letter of 4 March 1880, George had enclosed a letter from John Ferguson McLennan.

⁸ *Erasmus Darwin*.

To T. H. Farrer 5 March 1880

[6 Queen Anne Street, London.]

Postscript Mar 5th 1880.¹

I have re-read my letter dated Mar 7 1878 & have nothing to withdraw.² I may add that I have read two letters published by strangers in a Belfast newspaper, who

declare, after examining Mr Torbitt's ten-acre field, that there is the greatest difference between the several varieties in their liability to the disease, & that some seem to have quite escaped.³ Mr Torbitt himself assures me that some of the varieties are entirely free from the fungus, both in their foliage & tubers. But as young varieties seem to resist the disease better than those which have been long cultivated, it is especially desirable that these should be cultivated for several more generations, & that fresh seedlings should be raised from them. Mr Torbitt informs me that his business has been so unprofitable of late that he cannot continue his experiments, which are, & must be, very expensive, by his own means alone, as he had originally hoped to do.⁴

I may repeat my conviction that no one else is likely to undertake so laborious a task as the selection & cross-fertilization of the plant during many successive generations. As this letter may be seen by strangers, I will add that I have no sort of pecuniary interest in Mr Torbitt's success. He is personally unknown to me, but his letters have inspired me with so much confidence, & his plan seems so much more hopeful than any which has ever been tried, that I have been glad to aid him to a small extent with funds.⁵

Unless Mr Torbitt is assisted *immediately* with a few hundred pounds, he will be compelled, as he informs me, to sell his whole stock of potatoes; & this to the best of my judgment would be a national misfortune & disgrace

Charles Darwin

LS

Linnean Society of London (MS 489)

¹ The letter is a postscript to a letter to T. H. Farrer dated 7 March 1878 that CD had meant Farrer to use to raise support for James Torbitt's potato-crossing experiments (see *Correspondence* vol. 26, enclosure to letter to T. H. Farrer, 7 March 1878). Torbitt had asked again for CD's support (telegram from James Torbitt, [3 March 1880]) and CD had met Farrer in London to discuss the matter (letter to James Torbitt, 4 March 1880). CD wanted Farrer to use the 1878 letter with this postscript to raise support for Torbitt.

² See *Correspondence* vol. 26, enclosure to letter to T. H. Farrer, 7 March 1878.

³ The letters were probably those sent to Farrer with CD's letter of 23 October 1879 (*Correspondence* vol. 27). They appeared in the Belfast *Northern Whig*, 15 October 1879, p. 6, and 20 October 1879, p. 6.

⁴ For Torbitt's latest report on growing disease-free potatoes from seed, see *Correspondence* vol. 27, enclosure to letter from James Torbitt, 15 November 1879. Torbitt had reported his need for funds to continue his experiments in his letter of 12 February 1880.

⁵ In 1878, CD had sent £100 to assist with Torbitt's research; see *Correspondence* vol. 26, letter to James Torbitt, 4 March 1878.

To T. H. Huxley [5 March 1880?]¹

6. Queen Anne St | W.
Friday night

My dear Huxley

I want so very much to see you, that you must let me call on you on Sunday morning between 10 & 11. (i.e. if I am well) & stay a half-hour.—² I have much to do & very little strength to do it & so I propose coming on Sunday morning—

If I do **not** hear, you may expect me.—

Ever yours | C. Darwin

Imperial College of Science, Technology and Medicine Archives (Huxley 5; 354)

- ¹ The year is conjectured from the year given in Dawson 1946, p. 31, but CD was not staying at 6 Queen Anne Street, London, on the day and month given by Dawson, 3 December. The only date in 1880 on which CD could have written this letter is 5 March (see n. 2, below). In 1880, 5 March fell on a Friday.
- ² CD stayed with his brother, Erasmus Alvey Darwin, at 6 Queen Anne Street, London, from 4 to 8 March 1880 (CD's 'Journal' (Appendix II)).

From James Torbitt 5 March 1880

*J. Torbitt, | Wine Merchant. | 58, North Street, | Belfast,
5 Mar 1880.*

Ch Darwin Esqr. | 6 Queen Anne Street | Cavendish Square.

My dear Sir,

Letter of yesterday received—I have been thinking of Lord Derby myself. If he could be induced to advance the money I think he would not lose it.¹

May I consider that if nothing else is done you will advance the £50²—it would grow perhaps 500 varieties—The new potatoes to repay the £150 if they are worth that amt

I think that in a few years, if not lost, they will be worth some millions per annum to England.

For myself—my honor is pledged not to lay out more money on this matter.

Most respectfully | my dear Sir | James Torbitt

DAR 178: 161

- ¹ In his letter to James Torbitt, 4 March 1880, CD said he could possibly influence Edward Henry Stanley to support Torbitt's scheme to breed blight-resistant potatoes.
- ² CD said he would subscribe £50 in his letter to Torbitt of 4 March 1880.

From James Torbitt 5 March 1880

*J. Torbitt, | Wine Merchant. | 58, North Street, | Belfast,
5 Mar 1880.*

Respectfully in continuance of my note by last mail—¹

Some two thirds of the potatoes are growing and should be planted as soon as possible, but they can be grown from a second set of buds.

The remaining one third are not yet germinating, and I may be able to keep them back for a month, as I can now expose them to the air and light without much risk of frost.²

Speculating on the possibility of some help, I have sown seed twice, and tomorrow propose to sow a third lot.

I will also select a tuber from a hundred of the best varieties of '79, '78 and '77 for crossing so as to be prepared for continuance of the work.

Most respectfully | J. Torbitt

DAR 178: 162

¹ See first letter from James Torbitt, 5 March 1880.

² In his letter of 4 March 1880, CD had said that he supposed that Torbitt would not need to plant his varieties of potato for a month.

From G. H. Darwin 6 March 1880

Trin. Coll. Camb.
Sat. Mar 6. 80

My dear Father,

The enclosed letter has come to me & as I don't understand what it is about I think it must be meant for you.¹

I received a letter yesterday morning from Reginald Darwin enclosing one of D: D's visiting cards, of which a packet was found by one of his sisters. It is a curious looking card & I will send it on shortly. I have written to thank him & to tell him that I shall send on a copy of the pedigree etc. shortly.²

I have sent off my paper to the R.S.³ & have begun trying another point but am sadly afraid it is too hard for me—for it turns on purely mathematical difficulties. I w^d give a great deal to be able to solve it, as I feel convinced that it contains the physical meaning of Bode's Law—an empirical law concerning the mean distances of the planets from the sun.⁴ I am afraid the difficulties are of a kind which if insurmountable soon are not to be got over at all. I said that Tait was reporter on my paper, but I now feel certain that it is a mistake for Thomson—for Thomson has reported on all the others & Tait is not an F.R.S & has indeed a sort of quarrel or contempt for the Society. I shall be glad if this is so.⁵

I expect we shall get to work at our pendulum again next week but there has been more bricklaying &c than I thought at first there w^d be. Horace & Ida go to Oxford today.⁶ I suppose I shall be home in about a fortnight. I sent off a tea-service to Jackson at 6 Q.A. yesterday & hope it will have come safe. I'm getting on tol. well with my cold. I hope Mother is standing London well⁷

Y^r affec son | G H Darwin

DAR 210.2: 83

¹ The enclosure has not been found.

² Erasmus Darwin's visiting card has not been found in the Darwin Archive–CUL. George said he would copy the Darwin pedigree prepared by Joseph Lemuel Chester; see letter from G. H. Darwin, 4 March 1880 and n. 1.

³ G. H. Darwin 1880; see letter from G. H. Darwin, 4 March 1880 and n. 3.

⁴ For more on Bode's law, developed by Johann Elert Bode, and George's use of it, see Nieto 1972, pp. 55–7.

⁵ Peter Guthrie Tait. For William Thomson's previous support of George's work, see *Correspondence* vol. 27, letter to G. H. Darwin, 31 May [1879]. In fact G. H. Darwin 1880 was not refereed and the paper was voted to be published in abstracted form at a meeting a week before it was read to the fellows of the Royal Society of London (Royal Society archives, GB 117 MS/421).

⁶ See letter from G. H. Darwin, 4 March 1880 and n. 4; Horace and Ida Darwin.

⁷ William Jackson was the butler at Down House; on 31 March 1880 he married Sophia Steer at St Mary's, Down. From 4 to 8 March, CD and Emma stayed at 6 Queen Anne Street, London, the home of CD's brother Erasmus Alvey Darwin (CD's 'Journal' (Appendix II)).

From T. H. Farrer 6 March 1880

M. T. Hotel
6 March/80

My dear Mr Darwin

I saw both Caird and Hooker yesterday—¹ Both think it useless to approach the Gov^t—especially as they are buying at great expense “Champion” potatoes to send as seed to Ireland, which Caird says—have proved good.²

Both too think that Mr Torbitt, should now make—or still better—get some one else to make some precise report on what he has done shewing what progress he has made— Further they think that £100 would be enough to give at present— This there would be no difficulty in procuring; for Caird says he will make himself responsible for £75 and I will gladly add £25. Will this enable you to say anything encouraging to Mr Torbitt.— Would not a careful report of what he has done and its success in the form of a letter to Nature or to one of the Agricultural papers be useful. If accompanied by a few lines from you it would be sure to attract attention: and if he has any good potatoes to *sell* as seed, it would bring them forward.

Ever yrs sincerely | T H Farrer

DAR 164: 97

¹ James Caird and Joseph Dalton Hooker. CD was trying to help James Torbitt obtain funding for his blight-resistant potato breeding scheme; see letter to James Torbitt, 4 March 1880.

² Torbitt remarked that the Champion variety was increasingly susceptible to disease (see letter from James Torbitt, 12 February 1880). Champion’s blight resistance did decline after a few decades (Salaman 1926, pp. 117 and 231–2).

To James Torbitt 6 March 1880

Down, [6 Queen Anne Street, London.]
March 6, 1880.

My dear Sir

Your letter of the 5th received.¹ I have been doing all that I can in your affair. Mr Farrer met Mr. Caird and Sir J. Hooker yesterday, and they are all convinced that it will be hopeless to apply to the Government.² Mr. Caird says he will guarantee £75, and Mr. Farrer £25 and this with the enclosed cheque for £50 will permit you to go on in your experiments on a limited scale, to the amount of £150.³ The gentlemen above named are strongly of opinion, that you ought next autumn to get some one or two agriculturists of independent position and character to examine your experimental ground and the tubers when dug up and make a brief Report on the state and result of your experiments. They think if this were published and it were favourable, you might obtain further assistance. Otherwise I doubt whether you will be able. You know my firm conviction is that you are on the right road whether or not you are as yet fully successful.

On further reflexion I should not like to apply to Ld. Derby,⁴ and as far as my advice is worth anything I would recommend you to do the best with £150.

I wish I could have helped you more and remain | My dear Sir | Yours very faithfully | Ch. Darwin.

Copy

DAR 148: 112

¹ Letters from James Torbitt, 5 March 1880.

² Thomas Henry Farrer, James Caird, and Joseph Dalton Hooker; see letter from T. H. Farrer, 6 March 1880. CD had been trying to help Torbitt obtain a government grant for his blight-resistant potato breeding scheme; see letter to James Torbitt, 4 March 1880.

³ Under the heading 'Gifts and annual subscriptions', CD recorded a payment of £50 to Torbitt on 6 March 1880 (CD's Classed account books (Down House MS)).

⁴ Edward Henry Stanley; see first letter from James Torbitt, 5 March 1880 and n. 1.

To James Torbitt 6 March [1880]¹

Down [6 Queen Anne Street, London.]
March 6th.

P.S. to letter written and posted this morning—

Here is bad news: Mr. Farrer forwards letter from Mr. Caird, that he finds his agricultural friends are so poor from low rents that they are little inclined to subscribe; and Mr. Caird cannot pledge himself for £75.²

On the other hand my brother E. A. Darwin of 6 Queen Anne St. will subscribe £25 and my brother-in-law H. Wedgwood of 31 Queen Anne St. will subscribe £10—so you are sure of £110 and I cannot doubt something from Mr. Caird.³ Mr. Wedgwood wants me to write to the Times stating that I believe your scheme is a hopeful one and giving a sketch of it—stating what subscriptions are promised and saying that I would receive others for the object. I dislike the thought of doing this, but should be glad to hear what you think of it. If you approve I will try and screw up my courage, but would first consult Mr. Farrer (who unfortunately has left London to-day) and in this case pray give me a few particulars for how many generations have you in any case selected? Do the later generations appear freer of the disease than the earlier generations? Can you advance any evidence with names and addresses of agriculturalists or market gardeners who saw your plants growing and the tubers? My memory has grown rather uncertain and I dare not trust it. Any report had better be as short as possible. I am very tired and can write no more

C. Darwin

I do not promise to write to the Times.

Copy

DAR 148: 113

¹ The year is established by the relationship between this letter and the letter to James Torbitt, 6 March 1880.

² See letter to James Torbitt, 6 March 1880. The letter from James Caird to Thomas Henry Farrer has not been found in the Darwin Archive–CUL.

³ Erasmus Alvey Darwin and Hensleigh Wedgwood. CD had sent £50 and Farrer had pledged £25; see letter to James Torbitt, 6 March 1880.

To Leopold Württenberger 6 March 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
[6 Queen Anne Street, London.]

March 6th 1880

My dear Sir

I am very much obliged to you for your present of your “Studien Ammoniten”, & am delighted that you have found time to publish your valuable results.—¹ I hope soon to read your book, but German is always slow work with me.—

With much respect, I remain | Dear Sir | Yours faithfully | Charles Darwin

Eidgenössische Technische Hochschule Bibliothek, Zürich (Württenberger Nachlass Hs 709: 9)

¹ Württenberger had sent his *Studien über die Stammesgeschichte der Ammoniten. Ein geologischer Beweis für die Darwin'sche Theorie* (Studies on the phylogeny of ammonites: a geological proof of Darwinian theory; Württenberger 1880); there are copies in the Darwin Library–CUL and Down. In 1879, CD had given Württenberger £100 to support his research (see *Correspondence* vol. 27, letter from Leopold Württenberger, 7 February 1879).

To T. H. Farrer 7 [March 1880]¹

6 Queen Anne St
Sunday 7th

My dear Farrer.

I have thought that you would like to see enclosed copy of letter received by Sir J. Lubbock. but I suppose it is vox et præterea nihil.²

I had despatched a letter to Torbitt before receiving M^r Caird's, but it did not signify as I was able to send another by same post. explaining why M^r C was obliged to withdraw to a certain extent.³

Hensleigh who was present offered to subscribe 10£ & Erasmus 25£⁴ so that 110£ is assured with your generous offer of 25£; & I suppose M^r C. will get something

Hensleigh & Erasmus strongly advise me to write to Times during Easter recess, describing what M^r T has done & what is wanted—I am not sanguine of success. & dislike *much* the thought of writing to any paper; but I would do so if thought advisable.— If I could get 400 or 500£ I would pay $\frac{1}{2}$ per year to M^r T so as to make it last 2 years—and he would then raise several thousand new varieties— Will you tell me at Down what you think.— Hensleigh was strong that if I did write I should insert subscriptions promised; if M^r Caird subscribes anything himself, & would allow me to use his name it would be a tower of strength. & would insure insertion of letter in Times.— I would of course, if you think fit, send any letter which I might write for your approval.— I have written to M^r Torbitt for various particulars

& asking him whether he would object to my writing to Times—that is if I screwed my courage up. & I told him that I should first consult you.⁵

I had hoped never to trouble you again. | Yours very sincerely, Ch. Darwin.

Mr. T. writes that about $\frac{1}{3}$ rd of his tubers are sprouting & ought to be planted next week & he can now do this.—⁶

P.S. In case I do write to Times, the letters which I wrote to you & which you sent me might come in of use, so will you send it me to Down.⁷

[Enclosure]

Board of Trade
5th Mar. 1880

Copy of letter from Lord Sandon's secretary to Sir J Lubbock—
Sir,

I am directed by Lord Sandon to acknowledge the receipt of your letter of yesterday's date.⁸

Lord Sandon wishes me to say that he will carefully consider the subject to which you refer.

I am Sir | your obedient servant | T. W. P. Blomefield⁹

Copy

DAR 144: 101; DAR 177: 340

¹ The year and month are established by the relationship between this letter and the letter from T. H. Farrer, 6 March 1880.

² John Lubbock. *Vox et praeterea nihil*: a voice and nothing else (Latin).

³ In his first letter of 6 March 1880 to James Torbitt, CD had said that James Caird would pledge £75 to help Torbitt to continue his blight-resistant potato breeding programme. The letter from Caird to Farrer has not been found. In CD's second letter to Torbitt, of 6 March [1880], he said that Caird had withdrawn his pledge but would probably give something.

⁴ Hensleigh Wedgwood and Erasmus Alvey Darwin.

⁵ See letter to James Torbitt, 6 March [1880].

⁶ See second letter from James Torbitt, 5 March 1880.

⁷ CD is probably referring to the enclosure to his letter to Farrer of 7 March 1878 (*Correspondence* vol. 26), which went through several drafts.

⁸ See letter to T. H. Farrer, 14 February 1880; Farrer had asked Dudley Francis Stuart Ryder, Viscount Sandon and minister for the Board of Trade, to seek government aid for Torbitt.

⁹ Thomas Wilmot Peregrine Blomefield.

From Anthony Rich 7 March 1880

Chappell Croft, | Heene, Worthing.

My dear Mr. Darwin,

I have been thinking for many a long day that I would write and ask how you and your belongings and surroundings—fauna and flora—had survived the persistent fogs and frosts of the last four months.¹ Bravely, I hope, and in that faith I shall steadfastly continue unless, or until, I hear any thing to the contrary. Verily you

deserve the Victoria Cross, and M^{rs} Darwin the Star of India, for taking that plunge into the Cimmerian gloom of London streets at such a season during such a winter.² My sister,³ who lives in rather an out of the way part of London, Woburn Square, but which is immediately connected with large open spaces, wrote me word that they had passed five consecutive days under gaslight the whole time! You escaped such an infliction as that, I trust; but anyhow I may safely greet your joint resolution, about visiting at mid winter, with the sententious response which Charles Dickens received from a Dublin juvenile, aged six years, upon telling him that he liked talking to little boys—"Yer right".—⁴

I heard not very long since from your son William, who has very good naturedly interested himself in my behalf with some importers of cattle from the Channel Islands: He told me that his brother George had passed a wretched time of it on the journey to Davos, being laid up even for a week on the way; and after all found that the place did not agree with him. How often it happens that those localities, which rise into sudden and exaggerated repute, gain an undeserved notoriety only by the puffs of lodging house and hotel keepers, people who have land to sell in the vicinity, and a doctor with a *specialité*! He will soon, I expect, be homeward bound—perhaps at this moment is resting with you at Down—and, without a perhaps let me trust, thoroughly recovered from previous ailments. When he comes please to give him kind remembrances, and all sorts of good wishes from me. He spent as you know a day with me on his way to Bassett—a very pleasant one to me; We fished out some Epigrams that were current at Cambridge in my day, and which, he told me, were written by a relative of his own.—⁵

If I had had a proper sense of the *convenances* I should have taken care before this time to congratulate both M^{rs} Darwin and yourself upon the marriage of your son Horace.⁶ But an "outside barbarian"⁷ such as I am is sure to make a hole in his manners three hundred and sixty five times in the year at least. It must be a thorough gratification for parents to see their children settling themselves well and comfortably in life; and all without you're being compelled to, "assist at" the wedding in person!

I'm sure they're a charming couple, And *you're* a most fortunate man.—

I hope that they never would come into this neighbourhood without giving me an opportunity of making their acquaintance.—

The "Cray-fish" I found rather hard of digestion. Not from any fault in the dish itself, nor of the *Chef* by whom it was prepared; but because the intellectual stomach at the age of 76, is not so able to assimilate such concentrated technical food without some previous educational discipline of which mine is altogether wanting.⁸ So I merely skimmed over the surface in a half capricious way—like Horace's town mouse, *dente superbo*—when I ought to have dived seriously down to the bottom of the *plat*.⁹ Professor Huxley's project for a series of "Biographies of men of Science" will be, I presume, somewhat upon the plan of John Morley's "Men of Letters", which report says has been very successful.¹⁰ The former, however, will have the more difficult task, both from the nature of his subject and the smaller number of

persons capable of being interested by it. But **H.** does everything so well that one cannot but anticipate success for him in all his undertakings. I should look forward with intense interest to any Political writings from such a pen as his. Indeed some robust specimens of that description of literature are much wanted just now if we are not all of us to become Orientalized and adopted into the “higher Jewish caste of the Sephardim”, with our ruling Knight of the Garter.¹¹

Macvey Napier’s Correspondence afforded me as much pleasure as it did you. He must have been a man of wonderful patience and self command to have kept that gibbing Scotch nag from kicking right over the traces.—¹² The Memoirs of Madame de Remusat is another interesting book, which I dare say you will have read. It will have rejoiced the Shades of Sir Hudson Lowe if some of our Spiritists would only inform him that the world has been duly informed what a domestic ruffian he had in his keeping.—¹³

I read the other day in the Life & Letters of Caroline Herschell that at the age of 92, as an instance of her great physical power, she “put her leg behind her back and scratched her ear with it, like a dog”.— You wrote me word that you found it a hard job to know what to believe ... Try that!¹⁴

From books to “kitchen stuff” is an awful leap; but I must take it or I shall die of despair; and you, who know so much about every thing, must be called in, if you will, as the doctor who is to save me from myself. It is customary about here to cover the sea cale with sea weed in the winter, and I have done so for many years with complete success. But this spring we have found an immense number of white maggots about the third of an inch long under the weeds; and they have eaten round the stems of the cale close to the ground so that the plant breaks off and rots away. My gardener, who never saw the like before, thinks that these destructive things are engendered in the sea weeds. He says that he had observed a large number of flies, about the size of a house fly, but with longer & darker bodies than them, disporting themselves over the bed during the winter—and these he pronounces to be the *fons et origo mali*. But the weeds came up dripping wet just out of the sea, just as they were thrown up at the moment, and I do not suppose that the fathers of insects can be produced under the sea—to live and flourish upon dry land. The beds were covered over at least six weeks later than usual, because the times had been so calm that no weeds were to be obtained; but as the frosts were severe and enduring the autumn sweepings of dead leaves from the elm trees were laid over the beds for protection. Did they conceal the germs from which my enemies have sprung? *Aiutatemi, caro Signore!*—¹⁵

My friend or foe, I don’t know which to call him, the Gulf Stream, seems to have found his way into the Channel at last. I wonder whether there is any chance of his being diverted into the Pacific when M^r Lesseps has made his interoceanic canal?—¹⁶ But really—notwithstanding your exceeding courtesy and good nature—I do begin to feel that this long rigmarole exceeds the bounds of discretion. So no more. You can’t see my blushes. Fancy them; scarlet as the wattles of an angry turkey cock. The philadelphus flourishes; so does its present owner; so, I trust, does its donor;

and so, I hope, does that lady, who keeps a strict hand upon your vagaries, and amongst whose constituents I shall be anxious to be enrolled when the wrongs of animals and the rights of women have been removed to the “field of practical politics”¹⁷

Until then I must be content to sign myself hers and yours | Very sincerely |
Anthony Rich

March. 7. 1880.—

DAR 176: 141

¹ The period from November 1878 to January 1880 was exceedingly cold (Manley 1974, p. 396).

² Victoria Cross: British military and naval decoration bestowed for conspicuous bravery in battle; Star of India: order of knighthood (e.g. Knight Companion of the Star of India); Cimmerian: of or belonging to the legendary Cimmerii, who were said to live in perpetual darkness. Proverbially used as a qualification of dense darkness, gloom, or night, or of things or persons shrouded in thick darkness (*OED*). CD and Emma were in London from 4 to 8 March 1880 (CD’s ‘Journal’ (Appendix II)).

³ Rich’s sister, Emma Burnaby, lived at 26 Woburn Square, Bloomsbury, London (Census returns of England and Wales 1881 (The National Archives: Public Record Office RG11/320/22/40)).

⁴ The story appeared in a letter from Charles Dickens (see Dickens 1880–2, 2: 61).

⁵ George Howard Darwin went to Davos, a well-known spa town, from 21 January to 13 February 1880 to visit John Ferguson McLennan, who was suffering from consumption; George visited Rich and William Erasmus Darwin at Bassett, Southampton, before setting off (Emma Darwin’s diary (DAR 242), letter from Elizabeth Darwin to Ida Darwin, 15 January [1880] (DAR 258: 564), and letter from Elizabeth Darwin to G. H. Darwin, 3 February 1880 (DAR 251: 1412)). The Cambridge epigrams were possibly those of Harry Wedgwood; see *Emma Darwin* (1904) 1: 74 and 266.

⁶ Horace Darwin married Ida Farrer on 3 January 1880 (Emma Darwin’s diary (DAR 242)).

⁷ The Chinese often referred to foreigners as ‘outside barbarians’ (Gützlaff 1838, 2: 542).

⁸ In December 1879, Rich had procured Thomas Henry Huxley’s *The crayfish* (T. H. Huxley 1880a); see *Correspondence* vol. 27, letter from Anthony Rich, 28 December 1879.

⁹ A reference to Horace, *Satires* 2.6.85–7 (the town mouse and the country mouse). *Dente superbo*: with haughty tooth (Latin), i.e. disdainfully.

¹⁰ Huxley had recently written a biography of David Hume (T. H. Huxley 1879) for a series edited by John Morley on English men of letters; he contemplated editing a similar series on men of science but the project never materialised (see A. Desmond 1994–7, 2: 118).

¹¹ Benjamin Disraeli, prime minister of Great Britain, was from a Sephardic Jewish background but was baptised an Anglican in 1817. He became a knight of the garter in 1878. (*ODNB*.)

¹² Macvey Napier’s correspondence was published in 1879 (Napier 1879). Napier was editor of the *Edinburgh Review* from 1829 to 1847, but his predecessor Francis Jeffrey continued to influence editorial policy (*ODNB*). Thomas Carlyle also criticised Napier’s editorship.

¹³ *Memoirs of Madame de Rémusat* (Vergennes 1880) was an account of a *dame du palais* (lady-in-waiting) to the wife of Napoleon I. Hudson Lowe was governor of St Helena and gaoler to Napoleon I.

¹⁴ Caroline Lucretia Herschel was reported to have performed this feat at the age of 88 or 89 in Herschel 1876, p. 295. The letter from CD has not been found.

¹⁵ Rich’s gardener has not been identified. *Fons et origo mali*: the source and origin of the evil (Latin). *Aiutatemi, caro Signore*: help me, dear Sir (Italian).

¹⁶ After his successful construction of the Suez canal, Ferdinand de Lesseps embarked on a similar project in Panama from 1879 but the project failed.

¹⁷ In Rich’s letter of 28 December 1879 (*Correspondence* vol. 27), he reported on the health of a plant of *Philadelphus* (the genus of mock-orange) that CD had sent him. Debates over animal protection and women’s rights were sometimes conjoined by writers such as Frances Power Cobbe (see Mitchell 2004 and L. Williamson 2005).

From T. H. Farrer 8 March 1880

Board of Trade, | Whitehall Gardens. | S.W.
8-3-80

My dear Mr Darwin

I inclose some copies of your letter: and will write again after seeing Caird. I quite agree with the suggestion that you should write to the Times.¹

We had a very pleasant visit at Oxford—infinite chatter²

Ever yrs | T H Farrer

If you want more copies please say so

DAR 164: 99

¹ In his letter to Farrer of 7 [March 1880], CD had asked for copies of his earlier letters to Farrer (probably including the enclosure to his letter to Farrer of 7 March 1878 (*Correspondence* vol. 26)) to assist him in writing a letter to *The Times* to invite subscriptions to enable James Torbitt to continue his blight-resistant potato research. James Caird was collecting subscriptions; see second letter from T. H. Farrer, 8 March 1880.

² In Oxford, Farrer met with his daughter, Ida Darwin, and her husband, CD's son Horace (letter from G. H. Darwin, 6 March 1880 and n. 6, and letter from Emma Darwin to Ida Darwin, 26 February 1880 (DAR 258: 627)).

From T. H. Farrer 8 March 1880

Board of Trade S.W.
8-3-80

Dear Mr Darwin

I inclose you a note from Caird: and a cheque for £85 including his subscriptions & my own £25—¹

I have also written to him about the use of his name, & told him that—if you do use it you or I will shew him the letter first²

Ever yrs | T H Farrer

[Enclosure]

Board of Trade S.W.
March 8. 80

Dear Farrer

I bring you the produce of my labours—£25. each from M^r. Charles & M^r. Alfred Morrison—£5. from Sir Julian Goldsmid & £5. from myself. £60. altogether.

I dont think more than £100. should be given to the Experimenter unless he can show good reason. It is his own time only that has to be paid for. Mess Morrison w^d like to know result—& if you have anything written that I could show them on that point I should be glad. What is his name & address?

Believe me | Very truly Yours | James Caird

DAR 161: 2; DAR 164: 98

- ¹ James Caird was collecting subscriptions to enable James Torbitt to continue his development of blight-resistant potato varieties; see enclosure and letter from T. H. Farrer, 6 March 1880.
- ² CD was thinking about writing a letter to *The Times* to invite subscriptions to Torbitt's fund and wanted to use Caird's name; see letter to T. H. Farrer, 7 [March 1880].

To T. H. Farrer 9 March 1880

[Down.]

My dear Farrer—

Once again I thank you heartily & I hope that you will give my true thanks to M^r Caird.¹

It seems to me that Hooker & M^r Caird hardly understand the scale on which M^r Torbitt works when they say that 100 w^d be sufficient; in which success of his scheme depends on his raising annually many thousand [*illeg*] seedlings: I have been informed not by Mr T. only that he has 10 acres of land full of varieties, & that he devotes the whole of a greenhouse to propag forward the best seedlings which are first started in flower-pots.— The labour of harvesting separating labelling examining &c [*illeg*], saving the seed &c & keeping an account of all the varieties must be immense. I ⟨have⟩ now got with your cheque for £85 170£, which I think will be ample.² Though it appears to me useless to write to the Times, [nevertheless], as soon as I receive an answer to some queries sent to M^r Torbitt, I will draw up a letter, such as I had thought of writing to the Times, & will send 2 or more copies to you & M^r Caird so that his [generous] coadjutor may understand M^r Torbitts plan & see what he has done & why I think favourably of the scheme.— I can keep the letter to the Times in reserve, if next year it sh^d still appear to me desirable that M^r T. sh^d continue his work.³ Anyhow neither you nor Mr Caird shall be again troubled with the subject.—

I am, however, perplexed, as M^r Caird's writes "I dont think the 100£ sh^d be given to the experimenter unless he can show good reason." I will not now not send your cheque of £85 until M^r Caird & you have read what I will write to the Times & you can then decide whether I shall return the cheque to you

I have from yourself brother & Huxley 85 £.—⁴

Yours sincerely

March 9th 1880 | To T. H. Farrer

ADraft

DAR 202: 46

- ¹ See first letter from T. H. Farrer, 8 March 1880, and second letter from T. H. Farrer, 8 March 1880. James Caird had been collecting subscriptions to enable James Torbitt to continue his blight-resistant potato research.
- ² Joseph Dalton Hooker and Caird thought that £100 would be enough for Torbitt's research and wanted Torbitt to write a precise progress report before he received more; see letter from T. H. Farrer, 6 March 1880. CD had raised an additional £85 making a total of £170 (see letters to James Torbitt, 6 March 1880 and 6 March [1880]).

³ In his letter to Farrer of 7 [March 1880], CD had suggested writing to *The Times* to invite further subscriptions to Torbitt's fund. For CD's queries to Torbitt, see the letter to James Torbitt, 6 March [1880].

⁴ Erasmus Alvey Darwin and Thomas Henry Huxley. See enclosure to second letter from T. H. Farrer, 8 March 1880.

To T. L. Brunton 10 March 1880

Down | Beckenham, Kent—
Mar. 10/80

My dear Sir

I am very much obliged for your kind present of your *Pharmacology*, which I am sure that I shall read with interest. All medical subjects fascinate me, owing I suppose to so much doctorial blood flowing in my veins—¹

yours very sincerely | Charles Darwin

Copy

DAR 143: 169

¹ Brunton sent his *Pharmacology and therapeutics, or, Medicine past and present* (Brunton 1880); a copy is in the Darwin Library—Down. CD's father, Robert Waring Darwin, and his grandfather Erasmus Darwin were physicians.

To James Torbitt 10 March 1880

Down,
March 10, 1880.

My dear Sir

Your letter of the 8th received.¹ I enclose cheque for £25 and shall certainly be able to send you £110 and hope £150. But I shall not know the amount with certainty until I have written a statement for Mr. Caird, showing why I think favorably of your scheme, what you have done, &c. I am now pledged in honour to do this.² The letter to the *Times* can be kept in reserve till next year.³ Be so kind as to answer *as soon as ever* your work permits you all my queries; especially which year you commenced work and what you have expended. I will then write a statement, making it as brief as possible, and have copies sent to Messrs. Farrer,⁴ Caird &c.

My dear Sir | Yours sincerely | Ch. Darwin.

Copy

DAR 148: 114

¹ The letter from Torbitt has not been found.

² For the latest funds that CD had raised for Torbitt to continue his research developing blight-resistant potatoes and for James Caird's reservations about sending the money to Torbitt, see the draft letter to T. H. Farrer, 9 March 1880 and n. 2.

³ See draft letter to T. H. Farrer, 9 March 1880 and n. 3.

⁴ For CD's earlier queries to Torbitt, see the letter to James Torbitt, 6 March [1880]. Thomas Henry Farrer.

From Asa Gray 11 March 1880

Herbarium of Harvard University, | Botanic Garden, Cambridge, Mass.
March 11 1880.

Dear Darwin

I send you “one more” of the flat seeds, which has been about 10 days in damp sand. There are 3 others, perhaps not sound, we will watch¹

How *could* the plumule of *Delphinium nudicaule* get out, but through the united petioles?²

I sent you—to laugh at a notice in *The Nation.*, of a Philadelphia lawyer’s Refutation of Darwinism. The adage is that “a Philadelphia Lawyer is a match for the Devil”. But a mere imp is a match for this one.³

As to the names of the species by the seeds, it is not clear— But, according to Watson, who has done his best with them, the one with large ovate turgid seeds rather pointed at one end, the germination of which is figured in *Amer. Jour. Sci. & in Text-Book* is *Megarhiza Californica*, I suppose.⁴

The *M. Oregana* has flattish seeds;⁵

I have stopped & looked into this matter. There are two species clear.

1. A Californian one (*M. Californica*, with obovoid seeds & hilum at the small end—well figured by Naudin in *Ann. Sci. Nat. ser. 4, 12, t. 9*. The *last seeds* sent you must be of this, & it must include at least Watson’s *M. Californica* & *M. muricata*.⁶

2. *M. Oregana*, with oblate and flatter seeds, the hilum at middle of a long side, *seed sent*. And these seeds, supplied by a florist—must have come from Oregon.

The particular source of the seeds I germinated is uncertain, but surely Californian. If from San Francisco then probably *M. Marah*, a 3rd species. We will try to get them in cultivation. But tho’ they will grow here, we have not been able to flower them!

Yours ever | A. Gray

DAR 209.6: 202

¹ In his letter to Gray of 17 February 1880, CD had reported that none of the flattened seeds of *Megarhiza* that Gray had sent had germinated.

² *Delphinium nudicaule* is red larkspur; for CD’s description of the emergence of the plumule through a split base of confluent petioles, see the letter to Asa Gray, 20 January [1880] and n. 3.

³ Gray’s anonymous review of *The refutation of Darwinism* (O’Neill 1880) appeared in the *Nation*, 4 March 1880, p. 182; see letter to Asa Gray, 24 March [1880]. CD’s copy is in DAR 226.1: 104. The author was T. Warren O’Neill. For more on the adage ‘three Philadelphia lawyers are a match for the very devil himself’, see Rawson and Miner eds. 2006, p. 376.

⁴ Sereno Watson illustrated *Megarhiza californica* (a synonym of *Marah fabacea*, California manroot) in Gray’s article in the *American Journal of Science and Arts* (A. Gray 1877, pp. 23–4), and in *Gray’s botanical text-book* (A. Gray 1879, p. 21). See also letter from Asa Gray, 3 February 1880 and n. 2.

⁵ *Megarhiza oregana*, a synonym of *Marah oregana*, is western wild cucumber or coastal manroot.

⁶ Charles Victor Naudin had described and figured this species under the name *Echinocystis fabacea* in Naudin 1859, pp. 154–6 and tab. 9. For the complex synonymy given by Gray of these species, see A. Gray 1877; Sereno Watson had identified five species of *Megarhiza* in Brewer *et al.* 1876–80, 1: 240–2, including *M. californica*, *M. oregana*, *M. marah*, and *M. muricata* (the last two are synonyms of *Marah watsonii*, taw manroot).

From Charles Layton 11 March 1880

16 Little Britain (City)

March 11th/80

Dear Sir

I enclose cheque on Union Bank for £57.11.2 in payment a/c. of Sales from New York—¹

a receipt will oblige,

Yours Respectfully | for D. Appleton & Co | Charles Layton—Agent

To Charles Darwin Esq—

Sales by by D. Appleton & Co for a/c

Cha^s Darwin to Feby 1/80

Climbing plants

Aug 1/79	On hand	378		
Feby 1/80	"	366		
	Sold	12	10% of \$1 ²⁵	\$1.50

Orchids

Aug 1/79	On hand	508		
Feby 1/80	"	476		
	Sold	32	10% of \$1 ⁷⁵	5.60

Different forms of Flowers

No sales

Cross Fertilization

Aug 1/79	On hand	279		
Feby 1/80	"	258		
	Sold	21	10% of \$2	4.20

Origin of Species

Aug 1/79	On hand	230		
Oct	Printed	500		
Feby 1/80	On hand	523		
	Sold	213	5% of \$2	21.30
			Forw ^d	<hr/> \$32.60

Expression of Emotions

Aug 1/79	On hand	1038		
Feby 1/80	"	944		
	Sold	94	10% of \$3 ⁵⁰	32.90

Descent of Man

Aug 1/79	On hand	523		
Feby 1/80	"	292		
	Sold	231	10% of \$3	69.30
add copyright on 1466 copies sold since				
Feby 1/76.	1 Vol Ed price \$3 instead of \$2	10% of \$1.		146.60
Stg value exch 4[.]88 £57. 11. 2				

DAR 159: 105

¹ An entry dated 15 March 1880 in CD's Account book—banking account (Down House MS) records the receipt of £57 11s. 2d. for 'Appleton sale of books for 1/2 year'.

To James Torbitt 11 March [1880]¹

From Mr. Darwin, Down, Beckenham.

When you answer my queries be so good as to state how much land has been used in cultivating the varieties.—² I sh^d like to at least allude to this point. How many seedlings have you raised in any one year?

C. D.

March 11th.

ApcS
Bonhams (dealers) (9 November 2016)

¹ The year is established by the relationship between this letter and the letter to James Torbitt, 10 March 1880.

² See letters to James Torbitt, 6 March [1880] and 10 March 1880.

From T. H. Farrer 12 March 1880

Please Read & burn or keep
THF ¹

12.3.80

Copyhold Inclosure & Tithe Commision | 3, St. James' Square. S.W.
Mar 12.80

My dear Farrer

Pray say to Mr. Darwin that we have entire confidence in his appropriation of the money. I had no idea that Mr. Torbitts experiments were on so large a scale.²

I keep Mr. D's letter to show it to M^r. Morrison.³

Great enjoyment attend you on your *going over* to Rome.⁴ | Yours sincerely |
James Caird

DAR 161: 3

¹ Farrer wrote the comments at the top of letter before forwarding it to CD.

² In his draft letter to Farrer of 9 March 1880, CD had expressed doubt whether Caird had understood the scale of James Torbitt's breeding experiments on blight-resistant potatoes. Caird had been collecting subscriptions to enable Torbitt to continue his work.

³ See draft letter to T. H. Farrer, 9 March 1880. Charles and Alfred Morrison had given £25 each to support Torbitt's scheme; they had asked to know the results (see enclosure to second letter from T. H. Farrer, 8 March 1880).

⁴ 'Going over to Rome': a joking allusion to converting to Catholicism. Farrer planned to visit Rome for six weeks with his wife Katherine Euphemia Farrer; see letter to James Torbitt, 20 March 1880 and n. 2.

From James Torbitt 12 March 1880

J. Torbitt, | Wine Merchant | 58, North Street, | Belfast,
12 Mar 1880

Charles Darwin Esqr | Down

My dear Sir,

I have your valued letter of 10th and post card of yesterday with check from Mr E A, Darwin for £25 enclosed and for which I should like him to have my best thanks—please do not send any more money till the autumn.¹

I am sorry my report is so long but hope to finish tomorrow, I have to stop now to look up accounts &c to reply to your queries.²

I have just finished pricking out 2000 seedlings, they are the same as last years seedlings second cross—The crossing attempted last year was a failure, none of the plants operated on having produced fruit.

I remain my dear Sir | most respectfully and faithfully yours | James Torbitt

DAR 178: 163

¹ See letter to James Torbitt, 10 March 1880, and postcard to James Torbitt, 11 March [1880]. CD's brother, Erasmus Alvey Darwin, had subscribed £25 to enable Torbitt to continue his blight-resistant potato experiments; see letter to James Torbitt, 6 March [1880] and n. 3.

² CD wanted Torbitt to write a report in response to his queries; see letter to James Torbitt, 10 March 1880 and n. 4.

To James Torbitt 12 March [1880]¹

Down,
March 12,

My dear Sir

Your MS. just received.² I have had a couple of sentences copied just for the sake of asking you whether the figure "75's." (scored with red) is not an erratum

for 77's., for on the next and last page you describe how much more luxuriantly the 77's appeared than the 75's. When you speak of the 75's having undergone 4 years selection and the 77's only 2 years selection, I suppose that you refer to selection of the tubers and not to successive generations by seeds.

My dear Sir | Yours sincerely | Ch. Darwin

Copy
DAR 148: 115

- ¹ The copyist wrote the date as 'March 12, 1880', but someone, probably Francis Darwin, bracketed '1880' in pencil; this probably means that 1880 was not in the original text but is nevertheless correct.
- ² Torbitt was preparing a report on his potato-breeding experiments in response to CD's queries (see letter from James Torbitt, 12 March 1880). Torbitt evidently sent part of the report before completing it; see letter to James Torbitt, 17 March [1880]. For CD's queries, see the letter to James Torbitt, 6 March [1880], and the postcard to James Torbitt, 11 March [1880]. Torbitt had been carrying out his experiments since 1873 and corresponding with CD about them from 1876 (see Torbitt 1876 and *Correspondence* vol. 24). Torbitt's manuscript was returned to him; see letter to James Torbitt, 28 March 1880.

From Arthur Nicols 15 March 1880

11. Church Row | Hampstead— | N.W.
March 15th 1880

D^r Chas: Darwin,
Dear Sir,

Although that which is a new observation to me is likely to be quite familiar to you, I venture to send you some of the tail coverts of a male pheasant, which simulate, especially in the small pseudo "ocelli", the corresponding feathers of the peacock. The bird was killed just at the end of the season, and was in magnificent nuptial plumage, having the white ring on the neck and being, as I should judge from his spurs, three years old.

Comparing him with examples of *P. Colchicus*, *P. versicolor*, and *P. torquatus*, he seems to share the blood of all, but the characters of the latter prevail. On the other hand he is (unlike the pure *P. torquatus*) a very large individual.¹ The point that struck me in this bird was the (to me unusual) distinctness of the pseudo "ocelli" of the tail coverts. Whether this has any significance I must leave you to decide.

Perhaps these markings should not be termed ocelli at all, but the semi-lunar green mark incloses an irregular spot differently coloured, and the barbs are separated as in the peacock's feathers.

Looking down the back of this bird I was much struck by the general resemblance of the tail coverts to those of the peacock, when not spread out.

yours faithfully | Arthur Nicols.

DAR 172: 67

¹ The feathers have not been found in the Darwin Archive-CUL. *Phasianus colchicus* is the common pheasant; *P. versicolor*, the green pheasant; *P. colchicus torquatus*, the Chinese ring-necked pheasant. CD had discussed the variation of ocelli in pheasants and peacocks in *Descent* 2: 132–51.

To [William Whitaker?]¹ 16 March 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
March 16th 1880

Dear Sir

I must send one line to thank you for thinking to send me the article on inheritance, which is a subject which always interests me.²

Dear Sir | Yours faithfully & obliged | Ch. Darwin

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

¹ The correspondent is conjectured from a note in an unknown hand at the foot of the letter ('W. Whitaker').

² The article has not been identified.

From Williams & Norgate 16 March 1880

14, Henrietta Street, Covent Garden, | London, W.C.
16/3 1880

Messrs. Williams & Norgate present their compliments and beg to inform you, that they have just received copies of the undermentioned Work, to which they take the liberty of calling your attention.

Netter, A., *De l'Intuition dans les découvertes et inventions, ses rapports avec le positivisme et le Darwinisme*. pp 8^{vo} Strasbourg 1880 3/6¹

pc

Sotheby's (dealers) (11 July 2017)

CD ANNOTATION

2.1 Netter ... *Darwinisme* 2.2] 'Please send me'² *blue crayon circled blue crayon*

¹ *De l'intuition dans les découvertes et inventions: ses rapports avec le positivisme et le Darwinisme* (On intuition in discoveries and inventions: its relation to positivism and Darwinism; Netter 1879) was published in 1879. CD's copy is in the Darwin Library–Down.

² CD probably returned this card to Williams & Norgate.

To James Torbitt 17 March [1880]¹

Down,
March 17,

My dear Sir

I write now only one word to acknowledge receipt of letter of 15 and end of MS.² But you have *not* yet returned the page with some words not clearly legible and about "berries".

Can you tell me more definitely what your experiments have cost you?

Did the Bishop of Down, or any of the others, *write* anything which I could quote.³ As soon as I get your answer I will draw up my statement which, however, I must somehow make brief. In the autumn you can have more money advanced certainly up to £100 and almost certainly up to £150.⁴

In Haste | Yours — — | C. Darwin.

Copy
 DAR 148: 116

¹ The year is established by the relationship between this letter and the letter to James Torbitt, 12 March [1880].
² Torbitt's letter has not been found; his manuscript was returned to him (see letter to James Torbitt, 28 March 1880). In his letter of 12 March [1880], CD had sent comments on the earlier part of Torbitt's manuscript, a report on his potato breeding experiments.
³ Robert Bent Knox, bishop of Down, Connor, and Dromore, had grown a disease-free, red, round variety of potato from seeds Torbitt had sent him; see *Correspondence* vol. 27, enclosure to letter from James Torbitt, 15 November 1879.
⁴ CD had sent Torbitt cheques for £50 and £25 and had further donations to Torbitt's scheme in hand; see letters to James Torbitt, 6 March 1880 and 10 March 1880.

From James Torbitt [18 March 1880]¹

[Enclosure]

many varieties of the potato which are so prolific and so free from the disease, that they leave, after separating the few diseased tubers, a larger crop of sound tubers than the common old varieties give of sound and diseased tubers taken together.

Knight found (1)34 tons potatoes per statute acre.² I have found as much as (2)24 tons tubers, and (3)13 tons berries, while the average for Ireland last season according to statistics just published was 26(4) cwt. And no berries at all— now what would be thought of a crop of strawberries which was all roots and foliage and no berries?(5) And yet the capacity of sexual reproduction, is as well developed in the potato during the second and third years of life, as it is in the strawberry. Why does it fail in the 17th year of life as with the Champion?³

1 }
 2 } at the rate of
 3 }
 4 hundred weight
 5 fruit of the plant

AL incomplete
 DAR 52: E15

CD ANNOTATIONS
 1.1 many ... together. 1.3] *scored red crayon*

- 2.1 Knight] *double underl pencil*; 'Knight' *added pencil*
 2.2 berries] *underl pencil*
 2.3 was] *double underl pencil*
 2.3 cwt] *underl pencil*; 'cwt' *added pencil*
 2.3 no berries] *double underl pencil*; '?' *in margin pencil*; 'tons ?? | cwt' *interl pencil after 'no'*
Top of letter: 'has been recopied'¹⁴ *pencil*

¹ The date is established by the relationship between this letter and the letters to James Torbitt, 17 March [1880] and 20 March 1880; see also n. 4, below.

² Torbitt had previously referred to the potato experiments of Thomas Andrew Knight in the enclosure to his letter of 24 February 1878 (*Correspondence* vol. 26).

³ On the Champion potato, see the letter from James Torbitt, 12 February 1880, and the letter from T. H. Farrer, 6 March 1880.

⁴ CD had asked Torbitt to return a page of manuscript about 'berries' (see letter to James Torbitt, 17 March [1880]). Torbitt added the last two sentences and the footnotes before enclosing the page in a letter of 18 March 1880 that is now missing (see letter to James Torbitt, 20 March 1880). The page is numbered '9' and is part of a larger manuscript, the rest of which has not been found. Torbitt's footnote numbering is reproduced here in bold and his footnote markers, interlined in the original, are enclosed in parentheses.

From G. S. Ffinden 19 March 1880

Downe.
March 19.1880.

C. R. Darwin Esq^{re}.

Sir,

I beg hereby to tender my resignation as a Member of the Downe Schools' Committee as from the 2 [o] inst.¹

I will forward a cheque for the balance in hand, with the Books &c, if you will let me know to whom they should be sent.

I am, Sir, | Your obedient Servant, | G. S. Ffinden

Bromley Central Library, Local Studies Library and Archives (P123/25/2)

¹ Ffinden was vicar of Down and had been chairman of the Down School Committee. The Darwins had previously been involved in a series of disputes with Ffinden, including one over Ffinden's objection to the use of the Down schoolroom as a reading room for working men in the evenings; see *Correspondence* vol. 23, letter to J. B. Innes, 10 May [1875] and n. 6. Ffinden's resignation from the committee had been prompted by Francis Darwin, now serving on the committee, seeking the election of a new chairman, to whom Ffinden refused to be subordinate; see Moore 1985, p. 472.

To Asa Gray 20 March [1880]

Down Beckenham | Kent
March 20th

Will you be so kind as to tell me whether M: Thompson of Ipswich is right that *Ipomœa leptophylla* makes a great tuber as big as a manget-wurzel.—¹ A word on post-card w^d suffice. Petioles of Cotyledons behave partly like those of *Megarrhiza*.²

C. Darwin

ApcS

Postmark: MA 20 80

Archives of the Gray Herbarium, Harvard University (Walter Deane Autograph Collection)

- ¹ In his letter of 6 November 1879 (*Correspondence* vol. 27), Gray had recommended that CD try to obtain *Ipomoea* seeds from William Thompson (1823–1903). CD later reported that the seeds he had of *Ipomoea leptophylla* (bush morning-glory) had not germinated (letter to Asa Gray, 19 January 1880). The mangel-wurzel or mangold wurzel is a variety of *Beta vulgaris*.
- ² In his letter 19 January 1880, CD described the early growth of *Megarrhiza* from seeds that Gray had sent him. For CD's discussion of the movement and growth of seedling plants of *Megarrhiza californica* and *Ipomoea leptophylla*, see *Movement in plants*, pp. 81–5.

To James Torbitt 20 March 1880

Down,
March 20, 1880.

My dear Sir

I have received your letter of the 18th and all the documents. All the papers shall in a week or so be carefully returned to you.¹

I have sent my letter of 5 folio pages. (giving an abstract of your results and of my reasons for thinking favourably of your plan) to be copied, and it shall then be sent to Mr. Caird to be shown to those who are inclined to aid your work, and afterwards to Mr. Farrer who has gone with my niece for 6 weeks to Rome.²

I can easily add to the copy a few words about the Testimonials just received. Also if I can hear in time, about the cost of your experiments; I have said at present nearly £1000 from the commencement.³

The potatoes received: they seem very fine, but I am no judge. I have too much work in hand to undertake growing them. I will however show them to my Gardener,⁴ but nothing really succeeds without the master's eye, and I have no strength to undertake anything new.

I trust I shall give no more trouble. | My dear Sir | Yours sincerely | Ch. Darwin

Copy

DAR 148: 117

- ¹ Torbitt's letter has not been found, but for the copied page sent with it, see the enclosure to the letter from James Torbitt, [18 March 1880]. CD acknowledged receipt of the end of Torbitt's manuscript in his letter of 17 March [1880]. Torbitt's manuscript and documents were returned to him; see letter to James Torbitt, 28 March 1880.
- ² CD had written a statement in support of Torbitt's experiments to develop blight-resistant potato varieties using the report and testimonials that Torbitt had sent him; see letter to James Caird, 24 March 1880. James Caird and Thomas Henry Farrer had been raising money for Torbitt's research and CD had promised them copies of the statement (draft letter to T. H. Farrer, 9 March 1880). Farrer had married CD's niece Katherine Euphemia Farrer in 1873.
- ³ In his letter of 17 March [1880], CD had asked Torbitt about testimonials he could quote from and how much Torbitt's potato experiments had cost him.
- ⁴ CD's new gardener was William Duguid.



James Torbitt, *c.* 1860.

PRONI D3562/13

By permission of the Deputy Keeper of the Records,
Public Record Office of Northern Ireland.

From Alfred Tylor 20 March 1880

22a Queen Annes Gate | Westminster
Mar 20 80

Dear M^r Darwin

If one of your sons would come to the Anthropological Institute on Mar 23rd next Tuesday my paper on what I believe is a new law directing change of Species I should be very glad to see him.¹ I hope you have passed a good winter

A R Wallace complains of cold and the East Wind

Is it not possible that some small appointment should be found for him? He feels the labour of working for the Booksellers rather trying I fear when he is not very strong He is 57 years of age and has been much discouraged since he was unsuccessful in his application for the manager of Epping Forest in November last—²

Believe me to remain | Yours very truly | Alfred Tylor | of Carshalton

DAR 178: 200

¹ CD's sons were William Erasmus, George Howard, Francis, Leonard, and Horace Darwin. Tylor read his paper 'On a new method of expressing degree of changes of specific form in the organic world, especially referring to the development of the mind and body of man' at an ordinary meeting of the Anthropological Institute of Great Britain and Ireland (*Journal of the Anthropological Institute of Great Britain and Ireland* 10 (1881): 436); it was not published.

² Alfred Russel Wallace had told CD that he was seeking 'some easy occupation for [his] declining years with not too much confinement or desk-work'. Wallace regularly wrote reviews and articles for periodicals. CD had supported his unsuccessful application to become superintendent of Epping Forest (see *Correspondence* vol. 26, letter to A. R. Wallace, 16 September 1878, and this volume, letter from A. R. Wallace, 9 January 1880).

From Ernst Krause¹ 22 March 1880

Berlin N.O. Friedenstrasse 10. II.
den 22.3.80.

Hochverehrter Herr!

Sie werden sich gewiss darüber sehr wundern, dass Sie die deutsche Ausgabe von Erasmus Darwin immer noch nicht erhalten haben. Die Schuld liegt aber lediglich an Herrn Murray' in London, welcher meinem Verleger die Phototypie und die Galvanos vorenthält, trotzdem derselbe, wie er mir versichert, bereits vor langer Zeit den Preis für Beides eingeschendet hat. Das Buch liegt seit sechs Wochen fertig gedruckt da, kann aber nicht brochirt oder versandfähig gemacht werden, weil die auf dem Titel und im Texte erwähnten Beilagen fehlen. Wenn auch Herr Alberts zu dieser Handlungsweise des Herrn Murray einige Anlass gegeben haben mag, so kann man doch diese gegenseitige Hinderung der beiden Buchhändler der Sache wegen nicht anders als bedauern.²

Im Aprilhefte des Kosmos habe ich die beiden Artikel, welche Sie jüngst in der Nature veröffentlicht haben, und die mir leider sehr spät zu Gesicht gekommen waren, zum Abdruck gebracht. Ich bitte Sie, freundlichst entschuldigen zu wollen, dass dies ohne Quellenangabe und so geschehen ist, als ob Sie uns diese Aufsätze

direct zugesandt hätten. Da Sie uns gütigst erlaubt haben, Ihren Namen auf unsern Titel zu setzen, war es mir kaum möglich, anders zu verfahren, und ich hoffe, Sie werden mir diese kleine Entstellung wegen des guten Zweckes, diese wichtigen Beobachtungen zur Kenntniss unserer Leser zu bringen verzeihen.³

In demselben Hefte beginnt Moritz Wagner eine Reihe gegen die Zuchtwahl-Theorie gerichteter Artikel. Er ist neuerdings zu der Ueberzeugung gelangt, dass sich seine Migrations- oder wie er sie jetzt nennt,—Absonderungs-Theorie gar nicht mit der Zuchtwahl-Theorie vereinigen lasse; eine von beiden könne nur richtig sein.⁴

Mit dem Wunsche dass diese Zeilen Sie in gutem Wohlsein treffen, zeichne ich hochverehrter Herr, | Ihr | ergebenster | Ernst Krause

DAR 92: B57

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

² John Murray had published the English edition of *Erasmus Darwin* in November 1879 (*Correspondence* vol. 27, letter from Reginald Darwin, 12 November 1879); Karl Alberts's company, Ernst Günther, was publishing Krause's extended German edition (Krause 1880). In 1879, CD thought he had been treated unfairly when he paid in advance for photographs of the frontispiece for the German edition of *Erasmus Darwin*, and Alberts then cancelled the order to reduce costs; see *Correspondence* vol. 27, letter to Ernst Krause, 4 November 1879, and letter from Ernst Krause, 6 November 1879.

³ German translations of CD's short letters 'Fertility of hybrids from the common and Chinese goose' and 'The sexual colours of certain butterflies' (*Nature*, 1 January 1880, p. 207, and 8 January 1880, p. 237) appeared in *Kosmos* 7 (1880): 72–74, 77–8. The full title of the journal was *Kosmos: Zeitschrift für einheitliche Weltanschauung auf Grund der Entwicklungslehre in Verbindung mit Charles Darwin und Ernst Haeckel* (*Kosmos*: journal for uniform worldview based on the theory of development as proposed by Charles Darwin and Ernst Haeckel). See also *Correspondence* vol. 27, letters to *Nature*, 15 December [1879] and 16 December 1879.

⁴ Wagner argued that different species evolved as a result of geographical segregation (Wagner 1880). CD had previously discussed his objections to Wagner's theory; see, for example, *Correspondence* vol. 16, letter to August Weismann, 22 October 1868, *Correspondence* vol. 24, letter to Moritz Wagner, 13 October 1876, and *Correspondence* vol. 26, letter to C. G. Semper, 30 November 1878.

To Ernst Krause 23 March 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Mar 23 1880

My dear Sir,

I am exceedingly sorry to hear about the delay & have written by this post to Mr Murray. I feel sure that it has not been through any wilful neglect. Perhaps it may be due to the whole photographic establishment having been lately burnt to the ground.¹ As soon as I hear I will write again. It never crossed my mind that you would care about my two short letters to *Nature*; otherwise you may rely on it that I would gladly have sent you copies when they were published.²

I fear that you will hate the name of Erasmus you have had so much trouble with the whole subject.³

My dear Sir, | Yours sincerely | Charles Darwin

LS

The Huntington Library (HM 36204)

- ¹ See letter from Ernst Krause, 22 March 1880 and n. 2. Krause had reported that John Murray was withholding a phototype and electrotypes and holding up the publication of the German edition of *Erasmus Darwin* (Krause 1880). CD's letter to Murray has not been found. The London premises of the Autotype Company, which produced the images for *Erasmus Darwin*, were totally destroyed by fire on 8 December 1879 (*The Times*, 17 January 1880, p. 3).
- ² 'Fertility of hybrids from the common and Chinese goose' and 'The sexual colours of certain butterflies' (*Nature*, 1 January 1880, p. 207, and 8 January 1880, p. 237); see letter from Ernst Krause, 22 March 1880 and n. 3.
- ³ Krause had written the second part of *Erasmus Darwin* and expanded it for the German edition (Krause 1880).

From E. S. Morse 23 March 1880

Salem Mass.

Mar 23rd 80

My dear Sir.

I have been much annoyed by a review of my Omori Mound Memoir in the pages of *Nature* March 12th by a Mr. Dickins. I had hoped that the Editors of that journal would have at least done me the simple justice of placing my Memoir in the hands of some Archaeologist. I am sure that Mr Dickins has never made a contribution either on Archaeology, Zoology or Ceramic studies.¹

I do desire above all things a fair review from one competent to judge the leading points of the work.

I dislike to defend my work against such an ill spirited and untruthful review as this one of Mr Dickins I write fully aware of your precious time, yet thinking that possibly you might induce some one to notice the leading features of the memoir, namely the full illustration of the pottery tablets etc. the evidences of Cannabilism: which none dispute. the remarkable change in fauna which you so kindly wrote to me about.²

I desire such a review especially for those kind japanese friends who take "Nature" and who published the memoir for me at great trouble and expense.

I take the liberty of sending you my Review of Mr Dickin's article thinking that if you sent it to *Nature* it might more promptly or likely appear.³

Asking you to excuse this intrusion | I remain | with profound respect | Very faithfully y^{rs} | Edw^d S. Morse.

[Enclosure]

IN NATURE, vol. xxi. p. 350, is a review of my memoir on "The Omori Shell Mounds" by Fredk. V. Dickins.⁴ I do not now heed the spirit in which it is written, nor would I deem it worthy of notice did it not occur in the pages of your widely-read magazine. One expects in a reviewer some knowledge of the subject he reviews. Mr. Dickins, by a series of mistakes, betrays his ignorance of the whole matter. The extraordinary blunder he makes regarding the Ainos has already been promptly corrected by a Japanese gentleman residing in London.⁵ It is charitable to assume that Mr. Dickins has not lived in Japan, otherwise he would not, in common with

so many of his countrymen, commit the wilful blunder of calling the principal city of the empire by its wrong name.⁶ On the other hand, it is impossible he could have seen the Omori deposits, otherwise he would not make another blunder by expressing his belief that they have been completely swept away, when in truth but a small portion of them have been removed. He says: "These mounds consist for the most part of shells, little, if at all, distinguishable from what are still found in abundance along the shores of the Gulf of Yedo". Had he taken the trouble to read the memoir he attempted to review he would have seen that all the species occurring in the mounds vary in size, proportion of parts, and relative abundance of individuals from similar species living along the shores to-day. That some species extremely abundant in the mounds are scarcely met within the vicinity, while one species has never been found within 400 miles of Omori; indeed, it belongs to a different zoological province!

His complaint at the large number of plates given to the illustration of pottery, tablets, &c., shows how incapable he is of appreciating that part of the work which has received the highest commendation from archæologists, namely, the presenting as far as possible an exhaustive illustration of every form of vessel and variety of ornamentation. He laments the absence of a plate giving figures of the bones and shells, especially of the latter, which are stated to belong to extinct species. Had he looked at the last plate (a copper plate, by the way, and not a lithographic one, as he calls it) he would have seen every species, with one exception, figured, when similar forms from the neighbouring shores could be got for comparison.⁷

I did not feel justified in comparing shell-mound forms with similar forms from Niigata, Kobe, or Nagasaki, and the reason will be obvious to anyone having the slightest familiarity with the variations that species show in widely separated localities. As to figuring fragments of bones, I did all that my limited knowledge of mammalian osteology would permit in identifying the common mammals, and in giving a list of them as other writers have done in similar investigations. Possibly Mr. Dickins may here find a fruitful field for investigation, in which he may establish the recent nature of the deposits. I cheerfully proffer to him a large accumulation of fragments of bones in Tokio waiting to be put together!

His comparison of the Omori pottery with Banko will greatly amuse anyone at all familiar with Banko, or its associate forms, Hansuki, Otagakuan, Miki, Bashodo, Tokonabe, or their imitators either ancient or modern.

His review being thus occupied with a series of misstatements, he naturally finds no room to discuss my evidences of cannibalism or platycnemic tibiæ.

Finally, his ungenerous complaint of my well-merited compliment to the Japanese printers and binders who made the pamphlet, illustrates a lamentable but too common trait of the ordinary Briton in Japan, namely, that which manifests itself in a childish delight at the failures of the Japanese and in sneers at their successes.

EDWARD S. MORSE

Salem, Mass., U.S., March 25

DAR 171: 247; *Nature*, 15 April 1880, pp. 561-2

- ¹ Frederick Victor Dickins's review of Morse's *Shell mounds of Omori* (Morse 1879) was published in *Nature*, 12 February 1880, p. 350. The editor of *Nature* was Joseph Norman Lockyer. Dickins was a naval surgeon who collected plants and translated Japanese works (*ODNB*).
- ² See Morse 1879, plates 1–15, and pp. 17–19. CID had commented on a proof-copy of Morse 1879 (see *Correspondence* vol. 27, letter to E. S. Morse, 21 October 1879).
- ³ The original enclosure has not been found; the text has been transcribed from the published version in *Nature*, 15 April 1880, pp. 561–2. For CID's covering letter, see letter to *Nature*, 9 April [1880].
- ⁴ See n. 1, above.
- ⁵ In a note in *Nature*, 19 February 1880, p. 371, Shigetake Sugiura said that Dickins was incorrect in arguing that Omori heaps found Eastern region of the main island were works by an Aino race dating to the thirteenth or fourteenth century because Ainos were expelled from the area long before.
- ⁶ In *Nature*, 12 February 1880, p. 350, Dickins had used 'Yedo', a romanised form of the former name for Tokyo.
- ⁷ See Morse 1879, plate 18.

To James Caird 24 March 1880

To J. Caird Esq C.B.

My dear Sir

I enclose a statement for those who have generously subscribed in aid of Mr Torbitts experiments, giving an account of what he has already done & the reasons which make me think favourably of the plan. The statement is as much condensed as I c^d make it & has been drawn from various documents sent me by Mr Torbitt, & confirmed by articles in the Belfast newspaper., not written by him.¹ Anyone who will read the statement will be able to form as good a judgment as I can do how far the experiments are worth carrying on.— I have not thought it worth while to enclose a bundle of letters & testimonials from farmers & others with respect to the varieties of the potato given to them by Mr T. as they possess but little interest. If however, you or anyone w^d like, to see them, they shall be forwarded

(I am *extremely* sorry to trouble you, on one point which perplexes. I have sent Mr T 75£ (50 from self & 25 from my brother) & he now writes that he has just made an arrangement with some farmer who will grow his *older* varieties free of charge & give him $\frac{1}{2}$ the product; & this arrangement will save him some expense.² Therefore he writes [peremptorily] that he shall not want any more money, until the autumn. I hold 25. from Mr Farrer & 60£ collected by me (viz. 25£ from Ch. Morrison— 25£ from Mr A. M— 5£ from you & 5 from Sir J. Goldsmid) Now what I do with this 60£?— shall I send you a cheque for the amount, or retain it until the autumn (sending you a receipt [3 words illeg]) when a part or the whole shall be returned? Mr T. now thinks that 150£ will be ample, & I have collected 180, but this includes a promise of 10£ from my brother-in-law, Mr W. from whom I sh^d like to take only 5£.³

Will you be so kind as to send me your instructions whenever convenient in regard to the 60£.—

I will beg one other favour, viz to let Mr Farrer see this letter & the Statement on his return from Rome;⁴ it will save me some time, as I am occupied with [illeg] work

Thanking you cordially for all your kindness, I remain my dear Sir | yours sincerely | C. D.

J. Caird Esq | Mar 24th 1880

[Enclosure]

(Statement with respect to M^r Torbitt's experiments on the Potato.)

M^r Torbitt of Belfast asked me in 1875, whether I thought favourably of a plan which he had already commenced for combating the potato disease.⁵ His plan was to raise many thousand seedlings, to destroy all which were tainted, to preserve the varieties which seemed to resist best, to raise fresh seedlings from them, & so onwards for successive generations. In fact he intended to apply to the potato the principle of Selection which has yielded such wonderful results with animals in the hands of British agriculturists. This plan appeared to me hopeful, because with plants long cultivated every or almost every character becomes more or less variable; & there seemed no extreme improbability in a variety arising which from the structure of its tissues or nature of its juices might successfully resist the fungus. We have analogous cases in certain species & *varieties* of the American vine having naturally arisen which can resist the Phylloxera; in certain varieties of the apple not being attacked by the Coccus both in England & Australia; & in some varieties of the Peach resisting mildew better than others. Sir J. Hooker informs me that the Liberian coffee withstands the White fly better than the other kinds, & he has urged cultivators of Coffee & Tea not to rely on any single variety.⁶ We see something of the same kind even with man, in different individuals being more or less liable to certain infectious germs; & this liability is said to run in families. It may, however, be justly opposed that as the fungus of the potato attacks at least one other distinct species of the genus, namely the Tomato, it is very unlikely that a fungus-proof variety of the potato should arise. On the other hand the probability of such a variety arising is greatly strengthened by the notorious fact that the already existing varieties differ much in their liability to be attacked; & this has held good in a conspicuous manner with the many new varieties raised by M^r Torbitt.

In my first letter to M^r Torbitt, I suggested to him the advantage of cross-fertilising his seedlings in each generation, as I knew by experience that this would largely increase their fertility & constitutional vigour; the latter being shown by such seedlings resisting sudden changes of temperature & obtaining food from soil clogged with the roots of other plants.⁷ M^r Torbitt has acted on this suggestion, with, as he believes favourable results.)

M^r Torbitt has worked on a large scale, & this alone offered a fair chance of success; for he has raised each year about 5000 seedlings, & latterly has had ten acres of land under cultivation for his varieties. He has raised two successive generations from selected & cross-fertilised plants, & this year intends to raise a third generation. He informs me that the cross-fertilised seedlings of 1877 were "unquestionably more

free from the disease than those raised in 1875.”⁸ The difference between the same two sets of plants in 1879 was so great, that it could be perceived at the distance of nearly a mile; as the leaves of the 1875 plants were of a paler colour & had not grown sufficiently to conceal the ground. The leaves of the twice-crossed young seedlings of 1879 suffered much from the disease; & this is a bad feature in the case, but as far as M^r Torbitt can judge the tubers of some of the varieties are quite sound. Looking to all the varieties which he has raised, he states positively that many of them “are so prolific & so free from the disease, that they leave, after separating the few diseased tubers, a larger crop of sound tubers than the common old varieties of sound & diseased tubers taken together”.⁹

I append a statement by M^r Torbitt dated Nov^r. 1879, & printed for private circulation. He has sent me several letters & testimonials, all more or less favourable, with respect to the earlier varieties; & 8 additional ones are dated 1878 & 1879.¹⁰ In some of these it is stated that certain vars. are quite free of the disease. He endeavoured last autumn to get two well-known agriculturists to report on his crops; but they could not spare the time. He hopes to succeed in getting authenticated reports this autumn.

Finally M^r Torbitt estimates as nearly as he can, that his experiments have cost him, since their commencement, nearly 1000^l. He has given away new varieties largely, & has realised only a few pounds by the sale of superfluous tubers. He has received no pecuniary aid (before the present spring) except 100^l which I sent him in March 1878, & I here mention this as evidence that to the best of my judgment his plan is hopeful.¹¹ No one who has not carried on analogous experiments can be aware of the time & labour requisite for selecting, keeping separate & labelling numerous varieties,—for cross-fertilising the flowers, saving the seed, raising seedlings & potting them in a greenhouse &c. M^r Torbitt cannot afford to continue his experiments any longer without some aid; & it appears to me that it would be a great misfortune should the labour already taken be almost thrown away. According to all analogy the chance of raising sound varieties will be stronger in each successive selected generation. It is more especially desirable that the experiments should be continued, because there is reason to believe that lately raised varieties resist the disease better than those which have been long cultivated; so that until some of M^r Torbitts new varieties have been tested for a few more years, it is impossible to rely on them with confidence.

Charles Darwin

March. 24th 1880 | Down Beckenham Kent

P.S. It may be well to add that I am not personally acquainted with M^r Torbitt, but from our correspondence I have formed a high opinion of his probity.—

My letter←M^r Caird

ADraftS

DAR 52: E7a, E9–14, E16v

- ¹ In his letter to James Torbitt, 20 March 1880, CD said that he was having his statement in support of Torbitt's experiments to grow blight-resistant potatoes copied before sending it to Caird and that he had received all the documents in support of the scheme from Torbitt. For Torbitt's recent reports, see *Correspondence* vol. 27, enclosures to letters from James Torbitt, 4 November 1879 and 15 November 1879. For the Belfast newspaper articles, see the letter to T. H. Farrer, 5 March 1880 and n. 3.
- ² CD's brother was Erasmus Alvey Darwin. See letter to James Torbitt, 6 March 1880, and letter from James Torbitt, 12 March 1880. The letter from Torbitt about his arrangement with the farmer has not been found.
- ³ Thomas Henry Farrer had given £25 to Torbitt's scheme, Charles and Alfred Morrison £25 each, Caird £5 and Julian Goldsmid £5; see letter from T. H. Farrer, 8 March 1880 and enclosure. CD's brother-in-law was Hensleigh Wedgwood; see letter to T. H. Farrer, 7 [March 1880].
- ⁴ Farrer was in Rome for six weeks; see letter to James Torbitt, 20 March 1880 and n. 2.
- ⁵ The first extant letter from Torbitt to CD is dated 24 January 1876 (*Correspondence* vol. 24); it referred to an article on potato cultivation that Torbitt had published in 1875 (Torbitt 1875).
- ⁶ This sentence is taken almost verbatim from CD's earlier letter of support (*Correspondence* vol. 26, enclosure to letter to T. H. Farrer, 7 March 1878). *Vitis* is the genus of grapevines; phylloxera (*Daktulosphaira vitifoliae*) is a small sap-sucking insect native to North America, accidentally introduced in the mid nineteenth century to Europe, where it devastated native grapevines because it attacked the roots. In American vines, the insect usually only affected the leaves. *Coccus viridis* is a soft scale insect that is hosted by apples and other fruits and vegetables. *Podosphaera* is a genus of fungi that causes powdery mildew in peaches and other rosaceous plants. For Joseph Dalton Hooker's statements about coffee varieties resisting woolly whitefly (*Aleurothrixus floccosus*), see *Correspondence* vol. 26, letters from J. D. Hooker, 2 March 1878 and 12 March 1878.
- ⁷ CD's first known letter to Torbitt was about what constitutes an individual; see *Correspondence* vol. 24, letter to James Torbitt, 26 January 1876. For CD's advice on cross-fertilising plants, see *ibid.*, letter to James Torbitt, 14 April 1876.
- ⁸ For a report of Torbitt's improvements since 1875, see *Correspondence* vol. 26, enclosure to letter from James Torbitt, 24 February 1878.
- ⁹ See enclosure to letter from James Torbitt, [18 March 1880].
- ¹⁰ For Torbitt's printed statement, see *Correspondence* vol. 27, enclosure to letter from James Torbitt, 15 November 1879. The statement included a testimonial from one grower; CD had received more testimonials but he returned them to Torbitt; see letters to James Torbitt, 20 March 1880 and 28 March 1880.
- ¹¹ The letter from Torbitt about his costs to date has not been found. CD sent a cheque for £100 with his letter to James Torbitt, 4 March 1878 (*Correspondence* vol. 26).

To Asa Gray 24 March [1880]¹

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

March 24th

My dear Gray

I thank you much for the 2 seeds of Megarrhiza: I hope that they may germinate for I sh^d. very much like to see a longitudinal section & the proportion of the parts, when the (apparent) root is only $\frac{1}{2}$ or $\frac{1}{3}$ of inch out of the seed-coats.²

You must not suppose that what is obvious to you is so to me; for as the confluent petioles of the Cots. of the Delphinium are not tubular at first, I was astonished to see the young leaves coming out of a hole or slit at their base.³

Very many thanks for all your information about the Megarrhiza, the germination of which has interested me greatly.—⁴

I was much amused by your little article on the Philadelphia lawyer. You are a first-rate hand in touching up a fool.—⁵ The lawyer is a cool man in trying to make me out a rogue; but this seems the fashion & according to M^r S. Butler in

the Athenæum I am a rogue of the deepest dye, because I forgot to state that D^r Krause had altered his article on Erasmus Darwin before sending it to England for translation.⁶

Ever yours sincerely | Ch. Darwin

Archives of the Gray Herbarium, Harvard University (130)

¹ The year is established by the relationship between this letter and the letter from Asa Gray, 11 March 1880.

² Gray had sent seeds of *Megarrhiza* with his letter of 11 March 1880.

³ *Delphinium nudicaule* (red larkspur); see letter from Asa Gray, 11 March 1880 and n. 2.

⁴ See letter from Asa Gray, 11 March 1880 and nn. 4 and 5.

⁵ Gray's anonymous review of T. Warren O'Neill's *Refutation of Darwinism* (O'Neill 1880) appeared in the *Nation*, 4 March 1880, p. 182; see letter from Asa Gray, 11 March 1880 and n. 3.

⁶ Samuel Butler published a letter in the *Athenæum*, 31 January 1880, p. 185 (see letter to H. E. Litchfield, 1 February [1880], enclosure 1), comparing Ernst Krause's original German essay on Erasmus Darwin (Krause 1879a) with the translated version for *Erasmus Darwin*. Butler suggested that some of the new material made critical reference to Butler 1879 and blamed CD for not acknowledging its target.

From James Caird 25 March 1880

8, *Queen's Gate Gardens*. | S.W.

Mar 25. 80

My dear Sir

I am extremely obliged to you for sending me such full particulars of M^r. Torbitts experiments.¹ I hope soon to be able to explain to the several gentlemen interested all that you have so kindly communicated. The scale on which the experiment is carried on is much larger than I imagined—and one can more easily understand the need of pecuniary help. As to the £60— sent by me. (which you may count as £50—from the Messrs Morrison & £10— from me— Sir Julian Goldsmid not having paid his—we shall keep him in reserve for a future application if need be:) pray retain, till you think proper to make use of it for the object in view. You need not send any other receipt than the mention you make of it in your letter.²

If M^r. Torbitt succeeds in getting a plant more than commonly capable of resisting the fungus we shall be all well repaid—& my friends & myself are perfectly satisfied to follow your lead in the matter.

M^r. Mulholland, M.P., a friend of mine, who lives near Belfast, has kindly undertaken as soon as his election is over, to visit M^r. Torbitts experimental fields, and report the result.³ This I shall communicate to you.

I notice that M^r. Torbitt promises that his vigorous potatoes will enable the British farmers to compete successfully with America in all agricultural live stock! If he can give us a potato that will supply a sound vegetable for our people we shall be quite content.⁴

Believe me Dear Sir | Very sincerely yours | James Caird

Charles Darwin Esq

DAR 16i: 4

- ¹ James Torbitt; see letter to James Caird, 24 March 1880.
² Charles and Alfred Morrison and Julian Goldsmid; see letter to James Caird, 24 March 1880 and n. 3.
³ John Mulholland was MP for Downpatrick.
⁴ For Torbitt's hope about competing successfully with the United States 'in the production of beef, pork, butter, and cheese', see *Correspondence* vol. 27, enclosure to letter from James Torbitt, 15 November 1879.

From C. C. Graham 28 March 1880

Respected Sir,

Your letter to me acknowledging the reception of the little book I sent you has been beautifully framed and hangs in the fire proof gallery of the Kentucky State House at Frankfort where it may be read for centuries after we are gone to that unknown land.¹ We have all your writings in our great Public Library at Louisville of which I am a trustee. No answer requested.

Kindly and sincerely, | C. C. Graham, M.D.

Louisville, Ky. | March 28th, 1880.

DAR 201: 12

- ¹ Graham had sent a book (possibly Graham 1869) with his letter of 30 January 1877 (*Correspondence* vol. 25). No letter from CD to Graham has been found.

To James Torbitt 28 March 1880

Down,
 March 28, 1880.

My dear Sir

I am obliged for the correction of sum expended and have asked Mr. Caird to correct the figures in my Statement.¹ Mr. Caird has written and seems satisfied with what I have said of your method and the results hitherto attained. He says "If Mr. Torbitt succeeds in getting a plant more than commonly capable of resisting the fungus we shall all be well repaid, and my friends and myself are perfectly satisfied to follow your lead in the matter".² I can now *positively* promise to send in the autumn £90 besides the £75 already sent; but I shall be very glad if you can manage to draw rather less, as I have told Mr. Caird's friends that I believed you could do with less.³ Good fortune attend your efforts.

Yours sincerely | Ch. Darwin.

P.S. I now return all the documents together with your MS. which you had better preserve, as it may come in useful.⁴

Copy
 DAR 148: 118

- ¹ CD had estimated Torbitt's expenses on potato experiments at nearly £1000 (see enclosure to letter to James Caird, 24 March 1880. The letter from Torbitt amending this amount has not been found.

² See letter from James Caird, 25 March 1880.

³ See letter to James Caird, 24 March 1880 and n. 3.

⁴ Torbitt had sent CD a report and testimonials on his breeding experiments.

To C. W. Fox 29 March 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
March 29th 1880

My dear Mr Fox

I had heard that your Father was out of health, but had not the least idea that he was seriously ill. Your letter grieves me much & you all have my deep sympathy.¹ It has touched & gratified me much that your Father should have thought of me at such a time, but he was always full of sympathy?

I saw a great deal of him in old days at Cambridge, & we used to breakfast together daily.² In the course of my life, now a long one, I can truly say that I have never known a kinder or better man.— I can therefore feel what a loss he will be to you all. I gather from your letter that he does not now suffer much, & this is some comfort.

Believe me my dear Cousin, for we are cousins though in a remote degree, that I am grateful to you for having written & I remain | Yours very truly | Charles Darwin

University of British Columbia Library, Rare Books and Special Collections (Pearce/Darwin Fox collection RBSC-ARC-1721-1-12)

¹ Fox's letter has not been found; his father, William Darwin Fox, was CD's second cousin.

² CD and Fox had been undergraduates at Christ's College, Cambridge; see *Correspondence* vol. 1.

To James Torbitt 30 March 1880

Down,
March 30, 1880.

My dear Sir

I send rough copy of my letter, which please return, as I have no other.¹ I do not think it would be worth your keeping a copy, but of course you can if you like. I have a copy by this post to Mr. Farrer.² Sir J. Hooker writes to me that he has sent letter to Mr. Farrer, telling him that he thinks your plan deserves as the best, but I have not seen the actual letter.³ I have told Farrer that we will agree to whatever he or Mr. Caird thinks best.⁴

With all good wishes and in haste | Yours sincerely | Ch. Darwin.

Copy
DAR 148: 119

¹ CD probably sent a copy of his statement of support for Torbitt's potato experiments (see enclosure to letter to James Caird, 24 March 1880).

² For Thomas Henry Farrer's earlier support for Torbitt's scheme, see the letter to T. H. Farrer, 9 March 1880.

³ The letter from Joseph Dalton Hooker has not been found.

⁴ For James Caird's positive assessment of Torbitt's experiments, see the letter from James Caird, 25 March 1880. The two gaps indicate where the copyist could not read the text of the original letter.

From Asa Gray [1 April 1880]¹

Yes, *Ipomœa leptophylla* makes a root as *big* as a *man*. So does *Ipomœa pandurata*,—which, I *believe*—but dont know—germinates normally,² That must be looked to. If it has the same trick it would well confirm your notion of the meaning of the thing.³

A Gray

Postcard

Postmark: APR 1

DAR 209.6: 203

CD ANNOTATION

1.1 *pandurata* 1.2] '*pandurata*' *above ink*

¹ The year is established by the relationship between this letter and the letter to Asa Gray, 20 March [1880].

² See letter to Asa Gray, 20 March [1880]. CD's own seeds of *Ipomœa leptophylla* (bush morning-glory) had not germinated (letter to Asa Gray, 19 January 1880). Like *Ipomœa pandurata* (man-of-the-earth or wild potato vine), it is native to North America; both species, which are perennial, have large tuberous roots. Gray's comment about the manner of germination refers to earlier correspondence about *Megarrhiza californica* (a synonym of *Marah fabacea*, California manroot). CD had described his observations of the germination of plants of this species, noting that soon after germination the growth of the radicle was arrested and the tubular petioles penetrated the ground to a depth of $\frac{1}{2}$ inches beneath the surface (letter to Asa Gray, 19 January 1880).

³ CD had hypothesised that the tubular petioles of *Megarrhiza* acted functionally like a root in order to hide the (true) enlarged root from predators during the early stage of its development (letter to Asa Gray, 19 January 1880).

From James Torbitt 1 April 1880

J. Torbitt, | Wine Merchant. | 58, North Street, | Belfast
1st April 1880.

Charles Darwin Esqr. | Down

My dear Sir

I have received back all the documents together with your letter of 28th ulto. and can only repeat my thanks.¹

I am flattered by Mr Caird's opinion, and hope to show, next autumn, some varieties absolutely fungus-proof in the foliage as well as in the tubers.²

I have to day induced Sir Richard Wallace to grow to the extent of an acre, I to have placed at my disposal one half the produce.³

He is about the fifteenth landowner who is growing the potato for me on those terms.

I am growing six acres, and about 2,500 seedlings twice crossed, and a few seeds of twice crossed seedlings, themselves possibly thrice crossed.

I shall keep the expenditure under £150 or less if you wish, and hope to report again in a few days.⁴

Most respectfully & faithfully | James Torbitt

DAR 178: 164

¹ See letter to James Torbitt, 28 March 1880. On the documents, see letter to James Caird, 24 March 1880 and n. 1.

² James Caird had told CD: 'If Mr. Torbitt succeeds in getting a plant more than commonly capable of resisting the fungus we shall all be well repaid' (see letter from James Caird, 25 March 1880, and letter to James Torbitt, 28 March 1880).

³ Wallace's Irish estate was at Lisburn, near Belfast.

⁴ CD had told Caird's friends that Torbitt could conduct his experiments without using all of the money that was subscribed (see letter to James Torbitt, 28 March 1880 and n. 3).

From S. M. Herzfeld 2 April 1880

51. St. Thomas' Road, | Finsbury Park. | London
April 2. 1880.

Professor Darwin,

I applied to you two years ago. A stranger and a foreigner, I then had no other reason for doing so, than the high opinion such greatest men, both in France and in Germany, as are best able to appreciate the immense step forward your genius caused science to make, have conceived of you. I have devoted my whole life to the natural sciences, but I must only revere you as a pupil would a greatest and most beloved teacher; a pupil too humble to allow himself to bear a judgment on a man of your description. Well the opinion of those greatest men made me think, that your heart must be as good as your mind is vast and noble. I was not mistaken.¹ And now after having done all a gentleman can do to help himself, I apply again to you, under the following circumstances.

The subscription two years ago was to help me, but the letter of Professor J. H. Gladstone will tell you that it left me where I was, for when one month after you had put your name to it, I stopped it myself, and wanted to go to France, I was in such distress that I had to apply to him for the means to leave this country.² I am a German Doctor but having also a highest degree of the French University I went to France to apply for a professorship. The noble-minded and best-hearted senator once Minister of public instruction "Jules Simon" did all he could but I, unfortunately could not wait.³ I lectured on different natural-scientific subjects, returned again to England, and left it again with my library and apparatus, twenty-two large boxes, weighing more than two tons. 17 of these boxes, are still in the custom-house at Amsterdam, for I could not pay the charges on them about £35, and they will be sold, if in the shortest possible time, I do not redeem them. If they are sold, I shall lose my apparatus, without which I could not lecture, or work, and also the greater part of my library. I should lose what would cost me more than £200 if I had to buy it again. Besides these 17 boxes, I had five boxes containing the botanical part of my library. These five boxes I had with me for I intended to

lecture on exotic botany at Cologne. I send you the prospectus, because there is a very beautiful recommendation of myself and the lectures, Professor D^r. Schellen wrote on the second page of my prospectus.⁴ Cologne, where I wished to deliver these lectures, is unfortunately not a town, which encourages scientific pursuits, and I did not succeed. I was compelled to return again to England. These five boxes, are in London with Sutton & Co, 17 Aldersgate S^r. City, only £5 are the charges, but I have not been able to redeem them and they have been there the last five months; and all that time, I have again been in very great distress and without a book, and even without the greatest part of my clothes and linen etc. These boxes too are in danger, the warehousing will eat them up. The money-lender has my watch and all I could pledge, for I do not apply to others, if I can help. I want £12 and I applied to a friend, Gathorne Hardy (now Viscount Cranbrook)⁵ I send you his letter.

Professor Darwin, these circumstances of bitter distress and danger must excuse me, if I apply again to you. I know I have not the slightest claim nor right but I love you as a most grateful pupil can love a best teacher. I may also say that all your works, and it was with great difficulty I bought them are; at least, the botanical ones, in the five boxes I have here in London at Sutton's & Co. If I had £50 I could save my 17 boxes at Amsterdam, and also all I have in London, but I cannot ask so large a sum, though it would save as it were, my future, for I naturally want my chemical and other apparatus and books; but for the present, it would already be a great benefit, if I had £12 or £15, to redeem the botanical part of my library and my clothes which are at Sutton & Co here in London, and other things which are with the money-lenders and without which I cannot do.⁶ M^r. Jules Simon has recommended me to one of his once Inspectors of public instruction. I hope, I shall soon have occupation till then I must continue in distress, but it would be very bitter if I lost all I have, and so put in danger my future, as then I could hardly accept occupation even if I found it.

It is only such deepest distress which can excuse my applying to you, I know that, but, what can I do?

Professor Darwin, whatever your decision might be, I pray you would be as kind as to return the papers I enclose.⁷ Time must be of immense importance to me. Is it not so?

I might say that as soon as God helps, I should be happy to return any money I might be helped with; but, of course, I do not ask you to lend me money; for I know if you help, you help.

I have the honor to be | Professor Darwin | your most thankful servant | and pupil | S. M. Herzfeld | Doctor etc

51. S^t. Thomas' Road | Finsbury Park. London.

DAR 166: 191

¹ No earlier correspondence with Herzfeld has been found, but on 18 January 1878, CD recorded the payment of £5 to Herzfeld under 'Charities' in his Classed account books (Down House MS).

² John Hall Gladstone, who had studied chemistry in Germany, was known for his philanthropic endeavours, especially his work with the Young Men's Christian Association (*ODNB*).

- ³ Jules François Simon had been minister of instruction in the government of national defence of the Third Republic of France in 1870 (*EB*).
- ⁴ The prospectus has not been found; Thomas Joseph Heinrich Schellen was the director of the higher *Bürgerschule* (a city school focused on more practical education for students going into business or technical trades), Cologne (*ADB*).
- ⁵ Gathorne Gathorne-Hardy.
- ⁶ On 9 April 1880, CD recorded the payment of £12 to Herzfeld under ‘Charities’ in his Classed account books. Sutton & Co. was a firm of general carriers in London (*ODNB* s.v. Sutton, William Richard).
- ⁷ The papers, presumably the letters from Gladstone and Gathorne-Hardy, were evidently returned by CD (see letter from S. M. Herzfeld, 4 April 1880).

From Jules Rouquette¹ 2 April 1880

Montpellier | Rue des Balances, 25. (Hérault)
2 Avril 1880,

Illustre et honoré maître,

Etudiant en médecine à la Faculté de Montpellier, ardent à l'étude des sciences naturelles, j'ai dévoré votre oeuvre, et malgré moi, j'ai jeté sur le papier quelques vers, sous les auspices de votre grand nom.

Je vous les adresse; c'est un faible, mais sincère témoignage de mon admiration pour un si profond et si puissant observateur de la nature!

Je suis avec le plus grand respect, illustre M. Darwin, votre très humble serviteur
| J Rouquette S^t Geniez

Poésie— La Lutte pour la vie.
A. M. C. Darwin.²

Salut à toi, Darwin, salut à ton génie
Qui, longtemps, a fouillé les secrets de la vie,
Dévoilés hardiment par l'Evolution
Du Globe se pliant à la Sélection!—
Dès le jour où ta voix expliqua la nature,
Ses transformations, sa marche lente et sûre,
Et toujours progressive, un effrayant éclat
Resplendit sur ces mots: “La Vie est un combat!”
Indigne vérité pour l'homme et pour la brute!
Ah! malheur aux chétifs, car sanglante est la lutte!
Le Monde entier s'écrie: “Il faut vaincre ou périr!”
Regardez cet arbuste, on le voit se flétrir,
Car ce chêne l'étreint ... Tout tremble et fait silence,
Quand le Roi des forêts, de son antre s'élance!
La Lionne l'attend ... Mais un rugisse(m)ent
Annonce son rival! ... Alors, le(s crins au) vent,
Tous deux, vont au combat; et le plus fort se rue
Sur le faible, et bientôt le terrasse et le tue!
Au Vainqueur la Lionne accord son amour,

Voulant que ses petits quand ils naîtront au jour,
Soient vigoureux et beaux!

Vae victis!³ C'est la Guerre

Qui, sans trêve et merci, court, dépeuplant la Terre
Et qui grave partout: "Force prime le Droit!"
Inéluctable Loi qui vous saisit d'effroi;
Loi terrible, ici-bas, de l'injuste Nature!
Les faibles sont toujours des puissants la pâture!
Comme l'homme est fragile! et comme il se débat!
Tantôt contre l'été, contre un mortel climat,
Tantôt contre l'hiver, contre une maladie,
Contre l'Hérédité, contre une épidémie!!
Quelle lutte acharnée! . . . À ce prix seulement
On a droit à la vie!

Et bien plus, oh! tourment!

Le faible ne doit pas multiplier sa race,
S'il ne veut point laisser l'ineffaçable trace
De sa débilité! Qu'il périsse plutôt!
Sinon, qu'il soit maudit, s'il crée un idiot,
Un strumeux ne pouvant qu'être un traî(n)e-misère,
Au milieu de ce monde, hélas! qui dégénère,
Et va, se dégradant, grâce aux affreux excès
D'un siècle jouisseur, excitant les progrès
De la Corruption, et de l'Alcoolisme
Ce père d'une époque en proie au Nervosisme!⁴
Mais la Grande Coupable, ah! crions-le bien fort,
Est celle qui veut trop parer les coups du sort,
Cette noble science et toujours secourable,
Mais dont la Charité parfois est déplorable,
La Médecine enfin arrachant un enfant
Souffreteux à la mort!

Laissons dans le néant,

Les êtres maladifs dont la progéniture
Ne saurait être, un jour, qu'infecte pourriture;
Honte à Celui qui peut avoir l'iniquité
De procréer s'il est dénué de santé!
Nul ne doit engendrer, quand il n'est pas de taille
À mettre au monde un être armé pour la bataille!!!

J Rouquette S! Geniez⁵

Montpellier (Hérault) Rue des Balances, 25.

DAR 201: 33

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

² Rouquette's poem was published in a collection, *Joies et misères* (Joys and woes; Rouquette 1880, pp. 84–6).

³ *Vae victis*: woe to the vanquished (Latin).

⁴ *Nervosisme*: a rare term for a nervous disorder, especially neurasthenia (*OED*).

⁵ St or Saint Geniez was one of the pseudonyms Rouquette used in his writing (Bibliothèque nationale de France, http://data.bnf.fr/12443252/jules_rouquette/ (accessed 26 October 2018)).

From G. H. Schneider¹ 2 April 1880

Leipzig,
d. 2. April 1880.

Hochzuverehrender | Hochgeehrtester Herr!

Als ich im Jahre 1870 in Jena Naturwissenschaften, insbesondere Zoologie bei Herrn Professor Haeckel² studirte, erweckte die Descendenz- und Selectionstheorie ein ungemeines Interesse in mir. Ich sah sofort, dass diese Entwicklungsprinzipien für die psychischen Erscheinungen mindestens dieselbe Bedeutung haben müssten als für die morphologischen und beschloss sofort dem Studium der psychischen Phänomene vom Standpunkte der Descendenz- und Selectionstheorie aus mein Leben zu widmen.

Mein lebhaftes Interesse insbesondere für das von Ihnen entdeckte allgewaltige Lebensprinzip der natürlichen Auswahl im Kampfe ums Dasein gewann mir das Wohlwollen und die Sympathie des grossen Jenenser Zoologen, der nun meine Bestrebungen allseitig freundlichst unterstützte. Leider haben mich Nahrungssorgen jahrelang vielfach von meinen Arbeiten abgehalten. Nachdem ich nun aber fünf Jahre in Neapel gelebt und theils in dem Aquarium der zoologischen Station, theils in meinem kleinen Privataquarium Beobachtungen über die Willensäusserungen der niederen Thiere gemacht und mehrere kleinere Arbeiten theils als Broschüren, theils in wissenschaftlichen und populären Journalen veröffentlicht habe, bin ich nun endlich so glücklich Ihnen ein Exemplar meiner ersten grösseren Arbeit: "Der thierische Wille" ergebenst einschicken zu können.³ Damit ich einen Verleger dafür gewinnen konnte, musste ich freilich den Stoff zum grössten Theile in populärer Form bringen und die allgemeinen Erörterungen, in welchen der wissenschaftliche Werth liegt, auf ein geringes Maass beschränken. Beurtheilen Sie also gütigst das Buch mit Nachsicht.

Ich denke, ich habe das Problem des thierischen Willens *ganz in Ihrem Sinne aufgefasst*, indem ich, gestützt auf Ihre grossartigen und klassischen Untersuchungen, alle instinctiven Triebe wie alles zweckbewusste Wollen, resp. die Entstehung und Entwicklung desselben aus der Selection und aus successiven und simultanen Associationen erkläre.

Das Material über thierische Willensäusserungen habe ich systematisch geordnet, die Gewohnheiten zum Nahrungserwerb, zum Schutze, zur Begattung und zur Pflege der Nachkommenschaft zusammengestellt und sie nach ihrer psychologischen Werthigkeit eingetheilt:

1; in solche, welche auf Grund eines subjectiven oder durch unmittelbare Berührung hervorgerufenen objectiven Zustandes (Bewegungen auf Grund von *Empfindungstrieben*) erfolgen, 2, in solche, welche durch Wahrnehmungen der Objecte aus der Entfernung beruhen (*Wahrnehmungstriebe*) und 3, in solche, die auf einfachen

Reproductionen der Wahrnehmungen oder auf Vorstellungsverbindungen beruhen (*Vorstellungs- und Gedankentriebe*); und von diesen Erkenntnisstrieben habe ich noch zwei allein auf Bewegungsassociationen beruhende Hilfstriebe (Folgetrieb und Associationstrieb) unterschieden.

Um eine scharfe Grenze zwischen rein physiologischen und rein psychologischen Vorgängen zu haben, habe ich das Gebiet der Reflexe ganz in die Physiologie verwiesen und als reine Reflexe nur diejenigen Bewegungen bezeichnet, mit denen gar keine Bewusstseinserscheinungen, auch keine einfachen Empfindungen (d.h. das *Bewusstwerden* einer Nervenirregung) verbunden und die also rein physiologischer Natur sind; denn dadurch, dass man bisher so mannigfache Bewegungen, die eine ganz verschiedene psychologische Werthigkeit haben, als Reflexe bezeichnet hat, ist die Unklarheit sehr begünstigt worden.

Alles Triebleben, aller Instinct wie alles zweckbewusste Wollen beruht nach meiner Darlegung darauf, dass sich im Laufe der genetischen Entwicklung ganz bestimmte zweckmässige (d.h. der Arterhaltung günstige) Beziehungen zwischen gewissen Erkenntnissacten und bestimmten Gefühlen und Trieben ausbilden, so dass, wenn ein gewisser Erkenntnissact entsteht, durch dessen Associationsbeziehung auch der entsprechende Trieb hervorgerufen wird, so dass also z. B. eine bestimmte Wahrnehmung einen zweckentsprechenden Trieb und die zweckmässige Bewegung nothwendig zur Folge hat, auch wenn ein Bewusstsein des Zweckes fehlt.

Auf diese Weise finden zunächst *alle* Instincte ihre naturgemässe Erklärung aus der Descendenz- und Selectionstheorie. Aber auch die zweckbewussten Willensäusserungen, welche sich aus den instinctiven entwickeln, sind damit auf ihre Ursachen zurückgeführt.

Die ersten Associationsbeziehungen zwischen Erkenntnissacten und zweckentsprechenden Trieben sind solche zwischen Empfindungen und Empfindungstrieben z. B. zwischen dem Bewusstwerden einer angenehmen Berührung mit einem Nahrungskörper und dem Trieb zum Einschliessen desselben, oder zwischen dem Bewusstwerden einer unangenehmen Berührung und dem Trieb zur Contraction des ganzen Körpers etc.

Die Frage nach der ersten Entstehung solcher zweckmässiger Beziehungen fällt mit der Frage nach der Entstehung der ersten Organismen zusammen.

Wird nun mit einem Empfindungstrieb die *Wahrnehmung* des Objectes associirt, wird z. B. mit dem Nahrungsgenusse und dem Frestriebe, der ja auf einer unmittelbaren Berührung beruht, etwa die Gesichtswahrnehmung des Nährkörpers (sobald ein Sehorgan entwickelt ist) associirt, so vermag dann auch diese Wahrnehmung allein schon einen Trieb zum Fressen resp. zum Annähern an das Object zu erwecken; und so gehen die Wahrnehmungstrieb auf Grund von Associationen aus den Empfindungstrieben hervor. etc. etc.

Ich bilde mir ein mit dieser Arbeit ebenfalls einen nicht unwichtigen Beitrag zur Ausbaue ihres grossen Gedankens über die Entwicklung der Organismen, welcher Gedanke sicher die ganze zukünftige Philosophie beherrschen wird, geliefert zu haben.

Die specielle Entwicklung und Differenzirung der einzelnen Thiergewohnheiten (mit den Stammbäumen) werde ich in einem besonderen Buche bringen und

diesem eine grössere Arbeit über den menschlichen Willen, sowie eine solche über die thierische und menschliche Erkenntniss folgen lassen.⁴

Erlauben Sie mir, Hochgeehrtester Herr Darwin, gütigst an diese Mittheilungen eine Bitte anschliessen zu dürfen?

Da ich, wie gesagt, nur durch Ihre epochemachenden grossartigen Werke zu meinen Arbeiten angeregt worden bin und der Überzeugung lebe das Willensproblem ganz in Ihrem Geiste aufgefasst zu haben, so würde ich mich unendlich glücklich schätzen, wenn ich erfahren könnte, *wie gerade Sie über meine Arbeit denken*, wie Sie dieselbe beurtheilen; und durch eine auch noch so kurze Mittheilung würden Sie mich zu unendlichem Danke verpflichten.

Hoffentlich haben Sie mein Buch: “Der thierische Wille” sowie auch die früher an Sie abgeschickte kleine Schrift gegen Prof. Jaegers vermeintlicher Entdeckung der Seele von mir erhalten.⁵

Genehmigen Sie, Hochgeehrtester Herr, den Ausdruck der wärmsten Verehrung, mit der ich bin | Ihr | ganz ergebenster | G. H. Schneider

(Weststrasse 80 II in Leipzig)

DAR 177: 59

CD ANNOTATION

Top of letter: 'Jäger' pencil

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

² Ernst Haeckel was professor of zoology at Jena. Schneider studied philosophy at Jena but attended Haeckel's lectures (Lucidi 2009, p. 21).

³ Schneider was a teacher at the German school in Naples. ‘The cares of life’ is probably an allusion to his conflict with the director of the Zoological Station at Naples, Anton Dohrn, who had refused to grant him a place at the German table, ostensibly because of his lack of qualifications (Groeben ed. 1985, p. 298). CD's copy of *Der thierische Wille* (Animal will; Schneider [1880]) is in the Darwin Library–Down. In his introduction to the book, Schneider laid out his case against Dohrn and the funding practices of the station (*ibid.*, pp. vii–xv).

⁴ The only follow-up book to appear was *Der menschliche Wille* (Human will; Schneider 1882).

⁵ CD's copy of *Jäger's vermeintliche Entdeckung der Seele: eine Widerlegung* (Jäger's supposed discovery of the soul: a refutation; Schneider 1879) is in the Darwin Pamphlet Collection–CUL. Gustav Jäger had published an article in *Kosmos*, ‘Die Entdeckung der Seele’ (The discovery of the soul; Jäger 1878), in which he equated the specific smells emanating from an animal with its soul, and further, postulated that each individual possessed a unique odour.

To G. H. Schneider 3 April 1880

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

April 3rd 1880

Dear Sir

I am very much obliged to you for your great kindness in having sent me a copy of your “Der Thierische Wille”—. Everything about the minds of animals interests me greatly.—¹

I remain | Dear Sir | Yours faithfully & obliged | Charles Darwin

Alfred S. Posamentier (private collection)

¹ Schneider had sent CD a copy of *Der thierische Wille* (Animal will; Schneider 1880) and later sent a letter discussing his theories (letter from G. H. Schneider, 2 April 1880).

To Adolf Ernst 4 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S. E. R.
April 4 | 1880

Dear Sir

Your excellent letter has interested me much.¹ I have had much pleasure in ordering the two books to be sent to you.² It grieves me that you have so little time for observing, as I feel sure that you would make many interesting discoveries. That seems to me a good idea about punching out the glands of *Passiflora*.³ I wish you could get pollen of *Abutilona Striatum* from some other country or district as I should expect that with its aid you could raise vigorous seedlings from the weak plants long cultivated near Caracas.⁴ I still think, that if you can find the requisite time that it would be well worth while to test the fertility of illegitimate offspring from heterostyled plants. Should you hereafter be able to make any observations on the frequency of bloom-covered leaves on the dry plains I should be *particularly* glad to hear.⁵ I entirely agree with what you say about M. Bonnier's work which has now been published separately.⁶ I know nothing about *Triplaris*.⁷

Pray forgive brevity, as I have many letters to write My dear Sir | Yours faithfully
| Charles Darwin

P.S. Is not the *Turnera* defended by its ants against leaf-cutting ants or other enemies?⁸

LS(A)

State Darwin Museum, Moscow (GDM KP OF 8973)

¹ See letter from Adolf Ernst, 29 February 1880.

² See letter from Adolf Ernst, 29 February 1880 and n. 14. CD sent copies of *Coral reefs* 2d ed. and *Geological observations* 2d ed.

³ In his letter of 29 February 1880, Ernst had proposed removing the extrafloral nectaries on leaves of *Passiflora biflora* (twoflower passion-flower) to test whether fertilisation without insect aid was possible.

⁴ *Abutilon striatum* (a synonym of *A. pictum*) is painted Indian mallow. Ernst reported that the plant, which had been introduced as a single specimen from Peru, never produced fruit and seemed to be dying out.

⁵ Ernst wrote that he had not recently visited the interior plains of Venezuela, where CD hoped he would find plants with leaves covered in bloom, a waxy secretion (see letter from Adolf Ernst, 29 February 1880 and nn. 2 and 3).

⁶ Bonnier 1879b (see letter from Adolf Ernst, 29 February 1880 and n. 7); the separately published version was Bonnier 1879a. CD had earlier commented that Gaston Bonnier seemed to deny that nectaries were ever modified to encourage the visits of insects (see *Correspondence* vol. 27, letter to W. J. Behrens, 10 November 1879).

⁷ Ernst had described the relationship between ants inhabiting *Triplaris americana*, a species of knotweed, and scale insects (see letter from Adolf Ernst, 29 February 1880 and n. 10).

⁸ Ernst had described the extra-floral nectaries at the base of leaves of *Turnera ulmifolia* and observed large numbers of ants on the leaves (see letter from Adolf Ernst, 29 February 1880 and nn. 5 and 6). Fritz Müller had informed CD that ants that lived in plants of the genus *Cecropia* (embauba or trumpet tree) prevented herbivorous insects from attacking these trees (see *Correspondence* vol. 22, letter from Fritz Müller, 20 April [1874]).

From Asa Gray 4 April 1880

Herbarium of Harvard University, | Botanic Garden, Cambridge, Mass.

April 4 1880.

Dear Darwin

This just received¹

I fear that the elevation of the two seeds of my *Megarhiza* figured was unusual.²

They were placed in a pot, the bottom filled with crocks, then common potting soil: and the seeds near an inch underground. The pot 6 or 7 inches high.

Yours ever | A. Gray

[Enclosure]

San Francisco, Cal.,

March 29, 1880.

Dear Doctor Gray,—

I received your letter of the 17th. inst Saturday afternoon (the 27th.) and went immediately out to Lone Mountain where, on a sandy hill side, I found blossoming *Megarhiza* climbing over shrubby *Quercus agrifolia*.³ Under the vines were germinating seeds growing in almost pure sand. I put a few of the flowers and seeds into a cigar box which is now on its way across the continent.

Having just returned from botanizing near Niles, twenty five miles south of here, I happened to have in my plant case *Brodiaea* with forming bulbs, as well as bits of *Pellaea* &c. which I thought might be more interesting packing than moss.⁴ The two long racemes came from vines on which I could find no fertile flowers. The stem with fertile flowers must have grown from a root whose last year's growth produced, at least, two of the accompanying seeds. The seeds which have not begun to grow were found in a drift of oak leaves. The germinating seeds were covered to a depth of from one to three inches in sand as is shown by the appearance of the stems. In a few instances the seed was one or two inches to one side of the place where the sprout appeared above ground; but generally the plumule, preceded by the radicle, seems to have been pushed directly downward four to six inches by the elongation of the united cotyledon petioles, and these were split apart by the subsequent upward growth of the plumule.⁵ During the Christmas vacation I observed the germination of the same species(?) of *Megarhiza* in the Live Oaks of the Mokelumne River on the line of the Central Pacific R.R.⁶ None of the dozen or more seeds examined had sprouts more than four inches in length. All the seeds were lying on the surface of the ground (a sandy loam), and were lightly covered with leaves. In every case the sprout went directly down into the ground, and the plumule was found undeveloped near the end of the sprout which showed no signs of splitting. One seed, not so well covered as the rest, had its sprout blackened and wilted by the frost at the surface of the ground, but the underground portion seemed fresh and I doubt not the plumule would have lived. The seeds begin to grow soon after the first heavy rains; and if the plumule appeared as soon as the radicle had obtained a two or three inch hold upon the soil, as is the way with acorns, it would surely be killed by the frosts.

Some seeds which I planted a year ago in a crayon box and another shallow box invariably grew as represented in my botany; i.e., the growth was horizontal and the plumules came up four or five inches away from where the seeds were planted. Possibly the nearness of the bottom of the box—though they did not touch it—caused the sidewise growth. Possibly, too, a downward growing sprout might, when stopped by an obstacle, by its elongation push the cotyledons above ground. I cannot say certainly that the seeds I experimented with last year were of the same species as those I send you, but I think they were. Last summer, for the first I made a little effort to clear up my uncertainties regarding the several species of *Megarrhiza* but I was not successful. I then determined to begin early this season and study thoroughly. I shall secure seeds from many different localities from vines which shall have previously furnished blossoms and leaves. I shall send you specimens of all I collect.

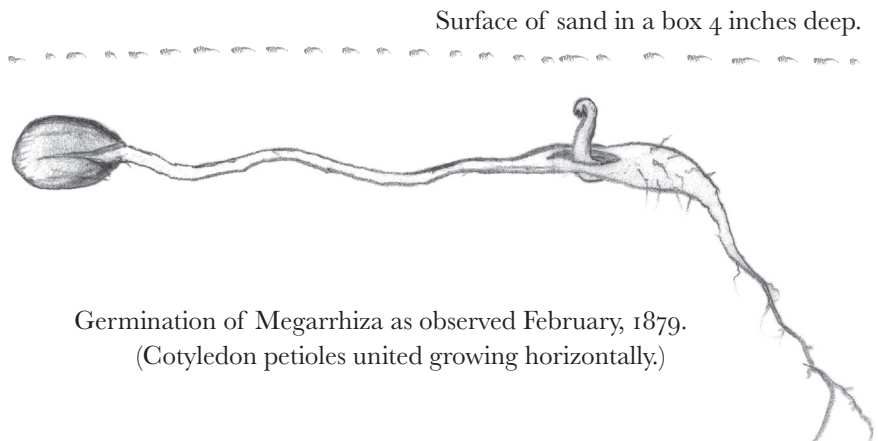
I shall try to find a bit of root in condition to grow and send it to you.

With this I send a package of *Lepidium* which puzzles me.⁷

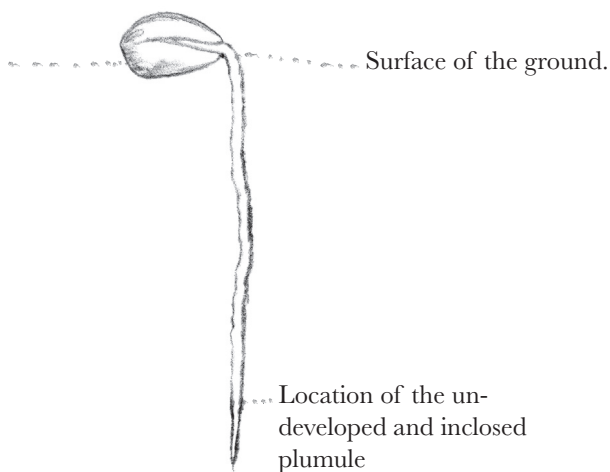
I collected it in San José a week ago. It seems to grow only on alkaline or salt flats. I found the same last year near Antioch. By-the-way I sent you in 1878 a package of *Lepidium oxycarpum*, Var. *Strictum*, Wat. of the Cal. Bot. collected in San Francisco. You did not acknowledge its receipt. I had previously sent specimens collected in San Joaquin Co. (Live Oaks) which were considered to be a possible variety of *L. Menziesii*. Isn't it a well marked species?⁸

Yours truly | V. Rattan.

Volney Rattan, | Girls' High School, | San Francisco, | Cal.⁹



Germination of *Megarhiza* seed when covered with leaves. Jan 2, 1880.



United petioles of the cotyledons growing vertically downward.

DAR 209.6: 204–6

CD ANNOTATIONS

End of letter: 'Roots of | *Ipomœa* | *I. pandurata*'¹⁰ *pencil*

Enclosure:

2.7 a depth ... stems. 2.8] *double scored pencil*

2.11 four ... petioles, 2.12] *double scored pencil*

2.12 split ... germination 2.13] *scored pencil*

2.15 seeds ... length. 2.16] *triple scored pencil*

2.17 In ... ground, 2.18] *double scored pencil*

2.20 had ... ground, 2.21] *double scored pencil*

2.23 as the ... frosts. 2.24] *scored pencil*

3.1 I ... shallow box] *triple scored pencil*

3.4 Possibly ... growth. 3.5] *double scored pencil*

¹ CD had disagreed with Gray's description of the germination of *Megarhiza californica* (a synonym of *Marah fabacea*, California manroot; see letter to Asa Gray, 19 January 1880 and nn. 1 and 2). Gray evidently requested seeds and information on the germination of the plant in its native habitat from Volney Rattan, whose response is enclosed.

² Gray had based the illustration and description in his *Botanical text-book* on *Megarhiza* grown in pots (A. Gray 1879, pp. 20–1).

³ See n. 1, above. Lone Mountain is in west-central San Francisco. *Quercus agrifolia* is the California live oak, a species associated with both *Marah fabacea* and *M. oregana* (western wild cucumber or coastal manroot; see Stocking 1955, p. 118). Gray had been unsure about the identity of seeds sent to CD (see letter from Asa Gray, 11 March 1880).

⁴ *Brodiaea* is the genus of cluster-lilies; *Pellaea* is the genus of cliff-brake, a type of fern.

⁵ Rattan's description of germination agrees with that of CD (see letter to Asa Gray, 19 January 1880).

- ⁶ The Mokelumne river is in northern California. The trees referred to are canyon live oaks (*Quercus chrysolepis*). RR.: railroad.
- ⁷ *Lepidium* is the genus of pepperweed.
- ⁸ *Lepidium oxycarpum* is forked pepperweed; *L. menziesii* is a synonym of *L. virginicum* ssp. *menziesii*, Menzies' pepperweed.
- ⁹ The diagrams are reproduced here at 60 per cent of their original size.
- ¹⁰ CD's annotation is a note for his reply to Gray (see letter to Asa Gray, 19 April 1880). *Ipomoea pandurata* is man-of-the-earth or wild potato vine (see letter from Asa Gray, [1 April 1880] and n. 2).

From S. M. Herzfeld 4 April 1880

51. S^t. Thomas' Road | Finsbury Park. | London
April. 4. 1880.

Dearest Professor Darwin,

I have received your letter, cheque, and papers.¹ My heart felt thanks. I should be so very happy to see you. What would it be, if again I had to return to the Continent, and if I, who lecture on natural sciences, could not say that I have most reverently kissed the hands of the great professor—not even seen him. I do not want you to give me money but I should be so very happy to see you.² Could you name a day? It would be most noble-minded man a fresh benefit you would confer upon me.

I have the honor to be, | Dearest Professor Darwin, | your most humble and thankful pupil, | S. M. Herzfeld | Doctor, | etc.

To Professor | Charles Darwin.

DAR 166: 192

¹ See letter from S. M. Herzfeld, 2 April 1880 and n. 6. CD sent Herzfeld £12. CD's letter to Herzfeld has not been found.

² No visit by Herzfeld has been recorded.

To W. E. Darwin 5 [April 1880]

March 5th¹

My dear W.

I have finished book on Laccolites, but doubt much whether it is worth your reading. It consists entirely of evidence on the Laccolites, & a good, but very long-winded discussion on terrestrial waste or denudation.—² I will send it if you like.—

What splendid news about the elections.— I have not been so much pleased for years. I was glad to see that you succeeded at Southampton.—³

Give my best love to dear Sarah..— It was a bad job that I was able to enjoy your visit so little.⁴ Yours affect | C. Darwin

Postmark: AP 5 80

DAR 210.6: 156

¹ The month and year are established by the postmark. CD wrote March in error.

- ² No book on laccoliths has been found in the Darwin Libraries at CUL and Down, but CD probably refers to a report made by Grove Karl Gilbert on the geology of the Henry Mountains, Utah (G. K. Gilbert 1877). Gilbert devoted a chapter of this work to the analysis of formations he termed laccoliths (now more usually referred to as laccoliths), which were masses of igneous rock intruded between rock strata causing uplift in the shape of a dome (*ibid.*, pp. 51–98). The following sections of the book focused on various types of erosion and land sculpture (*ibid.*, pp. 99–150).
- ³ A general election was held from 31 March to 27 April 1880 in which the Liberal party won by one of their largest majorities. Both Southampton parliamentary seats were won by Liberal candidates (Craig ed. 1989, p. 280).
- ⁴ Sara Darwin and William visited Down from 25 March until early April 1880; Emma's diary entry for 27 March reads, 'poorly as colds for a week' (Emma Darwin's diary (DAR 242); letter from Emma Darwin to H. E. Litchfield, 4 April 1880 (DAR 219.9: 229)).

From W. E. Darwin 6 April [1880]¹

Basset, | Southampton.

April 6

My dear Father,

I think I had better not have the book sent, as I have several geological books in hand that refer to places nearer home, and especially I am just beginning Geikie's ice age.² I forgot to say that they have just found some more flint tools on the common at about 7 ft deep, and the men talked as if they had found considerable numbers altogether at depths varying from 4 to 7 ft. How on earth did the flint tools get among this surface gravel without any relics of any kind being also found, as the flints have not been rolled enough to destroy all bones or teeth. The only other things that they ever find are "shepherds crowns".³

It is splendid about the election, and we are all triumphant here, I hear Gladstone is in all right, but I have not seen papers yet.⁴ This is the only really exciting election since I have come to years of discretion. S. has had a long letter from Hen. which is rather bitter, but she hopes you had your champagne.⁵

Goodbye dear Father, I hope you are better now. They are just beginning the verandah, & it will ready for you in the summer. S. sends you & Mother her best love your affect son | W E Darwin

Cornford Family Papers (DAR 275: 81)

¹ The year is established by the reference to the general election (see n. 4, below).

² CD had offered to send a book on the geology of the Henry Mountains, Utah (G. K. Gilbert 1877; see letter to W. E. Darwin, 5 [April 1880] and n. 2). James Geikie was the author of *The great ice age* (Geikie 1877).

³ 'Shepherd's crowns' was an old name for the fossils of some Cretaceous echinoids, resembling the ribs of a crown, on the downlands of southern England, where they may have first been found by shepherds (see Basset 1982, pp. 15–16).

⁴ The Liberal party had been victorious in the general election (see letter to W. E. Darwin, 5 [April 1880] and n. 3). William Ewart Gladstone became prime minister for the second time (*ODNB*).

⁵ Southampton, where William and Sara Darwin lived, returned two Liberal candidates, while the electoral district in which Henrietta Emma Litchfield lived, Westminster (London), returned two Conservatives (Craig ed. 1989, pp. 21, 280).

April 1880

To Henry Faulds 7 April 1880

DOWN, | BECKENHAM, KENT, | RAILWAY STATION,
ORPINGTON, S.E.R.

April 7th, 1880.

Via BRINDISI.

DEAR SIR,

The subject to which you refer in your letter of February 15th seems to me a curious one, which may turn out interesting; but I am sorry to say that I am most unfortunately situated for offering you any assistance. I live in the country, and from weak health seldom see anyone. I will, however, forward your letter to Mr. F. Galton, who is the most likely man that I can think of to take up the subject to make further enquiries.¹

Wishing you success, | I remain, dear Sir, | Yours faithfully,

CHARLES DARWIN.

Faulds [1912?], pp. 22–3

¹ See letter from Henry Faulds, 16 February 1880 and n. 1. Faulds had discussed the merits of keeping a record of fingerprints, focusing particularly on their possible use in identifying criminals. See letter to Francis Galton, 7 April 1880.

To Francis Galton 7 April 1880

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

Ap 7 1880

My dear Galton,

The enclosed letter and circular may perhaps interest you, as it relates to a queer subject.¹ You will perhaps say hang his impudence. But seriously the letter might possibly be worth taking some day to the Anthropolog Inst for the chance of someone caring about it. I have written to Mr. Faulds telling him I could give no help, but had forwarded the letter to you on the chance of its interesting you.²

My dear Galton, | Yours very sincerely | Ch. Darwin

P.S. The more I think of your visualising enquiries, the more interesting they seem to me.³

Copy

UCL Library Services, Special Collections (GALTON/3/2/1/24)

¹ CD enclosed the letter from Henry Faulds, 16 February 1880, and the sample form with hand prints that Faulds had included with his letter.

² See letter to Henry Faulds, 7 April 1880. Galton was a fellow of the Anthropological Institute of Great Britain and Ireland (*Journal of the Royal Anthropological Institute of Great Britain and Ireland* 30 (1900): 8).

³ In late 1879, Galton had sent CD a questionnaire on the faculty of visualising, which CD filled in and returned (see *Correspondence* vol. 27, letter from Francis Galton, 12 November 1879, and letter to Francis Galton, 14 November [1879]). Galton published the results of his survey in 'Statistics of mental imagery' in the July 1880 issue of *Mind* (Galton 1880a).

From Valentine Ball 8 April 1880

37 Northumberland Road | Dublin
8th April 1880

Dear Sir

I have just received from Mess^{rs} De La Rue a letter enclosing one from you acknowledging the receipt of a copy of “Jungle Life” which was forwarded to you by my direction¹

I have been encouraged to offer the copy for your acceptance in consequence of the favourable notice which the work has received from over twenty Reviewers both in England & India; and at the same time in recognition of the benefits I have received from the study of your works²

The particular branch of Zoology to which I have given most attention is Geographical Distribution and my collections are extensive including about 1000 species of birds—besides mammals, insects & shells &c

Should I return to India as I expect to be obliged to do in about six months, I shall ever be ready to conduct any line of inquiry or investigation, so far as I am able, with which you may think fit to entrust me.

I am anxious however to leave India in consequence of the Government refusing to relax the harsh rules under which Geologists have to serve namely *thirty* years active field work are required to secure a very small pension³

Yours faithfully | V. Ball

DAR 160: 37

¹ CD's letter has not been found; Thomas De La Rue & Co. had published Ball's *Jungle life in India* (Ball 1880). CD's copy is in the Darwin Library–Down.

² Ball's book was favourably reviewed in the *Athenaeum*, 13 March 1880, pp. 337–8. No other reviews have been found.

³ Ball gave up his position at the Geological Survey of India in 1881, when he became professor of geology and mineralogy at the University of Dublin (*Geological Magazine* 2 (1895): 382).

From C. W. Fox 8 April 1880

Broadlands | Sandown
Thursday, April 8. 1880.

Dear Mr. Darwin,

You will hardly be surprised to hear that my dear Father's sufferings are ended.¹ He died this morning, apparently in no pain, and conscious almost to the very last. I had only returned home last night; and during the night he suffered twice from a sort of spasm of the heart— then the breathing became somewhat laboured, and after a while it simply ceased. Those who were present scarcely knew when the last moment came— It is a great comfort to us all to think that he suffered less in the latter part of his illness, and that his death was so mercifully painless and free from conscious discomfort.

I can frame no better wish for myself, or for any one that loved him, than that they and I may be like him in life and in death. My poor Mother² still bears up

wonderfully—and was with my dear Father to the last. Almost his last words were expressions of affection and gratitude to her— I believe the very last words he uttered were “I am sorry to give you all so much trouble”—words which are so characteristic of him that I do not hesitate to quote them to you.

I must thank you sincerely for the kind letter which you sent in reply to my former letter.³ Your name has always been an honoured one in my Father’s house; and we shall always associate you with his memory— You will pardon a somewhat incoherent letter, I trust; the pressure of details today is very great.

Believe me to remain | Yours most sincerely, | Charles W. Fox

DAR 164: 172

¹ William Darwin Fox was CD’s second cousin.

² Ellen Sophia Fox.

³ See letter to C. W. Fox, 29 March 1880; Fox’s former letter to CD has not been found.

From Francis Galton 8 April 1880

42 Rutland Gate
April 8/80

My dear Darwin

I will take Faulds’ letter to the Anthro: & see what can be done.¹ indeed I myself got several thumb impresses a couple of years ago, having heard of the Chinese plan with criminals but failed, perhaps from want of sufficiently minute observation, to make out any *large* number of differences.² It would I think be feasible in one or two public schools where the system is established of annually taking heights weights &c also to take thumb marks. by which one would in time learn if the markings were as persistent as is said.³

Anyhow I will do what I can to help Mr Faulds in getting these sort of facts & in having an extract from his letter printed⁴

I am so glad that my “visuality” enquiries seem interesting to you. I get letters from all directions & the metaphysicians & mad-doctors have been very helpful.⁵

Very sincerely yours | Francis Galton

Our united kindest remembrances to you all.

DAR 105: A103

¹ CD had forwarded a letter from Henry Faulds discussing the use of hand- and fingerprints (see letter to Francis Galton, 7 April 1880 and n. 1). CD had suggested that Galton might take Faulds’s letter to the Anthropological Institute of Great Britain and Ireland.

² In his letter to CD of 16 February 1880, Faulds had mentioned the Chinese practice of taking fingerprints of criminals.

³ Faulds had argued that while a person’s face changed over time, finger rugae (ridges) remained the same (letter from Henry Faulds, 16 February 1880).

⁴ No article mentioning Faulds’s work, or extract from his letter, was published in the *Journal of the Anthropological Institute of Great Britain and Ireland*.

⁵ Galton had sent questionnaires on the power of visualising to various people including CD (see letter to Francis Galton, 7 April 1880 and n. 3).

From Henry Woodward 8 April 1880

129. LATE 117 *Beaufort Street*, | *Chelsea*, | *London. S.W.*

8 April 1880.

Dear Mr Darwin,

My chief Mr Waterhouse, under whom I have served in the Department of Geology & Palæontology for 22 years, has just tendered his resignation & in so doing has expressed his conviction of my fitness as his successor, & in this Prof. Owen concurs heartily.¹ Nevertheless the matter has to be referred first to the Treasury & then to the three Principal Trustees with whom the Appointment rests: (The Archbishop of Canterbury, The Lord Chancellor & the Speaker.)²

I shall have presently to apply to these august persons, & to accompany my letter by suitable Testimonials. May I venture to ask the great favor of a few lines of commendation from you?

Since my Brother's death, in 1865, the management of the Department has mainly rested upon me, & I have tried hard to carry out my official duties, & to contribute also my share to the Scientific work of the day. My Mon: on the Merostomata in the Pal. Soc. my numerous papers in the Quart. Journ. Geol. Soc. in the Geol. Mag., & elsewhere show what I have done.³

I was elected to the Royl. Socy in 1873., & made an Hon. LL.D. of St Andrews in 1878.,

If I am appointed I shall aim to make the Palæontological Collections in the New Museum the best arranged & most instructive series to be seen anywhere.

With kindest regards, | Believe me, Dear Mr Darwin | yours very sincerely |
Henry Woodward

DAR 181: 151

¹ George Robert Waterhouse was keeper of the geology department of the British Museum from 1857; Richard Owen was the superintendent of the natural history departments of the museum.

² Archibald Campbell Tait, Roundell Palmer (from 28 April 1880), and Henry Brand.

³ Woodward's brother, Samuel Pickworth Woodward, had been an assistant in the department of geology and mineralogy at the British Museum from 1848. Henry Woodward's monograph on the crustacean order Merostomata (Woodward 1866–78) was published by the Palaeontographical Society; Merostomata is now considered to be a class of the subphylum Chelicerata. Woodward was a co-founder and the editor of the *Geological Magazine* as well as a frequent contributor to it and to the *Quarterly Journal of the Geological Society of London*.

To E. S. Morse 9 [April] 1880¹

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

[*Abinger Hall, Surrey*]

March 9th 1880

My dear Sir

I have forwarded your letter to 'Nature' with a private one to the Editor & another which he can publish if he thinks fit.² Your letter, though I believe quite just, is rather

fierce, & whether the Editor will publish it, I do not at all know.— I hope that he may.— It is most wonderful & interesting that native Japanese gentlemen sh^d have aided you in your very interesting researches.—³

In Haste— Yours very faithfully | Ch. Darwin.

Peabody Essex Museum: Phillips Library (E. S. Morse Papers, E 2, Box 3, Folder 11)

- ¹ The month is established by the relationship between this letter and the letter from E. S. Morse, 23 March 1880. CD evidently wrote March in error.
- ² With his letter to CD of 23 March 1880, Morse had enclosed a rebuttal of an unfavourable review of his monograph on the Omori shell mounds in Japan (Morse 1879). CD's private letter to Joseph Norman Lockyer, the editor of *Nature*, has not been found. His other letter, dated 9 April [1880], was published in *Nature*, 15 April 1880, p. 561, along with the letter sent by Morse.
- ³ In his preface, Morse had cited several Japanese scholars by name and paid tribute to the work done by Japanese artists in producing the plates for the volume (Morse 1879, pp. iii–iv).

To *Nature* 9 April [1880]¹

[Abinger Hall, Surrey.]

The Omori Shell Mounds

I have received the enclosed letter from Prof. Morse, with a request that I should forward it to you.² I hope that it may be published, for the article in *NATURE* to which it refers seemed to me to do very scant justice to Prof. Morse's work.³ I refer more especially to the evidence adduced by him on cannibali[s]m⁴ by the ancient inhabitants of Japan—on their platycnemic tibiae—on their degree of skill in ceramic art—and beyond all other points, on the changes in the molluscan fauna of the islands since the period in question.⁵

It is a remarkable fact, which incidentally appears in Prof. Morse's memoir, that several Japanese gentlemen have already formed large collections of the shells of the Archipelago, and have zealously aided him in the investigation of the prehistoric mounds.⁶ This is a most encouraging omen of the future progress of science in Japan.

CHARLES DARWIN

Down, Beckenham, Kent, April 9

Nature, 15 April 1880, p. 561

- ¹ The year is established by the publication date of the letter in *Nature*.
- ² Edward Sylvester Morse had enclosed a letter responding to a negative review of Morse 1879 with his letter to CD of 23 March 1880.
- ³ Frederick Victor Dickins's review of Morse 1879 was published in *Nature*, 12 February 1880, p. 350.
- ⁴ The 's' is missing in the printed text.
- ⁵ On the evidence of cannibalism, see Morse 1879, pp. 17–19. Morse discussed the platycnemic or flattened tibiae characteristic of prehistoric humans and compared the Omori bones with those found at other sites (*ibid.*, pp. 19–21). On changes in the molluscs of the area, see *ibid.*, pp. 23–36.
- ⁶ Morse paid tribute to members of the Japanese Archaeological Society and mentioned some of the Japanese collections to which he was given access in Morse 1879, pp. iv, 3.

To C. W. Fox 10 [April] 1880

Abinger Hall | Dorking
March 10th 1880¹

My dear M^r Fox

I write only a word to thank you for your second most kind letter. I am glad that your poor dear Father's sufferings are over, & that his end was so tranquil.²

I have now before my eyes his bright face as a young man, so full of intelligence & I hear his voice as clearly as if he were present. Your mother³ must have gone through terrible suffering during his long illness.

My wife joins me in saying how deeply we sympathise with her. Pray believe me
| Yours very sincerely | Charles Darwin

Postmark: AP 10 | 80

University of British Columbia Library, Rare Books and Special Collections (Pearce/Darwin Fox collection
RBSC-ARC-1721-1-13)

¹ The month is established by the postmark. CD wrote March in error.

² See letter from C. W. Fox, 8 April 1880; William Darwin Fox had died on 8 April 1880.

³ Ellen Sophia Fox.

From Henry Woodward 10 April 1880

British Museum
Apl 10— 1880.

Dear Mr Darwin,

Pray accept my very sincere thanks for the friendly testimony you have been kind enough to bear to my fitness for the Keepership of Geology. It will, I feel sure, have great weight with the three principal Trustees, with whom the appointment rests.¹

Let me congratulate you on "the coming of age of the Origin of Species" on which Prof. Huxley so ably lectured on Friday night at the Royal Institution.² The crowded and overflowing theatre well expressed the earnest interest which *all* take in this great & fundamental principle which you were occupied in enunciating when I commenced my labours in the Museum (in 1858)—& which I have had the pleasure to see developed into a vast & universal law, applicable to every line of research on which the naturalist may enter. I have reason to be greatly rejoiced that I shall have the opportunity, when our collections are removed to the New Building, to set forth, by the aid of actual fossil remains, all that can be shown of the evolution of living forms, both Vertebrate & Invertebrate, as evidenced by the Geological Record; & I hope to do it thoroughly & well. It will be a labor of love.³

I can only earnestly express the desire that you may be able—as I am sure you will be willing—to look upon us in our New Residence, & to express your kindly approval of our labors. This will indeed be the Crown of rejoicing to my life.

With every earnest wish for your continued health & renown and with kindest regards, Believe me, | Yours very sincerely, | Henry Woodward.

Charles Darwin, Esq. F.R.S. | &c., &c., &c.

DAR 181: 152

- ¹ See letter from Henry Woodward, 8 April 1880 and n. 2. CD's letter of recommendation has not been found.
- ² Thomas Henry Huxley delivered the lecture at the Royal Institution of Great Britain on 9 April 1880; the text was published in *Nature*, 6 May 1880 (T. H. Huxley 1880c).
- ³ When the collection of fossils was moved to the new Natural History Museum in 1881, Woodward, as the keeper of geology, planned and supervised its rearrangement according to Darwinian principles (*Geological Magazine* 58 (1921): 481).

To T. H. Huxley 11 [April] 1880¹

Abinger Hall | Dorking
Sunday March 11th 80

My dear Huxley

I wished much to attend your lecture, but I have had a bad cough & we have come here to see whether a change w^d do me good, as it has done.² What a magnificent success your lecture seems to have been, as I judge from the reports in the *Standard* & *D. News*, & more especially from the accounts given me by 3 of my children.³ I suppose that you have not written out your lecture so fear there is no chance of its being published in extenso.⁴ You appear to have piled, as on so many other occasions, honours high & thick on my old head. But I well know how great a part you have played in establishing & spreading the belief in the descent-theory, ever since the grand review in the *Times* & the battle Royal at Oxford up to the present day.⁵

Ever my dear Huxley | Yours sincerely & gratefully | Charles Darwin

It was absurdly stupid in me, but I had read the announcement of your Lecture & thought that you meant the maturity of the subject, until my wife one day remarked, "yes it is about 21 years since the Origin appeared", & then for the first time the meaning of your words flashed on me!⁶

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 340)

- ¹ The month is established by the reference to Huxley's lecture (see n. 2, below). CD wrote March in error.
- ² Huxley delivered a lecture, 'The coming of age of the *Origin of species*', at the Royal Institution of Great Britain on 9 April 1880. The Darwins visited Abinger, Surrey, from 8 to 13 April 1880 (CD's 'Journal' (Appendix II)).
- ³ Articles on Huxley's lecture appeared in the *Standard*, 10 April 1880, p. 3, and the *Daily News*, 10 April 1880, p. 2.
- ⁴ The lecture was published in *Nature*, 6 May 1880 (T. H. Huxley 1880c).
- ⁵ An unsigned review of *Origin* by Huxley had appeared in *The Times*, 26 December 1859, pp. 8–9 ([T. H. Huxley] 1859). On the events that took place at the Oxford meeting of the British Association for the Advancement of Science, particularly the verbal encounter between Samuel Wilberforce, the bishop of Oxford, and Huxley on 30 June 1860, see *Correspondence* vol. 8, Appendix VI.
- ⁶ At this time the age of majority in Britain was 21.

To G. B. Ercolani 14 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
April 14th 1880

Dear Sir

I am much obliged for the honour which you have done me by presenting me with great work on the Placenta. The relations of its structure to the classification

of the mammalia has always seemed to me a very interesting subject, & I see that it is discussed by you.—¹

With the greatest respect, | I remain Dear Sir | Yours faithfully & obliged | Charles Darwin

Biblioteca Comunale dell'Archiginnasio, Bologna (Collezione degli autografi, XXIII, 6543)

¹ Ercolani had sent a copy of his work comparing the mammalian placenta with that of cartilaginous fishes and applying the results to taxonomy (Ercolani 1880). CD's copy is in the Darwin Library–CUL.

To F. E. Abbot 15 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
April 15th 1880

My dear Sir

I believe that I owe to you a copy of the *Literary World* with a capital review of a Lawyers book on evolution, & which I imagine is written by you.—¹ I had seen something about this book in a little article by Asa Gray, but did not know how abusive it was.² It appears to be a production worthy of an ignorant lawyer.—

For some time I have been intending to write & thank you for sending me the Index, & which I gratefully accept. But I now enclose a cheque for 5£ which will pay for copies for some little time.³ I always read a large part of your excellent *Journal* & sh^d certainly read every word had I time & strength sufficient. But reading much more than the papers &c which are necessary for my scientific work now-a-days tires me greatly.—

Most heartily wishing you success in your admirable endeavours in the good cause of truth & wishing you prosperity in all ways, I remain | My dear Sir | Yours sincerely | Charles Darwin

P.S. | Will you kindly send me a Post-card acknowledging receipt of the small cheque.—

Harvard University Archives (Papers of F. E. Abbot, 1841–1904. Named Correspondence, 1857–1903. Letter, C. R. Darwin to F. E. Abbot (15 April 1880), in folder Darwin, Charles and W. E. Darwin (son), 1871–1883, box 44. HUG 1101)

¹ Abbot sent CD a copy of an unsigned review titled 'A Philadelphia lawyer's views of Darwinism' (*Literary World*, 27 March 1880, pp. 104–5); the book reviewed was *The refutation of Darwinism* (O'Neill 1880). CD's annotated copy of the review is in DAR 226.1: 79–80.

² Asa Gray had sent CD his unsigned review of O'Neill 1880, which appeared in the *Nation*, 4 March 1880, p. 182 (see letter from Asa Gray, 11 March 1880). CD's copy is in DAR 226.1: 104.

³ Abbot was the editor of the *Index*, a weekly paper devoted to promoting 'free religion'; according to Abbot's letterhead, the annual subscription was \$2.

To W. T. Thiselton-Dyer 15 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Ap. 15th 80

My dear Dyer

If you chance to have seeds of *Impomœa pandurata*, a few would be of inestimable value to me.¹ Or indeed of any kind (except *I. leptophylla* which I have) which forms a great tuber-like root.²

Do not answer this.— I know that it is only a forlorn hope.—

Ever yours | C. Darwin

Asa Gray has told me of *I. pandurata* & admits that if its seeds germinate like those of *I. leptophylla*, a pet little theory of mine w^d be well confirmed.— He was, I think, at first inclined to treat my little theory with sovereign contempt.³

Royal Botanic Gardens, Kew (Darwin: Letters to Thiselton-Dyer, 1873–81: ff. 205–6)

¹ *Ipomoea pandurata* (man-of-the-earth or wild potato vine) is a species native to North America, notable for its large tuberous roots.

² CD recorded the germination of plants of *Ipomoea leptophylla* (bush morning-glory), another species with large tuberous roots, on 14 April 1880, noting that the petioles were buried about a half inch below the ground (DAR 209.6: 95).

³ See letter from Asa Gray, [1 April 1880] and n. 3. See also enclosure to letter from Asa Gray, 4 April 1880, in which a correspondent of Gray's confirmed CD's observations about the burying of petioles in another plant with tuberous roots. CD hypothesised that the buried petioles acted functionally like a root to hide the tuberous root from predators.

From William Tearle [before 16] April 1880¹

Cambridge Street | St Neots | Hunts.

Ap 1880

Sir

Having taken great interest in your work “The Origin of Species” and regretting, that its doctrine, as far as man is concerned, is antagonistic to the strict reading of the Bible; I have been trying if some solution to the two first chapters of Genesis cannot be found, which might bring those chapters in accord with your Theory.²

Whether my theory has been made before, or whether great minds will think it tenable, I leave you to judge. Briefly then I will explain.

1st “Let us make man in our image” May man not have been previously created, as an animal of a superior order, and God seeing that all living creatures required a head, and earthly master, he marked man as the most suitable, and then fashioned him after his own image. Had the text stood “Let us make man, and let us make him in our image” we could hardly argue that man had been previously created among the animals; but as the text now stands, and considering that the higher order of animals were created on the same day as man was I think my theory is not far fetched. It does not say that woman was made after gods own image, and the fact that she was not made *as woman* until afterwards, and then out of a man, helps to make my theory good, and to reconcile what has been considered two distinct creations. Man, male and female created he them, man after his own image afterwards, then he required a superior female and one was accordingly made. If any fossil remains have been found of the highest order of Mammalia, of which the male is wanting, such might have been the female to man in his primeval condition. For it is possible that the male being elevated from his original state would not be replaced but be entirely destroyed, leaving the females to die natural deaths. From what I have now said you may see the drift of my argument, which, after all may be

worthless, but in the space of a letter I am afraid that I have not made it so clear as I intended. I must apologise for trespassing on your time, but I hope the interest that I have taken in reading your works may be some excuse for the liberty that I have taken in addressing you.

I am sir | Your humble servant | William Tearle.

Professor C Darwin LLD FRS, Etc

DAR 201: 38

¹ The date is established by the relationship between this letter and the letter to William Tearle, 16 April 1880.

² CD did not discuss human evolution in *Origin*, but his work implied that humans, like other animals, were a product of evolution by natural selection.

To William Tearle 16 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
April 16th 1880

Private

Dear Sir

I am sorry to say that I can be of no assistance to you.—¹ Any remarks which I might make on your letter would as far as they had any influence, add to your doubts on subjects which you consider sacred.

In my opinion every man ought to weigh for himself impartially & anxiously all the arguments for & against any revelation ever having been made to man.—

Dear Sir | Yours faithfully | Ch. Darwin

American Philosophical Society (Ms.B.D25.572)

¹ See letter from William Tearle, [before 16] April 1880.

From C. C. Graham 17 April 1880

Louisville,
April 17th, 1880.

No answer requested

Dear Sir,

My letter was published without my knowledge or consent and I send you the paper to show you our backwoods style of Journalism on the “*Dark & Bloody Ground*” now called Kentucky; which in my early days was a howling wilderness of Savages and wild beasts.¹ There is a fire proof gallery in our State House at Frankfort now being finished where your handsomely framed letter may hang for ages after you are gone.² I am now near ninety six (96) but can and do walk my twenty miles a day.

Christopher Columbus Graham, M.D.

written by granddaugh.³

pc

DAR 165: 82

- ¹ The newspaper in which Graham's letter appeared has not been identified and no clipping has been found in the Darwin Archive–CUL. Graham probably refers to his letter to CD of 28 March 1880. In the last quarter of the eighteenth century, the Ohio River was often called 'that dark and bloody river' and the territory of the Ohio River Valley 'that dark and bloody land', an allusion to conflicts between settlers and indigenous people of the area (see Eckert 1995).
- ² CD's letter has not been found.
- ³ Graham's granddaughter has not been identified.

To [William Newton] 17 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Ap. 17th 1880

Private

Dear Sir

I can only repeat that I have never been able to follow easily abstract reasoning & of late years the labour is intolerable to me.¹ Therefore I cannot judge of the value of M^r Lloyds work.— I do not remember distinctly what Mr L. says against M^r Galton's book on Hereditary Genius, which I have always esteemed highly.—²

I am extremely sorry that you cannot give a better account of M^r Lloyd's health: your friendship must be a great support to him in his suffering state.³

I have received the 'Brigands Home', but have not yet had time to read it.⁴

Thanking you for your courteous expressions in regard to myself, I remain | Dear Sir | Yours faithfully | Ch. Darwin

I know that I am hated & abused by many; but I do not care much about this or about fame. It is the one advantage of advanced age.—

Archives of the New York Botanical Garden

- ¹ The letter to which this is a reply has not been found. The correspondent was probably William Newton, who had worked with Francis Lloyd (see n. 2, below).
- ² Lloyd had evidently sent CD a copy of his critique of Francis Galton's book on hereditary genius (Galton 1869, Lloyd 1876a); a copy is in the Darwin Pamphlet Collection–CUL (see also *Correspondence* vol. 25, letter to [Francis Lloyd], 1 May [1877]). Newton had written an introduction to Lloyd 1876a.
- ³ In the event, Lloyd died in Smyrna, Turkey, on 17 April 1880 (*Standard*, 22 April 1880, p. 1). CD had sent Lloyd £10 on 1 May 1877 (CD's Classed account books (Down House MS)).
- ⁴ No copy of *The Brigands' Cave on Salamis; tales and legends* (Lloyd 1876b) has been found in the Darwin Libraries at CUL or Down.

To Asa Gray 19 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
April 19th 1880

My dear Gray

It was **very** good of you to send me M^r. Rattan's letter which has interested me extremely.¹ I suppose that I may give some of his statements. It seems almost *certain* that the protection of the plumule from frost has determined the curious mode

of germination; & this agrees well with the drawing down in winter of so many seedlings by the contraction of their radicles. But I still think the protection of the enlarged roots from being devoured is probably a secondary advantage thus gained.² The “corks” explain the behaviour in your first case.³

What a curious fact of Mr. Rattan’s,—that of the confluent petioles growing out horizontally without coming into contact with the bottom of the shallow box. From what I have observed with other seedlings I believe I understand the cause, but it is too long a story.⁴ I wish I had a score of germinating seeds to experimentise on.—

Very many thanks for P. Card about roots of *Ipomœa leptophylla*: I have tried in vain to get seeds of *I. pandurata* from Kew.⁵

Ever yours very truly | Ch. Darwin

Mr Rattan seems to be a real good observer, & that is a rare species of animal.—

University of California Berkeley, Bancroft Library (BANC MSS 74/78 z)

¹ With his letter to CD of 4 April 1880, Gray had enclosed a letter from Volney Rattan with observations on the germination of *Megarhiza californica* (a synonym of *Marah fabacea*, California manroot).

² CD had hypothesised that the tubular petioles of *Megarhiza californica* acted functionally like a root to protect the (true) enlarged root against predators (see letter to Asa Gray, 19 January 1880). Rattan had observed that an unburied portion of one sprout was blackened by frost, but the buried part was fresh, leading him to conclude that the plumule would have survived (see letter from Asa Gray, 4 April 1880, enclosure). CD mentioned Rattan’s observation on the mode of germination in *Movement in plants*, pp. 82–3.

³ Gray had initially doubted CD’s description of the movement of the petioles to penetrate the ground in the germination of *Megarhiza californica* (see letter from Asa Gray, 3 February 1880 and n. 2).

⁴ In his letter to Gray, Rattan had included drawings of the manner of growth of the petioles during the germination of seeds that he had planted in a shallow box (see letter from Asa Gray, 4 April 1880, enclosure).

⁵ See postcard from Asa Gray, [1 April 1880]. CD requested seeds of *Ipomoea pandurata* in his letter to W. T. Thielton-Dyer, 15 April 1880.

From Ernst Krause¹ 19 April 1880

Berlin N.O. Friedenstrasse 10. II
den 19.4.80.

Hochverehrter Herr!

Endlich bin ich im Stande, Ihnen die deutsche Ausgabe des Erasmus Darwin vorzulegen.² Ich fürchte sehr, dass Ihnen die Ausdehnung des Stoffes nicht ganz gefallen wird, namentlich was die zahlreichen Anmerkungen betrifft. Ich habe dabei einerseits die Bedürfnisse und Gewohnheiten des deutschen Publikums im Auge gehabt und andererseits den Wunsch des Verlegers, nicht unter einer gewissen Seitenzahl zu bleiben, da Bücher zwischen 100–200 Seiten sich notorisch in Deutschland am schlechtesten verkaufen pflegen. Unter den Anmerkungen möchte ich Ihre Aufmerksamkeit auf N^o 92 (p. 231) zu richten mir erlauben, in welcher nachgewiesen wird, dass eine Theorie der Pangenesis von Hippocrates beinahe mit denselben Worten aufgestellt worden ist, wie von Ihnen.³

Indem ich Sie bitte, die Arbeit mit nachsichtigen Augen aufzunehmen, zeichne ich, hochverehrter Herr | In herzlicher Verehrung | Ihr dankbar ergebenster | Ernst Krause

x) Ich sende sie hierneben unter Kreuzband.

DAR 92: B58

CD ANNOTATION

Verso of letter: 'In conclusion let me thank you [^{most} *del*] cordially for your uniform & most k. consideration for every one of my wishes, & still more so for placing my grandfathers merit [^{on a} *illeg*] *interl & del*] on a sure & lasting foundation.'⁴ *ink*

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

² Krause 1880; CD's copy has not been found in the Darwin Archive.

³ Krause referred to the Greek physician Hippocrates (Krause 1880, p. 231). CD had been informed about similarities between his theory and that of Hippocrates by William Ogle (see *Correspondence* vol. 16, letter to William Ogle, 6 March [1868]). For a discussion of the theories of heredity of Hippocrates and CD, see *Dictionary of the history of ideas* 2: 622–4.

⁴ CD's annotation is a note for his reply to Krause of 21 April 1880.

To Horace Darwin 20 April [1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Ap. 20th

My dear old Horace

The advantage of a garden, if you have children & even without these appendages is so great, combined with the house itself suiting you & having a good landlord, that I if I were in your place, I w^d. wait for the chance of getting it.² For such advantages it w^d be worth while to economise in some other way; though this is a wicked line of argument & could not have been expected from your honoured & stingy old father.—

Perhaps the Belvoir House may not be let.³ My best of loves to Ida.

Your affect. Father | Ch. Darwin

DAR 185: 8

¹ The year is established by the relationship between this letter and the letter from G. H. Darwin, 28 May 1880.

² Horace Darwin was looking for a house in Cambridge; he and his wife, Ida, were married on 3 January 1880 (Emma Darwin's diary (DAR 242)). The house being discussed has not been identified.

³ CD evidently refers to a house in Belvoir Road, Cambridge.

From John Fiske 20 April 1880

Cambridge,
April 20, 1880.

My dear Mr. Darwin:

I am about to sail for England, to give some lectures at the Royal Institution, and shall be in London from May 16th until June 1st.¹ I am going to bring my wife with me

this time, for after 15 years with the children I think she ought to have a vacation.² While we are in London, I hope to get a chance to look at you again for a moment and shake hands.³ After finishing in London, I go to Edinburgh to give some lectures at the Philosophical Institution,—and shall be coming home again early in July.⁴ I hope you are still well and prospering in your great work.

I am unable to follow you in detail quite so closely as I used to, for year by year I find myself studying more and more nothing but history. But Huxley⁵ told me last year that he thought I could do more for the “doctrine of evolution” in history than in any other line. To say that all my studies to-day owe their life to you, would be to utter a superfluous compliment; for now it goes without saying that the discovery of “natural selection” has put the whole future thought of mankind on a new basis. When I see you I shall feel a youthful pleasure in telling what I would like to do, if I can.

I shall stay at Prof. Huxley’s, while in London (4 Marlborough Place, Abbey Road, N.W.); and any word from you will reach me there.

Ever, my dear Mr. Darwin, | Most sincerely yours, | John Fiske.

DAR 164: 127

¹ On 18 May 1880, Fiske delivered the first of three lectures at the Royal Institution of Great Britain on the topic of American political ideas viewed from the standpoint of universal history (*The Times*, 18 May 1880, p. 8).

² John and Abby Morgan Fiske had six children (*ANB*).

³ Fiske had spent a year in Britain in 1873 and visited CD at that time (see *Correspondence* vol. 21, letter to John Fiske, 3 November [1873]; see also J. S. Clark 1917, 1: 478–9, for a description of the meeting).

⁴ Fiske gave four lectures at the Philosophical Institute in Edinburgh on America’s place in history (*Edinburgh Evening News*, 19 June 1880, p. 2).

⁵ Thomas Henry Huxley.

To Edward Frankland 20 April 1880

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

Ap 20/80

My dear Dr. Frankland

Some time ago you were so good as to help me by analysing some water in which Pelargonium leaves had been immersed, & in which you found a considerable quantity of potassium.¹ You were at the same time so kind as to offer further assistance in the matter.² Would you allow my son to send one or two bottles of water for analysis? In order that the experiments may be of more value, an estimation of the potassium or of the soluble ash in the leaves used, ought to be made. This would of course considerably increase the work of analysis, but if it could be managed it would certainly add to the interest of the results.³

My son will make a considerable number of experiments of this kind during the spring & summer, which would entail more analysis than I could possibly ask you to undertake, & it would therefore be convenient to know of some trustworthy analyst who would professionally undertake such work. If you can recommend any one to

me I shall be grateful; & we could employ him at once without troubling you if your assistants are especially busy just now, or if it is in any way in the least inconvenient to you to undertake any analysis for us at present.

Pray excuse me for troubling you again & believe me | Yours very sincerely |
Charles Darwin

LS(A)

The John Rylands Library, The University of Manchester (Frankland Collection)

- ¹ In 1879, CD had asked Frankland to analyse water samples to identify the alkaline substance present after soaking leaves of various species of plants (see *Correspondence* vol. 27, letter to Edward Frankland, 8 February 1879). CD had hypothesised that the exudate was potash (potassium permanganate, KMnO_4); Frankland evidently confirmed the presence of potassium, but no letter from him on the subject has been found.
- ² No reply has been found to CD's letter to Frankland of 8 February 1879.
- ³ Francis Darwin's experiments related to his continuing research on the function of bloom, the waxy or pruinose coating on some leaves and fruit. One aspect of this work involved determining the nature of substances exuded by leaves soaked in water (see *Correspondence* vol. 27, letter to Edward Frankland, 8 February 1879 and n. 3).

To Ernst Krause 21 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
April 21st 1880

My dear Sir

I have been very glad to receive the German Translation. It is excellently got up & looks a good deal better than the English edition.¹ I rejoice that all your labour is now over, & I wish most sincerely for your sake & for that of the Publisher that the sale may be fairly good; but from the poor sale of the English Edit. I am fearful.² From what I can see by turning over the pages I do not doubt that you have added *much* to the value of the little book by your Addenda.—³

That is a very curious passage about Hippocrates.⁴ A very favourable review has just appeared in the U. States in the *Nation*, one of the best of the Literary Journals; but I have heard nothing about the sale there.⁵ In conclusion allow me to thank you cordially for your uniform & most kind consideration for every one of my wishes, & in a still higher degree for having placed my grandfather's merits on a sure & lasting foundation.

Believe me my dear Sir, yours ever | Very sincerely | Charles Darwin

The Huntington Library (HM 36205)

- ¹ Krause had sent CD the German edition of *Erasmus Darwin* (Krause 1880; see letter from Ernst Krause, 19 April 1880 and n. 2).
- ² On sales of *Erasmus Darwin*, see the letter to Ernst Krause, 9 February 1880 and n. 7.
- ³ For the German edition, Krause reinstated parts of his text not included in the English edition as well as adding over one hundred pages of notes (see Krause 1880, pp. 75–124, 180–286).

⁴ Krause pointed out similarities to CD's theory in the work of the Greek physician Hippocrates (Krause 1880, p. 231; see letter from Ernst Krause, 19 April 1880 and n. 3).

⁵ The review appeared in the *Nation*, 1 April 1880, pp. 253–4.

From G. J. Romanes 22 April 1880

April 22, 1880.

As soon as I received your first intimation about Schneider's book I wrote over for it, and received a copy some weeks ago.¹ I then lent it to Sully,² who wanted to read it, so do not yet know what it is worth. I, together with my wife—who reads French much more quickly than I can—am now engaged upon all the French books on animal intelligence which you kindly lent me.³ I am also preparing for my Royal Institution lecture on the 7th of May. I will afterwards publish it in some of the magazines, and, last of all, in an expanded and more detailed form, it will go into my book on Animal Intelligence.⁴

I went to see [Wallace] the other day on Spiritualism. He answered privately a letter that I wrote to 'Nature,' signed 'F.R.S.,' which was a feeler for some material to investigate.⁵ I had never spoken to [Wallace] before, but although I passed a very pleasant afternoon with him, I did not learn anything new about Spiritualism. He seemed to me to have the faculty of deglutition too well developed. Thus, for instance, he seemed rather queer on the subject of astrology! and when I asked whether he thought it worthy of common sense to imagine that, spirits or no spirits, the conjunctions of *planets* could exercise any causative influence on the destinies of children born under them, he answered that having already 'swallowed so much,' he did not know where to stop!!

My wife and baby⁶ are both flourishing. I noticed that the latter, at four days old, could always tell which hand I touched, inclining its head towards that hand.

E. D. Romanes 1896, p. 96

¹ CD had been sent a copy of Georg Heinrich Schneider's *Der thierische Wille* (Animal will; Schneider [1880]) by the author (see letter from G. H. Schneider, 2 April 1880).

² James Sully.

³ Romanes's wife was Ethel Romanes. The books have not been identified.

⁴ Romanes's lecture, delivered at the Royal Institution of Great Britain on 30 April 1880, was on mental evolution; the lecture given on 7 May was by William Henry Flower on fashion in deformity (*Morning Post*, 4 May 1880, p. 2, and 11 May 1880, p. 2). No magazine article based on the lecture has been identified; Romanes's book, *Animal intelligence*, was published in 1882 (G. J. Romanes 1882).

⁵ Alfred Russel Wallace's name is replaced with a dash in the printed source of this letter. Romanes's letter to *Nature* was a reply to a letter in the previous issue headed 'A speculation regarding the senses' and signed 'M.' (the author was St George Jackson Mivart; see Slotten 2004, p. 402); this letter suggested the possibility that one person could simultaneously experience another person's thoughts, and referred to the 'ascertained facts of clairvoyance and mesmerism' as examples (*Nature*, 5 February 1880, pp. 323–4). In his reply, Romanes invited any clairvoyant or spiritualist to allow him to investigate such phenomena (*Nature*, 12 February 1880, p. 348). Romanes was elected a fellow of the Royal Society of London in 1879.

⁶ Ethel Georgina Romanes.

To R. F. Cooke 23 April [1880]¹

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

Ap. 23rd

My dear Sir

I shall send my M.S to Printers in about a month's time, so I must settle about title.— My family shake their heads in the same dismal manner as you & Mr. Murray did, when I told them my proposed title of “The Circumnutating Movements of Plants.”²

Now will you kindly consider the following point. My book with title of “The Movements & Habits of Climbing Plants” is generally known, as merely “Climbing Plants” & is so lettered outside. Would it not be permissible under these circumstances to entitle the new book simply as

“The Movements of Plants”.—

This would be a quite correct title.—

Or I might put

“The Nature of the Movements of Plants”

but I do not like this so well.

One of my sons suggested

“A contribution to the Physiology of Plants:
Movements of Plants.”³

Do kindly give me your aid & believe me | Yours very sincerely | Ch. Darwin

National Library of Scotland (John Murray Archive) (Ms. 42152 ff. 366–7)

¹ The year is established by the relationship between this letter and the letter from R. F. Cooke, 27 April 1880.

² John Murray was CD's publisher; *Movement in plants* was published on 6 November 1880 (Freeman 1977).

³ CD refers to the title as it appears on the spine of *Climbing plants* 2d ed. The son mentioned was probably Francis Darwin, who had assisted CD in the research for *Movement in plants*.

From Henry Pitman 24 April 1880

41, John Dalton Street, | Manchester.

Apl 24 1880.

From | Henry Pitman.

To Charles Darwin Esq

I am writing a series of papers on “Shorthand Writers of Renown”, specimens of which I send; & desiring to include your illustrious grandfather, Dr. Erasmus Darwin.¹ I venture to ask if you can afford me more information than I find on page 17 of your Notice in the book by Ernst Krause.² You will see by the enclosed letter that I have tried elsewhere before troubling you.³ Can you oblige me with the loan of the letters, (or copies) written by Erasmus Darwin to Reimarus respecting shorthand?⁴

Yours truly | Henry Pitman.

DAR 99: 195

- ¹ The specimens have not been found, and the papers have not been identified. Pitman later included a section titled 'Shorthand writers of renown' in his essay 'Spelling reform and phonography'; the first writer mentioned in this section was Erasmus Darwin (see Pitman 1890, pp. 252–5).
- ² In *Erasmus Darwin*, p. 17, CD mentioned that, based on a passage in a letter from Erasmus Darwin to Johann Albert Heinrich Reimarus, it appeared that Darwin had corresponded about shorthand writing with Thomas Gurney. Darwin had taken notes as a student in Gurney's 'brachygraphy' course, and wrote a poem to Gurney that was published in the second and later editions of Gurney's *Brachygraphy: or short-writing* (Gurney 1750); see Ritterbush 1962 for more on Darwin's use of Gurney's system). CD did not mention the poem to Gurney, but he may have received a later edition of Gurney 1750 from Reginald Darwin (see *Correspondence* vol. 27, letter from Reginald Darwin, 17 November 1879 and n. 1).
- ³ The enclosure has not been found.
- ⁴ CD possessed copies of six letters from Darwin to Reimarus (DAR 227.1: 12–13; see *Correspondence* vol. 25, letter to E. H. Sieveking, 11 December 1877 and n. 2). No mention of shorthand occurs, except for that which CD had already mentioned.

From R. F. Cooke 27 April 1880

50A, Albemarle Street, London. W.

April 27 1880

My dear Sir

We are as much puzzled as yourself, as to a Title for your new work.¹

I have set up the enclosed for you to alter, correct, revise, add to, or curtail as you think best & then send it back & I will let you have a fresh proof to consider.²

Yours faithfully | Rob^t Cooke

Cha^s Darwin Esq

DAR 171: 504

¹ See letter to R. F. Cooke, 23 April [1880]. CD had not settled on a title for *Movement in plants*.

² The enclosure has not been found, but was probably a proof-sheet of the title page of *Movement in plants*.

From Hermann Müller 27 April 1880

Lippstadt

April 27th 1880.

My dear Sir

I thank you very heartily for your kind letter and for the Preface of the second edition of your "Forms of Flowers", which indeed contains many notes new to me.¹

I myself am very well, but my Brother Fritz has had a hard grief about 10 months ago and is as yet very depressed. One of his daughters, the most endowed of all, in Berlin in a paroxysm of melancholy has precipitated herself out of the window and immediately found her death.²

I am now preparing my work on alpine flowers which will contain numerous illustrations and wick, I hope, before the end of this year will be published.³

Some days ago I have read with the greatest interest and with sincere admiration for your grandfather your and Ernst Krauses “Erasmus Darwin” (second edition)⁴

With the best wishes for your further health, my dear Sir, | yours very sincerely
| H. Müller.

DAR 171: 314

- ¹ CD's letter has not been found. *Forms of flowers* 2d ed. was published in July 1880 (Freeman 1977); the preface to the second edition contained eight pages of new material based on information CD had received in correspondence, as well as on work published after the first edition. CD had mentioned articles by Müller on gynodioecy and androdioecy (species having both hermaphrodite and female or hermaphrodite and male flowers on different plants; see *Forms of flowers* 2d ed., pp. ix–x).
- ² Fritz Müller's daughter Rosa had travelled to Germany in 1876 and studied at the high school for girls in Regensburg. Although she did well in examinations, she failed to get a teaching position in Berlin, and on 12 June 1879, died in a fall from the window of her third-floor room (for more on her death and Fritz Müller's reaction, see West 2016, pp. 153–4).
- ³ Müller's work, *Alpenblumen, ihre Befruchtung durch Insekten: und ihre Anpassungen an dieselben* (Alpine flowers, their fertilisation through insect agency and adaptations for this; H. Müller 1881) was published in January 1881 (Möller ed. 1915–21, 2: 404).
- ⁴ The German version of *Erasmus Darwin* had just been published (Krause 1880; see letter from Ernst Krause, 19 April 1880).

To ? 27 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
April 27. 1880

Dear Sir

I am very much obliged to you for your kindness in writing, but such malformations, as you mention, are not very rare & therefore do not possess much novelty.¹

I remain | Dear Sir | Yours faithfully | Ch. Darwin

John Wilson (dealer) (July 1986)

- ¹ The correspondent and the precise subject of the letter have not been identified, but CD was interested in cases where a malformation caused by an injury appeared to have been inherited (see, for example, *Correspondence* vol. 26, letter to W. H. Flower, [4 March 1878]).

To J. V. Carus 28 April 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Ap. 28th 1880

My dear Sir

I shall be right well pleased if you will translate my new book, & sh^d certainly have communicated with you in due time.¹ I am now going over the m.s for last time,

& the whole will be sent to Printers in 2 or 3 weeks.— I grieve to say that it is a big book with nearly 200 woodcuts. The type will be of 2 sizes: the larger for conclusions & the smaller with an account of the observations & experiments.² The work is the result of much labour; that I fear will displease many of your justly renowned physiologists in Germany.³

M^r. Murray inserted the announcement in *Nature* without my knowledge & used a term which I have had to employ largely in the book, but which is not fitted for the Title.⁴ I enclose Title, as at present seems to me best. The work is an attempt to bring all the diversified movements of Plants under one general law or system. I will send Proofs when ready, but this will not be for some month or two, for I must have a few weeks rest. Of course if you & Herr Koch⁵ sh^d think the book not worth translating you can draw back.

I hope & believe that it contains a fair amount of new matter.

Believe me my dear Sir, Yours very sincerely | Charles Darwin.

[Enclosure]

The Power of Movement
in Plants
with woodcuts
by
Charles Darwin LLD. F.R.S.
assisted by
Francis Darwin

1880
John Murray

Staatsbibliothek zu Berlin – Preußischer Kulturbesitz (Slg. Darmstaedter Lc 1859: Darwin, Charles, Bl. 176–178)

¹ Carus had become CD's German translator when he translated *Origin* 4th ed. (Bronn and Carus trans. 1867); he was translating all of CD's works for a collected edition in German (Carus trans. 1875–87). *Movement in plants* was published on 6 November 1880 (Freeman 1977).

² CD had first used print of different sizes in *Variation* (see *Correspondence* vol. 15, letter to John Murray, 8 January [1867]); he also used this feature in *Movement in plants*. A large number of the woodcuts were diagrams of patterns of circumnutation.

³ CD's son Francis Darwin had spent two summers in the laboratory of Julius Sachs at the Botanical Institute, Würzburg. CD was aware that Sachs had different views on several aspects of plant movement, notably the function of the root tip (see, for example, *Correspondence* vol. 27, letter to Francis Darwin, 28 June [1879]).

⁴ John Murray announced the forthcoming 'Circumnutation of plants' in *Nature*, 22 April 1880, p. 596. A similar announcement appeared in the *Athenæum*, 10 April 1880, p. 475.

⁵ Eduard Koch was Carus's publisher.

From Thomas Meehan 28 April 1880

Commonwealth of Pennsylvania | Board of Agriculture | Harrisburg | Germantown
April 28th. 1880

Botanical Department. | Thos. Meehan, Botanist.

Dear Mr Darwin,

There has been much talk in our papers lately about Mr. Darwin owning himself wrong about sterility in hybrids,—and I have taken occasion to set your position right, as I understand your position to be, in my department of the New York *Independent*.¹

You may also be interested in the succeeding item on hybrids in Nature. My friend Isaac C. Martindale has gone closely over the Bartram oak question—long supposed to be a hybrid—and I have asked him to send you a copy.²

I think in some cases hybrids may be more fertile than their parents. We get here considerable seed (and raise seedlings) from *Magnolia Soulangeana*, while I cannot remember ever to have seen any seed on *M. conspicua*, which I suppose to be one of its parents.³

I hope time is behaving leniently with you. It seems I have occasionally to “propose an amendment” to some proposition of yours,—but I think there is no one here who more thoroughly appreciates the great value of your labors, or who more sincerely prays that you may live long to continue your good work.⁴

Very Sincerely yours | Thomas Meehan

DAR 171: 113

¹ Meehan wrote the ‘Science’ column in the weekly New York newspaper; his item on CD’s position on the fertility of hybrids appeared in the *Independent*, 29 April 1880, p. 7. No copy of the column has been found in the Darwin Archive–CUL.

² The next item in the same column discussed differing opinions on the occurrence and fertility of natural hybrids. George Clinton Swallow was cited as having stated that the Bartram oak (*Quercus* × *heterophylla*, Bartram’s oak or oddleaf oak) reverted to its parent species (*Q. rubra*, red oak, and *Q. phellos*, willow oak), while George Engelmann argued that many oaks hybridised naturally and produced fertile offspring. Martindale’s pamphlet *Notes on the Bartram oak* (Martindale 1880) has not been found in the Darwin Archive–CUL.

³ *Magnolia* × *soulangeana* (saucer magnolia) is a hybrid of *M. conspicua* (a synonym of *M. denudata*, lily tree or Yulan magnolia) and *M. liliiflora* (purple or Mulan magnolia).

⁴ Meehan probably alludes to his belief that insects played a limited role in plant fertilisation and that self-fertilised plants were as vigorous as and more productive than those dependent on insect aid (see, for example, *Correspondence* vol. 25, letter from Thomas Meehan, 1 July 1877).

To Williams & Norgate 28 April [1880]

From Mr. C. Darwin, Down, Beckenham.

I am very sorry to have troubled you but I had quite forgotten that I possessed Vol. I.¹

Kindly send me whenever convenient the 2^d Vol.—

C. D.

Ap. 28th—

ApcS

Endorsement: '1880 | Chas. Darwin Esq | May 1st'

eBay (22 August 2019)

¹ A note in the same hand as the endorsement reads 'Nathusius Vorträge II', a reference to *Vorträge über Viehzucht und Raßenkenntniß* (Lectures on cattle breeding and breed information; Nathusius 1872–80) by Hermann Engelhard von Nathusius. CD's annotated copy of Nathusius 1872–80 is in the Darwin Library–CUL (see *Marginalia* 1: 636). CD had bought a copy of the first volume in late 1871 (see *Correspondence* vol. 19, letter to Hermann Settegast, 20 October 1871).

To H. D. Garrison 29 April 1880

*Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.*Ap. 29th 1880

Dear Sir

I thank you for your very kind note & for sending me the interesting case of inheritance, which I have been glad to see, though it is not likely that I shall again write on the subject¹

Dear Sir | Yours faithfully | Charles Darwin

Spike Tyson (private collection)

¹ Garrison's letter has not been found; the case of inheritance has not been identified. CD had collected reports of cases of inheritance of acquired characters when he was working on *Variation, Descent, and Expression*.

From H. N. Moseley 30 April 1880

University of London, | Burlington Gardens. W.

April 30. 80

Dear M^r Darwin

My friend M^r F V Dickins who is an enthusiastic student of science is much discomposed at having fallen more or less under your censure in Nature with regard to the Japanese shells heaps.¹ I hope you will not mind my troubling you with a short account of him.

He is a MB and BSc of this University having taken both degrees in the same year a feat I fancy never performed here by anyone else. He took a Medal in Physiology. He served some time as a Surgeon in the Navy but gave up the Medical profession and was called to the Bar. After practising in London some time he went to Japan in consequence of ill health and practised many years at Yokohama as a barrister, taking up especially cases in which Japanese interests and laws were at conflict with those of Europeans. He is a Chinese and Japanese scholar, speaking Japanese fluently and reading it also. He has translated several Japanese books and has one now in the press. He was for some time Editor and proprietor of the Japan Mail and wrote the greater part of it himself.

He is an expert systematic botanist and well versed in the Flora of Japan. He has given valuable collections of plants to Kew. I travelled with him for a month in Japan

and have constantly corresponded with him till he came to live in England. He is well read in all branches of science and reads most European languages. I have always regarded him as a man of most remarkable ability.

Prof'r Morses theories as to the refuse-heaps were discussed in the *Japan Mail* when he first enunciated them and I read the discussion.² It seemed to be the general opinion in Japan that Morse was a charlatan I have spoken to M^r A Agassiz and others about him and I gather that he is not thought much of.³

Amongst scientific men in the U.S. M^r Dickins has told me in conversation that he believes he was even at fault in identifying human bones and certain that he was wrong about the identification of bones of mammalia. I think his conclusions ought at all events to be taken cum grano.⁴

Two points in Morses letter to you published in *Nature* strike me as singular. Firstly he professes ignorance as to whether M^r Dickins has ever been in Japan. I do not think it possible that he cannot have known Dickins name and something of his doings in Japan. Moreover when Morse first went to Japan he sent me a circular asking for copies of monographs for Yedo library and promising to send publications in exchange. In writing to him I advised him to make the acquaintance of Dickins as a man who would sympathize with him in all scientific matters.

Secondly the sneer at Dickins because he does not adopt the newfangled term for Yedo. "Tokio" shows that Morse can have learnt very little of the language or history of Japan.⁵

I should certainly take Dickins estimate of the Japanese ardour for antiquarian science in Yedo as far more likely to come near the truth than Morses. The Japanese are absolutely ignorant of their own early history and even I believe of that of the last few centuries in great measure.

Dickins has had many law pupils and it is highly improbable that Morse has anything like the facility in conversing with the Japanese in their own language that he has.

I hope you will excuse this long letter. Dickins has studied your books with care and though he does not mind what Morse says about him feels hurt at having fallen under your displeasure I promised to write and explain to you that he is a man of considerable and varied information.

Yours truly H N Moseley

DAR 171: 259

¹ See letter to *Nature*, 9 April [1880] and n. 2. Edward Sylvester Morse had written to CD, enclosing a letter to *Nature* responding to a review of his work by Frederick Victor Dickins (letter from E. S. Morse, 23 March 1880). CD forwarded Morse's letter along with his own letter to the journal.

² The *Japan Mail* was a fortnightly English-language newspaper, partly subsidised by the Japanese government (Perez 1999, p. 81). The review of Morse's memoir on the Omori shell mounds (Morse 1879) praised the author's ability and also noted that the production of the work had been done entirely locally (*Japan Mail*, 20 September 1879, pp. 496–7).

³ Alexander Agassiz was curator of the Harvard Museum of Comparative Zoology.

⁴ *Cum grano salis*: with a grain of salt (Latin).

⁵ 'Edo' or 'Yedo' was the name of the city of Tokyo during the Tokugawa Shogunate (1603–1868); the city was renamed Tokyo (sometimes transliterated as Tokio, meaning 'eastern capital') in 1868 with the restoration of imperial power (Meiji Restoration); for more on the effects of the restoration on the city, see Iwatake 2003.

To H. N. Moseley 2 May 1880

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

May 2^d 1880

My dear M^r Moseley

If you are disengaged will you give us the pleasure of seeing you here on Sunday the 9th— The Galtons¹ & Andrew Clark will be here.— Your best plan will be, if you can come, will be to start from Charing X by the 5^o. 2' Train on Saturday evening, which reaches Orpington a little before 6^o & I will endeavour to send a carriage to meet you.

If you do not object to going on Box there will almost certainly be a place. Or you can take a fly.²

Believe me | Yours very sincerely | Ch. Darwin

Christie's, London (dealers) (online 31 October – 8 November 2018, lot 14)

¹ Francis Galton and Louisa Jane Galton.

² The box was the elevated seat of a carriage on which the coachman sat; a fly was a light, covered carriage. The station at Orpington was about four miles from CD's home at Down.

To Williams & Norgate 3 May [1880]¹

From Mr. C. Darwin, Down, Beckenham.

Please to send me Ray Lankester's Lecture or Essay on Degeneration.²

C. D.

May 3^d.

ApcS

Sotheby's (dealers) (11 July 2017)

¹ The year is established by the publication date of Edwin Ray Lankester's essay (see n. 2, below).

² Lankester's essay *Degeneration: a chapter in Darwinism* had been announced in the *Athenaeum*, 17 April 1880, p. 502; the text had first been delivered at a general meeting of the British Association for the Advancement of Science in Sheffield in August 1879 (Lankester 1880, p. 1; *Report of the 49th meeting of the British Association for the Advancement of Science, held at Sheffield* (1879): lxxxvi).

To F. J. Hughes 5 May 1880

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

May 5th—1880

My dear Cousin

It is indeed a long time since we met, & I suppose if we now did so we sh^d not know one another; but your former image is perfectly clear to me.—¹

With respect to your Essay I feel bound to express my conviction that no good Scientific Journal would publish it.² Science progresses only by the discovery of

new facts & direct deductions from them. There have, moreover, been so many attempts to reconcile Genesis & Science, that every editor w^d look askance at any new attempt.—

The death of your brother, my dear & very old friend, has been a grievous loss to every one who knew him; for I do not believe that there ever existed a man with a sweeter disposition.³

Pray believe me | My dear Cousin | Yours sincerely | Charles Darwin

American Philosophical Society (Mss.B.D25.573)

¹ CD had seen Frances Jane Hughes, William Darwin Fox's sister, on visits to the Fox family home when he was a student (see *Correspondence* vol. 5, letter to W. D. Fox, 24 [October 1852] and n. 7).

² A scrap of paper with this letter reads, 'What C.D. alludes to was only a bit I sent him, to ask him if he did believe Genesis & Science were both true & divine in their origin'. Frances later published *Harmonies of tones and colours developed by evolution*, in which she attempted to show that the vibrations of sound (musical notes) and light (colours) were regulated by the same scientific laws in agreement with religious dogma from the Bible (Hughes 1883, p. 9). The book was condemned as valueless and ridiculous in the *Athenæum*, 22 September 1883, p. 378. The publisher's marked copy of the *Athenæum* records the reviewer as 'Frost' (City University, London); this was Henry Frederick Frost, a frequent reviewer for the periodical.

³ William Darwin Fox had died on 8 April (see letter from C. W. Fox, 8 April 1880).

To E. R. Lankester 6 May [1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S. E. R.
May 6th

My dear D^r Lankester

If you happen to be disengaged will you give us the pleasure of seeing you here next Sunday.— It is a very short notice, but we thought until this morning that every bed w^d be occupied.—

D^r Andrew Clark, F. Galton & we hope Moseley² will be here.— If you can come, your best plan will be to leave Charing Cross on Saturday by the 5^o 2' train, & we will have some vehicle at Orpington Station.—

Yours very sincerely | Ch. Darwin

Private collection

¹ The year is established by the relationship between this letter and the letter to H. N. Moseley, 2 May 1880.

² Henry Nottidge Moseley.

From James Dixon 7 May 1880

Harrow Lands, | Dorking.
May 7. 1880.

Sir,—

May I take the liberty of drawing your attention to a trifling—very trifling—inaccuracy which occurs in your *Descent of Man?* (1871, i, 19)— You say “the

platysma myoides ... cannot be voluntarily brought into action.” In my own case this muscle is as much under control as any other in my body.¹ I can use either lateral portion separately, or both portions together. The contraction influences the skin as far upwards as the angles of the mouth, both of which become slightly depressed; and in a downward direction the action extends far enough to move the nipples very slightly. I have known several persons who had the platysma under control, but I never met with anyone in whom this was the case to so great an extent as in myself.

I may observe that I know something about muscles inasmuch as I was Demonstrator of Anatomy at St Thomas’s Hospital during ten years.² I was a standing wonder to my pupils, who were highly amused at my ability to use many muscles over which they had not the slightest control. I could—and can—move everyone of my facial muscles; and my palmaris brevis is as active now, when I am past 60, as it was 40 years ago.³

I hope these confessions will not cause you to regard me as the “missing link.”

Excuse my thus addressing you, and believe me to be very faithfully y^{rs}. | James Dixon

Charles Darwin Esq. F.R.S. | &c &c

DAR 162: 185

¹ See *Descent* 1: 19. The platysma myoides is a sheet of muscle in the neck extending from the collar bone to the lower part of the cheek. CD did not change this passage in *Descent* 2d ed., but had noted in *Expression*, p. 298, that most people could voluntarily contract the muscle.

² Dixon had trained at St Thomas’s Hospital, London, and served as assistant surgeon there from 1847 to 1851 (Plarr 1930).

³ The palmaris brevis is the muscle on the inner side of the palm below the little finger; it strengthens the grip of the hand on an object.

To T. H. Huxley [7 May] 1880¹

Down, Beckenham, Kent
Friday 1880.

My dear Huxley

I have just read your “Coming of age” in *Nature*.² I am quite delighted with it. Like everything which you write it seems to contain the whole subject in a few words, and is unanswerable.—

Ever yours with admiration | Ch. Darwin

Copy, ALS³

DAR 145: 289; Janet Huxley (private collection)

¹ The day and month are established by the publication date of Huxley’s article (see n. 2, below), and the relationship between this letter and the letter from T. H. Huxley, 10 May 1880. In 1880, 7 May was a Friday.

² The opening article of the issue of *Nature* for 6 May 1880 was Huxley’s “The coming of age of the Origin of Species” (T. H. Huxley 1880c).

³ The text from ‘everything’ has been transcribed from the original; the rest is from a copy.

To James Dixon 8 May 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
May 8th. 1880

Dear Sir

I am very much obliged to you for your kind note.¹ Should I ever bring out a new Edit. of my *Descent of Man* or *Expression of the Emotions*, which is not probable, your information will be useful to me.—²

Dear Sir | Yours faithfully & obliged | Ch. Darwin

Cambridge University Library (MS Add 6604: 17)

¹ See letter from James Dixon, 7 May 1880.

² No new editions of *Descent* or *Expression* were published in CD's lifetime.

To W. E. Darwin [9 May 1880]¹

Down
Sunday

My dear W.—

Read this note & observe that it is private.² If you can find out these 2 mean & can anyhow by direct or indirect means, influence them, I hope that you will for the sake of Science & on public grounds.³ Could you venture to call on them if strangers to you.

We have a tremendous party here, which has gone off very pleasantly & St. Andrew has been extremely agreeable, but I am pretty well tired, though less than usual.⁴

your affect Father | C. Darwin

Postmark: 10 MY 80

DAR 210.6: 157

¹ The Sunday before 10 May 1880, the date of the postmark, was 9 May. This letter was written on the blank pages of the letter from John Lubbock, 9 May 1880.

² See letter from John Lubbock, 9 May 1880.

³ John Lubbock was standing for election as MP for the University of London, and, evidently to drum up support for his campaign, had asked whether William Erasmus Darwin would contact Edwin Hearne and Henry Hermann Carlisle, two graduates of the university, who lived in Southampton (see letter from John Lubbock, 9 May 1880). '2 mean' is a mistake for '2 men'.

⁴ Andrew Clark, Francis Galton, Henry Nottidge Moseley, and Edwin Ray Lankester had been invited to Down on 9 May 1880 (see letter to E. R. Lankester, 6 May [1880]).

From John Lubbock 9 May 1880

High Elms
9 May 80

Private

Dear M^r Darwin

All is going well as far as we can judge at the University, but Jessel is no doubt a very formidable opponent.¹

There is a D^r Hearne at Southampton Also

Rev. H. H. Carlisle

LLB²

Do you think your son could say a word to them?³

Ever | Yours very truly | John Lubbock

DAR 210.6: 157

¹ Lubbock was standing for election as MP for the University of London; George Jessel, master of the rolls and a graduate of the University of London, was one of his opponents (Finstein 1953–5, p. 278).

² Edwin Hearne and Henry Hermann Carlisle were graduates of the University of London (*Historical record of the University of London* 1: 343, 425).

³ William Erasmus Darwin was a banker in Southampton.

To James Torbitt 9 May 1880

Down,
May 9, 1880.

My dear Sir

If I were a minister of the Crown I should think it my duty to adopt some such plan as that which you suggest. But as far as I can see political men care only about their party quarrels, and I could not ask Mr. Farrer to undertake such a task as to interest the Government in your scheme.¹ He has often remarked to me on the extreme difficulty of getting anything new undertaken. I should think your best chance would be through Mr. Forster, who has I believe unbounded energy;² but I would suggest that you should simplify your scheme. You could state if you thought fit that I had aided you, and got others including Mr. Farrer and Mr. Caird to aid you in your valuable labours.³ I think that this would influence Mr. Forster. I doubt whether the Agricult. Socs in England would take any trouble about growing your vars. Mr. Carruthers reported to the R. Agricult Soc., to which he is Botanist, that in his opinion your attempt to raise a fungus-proof var. was hopeless.⁴ I am sure that you will believe that I regret that I cannot offer any assistance in urging the Government to take an active share in your work.

Believe me, my dear Sir | Yours sincerely | Ch. Darwin.

DAR 148: 120

¹ CD had asked Thomas Henry Farrer to try to get government support for Torbitt's experiments to develop blight-resistant potatoes (see letter to James Torbitt, 30 March 1880). Torbitt had sent CD a report on the experiments he proposed to make (see letter to James Torbitt, 20 March 1880). Torbitt's report has not been found, but his plans for raising blight-resistant potatoes were outlined by CD in the enclosure to his letter to James Caird, 24 March 1880.

² William Edward Forster was chief secretary for Ireland.

³ James Caird had collected subscriptions to enable Torbitt to continue his experiments (letter to T. H. Farrer, 9 March 1880).

⁴ William Carruthers was botanist to the Royal Agricultural Society (R. Desmond 1994). Potato blight was caused by the fungus *Phytophthora infestans*. Carruthers had long dismissed Torbitt's work, claiming that disease would affect potato plants regardless of the method used for the tuber's reproduction (DeArce 2008, p. 211).

From T. H. Huxley 10 May 1880

4 Marlborough Place | N.W
May 10th 1880

My dear Darwin

You are the cheeriest letter writer I know & always help a man to think the best of his doings¹ I hope you do not imagine because I had nothing to say about 'Natural Selection' that I am at all weak of faith on that article² On the contrary I live in hope that as paleontologists work more & more in the manner of that 'second Daniel come to judgment' that wise young man Mr. Filhol we shall arrive at a crushing accumulation of evidence in that direction also³ But the first thing seems to me to be to drive the fact of evolution into peoples heads—when that is once safe the rest will come easy

I hear that ce cher Elam is yelping about again—but in spite of your provocative messages (which Rachel retailed with great glee) I am not going to attack him nor anybody else—⁴

I wish it were not such a long story—that I could tell you all about the dogs—they will make such a case for 'Darwinismus' as never was⁵ From the South American dogs at the bottom (*C. vetulus cancrivorus* &c) to the wolves at the top there is a regular gradual progression the range of variation of each 'species' overlapping the ranges of those below & above Moreover as to the domestic dogs I think I can prove that the small dogs are modified jackals & the big dogs ditto wolves I have been getting capital material from India and working the whole affair out on the basis of measurements of skulls & teeth⁶

However my paper for the Zoological Society is finished & I hope soon to send you a copy of it⁷

We were at Balliol yesterday on a visit to the Master & of inspection of our boy who seems as happy as need be & is getting on very well⁸

With kindest remembrances to M^{rs} Darwin | Ever | Yours very truly
| T H Huxley

DAR 166: 352

¹ See letter to T. H. Huxley, [7 May] 1880.

² Huxley had published an article titled 'The coming of age of the Origin of Species' in *Nature*, 6 May 1880 (T. H. Huxley 1880c).

³ The Biblical character Daniel was renowned as a wise judge, but the phrase used here is from William Shakespeare's *Merchant of Venice*, 4.1.335. According to Huxley, Henri Filhol's fossil discovery of the common progenitor of cats, dogs, bears, civets, hyenas, and raccoons was among the evidence that contributed to making animal evolution 'no longer a speculation, but a statement of historical fact' (T. H. Huxley 1880c, p. 4).

⁴ *Ce cher*: that dear (French). Charles Elam published 'The gospel of evolution' in the *Contemporary Review* in response to Huxley's lecture 'On the coming of age of the Origin of Species'; Elam expressed his doubts about CD's 'doctrines', especially the theory of natural selection, criticised Huxley for claiming that evolution was now established as a fact, and concluded that organic evolution was a mere figment of the intellect (Elam 1880; T. H. Huxley 1880c). Rachel Huxley was Huxley's daughter.

⁵ On 6 and 13 April, at the Royal Institution of Great Britain, Huxley had delivered two lectures, 'On dogs and the problems connected with them', in which he gave a complete survey of the dog family,

both wild and domestic (L. Huxley ed. 1900, 2: 10). The lectures were never published, but a short report on the first one appeared in *The Times*, 7 April 1880, p. 7.

⁶ Huxley had been sent skulls from India by Joseph Fayrer (L. Huxley ed. 1900, 2: 11). *Canis vetulus* is a synonym of *Lycalopex vetulus* (the hoary fox); *C. cancrivorus* is a synonym of *Cerdocyon thous* (the crab-eating fox). Neither species is closely related to true foxes; they are more closely related to dogs.

⁷ Huxley refers to his paper 'On the cranial and dental characters of the Canidae' (T. H. Huxley 1880d).

⁸ Huxley's son Leonard Huxley had matriculated at Balliol College, Oxford, on 21 January 1880 (*Alum. Oxon.*). The master of the college was Edward Caird.

To T. H. Huxley 11 May 1880

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

May 11th 1880

My dear Huxley

I had no intention to make you to write to me, or expectation of your doing so; but your note has been so far "cheerier" to me than mine could have been to you, that I must & will write again.—¹ I saw your motive for not alluding to natural selection & quite agreed in my mind in its wisdom.² But at the same time it occurred to me that you might be giving it up, & that anyhow you could not safely allude to it, without various 'providos' too long to give in a Lecture. If I think continuously on some half-dozen structures of which we can at present see no uses, I can persuade myself that natural selection is of quite subordinate importance. On the other hand when I reflect on the innumerable structures, especially in plants, which 20 years ago would have been called simply 'morphological' & useless, & which are now known to be highly important, I can persuade myself that every structure may have been developed through natural selection.— It is really curious how many out of a list of Structures which *Bronn enumerated*, as not possibly due to natural selection because of no functional importance, can now be shown to be highly important.³ Lobed leaves was, I believe, one case & only 2 or 3 days ago, Frank showed me how they act in a manner quite sufficiently important to account for the lobing of any large leaf.—⁴

I am particularly delighted at what you say

($\frac{1}{3}$ of page) arrived in 'Var. of Dom animals' at exactly the same conclusion with respect to the domestic dogs of *Europe & N. America*.—⁵ See how important in another way this conclusion is; for no one can doubt that

($\frac{1}{3}$ of page) & how well this supports the *Papflasian* doctrine that domestication eliminates the sterility almost universal between forms slowly developed in a state of Nature.—⁶

($\frac{1}{3}$ of page) but (rest of line missing) own fault.

Ever yours | Ch. Darwin

Plants are splendid for making one believe in natural selection, as will & consciousness are excluded. I

($\frac{1}{3}$ of page) (mi)ght as will say that a pair of scissors or nutcrackers had been developed through external conditions as the structure in question.⁷

Incomplete

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 342); Janet Huxley (private collection)⁸

- ¹ See letter from T. H. Huxley, 10 May 1880.
- ² Huxley had not referred to natural selection in his article on ‘The coming of age of the Origin of Species’ (T. H. Huxley 1880c). He had initially had reservations about the mechanism of natural selection, but consistently defended CD’s evolutionary views (see, for example, [T. H. Huxley] 1860).
- ³ Heinrich Georg Bronn’s objections had appeared in an additional chapter appended to his translation of *Origin* (Bronn trans. 1860, ch. 15). The list of structures has not been located in Bronn’s work. It is possible this was a list sent by Bronn at the time he was translating *Origin* into German.
- ⁴ Francis Darwin was CD’s botanical assistant.
- ⁵ Huxley had recently given two lectures on the genus of dogs, arguing that small domestic dogs were modified from jackals and large dogs from wolves (see letter from T. H. Huxley, 10 May 1880). CD had made the same point in *Variation* 2d ed., 1: 34.
- ⁶ In *Variation* 2d ed., 1: 32, CD agreed with Pyotr Simon Pallas’s assumption that a long course of domestication eliminated the sterility exhibited by recently confined wild animals. Pallas had made this claim in his ‘Mémoire sur la variation des animaux’ (Pallas 1780). ‘Pafasian’ is a mistake for ‘Pallasian’.
- ⁷ CD was possibly responding to criticisms of the theory of natural selection made by Charles Elam (see letter from T. H. Huxley, 10 May 1880 and n. 4).
- ⁸ The letter as far as ‘I am delighted at what you say’ is at Imperial College; the rest is in a private collection. The top of the last sheet of the letter has been torn off.

From James Torbitt 13 May 1880

*J. Torbitt, | Wine Merchant. | 58, North Street, | Belfast,
13 May 1880.*

Charles Darwin Esqr. | Down.

My dear Sir,

I have now the honour and pleasure to report progress.¹

I have planted six acres new twice-crossed varieties of the potato, in place of eleven acres planted last year: this decrease will keep the expenditure within the limit.

For the purpose of crossing, I have planted some of the best tubers of some of the best and most dissimilar twice crossed varieties of 1878 and 1879, and in regard to this, I would wish to ask, may I rely on the pollen living a few days or a few weeks after having been collected, or must I use it immediately after collection?

Also, I have coming forward 5,000 seedlings grown from seeds of twice crossed varieties, and 1,000 seedlings grown from seeds of seedlings obtained from twice crossed varieties (perhaps thrice crossed, as they are grown in close proximity) Having all the means for growing these seedlings on hand, it would have saved but little restricting their number.

They are growing vigorously and I am protecting them against enemies much better than ever before.

They are beautiful, almost all I have looked at, as round as billiard balls—but they are only the size of peas as yet. In a few days I hope to commence planting them out into the field.

Beyond this I have induced some 14 Landowners to grow in the aggregate some 14 acres for me, the conditions being that they charge me nothing, and return to me one half the crop, and if this process of dissemination can be carried out on those terms for three or four years, we may be all repaid, as Mr Caird says, for our expenditure of time and money.²

I am my dear Sir, as always, most respectfully and faithfully yours | James Torbitt

DAR 178: 165

¹ Torbitt had sent CD his plans for producing blight-resistant potatoes (see letter to James Caird, 24 March 1880). He had been unable to secure government funding for his experiments, and was dependent on CD and other supporters for private donations (see letter to James Torbitt, 9 May 1880). He had emphasised that he would keep the expenditure within the limit of £150 if CD wished (see letter from James Torbitt, 1 April 1880 and n. 4).

² See letter from James Caird, 25 March 1880.

To John Fiske 14 May [1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
May 14th

My dear M^r Fiske

I suppose that you have reached England.² I did not write before, because we have had a succession of visitors & I absolutely require a day or two rest after any one has been here.—Some persons now in the house leave tomorrow evening & others are coming on Tuesday morning.³ If you & M^{rs} Fiske happen to be disengaged on Friday (21st) evening, would you come here to dinner & to sleep?⁴ There is a good train which leaves Charing Cross at 4^o 12' P.M.—

On Monday the 24th we leave home for a fortnight for me to rest.⁵ If it would be more convenient to you to come here, after June 8th or thereabouts it would suit us equally well, & we sh^d be very glad to see you & M^{rs} Fiske then.

In Haste to catch Post | Yours sincerely | Ch. Darwin

Very many thanks for all the kind expressions in your note.⁶

The Huntington Library (HM 8269)

¹ The year is established by the relationship between this letter and the letter from John Fiske, 20 April 1880.

² John Fiske had travelled from Cambridge, Massachusetts, to England in order to give lectures at the Royal Institution of Great Britain (see letter from John Fiske, 20 April 1880).

³ On 14 May, Mabel Wedgwood and Constance Rose Wedgwood and their fiancés Arthur George Parson and Johannes Hermann Franke were at Down House (letter from Emma Darwin to H. E. Litchfield, 14 May [1880] (DAR 219.9: 233)). It is unclear who the next set of visitors were on 18 May.

⁴ John and Abby Morgan Fiske visited Down from 21 to 22 May 1880 (Emma Darwin's diary (DAR 242), letter from Emma Darwin to H. E. Litchfield, [22 May 1880] (DAR 219.9: 234)).

⁵ CD was in Bassett, Southampton, from 25 May to 8 June (CD's 'Journal' (Appendix II)).

⁶ See letter from John Fiske, 20 April 1880.

From F. E. Abbot 15 May 1880

Office of The Index Association, | 231 Washington Street. | Boston, Mass.,
May 15, 1880

Dr. Charles Darwin, | Down, Beckenham, Kent:

Dear Sir,

Permit me to thank you very gratefully for your generous letter of April 15, with its enclosed bank-cheque of £5, for the *Index*.¹ To know that the paper possesses any

value in the eyes of one of whom the whole world is proud is a great honor, which I am keenly sensible of. As the issue of this week will inform you, I am to leave the editorial chair in a few weeks; but it is one of the precious memories of my long service to know that it has secured me the sympathy of one for whom my reverence is so great.

The article in the *Literary World*, which I took the liberty of mailing to you was not written by me, nor do I know the writer; but I imagined it might amuse you to see one of the fools so handsomely exposed.² But your reputation in America cannot be eclipsed by any Philadelphia lawyer. Every rising man of science is a Darwinian here. Indeed, I know nothing in the history of opinion more remarkable than the revolution you have made in so short a time. For myself, I am not a competent judge of the scientific aspect of the argument; what has excited my boundless sympathy and admiration is the splendid example of the love of truth for its own sake which you have set to mankind. The intellectual and moral *quality* of your work I can and do appreciate; and my soul's deepest desire is to do what I can to make that quality universal in the world.

I am, dear sir, | Yours gratefully, F. E. Abbot.

DAR 159: 6

¹ See letter to F. E. Abbot, 15 April 1880; CD had sent the money to cover the costs of receiving the *Index*, a radical religious periodical founded and edited by Abbot.

² Abbot had sent CD a copy of the *Literary World*, 27 March 1880, containing a review titled 'A Philadelphia lawyer's views of *Darwinism*' (see letter to F. E. Abbot, 15 April 1880). The book reviewed was *The refutation of Darwinism* by T. Warren O'Neill, a Philadelphia lawyer (O'Neill 1880).

From J. Harris¹ 16 May 1880

16 5 1880

Sir

I hope you will look upon the liberty I have taken in sending this note as a secondary matter, being prompted to respectfully ask if you can inform me of the reasons that explain the existence of "apes" in our day, or that material, out of which the writer as a member of the human family has emerged—why the mould, as it were, still remains. Can we look upon those living ancestral types as the residuum of development, at one period, they have I presume been supreme, and that process that has thinned their ranks will if I have patience eventually obliterate them.

A Friend who takes advantage of my interest in your writings put the question to me—"why have not the apes been swallowed up in victory"; (or anything lower than anthropoid apes.), perhaps the reason I was vague, is that much of what has emanated from your pen, is as yet, untrodden ground.

I hope Sir you will not think me inquisitive, in the generally understood manner by this letter, but I shall be extremely thankful & obliged to receive an answer if it is not inconvenient. I shall probably be made aware of an unsuspected weakness on my part considering the simplicity of my request.

Incomplete
Copy
DAR 198: 87

¹ J. Harris, who has not been further identified, sent this copy of a letter he had sent to CD to Francis Darwin in 1882. See also letter to J. Harris, 21 May 1880.

To B. J. Sullivan 16 May [1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
May 16th

My dear Sullivan

I have received from the U. States a book, giving an account of the adventures of 2 Missionaries in Patagonia, at the time when the Beagle was surveying. One of the missionaries was the Rev Titus Coan, who has done grand work in the Sandwich Isl^{ds}.—² The book has us, & therefore I send it by this Post to you, for the chance of your liking to read it.—³ Will you kindly return it some time, as some of our relations want to read it.—

Many thanks to you for sending me several numbers of the Missionary Journal.— I sincerely hope poor M^r Bridges will recover his health in this country.—⁴

Bishop Sterling is a relation of my son-in-law, M^r Litchfield.—⁵

Ever yours very sincerely | Ch. Darwin

Sullivan family (private collection)

¹ The year is established by the relationship between this letter and the letter from B. J. Sullivan, 20 May 1880.

² Titus Coan's *Adventures in Patagonia* details his and William Arms's efforts as missionaries in Patagonia from 14 November 1833 to 25 January 1834 (Coan 1880). Coan was a missionary in the Sandwich Islands (Hawaii) from 1835 to 1882.

³ Coan had referred to CD's and Robert FitzRoy's observations in Patagonia, Tierra del Fuego, and the Falkland Islands, giving emphasis to the attempt to establish the missionary Richard Matthews in Tierra del Fuego in 1833 (Coan 1880, pp. 269–86).

⁴ Thomas Bridges's ill health, and his departure for England with his family on 17 October 1879, was reported in the *South American Missionary Magazine*, 2 February 1880, pp. 34, 37–8; improvements to his health were reported the following month (*ibid.*, 1 March 1880, p. 52).

⁵ Waite Hockin Stirling, bishop of the Falkland Islands, was Richard Buckley Litchfield's cousin by marriage; Stirling was married to Louisa Jane Phinn, daughter of Litchfield's aunt Jane Phinn.

To Giovanni Canestrini 17 May 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
May 17th 1880

Dear Sir

I am very much obliged to you for your great kindness in having sent me your 'Teoria &c', which seems beautifully got up & very well illustrated by figures.—¹

With much respect | I remain Dear Sir | Yours faithfully & obliged | Charles Darwin

The estate of Sandro Onestinghel (private collection)

¹ Canestrini's *La teoria di Darwin criticamente esposta* (Darwin's theory critically explained; Canestrini 1880) contains twenty-five figures. CD's copy is in the Darwin Library–Down.

From E. A. Darwin 20 May [1880]¹

May 20

Dear Charles—

Thanks for the two pamphlets. That by Zöckler is I think entirely worthless. Just got up for a lecture out of you & Krause.² Being Prof of Theology he winds up by saying that he does not enter into the question whether your theories are true—or only castles in the air & a Fata Morgana.³ Poor Price must have a bad time coming & I will do what you suggest.⁴

Yours affec | EAD

DAR 105; B112

¹ The year is established by the publication date of Otto Zöckler's pamphlet (see n. 2, below).

² Otto Zöckler had published a pamphlet on Erasmus Darwin as a forerunner of Darwinism (Zöckler 1880); he evidently drew on *Erasmus Darwin*, which contained a translation of Ernst Krause's essay on Erasmus Darwin, and CD's biography of his grandfather. The second pamphlet was possibly the review of Erasmus Darwin that was published in *Revue scientifique de la France et de l'étranger* on 15 May 1880 (Ferrari 1880).

³ 'Fata Morgana': a mirage, due to a thermal inversion, that makes distant objects, such as ships, appear to be floating in the sky.

⁴ Possibly John Price, who was friends with both CD and Erasmus Alvey Darwin. A year later, in September 1881, CD wrote that he hoped that Price was suffering 'less than formerly' (*Correspondence* vol. 29, letter to John Price, 3 September [1881]).

From S. T. Preston 20 May 1880

25 Reedworth Street | Kennington SE. London

May 20th 1880

Dear Sir

I thank you much for your letter and kindness in expressing opinions on my paper.¹ There is however one point I should like to allude to, & which appears not to have conveyed quite the meaning I had intended—and which is from fault of inadequacy of expression on my part. I had meant the words "bound" in intelligence, "rapid" advance &c to possess a *relative* signification: i.e. in reference to the great length of period corresponding to man's antiquity. My main object was to call more particular attention to certain causes, which, if they alone acted, would seem to conduce to a *progressive* rise in the development of brain structure.

No doubt *secondary* causes (such as change of climate, of geological conditions, conquest of a people &c &c) are also in action;² sometimes in favour of progress, sometimes the reverse—and it would appear to me possible that the effects of these secondary causes would be those most noticed within so relatively short a period as 3 to 4000 years (say some 60 to 80 generations), and that therefore no reliable idea could probably be got of the *average* rate of progress of man by contemplating

that period (i.e. the historical period). The remarks in your letter have however shown me that a still closer study of the subject (which appears an interesting one) would be desirable; and also an additional perusal of works relating thereto, which circumstances have not permitted me to do so much as I should have liked.

In reading recently again your work on "The Descent of Man", I have been much interested in the part relating to the Evolution of morality.³ Having made this subject a special study for the last 2 or 3 years, I should like to make a few remarks, which I need not say have been very carefully considered, and which in view of the interest of the subject, might not perhaps be thought unworthy of attention.

It has appeared to me that the supposed *undesirability* of making Self or Self Interest the standard of morality is in reality questionable: and that the natural striving to avoid this *seemingly* undesirable conclusion has unconsciously (as it were) somewhat biased the judgment in the careful search after truth—though one would of course *wish* to avoid any bias. To me it seems quite impossible to conceive a *motive* for an action which does not affect *Self* in some way: or in the form of an axiom (and most fundamental truths, it may be observed, are founded on axioms) it may be said that 'a man cannot be affected by anything which does not affect *himself* (or touch his own individuality)'.

It will probably be admitted so far, that man has few instincts now (for external actions) which he cannot *at least test* by the light of his reason, so as to see whether they are desirable or not—at any rate it would not be a thing to be wished that man should be dominated by instinct (i.e. without rational *motive*). Now (as I venture to submit) the only rational—and even only possible—motive for an action (which is not instinctive or random) is *Self* or Self-Interest. A man asks himself before taking any course (i.e. if he is rational and weighs his actions)—"how does this course affect my interests"? I would even maintain (as it is known that some able minds have done) that he *could* not act without this (i.e. not rationally). For that which does not affect *him* (or touch his individuality) cannot make *him* act (or is not a *motive*). "Sympathy"—which it will be admitted is a mere phrase (or *effect*)—only can affect a man because it affects *Self*: and I would even venture to contend that it is *desirable* that this should be so. For where would friendship (or love) be without *Self* on each side? (I think the meaning of this will be gathered, though mode of expression on subjects of this kind is not always easy). Self Interest is (in my contention) the very essence of love or friendship. Is it not *because* of the pleasure felt (self-interest) on each side that friendship is a desirable thing? Remove the element of *Self* (the pleasure enjoyed) from each side, and where is the friendship (or the love)? Could there be friendship without interest? In the same line of argument—could a really "*disinterested*" action be said to be worthy of praise? (meaning by this a mere instinctive or random action). Is it not rather *because* of the self-interest (or the element of *Self* in the action) that praise is *deserved*? A man helps another because he takes pleasure in the esteem of that man (or of mankind generally). *Because* that was his object, he *deserved* the praise and esteem all the more: for he thus deliberately *earned* it. On the other hand, if that had not been his object, he would have deserved nothing—or (in other words), a

person who acted without any motive of *Self*, could not *deserve* any praise, because he acted by mere instinct or at random; and consequently could have undergone no sacrifice. The more strongly the value of the praise and esteem of others is weighed beforehand, and balanced against the disadvantage of the sacrifice, the greater will the sacrifice be, and consequently the more will the reward be *deserved*.

To sum up therefore, I would contend that Self-Interest as a motive for conduct is a thing to be commended—and it certainly is I think (at least I may conscientiously say that this is very clear to my mind) the only *conceivable* rational motive of conduct: and always *is* the tacitly recognised motive in all rational actions. The *effect* “sympathy” or sociability then shows itself to be the highest form of Self-Interest. Perhaps it may not be thought out of place if I state here that in my own experience on the subject of *Morality*, some errors have been so perseveringly drilled (as it were) or impressed on the brain for centuries that it becomes far more difficult to *unlearn* than to learn, and it is only after the most persistent thought that errors are discovered, which one afterwards wonders could have escaped detection so long.

I think it admits of being clearly proved that that desirable consummation, the greatest happiness (or interest) of the greatest number, can *only* be attained by each one consulting his own happiness (or interests)—and that consequently *Morality* is not in any way opposed to perfect individual Liberty.⁴

One of the points which has tended to prevent the general adoption of a common standard of *Morality* is (as it seems to me probable) the supposed idea that Self-Interest and Selfishness are synonymous. Whereas it may become clearly apparent on analysis, that Selfishness (or the attempt to advance oneself at the expense of others) is the very *opposite* of Self-Interest—from the fact that Sociability is one of the highest forms of Self-Interest (on account of the immense advantages gained therefrom). The apparent ease with which the Evolution of *Morality* would take place on the basis of Self-Interest, is perhaps too obvious to need special comment.

I cannot avoid the conclusion that the extraordinary circumstance of no universally recognised standard of conduct existing—in spite of the immense advance of the other sciences—has been greatly due to the supposed bugbear attaching to Self-Interest (or to the mistaking of this for Selfishness). In fact it is notorious that many great minds have felt themselves inevitably gravitating towards Self Interest as the natural standard of morality, but its *fitness* has apparently escaped them; or they have been frightened at the result—and the very ingenuity (sometimes almost desperate) of the efforts made to evade this deduction, are themselves surely among the best illustrations of its truth. It has been thought that if everyone were to follow his own interests, he would be in continual strife with his neighbours—forgetting that Sociability may be one of the strongest elements in Self-Interest. Hobbes (as Lange well relates in his “Geschichte des Materialismus”)⁵ was unable to contend against the logical conclusion that Self Interest was the motive of conduct: and he designed a scheme (as is known) by which the State was to have the dogmatic control over *Morality*—so as to prevent Self-Interest from causing people to “tear each other in pieces”.

In examining the question carefully, it will become apparent, I think, that the neglect to identify Morality with Self-Interest has caused great evils. It has acted as the greatest discouragement to Virtue by making it appear *against* ones interests, and has given rise to the invention of those dogmas (such as that evident injustice *eternal* punishment) as *supposed* checks—and the inculcation of which may do so much harm to the young (especially when of inquiring minds). If it were invariably taught that the path of virtue, or strict integrity, was absolutely in accordance with Self-Interest, (in fact that virtue is its own reward), and that thieving or deceit were to be avoided because they were *against ones interests*: instead of the absurd statement that they are “wicked” (which only makes them more attractive, from the intangible nature of the reason)—I am convinced that immense good would result. In fact it would be doing no more than making morality stand upon Reason—its only sure basis.

I have ventured to go into these points, on account of the time and attention I have devoted to the subject (with the former valuable co-operation of a friend). As the matter is an interesting one, I need not say that if the conclusions carefully thought out, should be deemed worthy of consideration, I should much esteem anything you might like to remark on the subject—though quite at your leisure and wish: as I should not like this letter to have any appearance of presuming on your kindness in commenting upon my last paper.

I am | Your's truly | S Tolver Preston

Charles Darwin Esqre F R S &c—

DAR 174: 61

- ¹ CD's letter to Preston has not been found; Preston's paper 'On a point relating to brain dynamics' was published in *Nature*, 13 May 1880, pp. 29–30.
- ² In the preface of *Descent* 2d ed., p. v, CD emphasised that changes in corporeal structure and mental power could not be attributed to natural selection alone, but to some degree depended upon the direct and prolonged action of changed conditions of life. In his paper, Preston had argued that the brain directed corporeal actions and was the physical site of reasoning processes as well as being the physical basis of individuality or identity.
- ³ CD had discussed the development of moral faculties in *Descent* 2d ed., pp. 127–45, arguing that they originated in the social instincts (p. 132).
- ⁴ Preston appears to be responding to CD's statements in *Descent* 2d ed., pp. 120–1, where CD argued that the foundations of morals did not lie in a form of selfishness based on pleasure or happiness, but in a deeply planted social instinct motivated by sympathy; most actions were directed towards the general good of the species rather than the general happiness.
- ⁵ Friedrich Albert Lange's *Geschichte des Materialismus* (History of materialism), first published in 1866 and translated into English in 1877, included a discussion of the views of Thomas Hobbes (Lange 1866, ch. 14).

From B. J. Sullivan 20 May 1880

Bournemouth
May 20/80

My dear Darwin

Thank you very much for sending me the book, which I now return.¹ It was indeed interesting. It is singular that we never heard of their visit to the Patagonians,

and especially so that at Port Louis, where they had seen them, we heard nothing of them. It must have been before our last visit there as they saw our tender, evidently, at the West Island, though they call her by her old name “Unicorn”.²

I shall pick out from the magazines of the last year a few pages showing the progress made with our Mission and send them to the publishers at New York for M^r. Field who may be able to forward them to M^r. Coan if still alive³

I have not been so well lately as I feel sometimes the old weakness in right leg and foot and frequent slight headaches. We hope this summer to spend two months at the old Hydropathic Establishment at Blarney, hoping it may us all some good My wife walking more stiffly than ever & my eldest daughter not allowed to go out of house except in a carriage or chair as her weak knee gets no better.⁴

Hamond & his wife have been with us lately;⁵ he had spent the Spring at Falmouth with much benefit to a weak chest, & Cough.

Our kind regards to M^{rs}. Darwin, and all your party. | Believe me | yours very sincerely | B. J. Sullivan

DAR 177: 311

¹ Coan 1880 (see letter to B. J. Sullivan, 16 May [1880] and n. 2).

² Titus Coan and William Arms had been in Patagonia from 14 November 1833 to 25 January 1834 to assess whether a mission might be established there; they were in Port Louis, Falkland Islands, from 5 to 12 February 1834, and on 15 February 1834 they saw the *Unicorn*, a vessel that Robert FitzRoy had purchased and renamed the *Adventure*, which was engaged in surveying the islands (Coan 1880, pp. 234–41).

³ The *South American Missionary Magazine* regularly reported on the mission station in Ushuaia, Tierra del Fuego. Henry Martyn Field had written the introduction to Coan 1880.

⁴ St Ann’s Hill Hydropathic Establishment, near Blarney, Ireland, was established in 1843; it is described in Parratt 1889, pp. 149–51. Sullivan had described his own lameness as well as that of his wife, Sophia Sullivan, and his eldest daughter, Sophia Henrietta Sullivan, in his letter of 9 June 1879 (see *Correspondence* vol. 27).

⁵ Robert Nicholas Hamond and Sophia Caroline Hamond.

To J. Harris 21 May 1880

Down Beckenham Kent
May 21. 1880

Dear Sir

No one could answer your question.¹ I do not believe one organism can be named, of which we know precisely the conditions of its existence. We cannot explain why one species is common and another rare in the same district, or why one is wholly absent from another district; we can give no explanation why certain species have increased in number during the last $\frac{1}{2}$ century in this country. Until we can explain such comparatively simple cases, it is not likely that we should be able to say why one form has been modified and not another; or why one species has become extinct and not another.

Dear Sir | Yours faithfully | Ch. Darwin

DAR 145: 5

¹ See letter from J. Harris, 16 May 1880. Harris has not been identified.

From William Spottiswoode 21 May 1880

Her Majesty's Printing Office, | East Harding Street, Fetter Lane, E.C.
21 May 80

Dear M^r Darwin,

one "S. M. Hersfeld, Doctor (Bonn & Berlin) &c", having been ineffectually to Mundella, comes to me begging help.¹ He says he has a quantity of apparatus & books which he has not funds to get out of the carriers' hands.² I wd. help if the man be deserving, & if I could see my way to doing him any good; but the case seems to be as hopeless as can be. The only thing which (as it appears to me) remains for him to do is to return to his home, & look to any relatives that he may have to help him. He showed me a note from you giving him some assistance, but telling him not to expect more.³ This is my apology for asking if you have any suggestion to make about him.

vy sincerely yrs | W Spottiswoode

DAR 177: 238

¹ S. M. Herzfeld had appealed for help from Anthony John Mundella.

² See also letter from S. M. Herzfeld, 2 April 1880.

³ CD's letter has not been found, but see the letter from S. M. Herzfeld, 4 April 1880.

To S. T. Preston 22 May 1880

Down,
May 22nd, 1880.

Dear Sir

Your letter appears to me an interesting and valuable one;¹ but I have now been working for some years exclusively on the physiology of plants and all other subjects have gone out of my head, and it fatigues me much to try and bring them back again into my head. I am, moreover, at present very busy as I leave home for a fortnight's rest at the beginning of next week.² My conviction as yet remains unchanged, that a man who (for instance) jumps into a river to save a life without a second's reflexion (either from an innate tendency or from one gained by habit) is deservedly more honoured than a man who acts deliberately and is conscious for however short a time that the risk and sacrifice give him some inward satisfaction.³

Wishing you success in your studies, I remain, Dear Sir | Yours faithfully
| Ch. Darwin

You are of course familiar with Herbert Spencer's writings on Ethics.⁴

DAR 147: 250

¹ See letter from S. T. Preston, 20 May 1880.

- ² CD was in Southampton from 25 May to 8 June (CD's 'Journal' (Appendix II)).
- ³ Preston had argued that morality originated in self-interest; CD believed that morality had originated in the social instincts (see letter from S. T. Preston, 20 May 1880 and n. 3). CD had given the drowning man example in *Descent* 2d ed., p. 110.
- ⁴ CD probably referred to Herbert Spencer's *The data of ethics* (Spencer 1879). His copy is in the Darwin Library–Down.

From William Spottiswoode 24 May 1880

41, Grosvenor Place. | S.W.
24 May 1880.

Dear Mr. Darwin,

Many thanks for your note.¹ I thought it likely that you had furnished Dr. Hertzfeld with money for getting his goods out of the carriers' hands; but he does not appear to have used it for that purpose. I also was grieved to see the man in his present plight; and this must be my apology for troubling you about him.² But I fear that his case is one of those in which it is impossible to do any real good; in fact, since he will not use any help given him, in a way calculated to enable him to help himself, I fear that any small donation would only draw him deeper in to slough of despond from which he can only be extricated by being sent home through his consul as "a distressed Austrian subject".

I fear that there is no chance of seeing you at our *Conversazione* at the R.S. on June 2nd.³

Yours very sincerely, | W Spottiswoode

TLS
DAR 177: 239

- ¹ CD's letter has not been found.
- ² See letter from William Spottiswoode, 21 May 1880, and letters from S. M. Herzfeld, 2 April 1880 and 4 April 1880, for S. M. Herzfeld's appeals for help.
- ³ William Spottiswoode was president of the Royal Society of London; CD did not attend the *conversazione* on 2 June 1880. The *conversazioni* were exhibitions of scientific discoveries, artefacts, and art works held at the society's premises in Burlington House.

From Francis Darwin [25–7 May 1880]¹

My dear Father,

I hope you got a telegram about *Convulvulus*.² The thing I found on working out the measurements is that they told nothing at all. A day is too long a period—the bits of stem which are growing well grow at a tremendous rate for a couple of days & then almost stop so that I could never get any fair comparisons³

Today I am measuring 4 plants every 4 hrs 7^{am}—11—3—7^{pm} I can measure to $\frac{1}{2}$ mm & I think with the rapid growth of 1 to 2 $\frac{1}{2}$ mm pr hour I ought to make out. They have alternate 4 hr periods of light & dark— If I see no good results today I shall shut up.

Please give my love to Sara & say I hope it will not matter my coming later instead of when I ought to I think I must go to Brittany by a boat leaving S'hampton Monday night⁴ I wonder if Ubbadub remembers S'hampton at all.⁵ | Yrs affec
F. D.

I shall certainly come on Friday⁶

DAR 274.1: 65

- ¹ The date is established by the references to CD's and Francis Darwin's visits to Southampton; Francis must have written after Emma and CD's departure for Southampton on 25 May and before his arrival there on 28 May (see nn. 4 and 6, below).
- ² The telegram has not been found. *Convolvulus* is the genus of bindweed. On 4 May 1880, CD recorded that he was working on sleep in cotyledons of *C. major* (a synonym of *Ipomoea purpurea*, common morning-glory; DAR 209.9: 12). Another undated note, about the growth of *C. major* in sand, records a task for Francis: 'Get Frank to give prescription about the nutritive fluids—' (DAR 68: 21).
- ³ Francis Darwin appears to have been trying to measure the rate of growth of plants relative to the amount of light present; he was possibly using the auxanometer (a self-recording instrument for measuring growth) built by Horace Darwin in 1876 (F. Darwin and Acton 1894, p. 140 n. 2). See plate on p. 210.
- ⁴ CD stayed with Sara and William Erasmus Darwin in Southampton from 25 May to 8 June 1880 (CD's 'Journal' (Appendix II)). Emma Darwin's diary records that Francis Darwin left for Brittany on Monday 31 May 1880 (DAR 242).
- ⁵ Ubbadub was a pet name for Francis's 3-year-old son, Bernard Darwin; Bernard and Francis lived with CD and Emma Darwin, and had accompanied them to Southampton on their previous annual visit (letter from Emma Darwin to H. E. Litchfield, [10 May 1879] (DAR 219.9: 196)).
- ⁶ Emma Darwin's diary records that Francis Darwin arrived in Southampton on Friday 28 May 1880 (DAR 242).

To Alphonse de Candolle 28 May 1880

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

[Bassett, Southampton.]

May 28th 1880

My dear Sir

I am particularly obliged to you for having so kindly sent me your Phytographie; for if I had merely seen it advertised, I should not have supposed that it could have concerned me.¹ As it is, I have read with very great interest about a quarter, but will not delay longer thanking you. All that you say seems to me very clear & convincing, & as in all your writings I find a large number of philosophical remarks new to me, & no doubt shall find many more. They have recalled many a puzzle through which I passed when monographing the Cirripedia; & your book in those days would have been quite invaluable to me.² It has pleased me to find that I have always followed your plan of making notes on separate pieces of paper: I keep several scores of large portfolios, arranged on very thin shelves about 2 inches apart, fastened to the walls of my study, & each shelf has its proper name or title, & I can thus put at once every memorandum into its proper place.— Your book will, I am sure, be very useful to many young students, & I shall beg my son Francis, (who intends to devote himself to the physiology of plants) to read it carefully.³



Auxanometer built by Horace Darwin in 1876.
© Whipple Museum (Wh.2766).

As for myself I am taking a fortnight's rest after sending a pile of M.S. to the Printers, & it was a piece of good fortune that your book arrived as I was getting into my Carriage, for I wanted something to read whilst away from home.⁴ My M.S. relates to the movement of Plants, & I think that I have succeeded in showing that all the more important great classes of movements are due to the modification of a kind of movement common to all parts of all plants from their earliest youth.

Pray give my kind remembrances to your son⁵ & with my highest respect & best thanks, believe me | My dear Sir | Yours very sincerely | Charles Darwin

P.S. It always pleases me to exalt Plants in the organic scale, & if you will take the trouble to read my last Chapter, when my book (which will be sadly too big) is published & sent to you, I hope & think that you also will admire some of the beautiful adaptations by which seedling plants are enabled to perform their proper functions⁶

Archives de la famille de Candolle (private collection)

¹ Candolle's *Phytographie* laid out the general principles and traditions of nomenclature from a practical point of view (A. de Candolle 1880). CD's copy is in the Darwin Library-CUL.

² CD had produced monographs on both living and fossil species of barnacles (*Living Cirripedia* (1851) and (1854); *Fossil Cirripedia* (1851) and (1854)).

³ Francis Darwin had worked as CD's botanical assistant and secretary since 1874 (*Emma Darwin* (1904), 2: 269).

⁴ CD was in Southampton from 25 May to 8 June 1880 (CD's 'Journal' (Appendix II)). He had just finished writing the manuscript of *Movement in plants* (see letter to J. V. Carus, 28 April 1880).

⁵ Casimir de Candolle.

⁶ Candolle received a copy of *Movement in plants* in November 1880 (see letter from Alphonse de Candolle, 23 November 1880).

From G. H. Darwin 28 May 1880

Trin. Coll. Camb.

May 28. 80

My dear Father,

I received the enclosed wonderful rigmarole a few days ago from M^{rs} Noel, & think it w^d. amuse you to see it.¹ Tho' it borders on the silly, yet there is something I like about it. I answered back at almost equal length, in great part in an endeavour to explain away y^r. treatment of W.A.D., with which she is obviously hurt.² I said that your information was almost wholly taken from E.D's correspondence & that W.A.D's name hardly occurred there, showing that there was but little intercourse between the brothers, & accordingly that you were literally correct in saying "of whom I know nothing", but I believed you regretted the form & implication thereof & w^d. probably modify in a future edit. wh. however w^d. probably not be called for. I also said you were anxious not to overburden the book with family details for which sort of statement the public do not care. I hope this will have expressed y^r. views fairly well & will have smoothed her down a little. It is strange that she sh^d. have

thought it worth while to send the catalogue of pictures sold, which must have been astonishing rubbish.³ The copies of the poems have come too & are as far as I can judge rather good.⁴

I have promised to send her copies of the M.S. when I go home & have begun by sending her the printed copy of the Waring Inventory.⁵ This I also sent to Reg^d. D. & received the enclosed letter back⁶ I have her two account books which do not possess much interest; they appear however to contain the original memo. as to the receipts for medical practice wh. you have given in the book.⁷

I am rather sorry Leo. did'nt ask young Noel over from Chatham once even tho' he is a beast.⁸ I seem to have got rather in to the thick of all these cousins & think I must pay a round of visits.

When I was at Glasgow Sir W. introduced me to a man in his Laboratory & said in his impulsive way that I sh^d be delighted to show him our lab^y here, to which I cd'nt but agree.⁹ I only saw the man for a minute & shd'nt have known him again. However he inflicted himself on me today. I gave him luncheon & walked him to the laboratory & after I had gone about a little, left him with the demonstrator. I have been so ill today that I had to explain that I c^d'nt go about at all. In fact I have been dosing in an armchair almost all afternoon & am dining in my room.

I really did think yesterday I was better as I did some work for a wonder, but it was only a case of "reculer pour mieux sauter" & I caught it worse than ever at night.¹⁰

Last night in hall I sat next a M^r. Gildersleeve Prof. of Greek at the Johns Hopkins University, a pleasant man and (you had better read altho' to Sara) a Southerner.¹¹ I am almost interested in hearing about America from the S. point of view. This man had been badly wounded & he said he suffered much more from deprivation in private life than he did with the army, showing the straits they were put to.¹² After dinner I came across a little Frenchman, a friend of Colvin's,¹³ who was rather amusing but somehow my French had run out of the heels of my boots. I've since heard that he is one of Gambetta's¹⁴ private secretaries.

Remember me to Miss Ashburner if she is with you.¹⁵ I sh^d think Sara must enjoy having her here

I hope Southampton is suiting you & mother.

I've not heard whether Frank is going to Brittany or not.¹⁶

I suppose H. & Ida will be back tomorrow. They will have to decide on the great house question then.¹⁷

Your affectionate Son | G H Darwin

Please return the letters.

DAR 210.2: 84

¹ The enclosure has not been found. Sarah Gay Forbes Noel was CD's cousin.

² William Alvey Darwin, Erasmus Darwin's brother, was Sarah Noel's grandfather.

³ The catalogue has not been found; it probably listed paintings at Elston Hall, Erasmus Darwin's birthplace.

⁴ The poems have not been identified.

⁵ George Howard Darwin had helped CD with the family history for *Erasmus Darwin*; his manuscript notes on Joseph Lemuel Chester's genealogy of the Darwin family are in DAR 14: 41. The Waring inventory has not been identified. Anne Waring was Erasmus Darwin's grandmother.

- ⁶ Reginald Darwin's letter to George has not been found.
- ⁷ In *Erasmus Darwin*, p. 25, CD gave details of a memorandum in Erasmus Darwin's hand listing the profits from his early medical practice in Lichfield.
- ⁸ Leonard Darwin was instructor in chemistry and photography at the School of Military Engineering, Chatham; 'young Noel' was possibly James Wriothesley Noel, Sarah Noel's youngest son.
- ⁹ George had probably visited William Thomson in Glasgow in May 1879 (see *Correspondence* vol. 27, letter from G. H. Darwin, 10 May 1879 and n. 5). The man in Thomson's laboratory has not been identified.
- ¹⁰ The phrase 'reculer pour mieux sauter' (French) literally means to draw back to make a better jump.
- ¹¹ Basil Lanneau Gildersleeve, first professor of Greek at Johns Hopkins University, Baltimore, was born in Charleston, South Carolina. Gildersleeve had served in the Confederate army during the American Civil War; George's American sister-in-law, Sara Darwin, came from Massachusetts, which was a centre for progressivist and abolitionist activity in the years leading up to the war.
- ¹² Gildersleeve was wounded in a skirmish in the Shenandoah Valley in 1864, when a bullet broke his thigh bone and gave him a life-long limp; he later said of the summer 1864 campaign: 'I lost my pocket Homer, I lost my pistol, I lost one of my horses, and finally I came very near losing my life' (Briggs ed. 1998, pp. 48–9). Gildersleeve had written about hunger in the midst of luxury, the exclusion of civilians from banquets, the 'starvation parties', the rationing of meat in the Confederacy, and his own experience of deprivation during the war (*ibid.*, pp. 167–8, 222–6, 230–4, 410, 413).
- ¹³ Sidney Colvin.
- ¹⁴ Léon Michel Gambetta was a French politician.
- ¹⁵ Probably Anne Ashburner, Sara Darwin's aunt.
- ¹⁶ Francis Darwin left for Brittany on 31 May 1880 (Emma Darwin's diary (DAR 242)).
- ¹⁷ Ida and Horace Darwin, who had married in January 1880, intended to live in Cambridge, where Horace designed scientific instruments (Cattermole and Wolfe 1987). CD had sent advice about the sort of house that would suit them best in his letter to Horace of 20 April [1880].

To G. H. Darwin 30 May [1880]¹

Southampton
Sunday May 30th

My dear old George.

I am glad that you said what you did to M^{rs} Noel, about her grandfather; it is all strictly true & you might have added, though this would not have been complementary, that I had utterly forgotten that he was her grandfather or anybody's grandfather.—² There is something nice about her note; it is so simple though rather foolish.— What a strange thing it was to sell the old portrait &c &c.;³ I have often heard my father⁴ speak with indignation about it.— You have had such luck in your genealogical researches, that I believe you will find something wonderful at Lincoln or Marton or the other village with very old registers.—⁵

I am so very sorry my poor dear old fellow to hear so bad an account of yourself.— You have indeed much to endure; but I always go on hoping that you will be better some day. It is not as if your lungs were affected.

Private We were tremendously interested by your letter about the Farrers' visit.⁶ Horace gave a rather better account of Effie than you did, but Ida gave to Henrietta as bad a one as yours.⁷ I am convinced that she is insane. She has been extra cordial & *confidential* with Leonard!⁸

We are enjoying ourselves here & to my surprise I find complete idleness not only endurable but very pleasant.⁹ The weather, however, has been dry & pleasant, though until to day extremely cold.— M^r Lowell is coming here today & he will be first-rate fun.—¹⁰ We like Miss Ashburne very much.—¹¹

The more I think of Sir Thompson, the more I am charmed with him; & it is a real pleasure to have seen so great a man.—¹² Be sure tell me whenever you have any luck or no luck with the pendulum.—¹³

I have subscribed 2.2.0 for the enclosed¹⁴

Farewell my dear old fellow; I do hope that you will get somewhat better soon.—
| Your affectionate Father | C. Darwin

Frank starts tomorrow or Tuesday for Brittany: he has been having good luck with his Potash experiments lately, & the fact is an extraordinary one.—¹⁵

P.S. If you sh^d find an Edit. of Botanic Garden about 1800, please look & see if there is a kind of Preface, headed

Apology

with some good philosophical remarks on use of Hypotoses.—¹⁶

DAR 210.1: 93

¹ The year is established by the relationship between this letter and the letter from G. H. Darwin, 28 May 1880.

² Sarah Gay Forbes Noel was upset that her grandfather William Alvey Darwin had not received more attention in *Erasmus Darwin* (see letter from G. H. Darwin, 28 May 1880). She had an interest in the genealogy of the Darwin family (see *Correspondence* vol. 27, letter from E. S. Galton, 31 March 1879). ‘(W^m Alvey Darwin)’ was added in purple crayon in an unknown hand above ‘grandfather’.

³ ‘(at Elston)’ was added in purple crayon in an unknown hand above ‘portrait’. Sarah Noel had sent George a catalogue of paintings sold, probably from Elston Hall (see letter from G. H. Darwin, 28 May 1880 and n. 3).

⁴ Robert Waring Darwin.

⁵ Manton is a village in Lincolnshire and the location of the Darwin family seat Cleatham Hall; Lincoln is the cathedral city of the county. George had long had an interest in the genealogy of the Darwin family (see *Correspondence* vol. 14, letter from E. A. Darwin, [before 20 February 1866?] and n. 2).

⁶ George’s letter has not been found. Thomas Henry Farrer had opposed his daughter Ida’s marriage to Horace Darwin. Emma Darwin thought this was because Farrer worried that all his family would be against it on either ‘worldly’ or religious grounds; she thought this placed Effie Farrer, Farrer’s second wife, Ida’s stepmother, and Horace Darwin’s cousin, in an awkward position, and that Effie, while disappointed by the match, was mainly concerned about losing Ida (letter from Emma Darwin to W. E. Darwin, [4 August 1879] (DAR 219.1: 125), and letter from Emma Darwin to Ida Farrer, [September 1879?] (DAR 258: 653)).

⁷ Horace’s account and Ida’s view expressed to Henrietta Emma Litchfield have not been found; they evidently concerned Effie Farrer’s health.

⁸ Leonard Darwin.

⁹ CD and Emma Darwin were visiting Sara and William Erasmus Darwin in Southampton from 25 May to 8 June 1880 (CD’s ‘Journal’ (Appendix II)).

¹⁰ James Russell Lowell, the poet, visited on 30 May 1880 (Emma Darwin’s diary (DAR 242)).

¹¹ Probably Anne Ashburner, Sara Darwin’s aunt.

¹² William and Frances Anna Thomson had lunched with the Darwins on 11 May 1880 (letter from Emma Darwin to H. E. Litchfield, [11 May 1880] (DAR 219.9: 236)).

¹³ George and Horace Darwin were constructing a pendulum to measure the lunar disturbance of gravity (G. H. Darwin 1907–16, 5: 1).

¹⁴ The enclosure was probably a request for contributions from fellows of the Royal Society of London for a portrait of Joseph Dalton Hooker, who had served as president of the society from 1873 to 1878. CD recorded a payment of £2 2s. on 31 May 1880 for ‘Hooker Portrait’ (CD’s Classed account books (Down House MS)). Hooker’s portrait was painted by John Collier in 1880.

¹⁵ Francis Darwin left for Brittany on 31 May 1880 (Emma Darwin’s diary (DAR 242)). Francis’s potash experiments probably related to his work on the bloom on leaves; in 1878, while Francis was working

in Julius Sachs's laboratory in Würzburg, Sachs had suggested that potash was exuded from leaves (as indicated by alkaline dew) and thus could provide a way of comparing leaves with bloom with those cleaned of bloom to see whether the function of bloom was to prevent water dissolving anything out of the leaves (see *Correspondence* vol. 26, letter from Francis Darwin, [4–7 August 1878], and *Correspondence* vol. 27, letter to Edward Frankland, 8 February 1879).

- ¹⁶ Hypotoses: hypotheses. The 1799 edition of Erasmus Darwin's two part poem *The botanic garden* (E. Darwin 1799, pp. xvii–xviii) contained an 'Apology' on the use of 'conjectures' and 'extravagant theories ... in those parts of philosophy, where knowledge is yet imperfect ... as they encourage the execution of laborious experiments, or the investigation of ingenious deductions'.

To W. E. Gladstone June 1880

TO THE RIGHT HONOURABLE W. E. GLADSTONE, M.P., FIRST LORD OF THE TREASURY, ETC.

We the undersigned beg to express an earnest hope that Her Majesty's Government will afford their support to the following Resolution, which will be submitted to the House of Commons by Mr. Roundell, on the 9th of July:—¹

"That this House, while fully recognising the obligation to make provision for the due fulfilment of the requisitions of Sections 5 and 6 of 'The Universities Tests Act, 1871' (relating to religious instruction, and to morning and evening prayer in Colleges), deems it inexpedient that, save in the case of the Deanery of Christchurch, any clerical restriction shall remain, or be attached to, any Headship or Fellowship in any College of the Universities of Oxford and Cambridge."

And that they will also support the following addition to such Resolution, to be proposed by Mr. Bryce:—²

"Or to the professorships of Hebrew and Ecclesiastical History in the University of Oxford, and the professorship of Hebrew in the University of Cambridge."

When the Universities Tests Abolition Bill of 1871 was before Parliament, it was only by small majorities that proposals for the abolition of clerical Headships and Fellowships were rejected; one reason assigned for the opposition of the then Government to the proposal being that a Commission would probably be appointed to examine the whole question of the tenure of Fellowship and College offices.³ When, in 1877, the Bill for the appointment of Commissioners, with power to make Statutes for the Universities of Oxford and Cambridge, and the Colleges therein, was brought in (by the Conservative Government), clauses providing for the abolition of clerical restrictions were rejected by the small majorities of nine, and twenty-two; notwithstanding the opposition of the Government, with its normally large majority.

At the present time, it is believed that in some of the Colleges the strong representations of the College authorities have induced the Commissioners to virtually assent to the abolition of the restriction, in the case of both the Headship and the fellowships. It is, however, understood that in other Colleges the clerical Headship will be retained, and that there will be a reduction only in the number of the clerical Fellowships.

The Statutes have not yet been settled for the whole of the Colleges, and none of the Statutes will have force until they have been laid before Parliament. We

are therefore desirous that, while the negotiations between the Colleges and the Commissioners are still in progress, there should be an expression of opinion on the part of the newly elected House of Commons that the principle of the Universities Tests Act of 1871 should be fully recognized by the Commissioners.

We are also of opinion that no sufficient reason can be advanced why the Chairs of Hebrew and of Ecclesiastical History at Oxford, and of Hebrew at Cambridge, should be filled only by clergymen of the Church of England, or ministers of any religious body, and that, in the interest of learning, and even of religion, as well as on the ground of justice, these Professorships should be thrown open to laymen.

We desire to call your special attention to the fact that Mr. Roundell's Motion fully respects the compromise, in regard to religious instruction and worship in the Colleges, upon which the Universities Tests Abolition Act was based; it being understood to be practicable to make provision for such instruction and worship without the retention of the existing system of clerical Fellowships.

June, 1880.⁴

Roundell 1880, pp. 10–11

¹ William Ewart Gladstone was prime minister; Charles Savile Roundell was MP for Grantham.

² James Bryce.

³ For the progress of the University Tests Act from 1871, see <https://api.parliament.uk/historic-hansard/acts/university-tests-act> (accessed 27 March 2019).

⁴ *Nature*, 15 July 1880, pp. 250–1, reported that the memorial was presented to Gladstone with 800 signatories, including CD, George Jessel, Henry Thompson, James Risdon Bennett, Thomas Henry Huxley, William Morris, Alfred Russel Wallace, William Benjamin Carpenter, Alfred John Carpenter, and Edwin Abbott Abbott.

From J. H. Comstock 4 June 1880

*J. Henry Comstock. | Entomologist to the | U.S. Department of Agriculture.
Washington, D. C.,
4 June 1880.*

PERSONAL

Mr. Charles Darwin, | Farnborough Hants, Kent Co., England.

Sir:

I have requested that a copy of my Report on Cotton Insects, recently published by this Department, be sent to you.¹ Knowing that your time is fully occupied I take the liberty of indicating the parts of the Report which I think will be of interest to you. The cotton worm (*Aletia argillacea*) is doubtless a tropical insect which has been introduced into our territory; as yet however, it is not fully naturalized and but very few are able to survive our winter.² Thus the first brood in the spring is so small that it is seldom noticed (see p.97). In the course of the season the insect passes through several generations, frequently increasing in numbers to such an extent as to sweep away the entire cotton crop of large sections of country. But of the

immense numbers thus produced only a few individuals ever survive the following winter (see pp.90, 91). Is not this a remarkable instance of the action of natural selection where hardly one insect in five hundred thousand is preserved?

Another point which I think will be of interest to you is the structure of the maxillae of the adult *Aletia* (see pp.86,87).³

I think you will also be interested in the extra floral nectar glands to which I have referred on pp.84,85 and which are discussed more at length by Mr. Trelease pp.319–333.⁴

Yours very respectfully | J. Henry Comstock.

TLS

DAR 161: 217

¹ An annotated copy of *Report upon cotton insects* (Comstock 1879) is in the Darwin Library–CUL.

² *Aletia argillacea* is a synonym of *Alabama argillacea* (cotton leafworm or cotton worm). It is a moth of the family Erebidae (the same family as the moth described in F. Darwin 1875; see n. 3, below).

³ In Comstock 1879, p. 87, Comstock cited F. Darwin 1875 and *Orchids*. Comstock had noted that in the adult moth, the maxillae possessed spines on the dorsal surface, enabling the organs to be used to pierce fruit. Francis Darwin had discussed similar adaptations in a related moth, *Ophideres fullonica* (a synonym of *Eudocima phalonia*, the Pacific fruit-piercing moth).

⁴ Comstock 1879, pp. 319–33, was a chapter by William Trelease, 'Nectar; what it is, and some of its uses'. It was followed by an extensive bibliography, including *Cross and self fertilisation* and *Variation*. Passages on these pages are scored in CD's copy (see *Marginalia* rev. ed.).

From Anthony Rich 4 June 1880

Chappell Croft, | Heene, Worthing,
June 4. 1880

My dear Mr. Darwin,

The sight of your hand-writing was an agreeable surprise to me yesterday,—for it is always an honour for me to receive a letter from you, and a pleasure to read your friendly talk and hear the news about all your family.¹ With a single exception all seems at present to be marked with a note of white chalk, and so far you may fairly be congratulated by all well wishers; and I can truly regret, because I well know what ill health means, that your son George should be the exception. If he is not a bad sailor, I should think that a summer's cruise in a fine yacht with so kind and distinguished a friend as Sir W. Thompson would be the very best prescription that he could have made up for him.² It is doubtless hard for a man of great ability to be forced against his will into a state of mental inactivity; but is it not best to yield willingly when resistance would be useless, or possibly prejudicial? Please to give him my salutations and cordial good wishes when you see or write to him.—

This morning's Paper brings me the announcement that Sir J. Lubbock has been elected Member for the University of London. That will give you pleasure I feel certain; as he is a neighbour of yours, and probably a personal friend. When I heard that he had lost his seat for Maidstone it at once struck me that the result would be

received with grief at Down.—³ *À propos* of election gossip I may mention that the new member for Mary-lebone is an old tenant of N^o. 24. on our Sacred Mount—the Hill of Corn—⁴ He does not occupy the premises himself, and never did, but sublets them in flats for a consideration which ought to be considerable to make such a transaction worth any body's while!

The title of your new book sounds as if it would come within the comprehension of such an ignoramus as myself, and I shall watch the announcement of its forthcoming with assiduous attention.⁵ I find that I get duller of intellect from year to year; and less and less able to fix my mind closely down upon any subject so as to retain a consecutive understanding of the matter I am reading about, if that happens to be greatly specialized or involves close or subtle argument. For this advancing years are, no doubt, partly to blame—but a dull torpid liver must add largely to that grievance, so that I find myself shirking books which require thoughtful reading, and looking out; much more than I used to do, for amusement than for instruction. But I anticipate both the one and the others in that book about the Movement of Plants. Ah! that reminds me that I shall see before long several strings of blossom bursting forth on the Philadelphus you gave me last autumn.⁶ Both of the plants survived the cold winter and dry spring—quite bravely.

I see that the Critics speak in flattering terms of your portrait at the Exhibition—the one I conclude for which you were sitting at this time last year.⁷ It must be a satisfaction for yourself as well as for your people to know that you have not been deformed nor caricatured by the artist who will hand down your features to the knowledge of coming generations.—

Those Wilberforces must have been, more or less, every one of them men of mark, to have sprouted out of the Clapham Sect into what they became. I used to meet one of them in Italy in my youth who afterwards joined the R.C. persuasion. He was then what we used to call “a Saint”—as was the lady whose house he frequented, a very great friend of mine, a most agreeable, and very worldly lady. She had small feet of which she was exceedingly proud, and I won her heart offhand at the tomb of Cecilia Metella on the Appian Way by offering her my lemon coloured kid gloves to put over her boots when she complained that she could not pass over the grass because it was too wet.⁸ When any of her pious lady friends wanted to convert me she used to say “its of no use your talking to M^r. Rich—I never can get him to be serious on the subject—he's hardened in sin”!—

Oh! if my kid glove days had not been dead and buried long, long ago, I should not want nor wait for two invitations to Down!⁹ Nothing could give me greater pleasure than to go and visit you at home; but the years that I have spent in a sort of monastic seclusion from social intercourse, if they do not render me unfit for its enjoyment, do make it increasingly difficult, I had almost said impossible, for me to resume the ordinary duties of life. I find too that my hearing is becoming somewhat defective; which renders me unable to follow the conversation going on when several people are talking together— It is now a year and a half since I have been in London; I get shy and nervous at having to go alone and mind my own

business; and scarcely anything short of compulsion can get me to move away from my own domicile. If I only had a thorough good body servant to travel with me and take all the trouble off my hands, I should go about much more than I now do. But then, such a man, if found at my time of life, would probably become, or try to become, himself the master—and might get to be a very domineering one.—I do not suppose that there is any chance of your passing this way on your return from Basset.¹⁰ Indeed I am far from sure that the very thought of such a thing may not appear to be somewhat conceited on my part—. *Pardon!* One half of my household has been disabled of late, but appears now to be convalescent. She goes to town on Wednesday next to see doctor, and as I hope for the last time—¹¹

That attack on you in the Athenaeum I did not see. You & the prime Minister may boast of having been in your day the best abused men in England—and both can look down with the pride of nobly earned triumphs upon the utter discomfiture of their opponents.¹² What satisfaction can be greater either intellectually or morally?—

Please to pass round my regards and respects to all your circle, and especially to the ladies, and believe me to be | Dear M^r. Darwin | Very sincerely yours | Anthony Rich

DAR 176: 142

¹ CD's letter to Rich has not been found.

² George Howard Darwin had been ill; see letter from G. H. Darwin, 28 May 1880 and n. 10. He had planned to visit William Thomson in Glasgow in May 1879 (see *Correspondence* vol. 27, letter from G. H. Darwin, 10 May 1879 and n. 5). Evidently, George did not go on the cruise in June 1880 (letter from James Thomson to G. H. Darwin, 25 June 1880 (University of Glasgow Special Collections, MS Kelvin T135)).

³ John Lubbock lived in Down and was Liberal MP for Maidstone, Kent, from 1870 to 1880, and for London University from 1880 to 1900 (*ODNB*; see also *The Times*, 3 June 1880, p. 11).

⁴ Daniel Grant was the newly elected Liberal MP for Marylebone in 1880 (Craig ed. 1989). Twenty-four Cornhill, London was one of the properties that Rich left to CD in his will; see *Correspondence* vol. 26, letter from Anthony Rich, 10 December 1878.

⁵ CD published *Movement in plants* on 6 November 1880 (Freeman 1977).

⁶ CD had sent Rich a plant of *Philadelphus* (the genus of mock-orange), which flowers from June to July and is very frost hardy; see *Correspondence* vol. 27, letter from Anthony Rich, 28 December 1879.

⁷ In June 1879, CD sat for his portrait painted by William Blake Richmond and commissioned by the Cambridge Philosophical Society to commemorate the honorary doctorate of laws (LLD) awarded to him by the University of Cambridge in 1877; see *Correspondence* vol. 27, letter to John Fiske, 10 June 1879, and Browne 2002, p. 451. It was exhibited at the Grosvenor Gallery, London; see also *The Times*, 1 May 1880, p. 8. The portrait now hangs in the Department of Zoology, University of Cambridge. See *Correspondence* vol. 27, frontispiece.

⁸ In the late eighteenth and early nineteenth centuries, William Wilberforce (1759–1833) was at the centre of the Clapham Sect, a circle of friends that provided emotional and practical support for an anti-slave-trade campaign and various religious initiatives (*ODNB*). His sons William, Robert Isaac, and Henry William Wilberforce converted to Roman Catholicism. The lady has not been identified. The tomb of Cecilia Metella is on the Via Appia Antica, three miles outside Rome (*Imperium Romanum*, Mausoleo di Cecilia Metella, <https://www.romanoimperio.com/2010/01/mausoleo-di-cecilia-metella.html> (accessed 15 March 2019)).

⁹ Rich had visited Down on 18 June 1879 (Emma Darwin's diary (DAR 242)).

- ¹⁰ CD's eldest son, William Erasmus Darwin, lived in Bassett, near Southampton, with his wife, Sara.
- ¹¹ In 1881, Rich lived with a cook and housekeeper, Harriet Hardwick, and a parlourmaid, Hannah Bagley (Census returns of England and Wales 1881 (The National Archives: Public Record Office RG11/1118/20/31)).
- ¹² CD was attacked in a letter by Samuel Butler published in the *Athenaeum*, 31 January 1880; see letter to H. E. Litchfield, 1 February [1880], enclosure 1. The prime minister was William Ewart Gladstone.

From James Torbitt 7 June 1880

Belfast
7th June 1880

Charles Darwin Esq. | Down.

My dear Sir,

Please to accept of my best thanks for your information regarding pollen, and for your advice, which shall be acted on.¹

The work is good, but in a national point of view, it is quite too slow, and besides, I want to repay everyone as well as myself, for what has been and shall be done. I wish therefore, if you Sir, and M^r Farrer approve of it, to make the following specific propositions to the Government,²

First. for a consideration of one pound per variety, I would propose to grow during the coming season (1881) one hundred thousand thrice crossed varieties of the potato, and hand them over to the Agricultural societies of the Kingdom for distribution.

Second. I would undertake that all these varieties should be of marketable appearance, of excellent qualities, and so prolific and so free from "the disease" that, after separating all unsound tubers, they would give a larger yield, than the old varieties give, of sound and diseased tubers taken together.

Third. I would propose that the Government send a Commissioner to inspect the twice-crossed varieties now growing, when they are in bloom, and when they are being dug up, and also to see the principles of cross-breeding and selection as applied to this years seedlings, in order to judge whether it is probable I should be able to carry out last proposition.

Fourth. I would propose that, under a vote of the House of Commons, one twentieth of the money (one shilling per variety) be paid in advance, the remaining nineteen twentieths to be held in reserve, as a guarantee fund against any failures in the varieties—that is, that all varieties which might fail to conform to fore going description should not be paid for.

And should these propositions seem to be practical, perhaps M^r Farrer would speak to Mr Chamberlain on the subject?³

Curiously enough, M^r Forster's (secretary for Ireland) father was a friend and guest of the father of the gentleman who is now growing these new crossed potatoes for us.⁴

I remain | my dear Sir | most respectfully & faithfully yours | James Torbitt

DAR 178: 166

- ¹ CD's letter has not been found, but in his letter of 13 May 1880, Torbitt had asked CD whether the pollen used in crossing potatoes would live for a few days, or weeks, or whether it had to be used immediately.
- ² CD had asked Thomas Henry Farrer, permanent secretary of the Board of Trade, to try to get government support for Torbitt's experiments to develop blight-resistant potatoes (see letter to James Torbitt, 30 March 1880).
- ³ Joseph Chamberlain was president of the Board of Trade.
- ⁴ William Edward Forster was chief secretary for Ireland; he had visited Ireland with his father, William Forster, in 1846 (*ODNB*). The man who was growing potatoes for Torbitt, and his father, have not been identified.

To H. B. Baildon 9 June 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
June 9th 1880

Dear Sir

I am much obliged to you for your courteous note & for the gift of your 'Spirit of Nature'.¹

I will read your work as soon as I have finished a book in hand, & I remain | Dear Sir | Yours faithfully | Charles Darwin

Cleveland Health Sciences Library (Robert M. Stecher collection)

¹ A copy of Baildon's book *The spirit of nature: being a series of interpretative essays on the history of matter from the atom to the flower* (Baildon 1880) is in the Darwin Library–Down. Baildon's note has not been found.

From R. F. Charles 9 June 1880

City of London School | Milk St | Cheapside | E.C.
June 9th 1880

Dear Sir,

I venture to ask your permission to use some extracts from the book "a Naturalist's Voyage round the World" in a series of English reading-books for schools that I am now bringing out. I ought to say that my publishers some time since asked Mr Murray for permission to print these extracts and others by other writers and he refused to allow extracts from any book published by him.¹

As I have no reason to suppose that he referred the matter to you I venture to ask if you will help me. The books are merely school "Readers" and are to be called "The Model Reading-books."—(Publishers—Mess^{rs}: Relfe & Co)²

The extracts I have marked are

1. pp. 450–51. "a Corrobery or native dance".—
2. p.p. 204–209. "Tierra del Fuego".—
3. p.p. 151–154. "Horsemanship of the Gauchos."—³

Should you be able to grant my request I shall feel very much obliged, and of course will take care to see that the extracts are properly printed and acknowledged.⁴

Trusting that you will excuse the liberty I am taking
Believe me | Yours faithfully | R. F. Charles.

Charles Darwin Esq.

DAR 161: 133

CD ANNOTATION

Top of letter: 'answered' pencil

¹ John Murray was CD's publisher; 'Naturalist's voyage round the world' is the spine title of *Journal of researches* (1860).

² The model reading books (Charles ed. 1880–3) were published by Relfe Brothers.

³ The extracts appeared in Charles ed. 1880–3, 5: 10–11; 6: 228–35 ('Narrative passages' section), and 72–7 ('Passages chiefly descriptive' section), respectively.

⁴ In Charles ed. 1880–3, 5: 7 and 6: 8, 'Mr Darwin' was thanked for his permission to use the extracts.

To W. E. Darwin 9 [June 1880]¹

Down—
9th

My dear W.—

I send by this post Asa Gray's Lectures on Nat. Science & Religion.—²

I have heard from Baxter of Bromley that he can supply me (at whole-sale price) with Permanganate of Potash at 3^s per lb—; the **best** kind is advertised at 4^s per lb, *whole sale price*.—³

We enjoyed our visit greatly with you, & it has done me a world of good. Give my best of loves to my dear Sara: it makes me happy to think how happy you seem at Bassett my two dear children.⁴

Ever yours affect. | C. Darwin

Your mother did not feel up to go to concert this morning.—⁵

Apples arrived, very good, you good Boy— mind that I repay you.—

P.S. | By an odd chance I came across this morning a list of all the property or shares which my Father made over to me as my share of my mothers property, & which I suppose w^d be the same as was made over to Caroline, & which must have included a share from Uncle Thomas Wedgwood.— I daresay Caroline may have her own list.—⁶

DAR 210.6: 159

¹ The month and year are established by the reference to CD's visit to Bassett, Southampton; see n. 4, below.

² A lightly scored copy of Asa Gray's *Natural science and religion* (A. Gray 1880) is in the Darwin Library–CUL.

³ William Walmisley Baxter owned a chemist shop at 40 High Street, Bromley, Kent. CD was using permanganate of potash (potassium permanganate, KMnO₄) in his experiments for *Movement in plants*; see letter to Asa Gray, 17 February 1880.

⁴ CD and Emma Darwin visited William and Sara Darwin at Bassett, Southampton, from 25 May to 8 June 1880 (CD's 'Journal' (Appendix II)).

⁵ In her letter to Sara Darwin, 9 June [1880] (DAR 219.1: 136), Emma Darwin wrote 'all my courage gave way about my early concert'.

⁶ The list of property and shares is in DAR 227.5: 100; a similar list for Caroline Sarah Wedgwood (CD's sister) is in DAR 227.5: 96. CD's father was Robert Waring Darwin and his mother, Susannah Darwin. Thomas Wedgwood was CD's mother's brother.

To Henry Johnson 9 June 1880

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

June 9th 1880

My dear Johnson.

Many thanks for your notes & enclosures.¹ I remember M^r Vivian perfectly & the interest which he took in prehistoric archæology.² I am glad to hear of the flint tool so low in the deposit. These old implements have a fascination for me, & I sh^d think that there was much probability in M^r Vivian's suggestion about breaking the ice. As for the contours of the head they surpass my powers of belief in their accuracy.— I have just returned home after a fortnights holiday & have a multitude of letters to write, so will say no more, except to send my very kind remembrance & thanks to your daughter.³

Good Heavens how many years it is since we used to walk together at Edinburgh.—⁴

with all good wishes | Yours very sincerely | Ch. Darwin

Torquay Museum Society (AR471)

¹ The notes and enclosures have not been found. Johnson had previously mentioned being present at excavations of caves at Longbury Bank, Penally, near Tenby; see *Correspondence* vol. 27, letter from Henry Johnson, 22 September [1879].

² Edward Vivian had worked on the excavations of Kent's cavern, near Torquay. He had sent CD information in 1872; see *Correspondence* vol. 20, letter to Edward Vivian, 23 August [1872].

³ CD visited his son William Erasmus Darwin at Bassett, Southampton, from 25 May to 8 June 1880 (CD's 'Journal' (Appendix II)). Johnson's daughter was Mary Elisabeth Johnson.

⁴ In the 1820s, CD and Johnson had been contemporaries at Shrewsbury School and at the University of Edinburgh; see *Correspondence* vol. 1, letter from Caroline & Susan Darwin, 2 [January 1826].

To John Murray 9 June 1880

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

June 9th 1880

My dear Sir

My son is going to publish a Lecture, compiled from my 'Climbing Plants' in the Q. J. of Science. M^r Dallas, the editor, had intended giving a Plate, but has now to give another & wants much 5 woodcuts.¹ Should you object to lend him 5 of them; for I doubt whether there is time to make cliches of them. My son's paper is in type & the number appears on July 1st.— I want much to oblige M^r Dallas & the drawings

are almost necessary for my son's article. Therefore I beg you to do what you can for me **at once**; & the originals or the clichés had better be sent to M^r. Dallas, Geolog. Soc. Burlington House.—²

My son's article will, also, serve as an advertisement.

The numbers of the Blocks, as given in my book, are 1, 2, 5, 11, 13.—³

I suppose the new Edit: of "The Different Forms of Flowers" is now completed, & I sh^d. like to have a copy for myself; & if you do not object (on account of old copies not yet sold) I sh^d. like a copy to be sent to 'Nature' & another to Linnean Soc. Burlington House.⁴

I have not yet received any proofs of my new Book.⁵

My dear Sir | Yours sincerely | Ch. Darwin

P.S. If original blocks from Climbing Plants are sent to M^r. Dallas, you had better enclose memorandum, asking for their prompt return, & if clichés, an account of their cost.

My absence from home has caused several day's delay.⁶

National Library of Scotland (John Murray Archive) (Ms. 42152 ff. 368–9)

¹ Francis Darwin was publishing his lecture on climbing plants, given to the Sunday Lecture Society at St George's Hall, London, on 25 January 1880 (Emma Darwin's diary (DAR 242). CD wrote *Quarterly Journal of Science* in error. Francis's article appeared in the July 1880 issue of the *Popular Science Review*, edited by William Sweetland Dallas (F. Darwin 1880c).

² Clichés were stereotype plates of the woodcuts. Murray had published the second edition of *Climbing plants* and CD had received permission from the Linnean Society to allow Murray to use the thirteen woodblocks made when the work originally appeared in the *Journal of the Linnean Society (Botany)*; see *Correspondence* vol. 23, letter to Linnean Society, 1 January [1875]. Dallas was assistant secretary to the Geological Society of London.

³ The illustrations required were in *Climbing plants* 2d ed., pp. 47, 54, 86, 148, and 165.

⁴ *Forms of flowers* 2d ed. was published in July 1880 (Freeman 1977); a presentation list for this work has not been found.

⁵ CD was expecting proofs for *Movement in plants*; see letter to J. V. Carus, 28 April 1880.

⁶ CD visited his son William Erasmus Darwin at Bassett, Southampton, from 25 May to 8 June 1880 (CD's 'Journal' (Appendix II)).

To G. H. Darwin 11 June [1880?]¹

A bundle of Essays in German & Dutch & French on Astronomical & Physical subjects by Oudemans has arrived here—for you marked from Author, too large for Post²

C. D.

June 11th

DAR 210.1: 94

¹ The year is suggested by "80" added in purple ink.

² Jean Abraham Chrétien Oudemans was a Dutch astronomer; the essays he sent have not been identified.

To John Murray 11 June 1880

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

June 11th 1880

My dear M^r: Murray

M^r Charles of the City of London School has asked me to permit him to give some extracts in his "Model Reading Books" from my *Naturalists Voyage*.¹

I should very much like to grant permission & sh^d. feel proud that any passage in any book of mine sh^d. be used for educational purposes; but I cannot of course grant permission without your consent, as the book is your property.

I hope, however, that you will oblige. me.— | Yours sincerely | Ch. Darwin

The desired extracts are

(1) p. 450–451—the corrobory dance of the Australians

(2) p. 204–209 Tierra del Fuego, account of.—

(3) p. 151–154—Horsemanship of the Gauchos.²

Private collection

¹ See letter from R. F. Charles, 9 June 1880. Robert Fletcher Charles, an assistant master at the City of London School, had asked CD for permission to publish extracts from *Journal of researches* (1860) in his textbooks (Charles ed. 1880–3).

² For the extracts, see the letter from R. F. Charles, 9 June 1880 and n. 3.

To Edward Vivian 11 June 1880

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

June 11th 1880

Dear M^r: Vivian

The enclosed, evidently intended for you, has by a mistake been addressed to me.¹

Pray believe me | Yours very faithfully | Charles Darwin

Torquay Museum Society (AR472)

¹ The enclosure has not been identified, but see the letter to Henry Johnson, 9 June 1880, in which Vivian is mentioned by CD when thanking Henry Johnson for his enclosures.

From R. F. Charles 12 June [1880]¹

City of London School | Milk S^t. | E.C.

June 12.

Dear Sir,

I write to thank you for the exceedingly kind letter you have written me about the extracts from your book.²

I am sorry that I have been the cause of giving you so much trouble, but I am very much obliged to you for writing to M^r: Murray.³

I am endeavouring to make these Readers more interesting than such books often are, and, as far as is possible in a great number of selections, to give the children a taste of books only that are really valuable—

With many thanks | Believe me | Yours faithfully | R. F. Charles

Chas: Darwin Esq.

DAR 161: 134

- ¹ The year is established by the relationship between this letter and the letter from R. F. Charles, 9 June 1880.
- ² The letter from CD to Charles has not been found. However, see the letter from R. F. Charles, 9 June 1880, in which Charles had asked CD for permission to publish extracts from *Journal of researches* (1860) in his textbooks (Charles ed. 1880–3).
- ³ John Murray; see letter to John Murray, 11 June 1880.

From R. F. Cooke 12 June 1880

50^A, Albemarle St | W.
June 12. 1880

My dear Sir

M^r Murray gives his consent with much pleasure to M^r Charles in regard to the Extracts he wishes to make from *The Naturalists Voyage*.¹

The Electros from “Climbing Plants” were sent to M^r Dallas on Thursday.²

We will do up a few copies of the *reprint* of “Forms of Flowers” & send you a copy, but it w^d be unwise to send one to *Nature* at present if they were to notice it, because we should have to take back any copies remaining in the hands of the booksellers.³

Yours faithfully | Rob^t: Cooke

Cha^s: Darwin Esq

DAR 171: 505

- ¹ In his letter to John Murray, 11 June 1880, CD asked for permission for Robert Fletcher Charles to publish extracts from *Journal of researches* (1860) in his textbooks (Charles ed. 1880–3).
- ² See letter to John Murray, 9 June 1880 and n. 2. CD had asked Murray to provide illustrations from *Climbing plants* 2d ed. for an article by Francis Darwin on climbing plants (F. Darwin 1880c) in the *Popular Science Review*, which was edited by William Sweetland Dallas.
- ³ In the letter to John Murray, 9 June 1880, CD had asked for copies of *Forms of flowers* 2d ed. to be sent to himself, the journal *Nature* and the Linnean Society. It was an accepted practice for publishers to replace existing editions when a new edition appeared; see, for example, the *Bookseller*, 28 October 1861, p. 575.

From W. E. Darwin to F. E. Abbot 13 June [1880]¹

Basset | Southampton
June 13th

Private

My Dear Sir,

My Father wished me to acknowledge with sincere thanks your letter, and the most kind expression of your feelings as regards his labours contained in it.²

He feels that there can be no higher tribute than the heartfelt appreciation of his work coming from one whose life he feels like his own to have been devoted to the search for truth.

I wished at the same time to express to you my very deep regret to learn that the *Index* is losing your superintendence; the paper has been a source of pleasure & instruction to me for years, and the high fearless tone of all your essays has always impressed deeply. When I had the pleasure of seeing you in the autumn of 1878 I feared that your connection with the paper was coming to an end.³

My Father desired me to say that it would be a considerable satisfaction to him, if you would kindly put a stop to the weekly advertizement of his appreciation of the *Index* tracts. He had no intention that his words should be used for this purpose, and he wishes now that they should be omitted.⁴

With sincere respect, I am dear Sir | Yours very faithfully | W. E. Darwin

F. E. Abbot Esq^{re5}

Harvard University Archives (Papers of F. E. Abbot, 1841–1904. Named Correspondence, 1857–1903. Letter, W. E. Darwin to F. E. Abbot [13 June 1880]), in folder Darwin, Charles and W. E. Darwin (son), 1871–1883, box 44. HUG 1101)

¹ The year is established by the relationship between this letter and the letter from F. E. Abbot, 15 May 1880.

² See letter from F. E. Abbot, 15 May 1880.

³ Abbot had been editor of the *Index* since 1870. William evidently met Abbot when he and his wife Sara Darwin visited her family in Massachusetts (Emma Darwin's diary (DAR 242) and *Correspondence* vol. 26, letter to B. J. Sullivan, 5 November [1878]).

⁴ The advertisement contained a modified version of a statement CD had made about a tract associated with the *Index* (Abbot [1870]) in his letter to Abbot of 27 May [1871] (*Correspondence* vol. 19): 'I have now read, Truths for the Times, & I admire them from my inmost heart, & I believe that I agree to every word'. For more on CD's dealings with the *Index*, see Browne 2002, pp. 391–2.

⁵ Abbot added a note to the bottom of the letter: 'Mr. Darwin forgets that he *expressly authorized* me to print his "endorsement", in his letter of Nov. 16, 1871. F.E.A.' For CD's permission, see *Correspondence* vol. 19, letter to F. E. Abbot, 16 November [1871].

To John Murray 13 June [1880]¹

Down, | Beckenham, Kent. | Railway Station, | Orpington. S.E.R.
June 13th

My dear M^r Murray

The Bearer of this note is my son-in-law, M^r Litchfield of the Ecclesiastical Commission, who wishes to speak to you about a map to interest some working men, whom he is going to take down to Cambridge for a day's excursion.—²

Yours sincerely | Ch. Darwin

National Library of Scotland (John Murray Archive) (Ms. 42153 ff. 60–1)

¹ The year is established by the date of the Working Men's College summer excursion to Cambridge (see n. 2, below).

² Richard Buckley Litchfield was a clerk to the Ecclesiastical Commissioners, and also taught at the Working Men's College in London. The college summer excursion for 1880 took place on 11 July and was to Cambridge, where Litchfield arranged a dinner at Trinity College (R. B. Litchfield, Record, personal and domestic, vol. 1 (DAR 248/1), p. 136).

From Thomas Meehan 14 June 1880

The Germantown Nurseries, | Chewstreet, below Gorgas, | Germantown, Philadelphia.

June 14th. 1880

Dear Mr Darwin

I am glad you are investigating the movements of plants. It will be worth while to look into the movements of the Stamens in *Portulaca Splendens*. The fact of the movement formed the subject of my first scientific paper in 1841—Marnock's *Gardener's Journal*,—but I have never been able to get any explanation of the movement.¹ The movement of a leaf of the sensitive plant when touched is always in one and the same direction; but when the stamen of the *Portulaca* is touched it may go to the right or to the left, upwards or downwards,—and one which you touch now and find moves to the left may move to the right when it is touched again the same day. The power of movement seems to last only one day. There is a similar movement in the stamens of *Opuntia Rafinesqui*, and *Opuntia vulgaris*, but so far as I can find always slowly upwards. *Portulaca oleracea*, a common weed here also has this movement of the Stamens; but the flower is so small that one has to lie flat on the ground with a large magnifier to see it, as the flowers remain open but a very short time.²

A very large number of Scrophulariaceous and Bignoniaceous plants here, which have cloven stigmatic plates, close these lobes when touched. I rarely see any of these but I expect at once to find the motion; but it is remarkable that some species of Orobanchaceae, with similar stigmas, have no motion,—*Aphyllon uniflorum*, is particularly in mind while writing.³

Sincerely yours as ever | Thomas Meehan

DAR 171: 114

¹ CD had finished his manuscript for *Movement in plants*; see letter to Alphonse de Candolle, 28 May 1880. *Portulaca splendens* is a synonym of *Portulaca grandiflora* (rose moss). Robert Marnock edited the *Floricultural Magazine*, and *Miscellany of Gardening* from 1836 to 1842; Meehan's paper has not been identified.

² *Opuntia rafinesqui* is a synonym of *Opuntia humifusa* (devil's-tongue); *Opuntia vulgaris* is a synonym of *O. ficus-indica* (Indian fig). *Portulaca oleracea* is common purslane; CD cited Meehan's work on the plant in *Movement in plants*, p. 189.

³ Scrophulariaceae is the family of figworts; Bignoniaceae is the family of bignonias. Many flowers in these families have two-lobed or bilamellate stigmas that close together after being touched by a pollinator, preventing further pollen reception. For a more detailed description of the action of the stigma in the Scrophulariaceae, see *Correspondence* vol. 25, letter from T. F. Cheeseman, 23 October 1877. Orobanchaceae is the family of broomrapes, which included *Aphyllon uniflorum* (a synonym of *Orobanche uniflora*, naked or one-flowered broomrape).

From James Torbitt 14 June 1880

58 North Street | Belfast

14th. June 1880

Charles Darwin Esqr. | Down.

My dear Sir,

I would not dream of asking you to ask Mr Farrer to take any step which he did not himself wish to take.¹

In writing to Mr Forster, and explaining to him the interest you take in the work, and your kindness in assisting me, and in inducing Mr Farrer and Mr Caird and others to assist me, could you confer the further great favour of permitting me to quote that part of your letter to Mr Farrer on this subject (and of which you permitted me to take a copy) in which you state that “Mr Torbitt’s plan of resisting the potato disease seems to me by far the best that has ever been suggested”—consisting as it does in—“raising a vast number of seedlings from cross-fertilized parents, subjecting them to infection, destroying those which suffer, saving those which resist best, and repeating the process in successive seminal generations” and could you also permit me to quote your letters to myself wherein you state that “I have remarked to Mr Farrer what a national misfortune it would be if you were compelled to throw up the work” and the last in which you say “if I were a minister of the Crown, I would think it my duty to adopt some such plan as you suggest”—that plan being Governmental assistance in the production and distribution of cross-bred varieties of the plant—²

I do not at all know if you can permit me to quote you thus, or in some modified form, but if you can, I should think the matter **must** be investigated and then let it stand on its own merits.

I should be quite ready to simplify my scheme and work on, on almost any terms which would secure an advance of cash next spring, sufficient to grow new varieties upon an extended scale.

I imagine that some of the Agricultural societies might be induced to receive the new varieties and distribute them amongst their subscribers, but I would not again attempt to interest the society with which Mr Carruthers is connected.³

This years seedlings are now planted out and are growing healthily, as are the plants for crossing, and the 6 acres new vars. The 14 acres which are being grown, one half the produce to be mine, or ours rather, free of cost, I have not yet heard about.⁴

I am distressed by trespassing so far on your invaluable time but there seems no help for it.

I remain my dear Sir | most respectfully and faithfully | James Torbitt

I see Major Nolan’s committee on the potato Disease has been reappointed, but I have not heard from him, although last session Mr Cave late M.P. for Barnstaple wrote to him, strongly advising him to have my evidence.⁵

DAR 178: 167

CD ANNOTATION

2.12 were ... Crown 2.13] *under pencil*

¹ CD’s letter has not been found, but in his letter of 7 June 1880, Torbitt sent for CD and Thomas Henry Farrer’s approval four propositions he wanted to make to the government about his scheme to grow blight-resistant potatoes.

² William Edward Forster was chief secretary for Ireland. In March 1880, CD had raised a subscription for Torbitt’s potato work with the help of Farrer and James Caird; see letter to James Caird, 24 March 1880. The quotations are from the enclosure to the letter to T. H. Farrer, 7 March 1878 (*Correspondence* vol. 26), the letter to James Torbitt, 1 November 1879 (*Correspondence* vol. 27), and the letter to James Torbitt, 9 May 1880.

- ³ In his letter of 9 May 1880, CD had told Torbitt that William Carruthers, botanist to the Royal Agricultural Society, thought Torbitt's attempt to raise fungus-proof varieties of potato was hopeless.
- ⁴ For details of Torbitt's growing plan for the year, see the letter from James Torbitt, 13 May 1880.
- ⁵ John Philip Nolan was MP for Galway and served on the seed potatoes (Ireland) committee (*Hansard Parliamentary Debates* 3d ser. 250 (1880): 651–8). In his letter of 20 November 1879 (*Correspondence* vol. 27), Torbitt wrote that Thomas Cave, Liberal MP for Barnstaple until the April 1880 election, had seen his potato varieties and would urge the government to assist in his work.

From C.-F. Reinwald¹ 16 June 1880

Paris
16 Juin 1880

Cher Monsieur

Ma dernière lettre était de 17 Avril 1879. J'ai été favorise depuis de vos honorées lettres de 20 et 27 Avril 1879.²

Nos réimpressions de vos ouvrages ont été retardées par une grave maladie de Mr. E. Barbier, votre traducteur; mais qui heureusement n'a pas eu des suites plus graves pour lui qu'une suspension de son travail de plusieurs mois. Malgré cela nous avons enfin pu terminer la nouvelle édition de *l'Origine des Espèces* et la nouvelle Traduction des *Variations des Animaux et des Plantes*. Je vous envoie aujourd'hui deux exemplaires de chacun de ces deux ouvrages, en un paquet que je remets aux Messageries franc de port.³ Il va sans dire que je suis a votre disposition pour d'autres exemplaires dont vous pourrez avoir besoin pour des amis en France ou ailleurs.

La *Descendance de l'Homme* est encore sous presse; la moitié en est imprimée et l'autre moitié se terminera tout doucement, vu l'état de santé de M Barbier d'ici au mois d'Octobre. C'est réellement une toute nouvelle traduction d'après votre dernière édition anglaise et elle se formera comme celle-ci qu'un seul volume, dont le prix sera moindre que celui des deux premières éditions.⁴

Nos traductions de vos divers traités botaniques ne peuvent se vendre aussi régulièrement que les volumes susdits. Je regrette donc que je ne puisse pas dès aujourd'hui vous annoncer la réalisation d'un bénéfice, dont je vous devrais le pourcentage. Cependant les nouvelles éditions de *l'Origine des Espèces* et de *Variation des Animaux et des Plantes* m'engagent a vous remettre ci inclus un chèque de *quarante livres Sterling* pour la part des bénéfices à venir et possibles dans ces deux entreprises.⁵

La nouvelle édition de *Variation* a été assez couteuse pour l'éditeur et la vente de cette nouvelle traduction ne pourra probablement s'effectuer avec plus de promptitude que celle de la première édition que nous avons publiée, comme vous le savez en 1868.⁶ Je pense donc que le pourcentage que je vous remets avec la présente répondra à la situation présente de l'éditeur et de l'édition.

Depuis l'année dernière j'ai eu la malheur de perdre mon neveu, jeune homme de 38 ans qui était associé a mes affaires.⁷ Je vous prie d'excuser le retard de la présente lettre en consideration des troubles et des travaux supplémentaires qui étaient la suite de cette perte déplorable. Aussitôt que la nouvelle traduction de la *Descendance*

sera achevée j'aurai lhonneur de vous envoyer quelques exemplaires. Quant a le Vie de votre aïeul Erasme Darwin, je n'en ai plus entendu parler pas M. Barbier à cause de sa récente maladie.⁸

Veillez bon agréer, cher Monsieur, l'expression de mes sentiments les plus distingués de reconnaissance et de dévouement avec lesquels je suis | yours truely | C Reinwald

A Charles Darwin Esq Down

DAR 176: 110

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

² See *Correspondence* vol. 27, letter from C.-F. Reinwald, 17 April 1879. CD's letters to Reinwald of 20 and 27 April have not been found; his last known letter to Reinwald is that of 29 October 1879 (*ibid.*).

³ The third edition of the French translation of *Origin* by Edmond Barbier was published in 1880 (Barbier trans. 1880). The first volume of Barbier's French translation of *Variation* was published in 1879, the second in 1880 (Barbier trans. 1879–80); the translation was from *Variation* 2d ed.

⁴ A third French edition of *Descent*, translated by Barbier from Murray's 1879 issue of the revised second English edition, was published in 1881 (Barbier trans. 1881).

⁵ The following of CD's books dealing with botanical topics had been translated into French and published by Reinwald: *Orchids* (Rérolle trans. 1870), *Climbing plants* 2d ed. (Gordon trans. 1877), *Insectivorous plants* (Barbier trans. 1877), *Cross and self fertilisation* (Heckel trans. 1877), and *Forms of flowers* (Heckel trans. 1878). CD recorded the receipt of £40 under the heading 'Reinwald profits on French translation' on 22 June 1880 (CD's Account books—banking account (Down House MS)).

⁶ The first French translation of *Variation* was Moulinié trans. 1868.

⁷ Reinwald's nephew Frédéric Buhlmeyer died in June 1879 (*Bulletins et mémoires de la Société d'anthropologie de Paris* 3d ser. 2 (1879): 430).

⁸ CD had sent the sheets of *Erasmus Darwin* to Reinwald for Barbier to consider translating; see *Correspondence* vol. 27, letter to C.-F. Reinwald, 11 September 1879. CD also wrote about a French translation in his letter to Reinwald of 29 October 1879 (*Correspondence* vol. 27), but no French edition was published.

To James Torbitt 16 June 1880

Down,
June 16, 1880.

My dear Sir

I have no objection to your quoting the sentences referred to; but I should like you to alter one, viz., where I wrote "if I were a minister of the crown" write "if I had the power"; for anyone might smile and say "a pretty fellow to be a minister of the crown". If I were in your place I would endeavour to make my letter to Mr. Forster as short as possible (for I have been told he is much overworked), and copied in clearest writing. On these grounds I would shorten the extracts from my letters.¹

I am not very sanguine of success, but I most truly wish you all the success which you amply deserve in your application to Government. How would it be simply to ask for assistance and leave Mr. Forster or his assistants to suggest some plan? I am sure I do not know which would be best.

Believe me. | Yours sincerely | Ch. Darwin.

P.S. Would it not be better to say that you had been assisted by Mr. Caird, C.B., and Mr. Farrer *of the Board of Trade*, without saying that this was through my intervention.² Their names would thus perhaps have greater weight. I do not think that this would be dishonourable. On the other hand if Mr. F. applied to these gentlemen, no doubt he would hear that they had acted on my advice.

Copy
DAR 148: 121

¹ Torbitt was planning to ask for support for his scheme to grow blight-resistant potatoes from William Edward Forster, chief secretary for Ireland; he asked CD for permission to quote from several of CD's letters praising the scheme (see letter from James Torbitt, 14 June 1880 and n. 2).

² CD had sought the assistance on Torbitt's behalf of James Caird (Companion of the Order of the Bath and member of the inclosure commission (*ODNB*)), and Thomas Henry Farrer (permanent secretary of the Board of Trade). They had recently helped CD raise a subscription for Torbitt to continue his potato work; see letter to James Caird, 24 March 1880.

[Williams & Norgate] 16 June [1880]¹
Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
June 16

Dear Sir

Be so good as to endeavour to get a book or pamphlet (I know not which) by W. Hofmeister, which I see referred to under following title

“Die bis jetzt bekannten Arten aus der Familie der Regenwürmer; Braunschweig 1845.”²

Dear Sir | Yours faithfully | Ch. Darwin

Photocopy
British Library (Surrogate RP 09897)

¹ The correspondent is conjectured from the fact that Williams & Norgate usually supplied foreign books to CD. The year is established by CD's work on earthworms around this date in 1880; see letter to A. C. Ramsay, 17 June 1880, and letter to W. E. Darwin, 18 June 1880.

² CD's annotated copy of Werner Hoffmeister's *Die bis jetzt bekannten Arten aus der Familie der Regenwürmer. Als Grundlage zu einer Monographie dieser Familie* (The presently known species from the family of earthworms. As the basis for a monograph of this family; Hoffmeister 1845) is in the Darwin Pamphlet Collection–CUL, with a separate page of notes by CD. *Earthworms* was published on 10 October 1881 (Freeman 1977).

To A. C. Ramsay 17 June 1880
Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
June 17th 1880

My dear Ramsay

Will you have the kindness to glance at your 2 enclosed notes (*which for the love of Heaven return to me carefully*) & answer me one simple question, viz whether any plants

or weeds grew in the interstices of the stones, & secondly whether trees were near so that leaves often or ever were blown into the court.—¹ I am putting together some notes on the action of worms & I find that naturalists differ much whether worms can live without obtaining dead vegetable matter *from the surface*,— indeed some deny that they get any nutriment out of the humus.— Your case of the little court interests me in other ways.—

I have not been lately in London, but during my two last visits I was so unlucky as to miss you in Jermyn St, & thus missed some pleasant talk²

Believe me | Yours very sincerely | Charles Darwin

I forget whether you have ever seen many worm castings in the court-yard, which I suppose is occasionally swept out.

DAR 261.9: 11 (EH 88205984)

¹ CD evidently enclosed Ramsay's letters of 27 December 1871 and 3 January 1872 (*Correspondence* vols. 19 and 20) describing a pavement running from his house to his garden that had subsided apparently as a result of the action of earthworms. The case appeared in *Earthworms*, pp. 192–3.

² CD was in London from 3 to 11 December 1879 and from 4 to 8 March 1880 (Emma Darwin's diary (DAR 242)). Ramsay was director-general of the Geological Survey of Great Britain, based at 28 Jermyn Street, London.

To W. E. Darwin 18 June 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
June 18th 1880.

My dear William

As the Printers have not sent me proof-sheets, I have been arranging my notes about worms, & I want your help on one point.— Possibly before the autumn you may wish to take some one to Beaulieu Abbey, or if not w^d you be so good as to go there in the early autumn, when the worms work most.¹

What I want is a packet of castings from above the tiles in the square cleared hole; for I find in my notes the clearest statement that the minute fragments of *brick* in the castings were rounded, as if by attrition in the muscular gizzard of the worms.² Now I very much wish to examine under the microscope more of such particles of brick, tile, slate or any other artificial object, which could hardly have been worn except in the worm's gizzard. Will you help me & keep this in mind.

Love to Sara³ | Your affect. Father | C. Darwin

DAR 210.6: 160

¹ CD was awaiting the proof-sheets of *Movement in plants*; see letter to J. V. Carus, 28 April 1880. William had examined the buried pavement at Beaulieu Abbey in Hampshire on 5 January 1872 and CD had visited on 22 June 1877 (*Earthworms*, pp. 193–7); see also *Correspondence* vol. 24, Supplement, letter from W. E. Darwin, [4 January 1872], and *Correspondence* vol. 25, letter to Francis Darwin, [10 June 1877]. William's notes on furrows on a slope at Beaulieu, made in January 1872, are in DAR 63: 77–8.

² CD's notes on the concretions in castings from Beaulieu, dated 20 January 1881, are in DAR 64.1: 81.

³ Sara Darwin.

To W. C. McIntosh 18 June 1880

Down. | Beckenham Kent
June 18th 1880.

My dear Sir

I hope that you will forgive me for troubling you with a simple question. Can you refer me to any monograph on British worms (Lumbricidæ.) by which I could find out, how many endemic species there are, and how many burrow in the earth.—¹ I want further to know whether there exist any burrowing species on the grassy slopes of mountains of some considerable elevation.

I have attended a little to some of the habits of worms & intend this autumn or winter to publish an essay on the subject; & it is in this relation that I want information. My essay will be barely scientific, but the subject has amused me.— I should like to give a copy on a woodblock of the whole intestinal canal of Lumbricus:— Can you refer me to any simple figure?— Perrier in Archives. Zoolog: Exp: gives an admirable one of Urocheta, but it would be better to give (if I do give any) a drawing of Lumbricus.²

I hope that you will excuse all this trouble & I remain | My dear Sir | Yours faithfully | Charles Darwin.

Copy
DAR 146: 351

¹ Lumbricidae is the family of earthworms that contains most European species.

² *Earthworms* was not published until October 1881 (Freeman 1977). A diagram of the alimentary canal of *Lumbricus* appeared in *Earthworms*, p. 18, copied from the *Quarterly Journal of Microscopical Science* n.s. 4 (1864): pl. 7. For Edmond Perrier's diagram of *Urochaeta* (a genus of earthworms native to South America), see *Archives de zoologie expérimentale et générale* 3 (1874): pl. 13. CD discussed worm-castings probably from a species of *Urochaeta* in *Earthworms*, p. 121.

To E. B. Tylor 19 June [1880]

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
June 19th

My dear Sir

I have come across more than one statement that animals learn to know poisonous herbs, & one case referred to Australia. I fancy that another was by Linnæus.¹ I have no doubt that I have references, but it would take me a week of labour to find them. I believed in them because there seems to be good evidence that animals learn by seeing their comrades suffer. It is notorious that you cannot long continue poisoning rats with the same kind of poison. I have received 2 or 3 accounts from trustworthy persons (besides some published notices) that when telegraph wires are first set up in any district, many birds, especially partridges, are killed by flying against them; but that after 2 or 3 years such deaths cease. On the other hand when no harm follows animals get accustomed to what is terrific: the late W. Thompson of Belfast (an excellent observer) told me that when a R^y was first made there, which passed

through some swamps, the wild birds were terrified at the trains, but that after a few months even herons remained stationary close to the train.—²

I wish that I could have aided you better & remain | Yours very sincerely | Ch. Darwin.

P.S. I have just thought of a place to look for references on cattle or sheep learning to avoid poisonous herbs

- (1) *Annals & Mag. of Nat. Hist* 2^d series, Vol. 2. p. 364.
- (2) *Amœnitates Academicæ* Vol. 7 p. 409. (I suppose Linnæus)
- (3) *Stillingfleet Tracts* p. 350
- (4) *Youatt on Sheep* p. 404.³

I do not now know details, but these works were consulted by me,

Postmark: JU 20 | 80

British Library (Add MS 50254 ff. 96–8)

¹ Carl von Linné (Carolus Linnaeus). CD had discussed domestic animals learning to avoid poisonous herbs in *Descent* 1: 36 and in his letter to George Harris, 27 April 1875 (*Correspondence* vol. 23). No letter from Tylor requesting information on the ability of animals to learn about poisonous substances has been found but see the letter from E. B. Tylor, 21 June 1880.

² CD corresponded with William Thompson (1805–52) in the late 1840s (see *Correspondence* vols. 3 and 4); no letter from Thompson detailing this behaviour of birds has been found.

³ The references are to: 'Extracts from a letter to Thomas Bell, Esq., F.R.S., from George Clark, Esq., of Mauritius', *Annals and Magazine of Natural History* (1848); Linnaeus 1749–90; *Stillingfleet et al.* 1762; and *Youatt* 1837.

From E. B. Tylor 21 June 1880

Linden | Wellington Som.

June 21 | 80

My dear Sir

It fits with your remarks about birds and telegraph wires, that a New Zealand colonist whom I asked about sheep and poison-plants offered me as more satisfactory for my purpose his experience of the first setting up of a telegraph, when at first numbers of birds were killed against the wires, but from the second year none.¹ This will answer my purpose as an illustration, and I will not trouble you further about sheep & poisonous plants. But so far as I learn from my friend Mr W. A. Sanford the geologist, who was in Australia, the sheep there (in the West) rush on the bright green poison-pea, and in a few minutes the whole flock begin to whirl round & then fall dead, so that there are really no survivors to benefit by the experience.²

With many thanks for your kindly looking into your evidence for me

I am | Yours very sincerely | Edward B. Tylor

Charles Darwin Esq

DAR 178: 206

¹ See letter to E. B. Tylor, [19 June 1880]; the colonist has not been identified.

² William Ayshford Sanford was colonial secretary of Western Australia from 1851 to 1855 (*Dictionary of Australian artists*).

From W. E. Darwin 24 June [1880]¹

Basset, | Southampton.

June 24

My dear Father,

Should you have any objection to forwarding the enclosed to Lord Derby, perhaps also the enclosed letter to Sara, would explain the case more fully.² It seems to me that L^d Derby can quite decline to ask any question notwithstanding his note if he thinks it better. If you do send it, it might be well to say that in case he did not receive the copy of the New York state survey mentioned by M^r Olmstead a copy can be sent him; I have one sent me by M^r O. which I would send if necessary.³

I have taken a memorandum about Beaulieu & will go there in the early autumn and get you a good parcel of castings.⁴

An acquaintance of mine told that he hatched 7 gallenes under a hen and two days ago when running about with about 100 little chickens the little gallenes being about a month old were attacked by a male & female gallene 3 were killed and 4 were saved by the son who was attacked by the gallenes. The female gallene had no chickens, why on earth should she & her husband hate the ones brought up by a hen.⁵

Please tell Bessy I will send a cheque book & that there is no need to settle about Buxton.⁶

Your affect son | W. E. Darwin

Cornford Family Papers (DAR 275: 82)

¹ The year is established by the relationship between this letter and the letter to W. E. Darwin, 18 June 1880.

² Edward Henry Stanley and Sara Darwin. For the enclosures, see the enclosures to the letter to E. H. Stanley, 25 June 1880.

³ Frederick Law Olmsted had sent copies of *Special report of New York state survey of the preservation of the scenery of Niagara Falls* (Gardiner ed. 1880).

⁴ CD had asked William to visit Beaulieu Abbey, Hampshire, to collect worm-castings; see letter to W. E. Darwin, 18 June 1880 and n. 1.

⁵ Gallene: a variant spelling of galeeny, guinea fowl (*OED*).

⁶ William was a banker in Southampton. Elizabeth Darwin went to Buxton with William and Sara in July 1880; see letter to W. E. Darwin, [19 July 1880] and letter from W. E. Darwin to Charles and Emma Darwin, 22 July 1880. See also letter to the Darwin children, 10 January 1880.

To E. H. Stanley 25 June 1880

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

June 25th 1880

Dear Lord Derby

You will perhaps recollect that I formerly applied to your Lordship with respect to a petition about Niagara, and that you were so kind as to say that you would not object to ask in the House of Lords any question on the subject.¹ I now inclose a

letter from M^r Olmsted to your Lordship, & a copy of one to my sons wife,² which latter will perhaps aid in explaining matters. My son informs me that a map of the district, published by the New York State Survey, was sent to you; but if not received, or lost, my son would send another copy if you would like to see it.³

Apologising for troubling your Lordship, I beg leave to remain | Yours very faithfully | Charles Darwin

[Enclosure 1]

209 W. 46th. St., New York,
11th. June, 1880.

My Lord;

Mr. Darwin having kindly sent me a copy of your lordship's note to him of last November, expressing interest in the movement to restore the natural scenery of Niagara Falls, I sometime since sent you a copy of the report of the New York Commission on the subject.⁴

I am sorry to say that though advocated by a great number of the more eminent men of letters and other esteemed citizens both of Canada and of the United States and received with considerable official favor, the legislative bodies of the Dominion, of the Province of Ontario and of the State of New York have all adjourned without taking favorable action upon the project.

A cautious policy with reference to the present presidential canvass had to do with the failure in New York.⁵ In Canada I am advised that the chief obstacle lay in the difficulty of gaining a serious interest among members of Parliament in a subject so far without the field of their ordinary political discussions.

The agitation will be revived in the autumn, and I beg to say that an inquiry upon the subject in the House of Lords as kindly proposed in your lordship's note to Mr. Darwin, would, as an indication of the interest of the subject to the world beyond Canada and the United States, have a valuable influence and be gratefully regarded by those who have here led the movement; writing in whose behalf, | I have the honour to be | Your lordship's | Very obedient servant | Frederick Law Olmsted.

The Right Honorable, | The Earl of Derby.

[Enclosure 2]

209 W 46th. St., New York,
11th. June, 1880.

Dear Mrs. Darwin;

I have left your note of 8th. March so long unanswered in hopes of being able to give you something definite and agreeable about the Niagara project.⁶ But I must confess at last that we have not only failed at every point legislatively, but that in an effort to keep afloat we are swamped by the Presidential tempest.⁷

I sent you some time since a copy of our report, which, if it should be possible to revive interest in the matter next year, will be a good magazine to draw upon. I do not mean that this shall be the end if I can help it.

I enclose a letter to Lord Derby, which I wish that you would submit to Mr. Darwin, and if approved, send it to him. A little talk in Parliament would undoubtedly have a good effect, especially in Canada, where, although the Governor General and the Princess⁸ showed as much interest as could be expected of them and the ministry was civil and made good promises, the subject seems to have had no serious consideration. Anything tending to show that the leading men of England really care for it and think it worthy of their earnest attention will help to overcome this provincial indifference.

Your friend Mr. Wardell⁹ called while I was in Boston whence I had to go immediately to Washington. On my return, when I called at his hotel, he had gone to Philadelphia. I left a note requesting him to let me know when he should be in town again, but am sorry to say I have had no reply.

With kindest regards and thanks to Mr. Darwin, I am | Very sincerely yours, | Fred^k Law Olmsted.

LS

Liverpool Record Office, Liverpool Central Library (920 DER (15) 43/89/21/2)

¹ For the petition, see *Correspondence* vol. 27, letter from W. E. Darwin, [9 November 1879].

² Frederick Law Olmsted and William Erasmus Darwin's wife, Sara Darwin.

³ In his letter of 24 June [1880], William had told CD that he could send Stanley a copy of *Special report of New York state survey of the preservation of the scenery of Niagara Falls* (Gardiner ed. 1880).

⁴ In 1879, Emma Darwin wrote to Stanley's wife, Mary Catherine Stanley, about the Niagara project; see *Correspondence* vol. 27, letter from Emma Darwin to M. C. Stanley, 12 November [1879]. Emma received a reply from Stanley declining to sign the petition but expressing sympathy with its aims (letter from E. H. Stanley to Emma Darwin, 13 November 1879 (F. L. Olmsted Papers: 1857–1952, Library of Congress, mss 35121, box 40; reel 36)). For the report, see n. 3, above.

⁵ Canvassing was under way for the United States presidential election, which took place on 2 November 1880.

⁶ The note from Sara Darwin has not been found.

⁷ See n. 5, above.

⁸ John George Edward Henry Douglas Sutherland Campbell (marquess of Lorne) was the governor-general of Canada from 1878 to 1883; he married Queen Victoria's fourth daughter, Louise Caroline Alberta, in 1871 (*ODNB*).

⁹ George Young Wardle, manager of Morris & Co., was travelling in the US on the firm's business (letter from Jane Morris to Sara Darwin, 28 March [1880] (Sharp and Marsh 2012, pp. 108–9 and n. 2)).

From the Spallanzani Monument Committee¹ 30 June 1880

Comitato Promotore | pel | Monumento | a | Lazzaro Spallanzani | in | Scandiano

Illustrissimo Signore,

Alcuni Scandianesi nell'intento di tributare un omaggio ben dovuto alla memoria del sommo naturalista LAZZARO SPALLANZANI loro concittadino, fin dal decorso anno si costituirono in

Comitato provvisorio, e in un primo programma sotto la data 10 Maggio 1879 fu proposto di erigere al grande scienziato un monumento statuario nel suo paese nativo.² Il favor grande che incontrò la proposta in Scandiano, in Reggio, in Modena, luoghi dove soltanto fino ad ora si cercarono e si ottennero adesioni ed offerte, incoraggiò la presa iniziativa, per guisa che ritenendosi pressochè assicurato il buon esito del progetto, vennero convocati il 30 Maggio decorso i Signori sottoscrittori di quel programma per stabilire in una seduta preliminare le massime principali che meglio possono condurre alla meta desiderata. In tale circostanza costituitisi i suddetti in Comitato promotore, vennero creati tre uffici direttivi nei tre centri summentovati nello scopo principalmente di procacciare nuove adesioni e allargare così la base del Comitato stesso coll'aggiunta di persone che per ingegno e coltura siano in grado di apprezzare i meriti sovrani del gran fisiologo che vuolsi onorare, procedere in seguito alla raccolta di offerte, e disporre infine a norma, dell'esito di queste, per esaurire le ulteriori incombenze che riflettono la esecuzione del monumento progettato.

Ciò premesso ad opportuna norma, per espresso incarico del Comitato suddetto, si fa invito alla S. V. Ill.ma perchè si compiacca abilitare i sottoscritti ad aggiungere il rispettato di Lei nome nell'elenco dei tanti altri illustri di cui il Comitato altamente si onora.

Le si acclude la scheda relativa che ci lusinghiamo voglia ritornarci sollecitamente firmata in segno di adesione dirigendola all'Ufficio del Comitato Promotore del Monumento Spallanzani in Scandiano e ci pregiamo attestarle il nostro profondo ossequio.

Scandiano il 30 Giugno 1880.

PEL COMITATO PROMOTORE

nella sezione di modena

Cav. Prof. Luigi Vaccà | Cav. Prof. Antonio Caruccio | Dott. Gisberto Ferretti

nella sezione di reggio

Prof. Giacomo Prandi | Prof. Alfredo Jona | Prof. Naborre Campanini

nella sezione di scandiano

Avv. Cav. Giuseppe Basini | Dott. Vincenzo Mattioli | Dott. Pietro Prampolini³

Document

DAR 177: 222

CD ANNOTATION

Foot of letter: 'Aug 4th 2.2.0 Promised'⁴ ink

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

² Scandiano is a town, and Reggio Emilia and Modena are cities, in the Emilia-Romagna region of Northern Italy. Spallanzani was born in Scandiano, taught at the ancient college, and later the university, of Reggio Emilia from 1755 to 1762, and was professor in Modena from 1763 to 1769 (*Complete dictionary of scientific biography*). The monument was inaugurated in the town square of Scandiano on 21 October 1888 (*History of medicine topographical database*, Lazzaro Spallanzani's monument, <http://himetop.wikidot.com/lazzaro-spallanzani-s-monument>, accessed 22 March 2019).

³ Giacomo Prandi, Vincenzo Mattioli, and Pietro Prampolini have not been identified.

⁴ Under the heading 'Gifts and annual subscriptions', CD recorded a payment of £2 2s. for 'Spallanzani monument' on 18 October 1880 (CD's Classed account books (Down House MS)).

To Theodor von Heldreich 1 July 1880

Down | etc. etc.
July 1. 1880.

Dear Sir

I am much obliged to you for your kindness in having sent me your Essay & Poem with notes.—¹ I beg you also to give my best thanks to D^r. Meliarakis for the honour which he has done me by publishing the account of my life & works—²

I remain, Dear Sir | Yours very faithfully | Charles Darwin.

Copy

DAR 145: 10

- ¹ Heldreich sent an offprint of his article 'Beiträge zur Kenntniss des Vaterlandes und der geographischen Verbreitung der Rosskastanie, des Nussbaums und der Buche' (Contributions to the understanding of the native range and geographical distribution of the horse chestnut, walnut and beech; Heldreich 1879). CD's copy is in the Darwin Pamphlet Collection–CUL. The poem was probably *Musinitza: eine Idylle vom Kōrax* (Heldreich 1880); no copy has been found in the Darwin Library–CUL or Down.
- ² Spyridon Miliarakis had translated into Greek William Preyer's biographical sketch of CD which had appeared in the special issue of *Kōsmos* celebrating CD's seventieth birthday (Preyer 1879; Miliarakis trans. 1880).

From Asa Gray 3 July 1880

July 3, 1880

Here is a confirmation of your idea, illustrating how well you hit.¹ I got a few seeds of *Ipomœa pandurata*, the species of Atlantic U.S. with a huge root. One seed only has yet come up. Its germination is same as of *I. leptophylla*, viz. caulicle remains shut and petioles of cotyledons lengthen & bring up the cotyledons.² The caulicle lengthens a little—is $\frac{1}{4}$ inch, the petioles are 2 inches long.

A. Gray

ApcS

DAR 186: 52

- ¹ See letter to Asa Gray, 19 January 1880. CD had described his observations on the germination of *Megarhiza californica* (a synonym of *Marah fabacea*, California manroot), noting that the growth of the radicle or embryonic root seemed to be arrested, while the tubular petiole penetrated the ground and grew to a depth of more than two inches.
- ² *Ipomœa pandurata* (man-of-the-earth or wild potato vine) and *I. leptophylla* (bush morning-glory) are North American species with large tuberous roots. Gray had earlier told CD he thought germination occurred normally in these species, but would investigate further (letter from Asa Gray, [1 April 1880]). In Gray's usage, the caulicle was the initial stem in an embryo (A. Gray 1879, p. 401).

From Francis Galton 5 July 1880

42 Rutland Gate
July 5/80

My dear Darwin

Best thanks for sending me *Révue Scientifique* with Vogt's curious paper, which I return with many thanks— The passage you marked for me makes me sure that

he would give help of the kind I now want & I will write to him.¹ (De Candolle & another Genevese, Achard by name, have already kindly done much.)²

I send an advance copy of those “Visualised Numerals” of mine, not to trouble you to re read what you know the pith of already, but because of the illustrations at the end and also for the chance of your caring to see there the confirmations from other sources of what Vogt says about the left hand executing with facility *in reverse* what is done by the right hand.³ I find that the Editor has cut out all Bidders remarks on this point—which I much regret.⁴ I made Bidder scribble flourishes with pencils held in both hands simultaneously & the reflexion of the one scrawl in a mirror was just like the other picture seen directly.

I have just published in “Mind” something more about mental imagery, & when I get my reprints I will send one, in case you care to glance at it.⁵

Enclosed is a reference that might be put among your D^r Erasmus Darwin papers, in the event of having again to revise the ‘Life’. I had not a notion until I began to hunt up for the reference, how much he had considered the subject of mental imagery, or the very striking experiment in part 1. Sect XVIII b (which in my edition of 1801 is in vol 1. p. 291.) which shews that he himself possessed the faculty in a very marked manner.⁶

We came back after a very successful Vichy visit; my wife improved at once on getting there, but for my part I have since been unlucky, & alas only just out of bed after a week’s illness of the same kind as Litchfield’s long affair—this partly accounts for bad handwriting.⁷

With kindest remembrances to you all from us both & from my sister Emma⁸ who is now with us for a few days | Ever sincerely y^{rs} | Francis Galton

DAR 105: A104–5

¹ Carl Vogt’s article ‘L’écriture considérée au point de vue physiologique’ (Writing considered from a physiological perspective; Vogt 1880) appeared in *La Revue scientifique de la France et de l’étranger*, 26 June 1880. CD’s copy has not been found, but Galton’s notes on the article are in GALTON/2/12/39, UCL Library Services, Special Collections.

² In *Nature*, 15 January 1880, Galton had published his initial research on the ability of some people to see numbers in their mind’s eye; at the end of the article, he solicited further information from readers (Galton 1880b, p. 256). Among the responses to the *Nature* article, Galton received two letters from Arthur Achard (letters dated 30 March and 10 April 1880; GALTON/2/7/2/6/3, UCL Library Services, Special Collections) and one from Alphonse de Candolle (letter dated 9 April 1880; GALTON/2/7/2/6/5, UCL Library Services, Special Collections).

³ Galton’s paper ‘Visualised numerals’ had been read and discussed at a meeting of the Anthropological Institute of Great Britain and Ireland on 9 March 1880; the published version appeared in the *Journal of the Anthropological Institute of Great Britain and Ireland* in January 1881 (Galton 1880c). An appendix with two plates of diagrams illustrated how several respondents visualised numbers (*ibid.*, pp. 96–7). CD’s offprint of Galton 1880c is in the Darwin Pamphlet Collection–CUL.

⁴ For George Parker Bidder’s comments in the published version, see Galton 1880c, pp. 97–8.

⁵ Galton’s paper ‘Statistics of mental imagery’ appeared in the July issue of *Mind* (Galton 1880a). CD’s copy has not been found.

⁶ The enclosure has not been found. CD had consulted Galton and other members of Galton’s family when writing the ‘Preliminary notice’ to *Erasmus Darwin* (see *Correspondence* vol. 27); no second edition was published in CD’s lifetime. Erasmus Darwin had discussed the imagination in sleep and in

contemplation in a section of *Zoonomia* on sleep (E. Darwin 1794–6, 1: 198–219; see pp. 203–3 for the section Galton cites in his edition).

- ⁷ Galton's wife was Louisa Jane Galton. CD's son-in-law Richard Buckley Litchfield had suffered from acute appendicitis while travelling in Switzerland in September 1877 (*Correspondence* vol. 25, letter to H. E. Litchfield, 4 October [1877] and n. 2). Vichy was a popular mineral spa town in Auvergne, France.
- ⁸ Emma Sophia Galton.

From W. S. Dallas 8 July 1880

*Geological Society,
Burlington House, W.
8th July 1880*

My dear Mr Darwin

You will see that your son's article is printed in the Pop. Sci. Review, & I think it makes a very nice paper.—¹ I read it carefully & made a few alterations which seemed necessary, but it did not require so much touching up as he seemed to expect when he returned the proof— I have to thank you for the loan of the woodcuts also, which shall be returned to Mess. Clowes,—& I have suggested to Mr Bogue that as you kindly *gave* us the MS. your son should have some separate copies of the article—² I have not heard from him (which is no novelty) & so do not know what he will do in the matter.—

I have yet another matter to write about— A friend of mine, Mr John Wise, told me the other day that he was in London to see about the publication of some poems, which he proposed to dedicate to you, his proclivities being all in favour of the theory of evolution, & his poems being written with a view to the demonstration of the fact that, contrary to the opinion frequently entertained by Artists (using the term in the broadest sense) there is no necessary antagonism between Science & poetry.— I have not seen his poems, but have no doubt they will be of respectable quality.— Of his literary powers I have a very high opinion,— in fact as he has been for some 20 years connected with the Westminster Review, & for a considerable part of that time as writer of the “Belles Lettres” section, there need be little doubt about his qualifications.— I hope, therefore, that if he should write to you you will kindly take his request into favourable consideration.³

Yours very truly | W. S. Dallas.

DAR 162: 30

- ¹ Francis Darwin's article ‘Climbing plants’ (F. Darwin 1880c) was based on a lecture given to the Sunday Lecture Society on 25 January 1880.
- ² William Clowes & Sons were printers to John Murray. David Bogue was the publisher of the *Popular Science Review*.
- ³ John Richard de Capel Wise dedicated his book *The first of May: a fairy masque* ([Wise] 1881) to CD; the dedication reads: ‘To Charles Darwin | From the author & the artist’ and features a quotation in Greek from Marcus Aurelius, *Meditations* 4.3, that translates as ‘The cosmos is change, life opinion’. The illustrator was Walter Crane. No correspondence between Wise and CD has been found.

From E. A. Darwin 8 July [1880]¹

July 8

Dear Charles—

I was rather puzzled about C.P.'s tithes, but that must be Mariannes share as what I pay to G H D is Catherines share & to the Parker Trust Susan's share, which with yours & mine make up 4 shares I receive.²

yours affe^c | Eras D

DAR 105: B113

¹ The year is established by the reference to tithes; an entry for 'Castle Morton Tithes', dated 28 June 1880, in CD's Account books—banking account (Down House MS) records the receipt of £17 11s. 'per Erasmus', and, under the same heading, the receipt of £3 3s. 6d. 'for Frank' (Francis Darwin). The only year in which tithes were paid in June was 1880.

² The payment was for tithes for CD's property at Castle Morton, Worcestershire. CD and each of his five siblings had inherited one sixth of a 'Castle Morton trust' from their father, Robert Waring Darwin, in 1837 (Robert Waring Darwin's Investment book, Down House MS). The share of CD's sister Marianne Parker evidently went to her son Charles Parker after her death. George Howard Darwin inherited the share of Catherine Langton and Susan Elizabeth Darwin's share was apparently left to the Parker Trust.

To E. H. Stanley 8 July 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
July 8th 1880

Dear Lord Derby

I am very much obliged for your Lordships great kindness in having taken the trouble to inform me about the Niagara affair, & for all that you have done.¹ I hope that the Governments of the U. States & Canada may be induced to take some active steps.—

I beg leave to remain | your Lordship's very faithfully | Charles Darwin

Liverpool Record Office, Liverpool Central Library (920 DER (15) 43/9/21/3)

¹ No letter from Stanley regarding the Niagara affair has been found, but see the letter to E. H. Stanley, 25 June 1880. According to notes attached to that letter, Stanley replied to CD on 27 June 1880 and again on 7 July 1880. The first note reads: 'Darwin, C. June 26/80 Ansd. June 27. Will ascertain how the matter stands. Ready to put question if of use, but no use if the answer is that nothing has been heard of the matter—Such would only show failure. The Cans. & Amers must take the initiative—we can only back them up. Wrote again July 7.' The second note reads: 'Ansd that nothing known of the matter here, they must make the first move. July 7.' In November 1879, CD had signed a memorial for the protection of Niagara Falls from commercial and industrial development (see *Correspondence* vol. 27, letter from W. E. Darwin, [9 November 1879]). For more on the campaign to preserve Niagara Falls, see Runte 1973.

To H. W. Jackson 9 [July 1880]¹

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Friday 9th

My dear Sir

I shall be happy to see the members of the Association here tomorrow, & I heartily wish that the place was more attractive than it is.—² Perhaps I c^d. make a few

remarks to the members on the Natural History of the district, which might interest them slightly,— anything of much interest I could not say. As the party will be rather large I think the best plan will be to receive them in the drawing room, which opens into a rather large verandah & I will have benches in the garden close in front, & by these together everybody will, I hope, be able to rest a little.³

My dear Sir | Yours faithfully | Charles Darwin

Copy

DAR 185: 26(ii)

¹ The month and year are established by the relationship between this letter and the letter to H. W. Jackson, 15 July 1880.

² Jackson was an honorary secretary of the Lewisham and Blackheath Scientific Association. CD had invited members and their friends to visit Down on the afternoon of 10 July 1880.

³ According to a report of the visit in the *Proceedings of the Lewisham and Blackheath Scientific Association* 2 (1880): 19–20, forty-three members and friends of the society visited Down. CD showed them the albums he received from German and Dutch naturalists on his 70th birthday and also showed some recent research on earthworms in his study. The visit was also described in a letter from Emma Darwin to Henrietta Emma Litchfield, [11 July 1880] (DAR 219.9: 242).

To John Murray 10 July 1880

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

July 10 1880

My dear Sir

I have received Revises of the 2 first sheets of my book, & I now want to beg a little advice. The book, I believe to possess some value, & I sh^d expect for some years a few copies would be sold.— I shall give away about 40 or 50 copies.— Now had 750 or 1000 copies better be printed off? The book will cost me a good deal & I do not want to waste more money than can be avoided. It would, however, be a pity if in the course of a year or two or three copies could not be bought.— Can you give me an idea of cost of paper & printing off of 250 copies?¹

Secondly, before long an Index, (& I sh^d wish a good one to be made) must be thought of— Can you find an Index-maker & arrange about fair remuneration for me?²

I will order a set of sheets for Index-maker, & 2 other sets for German & perhaps French Translations.³ I enclose a few instructions about making Index.—

Pray believe me | My dear Sir | yours sincerely | Ch. Darwin

National Library of Scotland (John Murray Archive) (Ms. 42152 ff. 370–1)

¹ CD was correcting proof-sheets of *Movement in plants*; he expected to pay the initial cost of publication (see letter to R. F. Cooke, 16 July 1880).

² The indexer was Matilda Smith; an entry in the John Murray copies ledger (National Library of Scotland, John Murray Archive & Publishers' Collections, MS.42733) recorded £8 paid to 'Mrs Smith for indexing The power of movement in plants'.

³ The German translation of *Movement in plants* was made by Julius Victor Carus (Carus trans. 1881); the French translation was made by Édouard Heckel (Heckel trans. 1882).

From C. L. van der Burg and H. Cretier¹ 15 July 1880

Kon. Natuurk. Vereeniging | in | Nederl. Indië.

S. D. Viro Egregio Ch R Darwin | Down Beckingham—Kent

Societatis Regiae Scientiarum in India Batava Orientali Praefecti Te, Vir Egregie,
propter merita Tua in Scientiam Naturae

Socium (Correspondeerend Lid) corporis sui creavere.²

Si id effecissemus ut, Tu Vir Egregie, diploma Societatis nostrae, quod his letteris
adjungimus, acceptares et tanquam specimen venerationis nostrae erga Te haberes,
existimabamus optandum quiddam nos esse assecutos.³

D^r van der Burg | Praeses | D^r H Cretier | Scriba

Scripsimus Bataviae | Idibus Juliis 1880

DAR 202: 107

¹ For a translation of this letter, see Appendix I

² CD was nominated as a corresponding member of the society at the meeting of 20 May 1880; his election was confirmed on 17 June (*Natuurkundig tijdschrift voor Nederlandsch Indië* 40 (1881): 378–9).

³ For the diploma, see Appendix III.

From R. F. Cooke 15 July 1880

50^A, *Albemarle St* | *W.*
July 15, 1880

My dear Sir

Your new work is calculated to run to 600 pages, which is a good deal more than
any of yr previous ones.¹

Do you *wish* to publish this at your own expense or do you wish Mr Murray to
undertake it on the usual terms?

I think you may fairly print off 1000 copies as we shall be able to dispose of those
sooner or later.²

I calculate that the expenses of 1000 copies will be about £300. & if these are sold
(& the retail price is fixed at **12/**) they may produce from £75 to £100.

But 12/- is a less price than we have generally fixed on your works. It might be
15/-³

Do you wish a set of stereotype plates to be sent to Mess^{rs}. Appleton?⁴

Have you returned any sheets to Mess^{rs}. Clowes⁵ for *press*?

We might print off 1250 No instead of 1000 No & then distribute the type.

Let me know your views & wishes. | Yours faithfully | Rob^t Cooke

Cha^s Darwin Esq

We must not use quite so thick a paper this time

DAR 171: 506

CD ANNOTATION⁶*Top of letter:*May 28th

Cooper 3. 11. 0

Feb. 3^d 137. 18.

£141: 11.9. 0 ink

¹ The previous longest physical volume of CD's works was the second volume of *Variation* 2d ed.; it was 499 pages.

² In the event, 1500 copies of *Movement in plants* were printed (Freeman 1977). John Murray was CD's publisher.

³ The eventual price was 15s. (Freeman 1977).

⁴ CD's US publisher, D. Appleton & Co., usually printed CD's works from stereotypes made by John Murray.

⁵ William Clowes & Sons were printers to John Murray.

⁶ CD's annotation is a note for his reply; see letter to R. F. Cooke, 16 July 1880. James Davis Cooper made the woodblocks for *Movement in plants*.

From James Geikie 15 July 1880

Balbraith, Perth
15th July 1880

My dear Sir

Some years ago you did me the honour to send me a most interesting letter containing an account of some observations made by you on the gravelly drift near Southampton—and which had led you to suggest an explanation of the vertically-placed stones in that accumulation.¹ You may remember that you attributed the peculiar position of those stones to differential movements in the drift itself arising from the slow melting of beds of frozen snow interstratified with the gravels. Your view explained also in a very satisfactory manner the wide distribution of the gravels over the flattish platforms or plateaus between the valleys. I have found this explanation of great service even in Scotland, and from what I have seen of the drift gravels in various parts of Southern England and Northern France I am inclined to think that it has a wide application. I write now to ask whether you will permit me to publish your view in your own words in a forthcoming volume of mine in which I treat of the climatic and geographical changes which have taken place in Europe since the commencement of the pleistocene period.² Should you think fit to grant my request I need not say that I will be extremely obliged. You will at the same time I am sure confer a favour on many other glacialists who have long been puzzled with the phenomena which your explanation so satisfactorily accounts for.

Do not trouble to write in reply unless you would rather that I did not use the letter which you were so kind as to send me. A post-card with the single word “No” will suffice if you do not approve.

With highest regards | Yours faithfully | James Geikie

DAR 165: 30

¹ See *Correspondence* vol. 24, letter to James Geikie, 16 November 1876.

² Geikie quoted a large section of CD's letter of 16 November 1876 in *Prehistoric Europe: a geological sketch* (Geikie 1881, pp. 141–2).

To H. W. Jackson 15 July 1880

Down, | Beckenham, Kent | (Railway Station | Orpington. S.E.R.)
July 15. 1880

My dear Sir,

I must write a line to thank you sincerely for the official & for your private note, both of them extremely kind.¹ I much wish that the weather had been better, & then perhaps I could have made the visit more agreeable to your members. I was very tired in the evening, but none the worse next day.

My dear Sir | Yours very faithfully | Charles Darwin.

Copy
DAR 146: 3

¹ Neither of the notes has been found, but they were evidently written to thank CD for having welcomed members and friends of the Lewisham and Blackheath Scientific Association to Down on 10 July 1880 (see letter to H. W. Jackson, 9 [July 1880] and n. 3). According to Emma Darwin, there were 'violent showers every hour' (letter to H. E. Litchfield, [11 July 1880] (DAR 219.9: 242)).

From Lawson Tait 15 July [1880]¹

7, Great Charles St. | Birmingham.
July 15 [1880]

My Dear Sir,

You must think me a nuisance, but I have so much at heart that you are intimately concerned with that you must forgive me. You get the "Midland Naturalist" so that I know you are familiar with our "Union". We (Management Committee have just decided to establish a prize for local observations in every of our departments of an original character and we want to call it the "Charles Darwin Prize".² Mr E. W. Badger our Secretary will write you formally and I hope you will consent.³ A Medal will be struck with your portrait.

We are further encouraging original research by running a fund from which we have already granted £150 a year to Dr George Gore F.R.S. for the continuance of his researches.⁴ This is by the Bmg^{ham} Philosoph. Soc. Knowing that you are interested in such a work, that a contribution from you would have its value multiplied tenfold by its source, guessing that you may be able to give and would like to do it, will you forgive me for begging. My own donation is £2.2 a year for 10 years & they go up to hundreds⁵

Yours truly, | Lawson Tait.

Please in your reply to Mr Badger do not mention my having written to you

DAR 99: 215–216

- ¹ The year is established by the relationship between this letter and the letter from E. W. Badger, 17 July 1880.
- ² The *Midland Naturalist* was the journal of the Midland Union of Scientific and Literary Societies. The scheme for the prize was adopted on 15 July 1880 (*Midland Naturalist* 3 (1880): 181–2). A copy of these pages from the journal is in DAR 226.2: 46.
- ³ See letter from E. W. Badger, 17 July 1880.
- ⁴ The Birmingham Philosophical Society gave George Gore a grant in aid of research at his Institute of Scientific Research at Easy Row, Birmingham; the institute was founded in 1880 (see *ODNB* s.v. Gore, George, and *Midland Naturalist* 4 (1881): 270; see also *Nature*, 1 July 1880, p. 203).
- ⁵ CD recorded a payment of £25 on 19 July 1880 to 'Birm Sc Fund' under 'Gifts [and] Annual Subscriptions' in his Classed account books (Down House MS); a payment of £2 2s. to 'Birm Nat Hist Soc' is recorded under the same heading on 23 September 1880.

To C. L. van der Burg [after 15 July 1880]¹

Sir

I beg leave to acknowledge the receipt of your courteous letter in which you announce that the Kon. Natuurkund. Vereeniging in N.-I. has conferred on to me the distinguished honour of being a corresponding m; & for this h. I return my very sincere thanks²

I have the hr. to remain | Sir | Your obliged & obed sevt | Charles Darwin

To the | President

Draft

DAR 202: 107v

- ¹ The date is established by the relationship between this letter and the letter from C. L. van der Burg and H. Cretier, 15 July 1880.
- ² See letter from C. L. van der Burg and H. Cretier, 15 July 1880 and n. 2. CD had been elected a corresponding member of the Koninklijke Natuurkundige Vereeniging in Nederlandsch-Indië (Royal Scientific Society in the Dutch East Indies).

To R. F. Cooke 16 July 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
July 16th 1880

My dear Sir

I am much obliged for your note.¹ I intended (& had as I thought so clearly expressed myself) to publish the book at my own cost & risk; but I did so merely because I did not think it fair that M^r Murray & you sh^d. have the risk of publishing a purely scientific work. I sh^d. certainly prefer to publish on the old or former terms, if, on full consideration, you decide to do so.² But I believe that you have omitted in your calculations that I have paid to M^r Cooper, for 199 or 200 woodcuts

	£	s.	
Feb. 3	137.	18.	0
May 28 th	3.	11.	0
	£141:	9:	0

The cuts were rendered more expensive, as several had to be photographed on wood.—³

In favour of the book, I think I can truly say that it contains much new & curious matter; but then there are very few persons who care for physiological Botany in this country.—⁴

With respect to price I sh^d be rather sorry at cost being 15^s, & if I publish at my own cost, I will fix 14^s & have printed off 1000 copies.—⁵

As soon as you have decided let me hear the result.—

I hope & think corrections will not be quite so heavy as on some former occasions; yet they will be considerable, notwithstanding I went *thrice* carefully over the completed M.S. The paper certainly must be thinner than usual.—⁶

Only 2 sheets have been returned to M^{rs} Clowes for Press.—

I do not yet know about stereotyped Plates for M^r Appleton: I have told them I could not judge whether it was worth their while to reprint the book.—⁷ There will be a German Edition & perhaps a French one.⁸

With many thanks for your uniform kind attention to my wishes I remain, My dear Sir | Yours sincerely Ch. Darwin

P.S. Please remember about Index-maker⁹

National Library of Scotland (John Murray Archive) (Ms. 42152 ff. 372–3)

¹ See letter from R. F. Cooke, 15 July 1880.

² CD had asked about publication costs in his letter to John Murray, 10 July 1880. John Murray usually published CD's books at his own expense and paid CD a percentage of the profits on publication.

³ Cooke had calculated the expenses for 1000 copies at £300 (letter from R. F. Cooke, 15 July 1880). James Davis Cooper produced woodcuts for the illustrations. For more on the process of using photography on wood, see Beegan 1995, pp. 266–9.

⁴ CD alludes to the fact that most British universities and botanic gardens focused on systematic rather than physiological botany. For more on the rise of physiological botany in Britain around this time, see N. Morgan 1980, pp. 142–55.

⁵ Cooke had calculated profits if the book were sold for 12s., but then suggested 15s. as the price (see letter from R. F. Cooke, 15 July 1880 and n. 3).

⁶ Cooke suggested thinner paper as the book would be about 600 pages (letter from R. F. Cooke, 15 July 1880).

⁷ D. Appleton & Co.; see letter from R. F. Cooke, 15 July 1880 and n. 4.

⁸ See letter to John Murray, 10 July 1880 and n. 3.

⁹ See letter to John Murray, 10 July 1880 and n. 2.

To Lawson Tait 16 July [1880]¹

My dear Sir

I sh^d be the most ungracious of men if I hesitated to accept the honour of the intended prize-medal, & will answer accordingly when I receive the official letter.—²

I saw something in the newspaper about the Fund, & admired greatly the noble spirit of Birmingham. I supposed that it was confined to the place; otherwise I hope & believe that it w^d have occurred to me spontaneously to have subscribed.— As it is, I shall be most happy to do so. I do not, however, understand whether it is an annual subscription; or a donation for a fund, the interest of which alone is employed.— I

sh^d be glad to subscribe 2.2.0 annually or what I sh^d prefer w^d be to give 25£ the interest of which w^d be about 1£ annually, & as I am an old man, this w^d be best plan for the Fund. Will you kindly take the trouble to advise me.³

I w^d give more, but in truth the claims on my purse are many.—

Pray Believe me | my dear Sir | yours very f. C. D.

P.S. You sent 2 surgical papers to my son F. (who at present is away from home) and I have read them with interest.⁴ Your success in ovariectomy is truly wonderful. I had a sister who many years ago died from this cause, and now she might have been saved!⁵ Surgery is indeed a grand scientific art.—

L. Tait Esq^{re}— July 16th

Draft

DAR 202: 86

¹ The year is established by the relationship between this letter and the letter from E. W. Badger, 17 July 1880.

² Letter from E. W. Badger, 17 July 1880. The Midland Union of Scientific and Literary Societies had resolved to offer a 'Darwin prize' or medal for original work on local topics (see letter from Lawson Tait, 15 July [1880] and n. 2).

³ An article in *Nature*, 1 July 1880, p. 203, reported on the fund established by the Birmingham Philosophical Society for the endowment of original research and on the first recipient of a grant, George Gore (see letter from Lawson Tait, 15 July [1880] and n. 4). For CD's contributions, see the letter from Lawson Tait, 15 July [1880] and n. 5.

⁴ Francis Darwin was probably in Wales at this time (see letter from Francis Darwin, [1 August 1880]); he went to Coniston in the Lake District from about 7 to 17 August (letter from Emma Darwin to W. E. Darwin, [18 August 1880] (DAR 219.1: 137). The papers sent by Tait have not been found, but were probably two recently published papers from the *Medico-Chirurgical Transactions* on surgical procedures (Tait 1880a and 1880b).

⁵ Tait was a pioneer in ovariectomy (surgical removal of ovaries) and performed one hundred procedures with the loss of only three patients (*ODNB*, Tait 1880a). CD's sister Marianne Parker died in 1858 of 'chronic disease of the ovarium' (Certificate of death, Marian Parker, 1858, Overton (General Register Office, England)).

From E. W. Badger 17 July 1880

Midland Union of Natural History Societies. | Midland Counties Herald Office | Birmingham.
July 17th 1880

Sir,

At a recent meeting of one of the Committees of this Union a resolution was passed requesting me to prefer a request to you, but before doing so I desire as briefly as I can to say a few words about the Union.¹

The first number of the *Midland Naturalist* gave an account of the origin & formation of the Union: the enclosed paper is a copy of that account.² The Union now consists of 23 Natural History Societies in various parts of the Midland Counties.

The Council (consisting of two delegates from each of the societies) has had under consideration for some time past how best to foster original work among the 3000 members of the Union; and at the last meeting (at Northampton) they submitted

a proposal for the establishment of an annual Prize. The idea was warmly approved of & the Management Committee were directed to consider the subject & were empowered to carry the proposal into effect if they should think it desirable to do so.

At a meeting of this Committee held at Birmingham on Thursday last D^r. T. Wright FRS (Cheltenham)³ in the chair, on the motion of M^r Lawson Tait FRCS it was unanimously resolved to offer a Medal or Prize of the value of £10 annually for the best local observations upon any subject within the Scope of the Societies in the Union, & the Subjects to which contributions shall be limited for the next three years were fixed to be 1881 Geology, 1882 Biology, 1883 Archæology.— It is desired to afford as much liberty as possible & hence it was settled that the Prize may be awarded to any paper contributed by a member of the Union which shall during the year have been printed in the journal of the Union (the *Midland Naturalist*) or which before a day to be fixed shall have been sent in for publication.

The following resolution was then unanimously passed:—That the Hon Sec be requested to ask M^r Charles Darwin for permission to use his name for the Prize and in the event of consent being given that the Prize be called “The Darwin Prize”.

It is for the purpose of making this request that I have troubled you with this letter; and I can assure you it will be a source of much gratification & encouragement to my Committee if you will yield your consent. In the event of your doing so it is our intention to have a Medal struck bearing your portrait for presentation to those who prefer a gold medal to the amount of the prize in money.⁴

I have the honour to subscribe myself | Sir Your obed. Servant | Edward W Badger | Hon Sec

Charles Darwin Esq., FRS &c

DAR 160: 14

¹ See letter from Lawson Tait, 15 July [1880] and n. 2.

² The enclosure has not been found, but see *Midland Naturalist* 1 (1878): 1–4 for the account of the formation of the Midland Union of Natural History Societies.

³ Thomas Wright.

⁴ The die for the medal was cut by Joseph Moore; one side showed a bust of CD and the other showed a branch of coral (*Midland Naturalist* 5 (1882):159).

To Hermann Müller 18 July 1880

Down, Beckenham, Kent
July 18. 1880.

My dear Sir

... I likewise enjoyed your well-merited castigation of M. Bonnier.¹ The book appeared to me a most unsatisfactory one; but I was interested by his evidence of at least the occasional reabsorption of nectar.—² ...

Yours very faithfully | Ch. Darwin

Copy

DAR 146: 441

- ¹ In the July 1880 issue of *Kosmos*, Müller had written an essay review of Gaston Bonnier's *Les nectaires: étude critique, anatomique et physiologique* (Nectaries: a critical anatomical and physiological study; Bonnier 1879a) titled 'Gaston Bonniers angebliche Widerlegung der modernen Blumentheorie' (Gaston Bonnier's supposed refutation of modern floral theory; H. Müller 1880b). In the highly critical article, Müller dismissed Bonnier's study as mere teleological speculation and a chain of logical errors.
- ² Bonnier had observed that in *Reseda odorata* (mignonette), nectar produced on the underside of the disc, which was abundant when the flower opened, was reabsorbed after fertilisation (Bonnier 1879a, pp. 108–9).

To E. W. Badger [19 July 1880]¹

I request that you will be so good as to inform the members of the Committee that their wish to name the medal after me is a very great honour, which I gladly accept. It is particularly pleasing to me to have my name connected, in however indirect a manner, with a scheme for advancing science, the study of which has been my chief source of happiness throughout life.²

Manchester Guardian, 2 May 1882, p. 6

- ¹ The date is established by the relationship between this letter and the letter to W. E. Darwin, [19 July 1880].
- ² See letter from E. W. Badger, 17 July 1880.

To W. E. Darwin [19 July 1880]

My dear old W.—

As you helped me at Southampton about the gravel, I have thought that you wd like to see the enclosed, which has pleased me.¹ I have told G. about the celts at Southampton, & about the musk ox & woolly elephant remains in angular gravel at Greenstreet Green, which must have been deposited during an almost arctic climate & which I do not doubt was washed down from about Knockholt Beeches over frozen snow, accumulated in the large valleys.—² We are very quiet here, & I hope it is not very dull for your mother. But I am tired with long letter to Geikie & another difficult one about a prize-medal bearing my name & face, which has been founded at Birmingham.—³ Please return Geikie's letter—

My best of loves to my dear “dutiful & affectionate daughter”, whom I do hope Buxton may do good to.—⁴ Love to Bessy & to Miss Ashburner, if I may presume so far⁵ | Your affectionate Father | C. Darwin

Postmark: JY 19 80
DAR 210.6: 161

- ¹ CD enclosed the letter from James Geikie, 15 July 1880. CD and William had discussed CD's ideas about upright pebbles and interstratified gravel at Southampton in 1876 (see *Correspondence* vol. 24, letter to W. E. Darwin, [after 20 November 1876], and *Correspondence* vol. 30, Supplement, letter from W. E. Darwin, 30 November [1876]).

- ² See letter to James Geikie, 19 July 1880 and n. 4. Knockholt Beeches was a woodland near Sevenoaks, Kent; Greenstreet Green was about six miles to the north, and about three miles north-east of Down.
- ³ See letter from E. W. Badger, 17 July 1880, and letter to E. W. Badger, [19 July 1880].
- ⁴ Sara Darwin was consulting a physician and taking the waters at Buxton (see letter from W. E. Darwin to Charles and Emma Darwin, 22 July 1880).
- ⁵ Elizabeth Darwin and Anne Ashburner.

To James Geikie 19 July 1880

Down | Beckenham Kent. &c
July 19th. 1880—

My dear Sir.

Your letter has pleased me very much. & I truly feel it an honour that any thing which I wrote on the drift &c should have been of the least use or interest to you.— Pray make any use of my letter; I forget whether it was written carefully or clearly, so pray touch up any passages that you may think fit to quote—¹

All that I have seen since near Southampton & elsewhere has strengthened my notion— Here I live on a Chalk platform gently sloping down from the edge of the escarpment to the S. (which is about 800 f^t in height.) to beneath the tertiary beds to the north— The beds of the large & broad valleys (& only of these) are covered with an immense mass of closely packed broken & angular flints; in which mass the skull of the Musk Ox & woolly elephant have been found—² This great accumulation of unworn flints must therefore have been made when the climate was cold, & I believe it can be accounted for by the larger valleys having been filled up to a great depth during a large part of the year with drifted frozen snow—over which rubbish from the upper parts of the platforms was washed by the summer rains—sometimes along one line & sometimes along another; or in channels cut through the snow all along the main course of the broad valleys.³

I suppose that I formerly mentioned to you the frequent upright position of elongated flints in the red Clayey residue over the chalk, which residue gradually subsides into the troughs & pipes corroded in the solid chalk— This letter is very untidy, but I am tired—

Pray believe me, My dear Sir. | Yours sincerely. | Ch: Darwin.

P.S. | Several palæolithic celts⁴ have recently been found in the great angular gravel-bed near Southampton in several places—

Copy
DAR 144: 332

¹ See letter from James Geikie, 15 July 1880 and nn. 1 and 2.

² The musk ox is *Ovibos moschatus*; an extinct relative, *Praeovibos priscus* (giant musk ox) also existed until the end of the Pleistocene epoch. The woolly elephant or mammoth (*Mammuthus primigenius*) became extinct in the early Holocene epoch.

³ Geikie quoted this paragraph in J. Geikie 1881, p. 142.

⁴ The Palaeolithic period in human prehistory was characterised by the development of stone tools, among which was the celt, a type of hatchet with a chisel-shaped edge. The period ended roughly around the end of the Pleistocene epoch.

July 1880

To Lawson Tait 19 July 1880

*Down, | Beckenham, Kent | (Railway Station | Orpington. S.E.R.)*July 19th 1880

My dear Sir

I have much pleasure in enclosing 25£ for your Scientific Fund in Birmingham—¹

Yours very faithfully | Ch. Darwin

Shrewsbury School, Taylor Library

¹ On 19 July 1880, CD recorded a payment of £25 to 'Birm Sc Fund' under 'Gifts [and] Annual Subscriptions' in his Classed account books (Down House MS). For more on the fund, see the letter to Lawson Tait, 16 July [1880] and n. 3.

From R. F. Cooke 20 July 1880

50A, Albemarle Street, London. W.

July 20 1880

My dear Sir

With the additional information which you have given regarding your new work, the prospect is rather more gloomy.¹

I have put down the cost of Illustrations as £150—& have also added a little more to the cost of corrections & the result is that the whole expense of 1000 Copies comes to £450.

If 1000 copies are printed & sold, the *retail* price being fixed at 14^s/- the produce will be £456.

Where is the profit for Author or publisher?

What is to be done? Can we venture to print 250 more, or can we raise the price to 16/-²

We are fairly puzzled

Yours faithfully | Rob^l CookeCha^s Darwin Esq

DAR 171: 507

¹ See letter to R. F. Cooke, 16 July 1880 and n. 3. Cooke had failed to take into account the cost of woodcuts for *Movement in plants*.

² Cooke had proposed a cost of 15s. but CD had suggested 14s. (see letter to R. F. Cooke, 16 July 1880 and n. 5).

To R. F. Cooke 21 July 1880

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

July 21. 1880

My dear Sir

I must take the risk & loss on my own shoulders. As I have made some money by science, I must now lose some for science. I will have 1000 copies printed off &

from what you say charge 15.¹ As soon as a moderate time has elapsed for the sale of whatever copies may be sold I will settle with you; & I shall, as I believe, in the course of a few years be partly or wholly repaid by future sales.

With many thanks for all your kind consideration of this affair, I remain | Yours sincerely | Ch. Darwin

National Library of Scotland (John Murray Archive) (Ms. 42152 f. 374)

¹ See letter from R. F. Cooke, 20 July 1880 and nn. 1 and 2.

From W. E. Darwin to Charles and Emma Darwin 22 July 1880

Buckingham House | Buxton

Thursday | July. 22 1880

Dear Mother | This also meant for you so please read it aloud

My dear Father,

I return M^r Geikie's letter which I was very much interested in reading.¹ I am extremely glad he wishes to adopt your idea of settlement of the gravel through the melting of snow. I must get his book when it is out, as I shall be curious to see whether he makes any suggestions as to the Celts, and as to the direction which the floods took originally that spread the flints & gravel over the snow; whether it was from the N.W. which would be the direction of the general drainage system when the Solent was a big river.²

I am trudging about here with a hammer & bag & map, and with the help of Ramsay's capital book one gets a fairly clear idea of the geology; but great areas of carboniferous limestone and millstone grit are rather uninteresting; I hope soon to take a day into a part where the carboniferous & permian are faulted tighter & see if I can make it out; and also I mean to get on the top of Kinder Scout to see the wonderful weathering of the grit.³

The country is disappointing as far as beauty goes, and though the effect of the wide bleak grass hills is fine it is much spoilt by numberless stone walls; the only really pretty part is the deep valley cut by the Wye in limestone which we shall see well in going down to Haddon.⁴ Miss Ashburner is set on castles so that we are to drive 12 miles to see Pevenil's Peak, tho' I fancy it is not much of a ruin, her interest in Kenilworth was extreme, and we were so sorry that Sara was not equal to going; but it was wiser not after our tiring day at Stratford; which interested us all four very much.⁵ I feared we should be plagued with crowds of wretches being shown over the house at the same time, but we most luckily had it all to ourselves; we did not gush or get sentimental, but when on the spot one felt it all the more astonishing that Shakespeare could have written and read all that he appears to have done, and I made Miss A. most indignant by saying that probably after all you were right & that Bacon was the man.⁶

Sara has consulted a dried up little Scotchman a D^r Robertson,⁷ who seems not to have much in him, but he wisely orders no physick, and is very cautious in the amount of bathing ordered, which is only to be every other day, beginning at 6

minutes, & not getting beyond 10. It is too cold for sitting out unluckily, and there are hardly any pretty short walks except in the public gardens, but after Droitwich she will be equal to any dullness, and I think the keen air after Southampton mildness will be a wholesome change. I am sure it will brace me up well as I was getting rather flabby.

I went yesterday and called on Reginald D. & found him very friendly & pleasant and hearty, and I much enjoyed seeing all the D. pictures & he is evidently deeply interested in the pedigree and all information about the family.⁸

He seems to be particularly taken with George, and said on several occasions what a first rate fellow he was, he had been alarmed at a Wrangler, and expected a tall thin man in spectacles, and was delighted to find an ordinary mortal who could laugh.⁹ He has the signs of having been a sportsman & small squire all his life, and has been chairman of the bench here for 20 years or more, so that he is one of the big people here. He & Mrs. D.¹⁰ called he today, she is a pleasant old lady & tried to be very friendly, but as I had before explained that S. was not strong & we are a large party, I think they will probably not ask us to dinner.

I am glad to get to know my relations, and also to see the picture of Erasmus which he has which is the finest I have seen.¹¹

Goodbye my dear Father | My love to Mother, you must feel an oddly small party your affect son | WED

Reginald D. seemed really smitten with George

What is it about a bronze of your head¹²

Cornford Family Papers (DAR 275: 76)

¹ CD had enclosed the letter from James Geikie, 15 July 1880, with his letter to William of [19 July 1880].

² James Geikie's book, *Prehistoric Europe: a geological sketch* (Geikie 1881), was published in January 1881, but CD received a copy in November 1880 (see letter to James Geikie, 27 November 1880). Geikie discussed the earlier extent of the Solent and Southampton Water in *ibid.*, pp. 341–2.

³ William had probably borrowed CD's copy of Andrew Crombie Ramsay's *The physical geology and geography of Great Britain: a manual of British geology* (Ramsay 1878). Ramsay discussed the character of Kinder Scout and the Millstone grit in *ibid.*, pp. 326–31.

⁴ The village of Over Haddon in the Peak District is about twelve miles south-east of Buxton.

⁵ Peveril Castle is a ruined eleventh-century castle overlooking Castleton, a village about ten miles north-east of Buxton. It was made famous in Walter Scott's novel *Peveril of the peak* ([Scott 1823]), as was Kenilworth Castle in his novel *Kenilworth* ([Scott] 1821). Anne Ashburner, Sara Darwin's aunt, was visiting from America.

⁶ William Shakespeare's birthplace in Stratford-upon-Avon was a popular attraction. William alludes to the idea, first presented in the late 1850s, that Shakespeare's plays were actually written by Francis Bacon (see D. S. Bacon 1857).

⁷ William Henry Robertson.

⁸ CD had consulted his uncle Reginald Darwin, who lived in Buxton, when he was working on *Erasmus Darwin* (see *Correspondence* vol. 27). George Howard Darwin had provided Reginald with a pedigree of the family and other family papers (see letters from G. H. Darwin, 6 March 1880 and 28 May 1880).

⁹ George had recently met Reginald Darwin; Emma Darwin wrote of the meeting, 'G. came home delighted w. Old Reginald who is quite as jolly as his letters seemed to be' (letter from Emma Darwin to H. E. Litchfield, [11 July 1880]; DAR 219.9: 242). When at university at Cambridge, George had been second in the mathematical honours examination; the position was known as 'second wrangler' (*Cambridge University calendar* 1868).

¹⁰ Mary Anne Darwin.

¹¹ Reginald possessed two portraits of Erasmus Darwin, one by Joseph Wright and one by James Rawlinson (see *Correspondence* vol. 27, letter from Reginald Darwin, 7 April 1879).

¹² CD had given permission for the Midland Union of Natural History Societies to award a Darwin Prize and Medal; the medal featured a bust of CD and the reverse showed a branch of coral (see letter from E. W. Badger, 17 July 1880 and n. 4).

From James Geikie 22 July 1880

Perth
22^d July 1880

My dear Sir

Pray accept my best thanks for your kind permission to use the notes you were so good as to send me.¹ They will need no “touching-up”—being perfectly clear and terse. It was extremely kind of you to reply to my letter. I am only sorry to have put you to that trouble. But I’m very glad to have the additional interesting information.² I am grateful and pleased more than I can say that you should have considered it worth your while to write to me. Nor would you wonder at this if you knew how great is the love and veneration with which I have looked up to you for many years. Pray excuse me saying so much.

With highest regards | Yours sincerely | James Geikie

DAR 165: 31

¹ See letter from James Geikie, 15 July 1880, and letter to James Geikie, 19 July 1880.

² In his letter to Geikie of 19 July 1880, CD had added further observations on differential movement in drifts. In *Prehistoric Europe: a geological sketch*, Geikie quoted from this letter as well as from CD’s earlier observations made in the letter to James Geikie, 16 November 1876 (*Correspondence* vol. 24; see also Geikie 1881, pp. 141–2).

From G. H. Darwin 27 July 1880

Trin: Coll: | Camb:
July 27. 80

My dear Father,

I am in some perplexity to know what I ought to do about the enclosed letter from Wrigley.¹ It would be exceedingly disagreeable to refuse my name. I really know absolutely nothing about the place for the last 10 or 12 years & what I do remember is that the discipline was far from good & that Wrigley was painstaking but far from brilliant as a teacher. He occasionally had good men as under masters more especially for the military boys²

What on Earth is the meaning of my being a referee. It would hardly be fair to Wrigley to admit my name & then to give a very luke-warm praise of him & how can I refuse.

I have just had a letter from Lady Thomson asking me to join them at Cowes at the end of the week. If I can only get a little better (& I do seem better today) I shall

certainly go. I feel it is very lazy to do so & that I ought to stop and look after the pendulum, as I've not done anything to speak of for so long³

I found poor Horace quite ill with his toothache yesterday but getting better in consequence of having his face lanced.— We had another thunderstorm & heavy rain again last night. I suppose you are quite alone now.⁴

Your affectionate son | G. H. Darwin

I think Wrigley might be fairly described as good for men not intending to read high mathematics

Pure trypsin (not thrypsin) not procurable.

Kühne probably the only man who ever had it & it w^d cost about £3 a gramme.⁵

Will send you tomorrow some stuff which is almost all trypsin out of wh. Lea makes pancreatic ferment.⁶

Horace better but still in bed with bad sore throat

DAR 64.2: 94; DAR 210.2: 85

- ¹ The enclosure has not been found; George had apparently been asked by Alfred Wrigley to write a reference for him.
- ² Wrigley was the headmaster of Clapham Grammar School and taught mathematics; the school had a special department for pupils preparing for admission to military colleges and the Indian Civil Service (*The Times*, 16 September 1867, p. 4). CD's four younger sons, including George, had been pupils at the school.
- ³ Frances Anna Thomson was the wife of William Thomson. Cowes is a seaport town on the Isle of Wight. George apparently did join the Thomsons for some time on their yacht, *Lalla Rookh*, on which they spent most summers (S. P. Thompson 1910, 2: 760). George and Horace Darwin were attempting to construct a pendulum to measure the lunar disturbance of gravity; the idea for the investigation had come from Thomson (see *Nature*, 3 November 1881, pp. 20–1, for a description of the construction of the bifilar pendulum; see also Longair 2016, p. 93).
- ⁴ Horace Darwin also lived in Cambridge. Francis Darwin was in Wales (letter from Francis Darwin, [1 August 1880]) and Elizabeth Darwin had gone to Warwick; however, Henrietta Emma and Richard Buckley Litchfield visited from 26 to 29 July 1880 (Emma Darwin's diary (DAR 242)).
- ⁵ The section of the letter from 'Pure trypsin' to the end was written on the back of the envelope. In 1876, Wilhelm Friedrich Kühne was the first to isolate and name the enzyme trypsin (Kühne 1876).
- ⁶ Arthur Sheridan Lea had gone to Heidelberg to study with Kühne around the time of Kühne's discovery (Geison 1978, p. 183).

From Ernst Krause¹ 27 July 1880

Berlin N.O. Friedenstrasse 10. II.
den 27.7.80.

Hochverehrter Herr!

Die deutschen Revuen sind von einer solchen Langsamkeit in der Besprechung literarischer Erscheinungen, dass mir noch nichts von Belang zu Gesicht gekommen ist, was über "Erasmus Darwin" gesagt worden wäre.² In verschiedenen Tagesblättern sind einige im Allgemeinen wohlmeinende, aber so kurze Besprechungen erschienen, dass es nicht verlohnte, sie Ihnen zu übersenden.

Dagegen ist mir eine zweite auf Grund Ihrer Lebensskizze gearbeitete Darstellung in diesen Tagen zugekommen, die ich Ihnen gleichzeitig mit diesem Briefe

unter Kreuzband sende.³ Sie ist darum interessant, weil sie von einem Ihrer vielen theologischen Gegner in Deutschland herrührt, und es ist erfreulich zu sehen, dass der Mann sich jetzt soviel anständiger und rücksichtsvoller gegen Sie benimmt, als er es früher in zahlreichen Streitschriften der theologischen Journale und in dem gelehrten Buche gethan hat, welches ich in meiner Studie über Erasmus Darwin citirt habe.⁴

Ferner ist mir eine Besprechung von Ferrari, in der *Revue Scientifique* zugekommen, die ich Ihnen sende, weil Sie dieselbe vielleicht nicht zu Gesicht bekommen haben.⁵

Mit dem herzlichsten Wunsche, dass diese Zeilen Sie im erwünschtesten Wohlsein erreichen, zeichne ich, hochverehrter Herr | Ihr dankbarlich ergebenster | Ernst Krause

DAR 92: B59

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

² The German version of *Erasmus Darwin* was published in April 1880 (Krause 1880; see letter from Ernst Krause, 19 April 1880).

³ Otto Zöckler's lecture, 'Darwin's Grosvater als Arzt, Dichter und Naturphilosoph' (Darwin's grandfather as physician, poet and natural philosopher; Zöckler 1880), drew heavily on *Erasmus Darwin* as well as on other sources (see *ibid.*, pp. 155–8). Two offprints of Zöckler 1880 are in DAR 133.5: 4 and DAR 210.11: 43.

⁴ Krause cited Zöckler's *Geschichte der Beziehungen zwischen Theologie und Naturwissenschaft* (History of the relations between theology and natural science; Zöckler 1877–9) in *Erasmus Darwin*, p. 151. For more on Zöckler's critique of Darwinism from the standpoint of theology, see Gregory 1991.

⁵ Two copies of *Revue scientifique de la France et de l'étranger*, 15 May 1880, containing Henri Ferrari's review of *Erasmus Darwin* (Ferrari 1880), are in DAR 133.5: 3 and DAR 210.11: 42.

From W. S. Dallas 28 July 1880

Geological Society, | Burlington House, W
28 July 1880

My dear M^r: Darwin

I communicated your letter to my friend M^r: Wise, & he was very much pleased to find that you so kindly agreed to accept the dedication of his poetical effusion.—¹ I have not seen the poem, but he called upon me on Sunday with some of M^r: Walter Crane's drawings which are exceedingly beautiful.— The drawings, which are in outline, will be reproduced by one of the Photographic printing processes, & the book, when completed, will certainly be what the French call an *ouvrage de luxe* & one of very high artistic character—²

Of the poem itself, I have, as already stated, seen nothing, unless you reckon the four opening lines as sufficient to found an opinion upon.— From the nature of the illustrations & from what M^r: Wise told me, its construction is that of a Fairy Masque, but in what way the theory of evolution is to be illustrated by it, not being myself a poet, I am unable to conceive.—

Author & artist have some little difficulty just now in coming to a business arrangement with a publisher, but I believe as soon as they have got matters in train,

M: Wise will write to you, & I suppose, submit some portion of the work to your inspection.— I feel quite sure that you will be pleased with it so far as the artistic part is concerned, & from my knowledge of the author I hope the poem may satisfy you equally well.—

Believe me | Yours very truly | W. S. Dallas.

DAR 162: 31

¹ CD's letter has not been found, but see the letter from W. S. Dallas, 8 July 1880 and n. 3, for the dedication in John Richard de Capel Wise's *The first of May* ([Wise] 1881).

² Each page of [Wise] 1881 has illustrations by Walter Crane.

To Francis Darwin 28 July [1880]¹

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
July 28th

My dear Frank

Many thanks for all your very useful criticisms, all of which adopted except one & has been partly adopted.— It is a good job for you, but an accursed one for me that the Printers have quite ceased sending me slips.² I shall get my Worm little book done first, if they do not look sharp.—³

Hooker was very much interested about the pulvinus of *Desmodium* & about the young plants not bearing lateral leaflets. & about their movement at a lower temp. on seedlings.⁴ By the way the big envelopes which you use are made of such atrociously bad paper, that *everyone* has arrived more or less burst— one came in fragments, patched up with sealing wax by Post office at Stafford I think.⁵ Anyone c^d pick out anything from within & you had better not use them.—

I send paper by Stahl; he has sent me a copy; if you write pray thank him for me & you may say, *if you like*, that I fully appreciate the interest of his observations.⁶

Good Bye— I am tired with writing.—

My kindest remembrances to all your party— you were quite right to send Bernard⁷ home in my opinion.— | Yours affect. | C. Darwin

DAR 211: 65

¹ The year is established by the reference to proof-sheets and worms (see nn. 2 and 3, below).

² Francis was assisting with CD's corrections of proof-sheets for *Movement in plants*. William Clowes & Sons were printers to John Murray, CD's publisher.

³ CD mentioned that he was 'putting together some notes on the action of worms' in his letter to A. C. Ramsay, 17 June 1880. He recorded in his journal for 1880 (Appendix II) that he 'Began in Autumn on Worms'. *Movement in plants* was published on 6 November 1880 (Freeman 1977).

⁴ Joseph Dalton Hooker probably saw proof-sheets of *Movement in plants* when he visited Down from 24 to 26 July 1880 (Emma Darwin's diary (DAR 242)). CD discussed the length of the pulvinus (a swelling at the base of the petiole that acts like a joint) relative to the size of the leaf blade in lateral leaflets of *Desmodium* as the proximate cause of rapid circumnutating movement (*Movement in plants*, pp. 364–5). CD had also noted that a sudden fall in temperature caused the terminal leaflet to sink downwards (*ibid.*, pp. 359–60).

⁵ Francis was in Wales (letter from Francis Darwin, [1 August 1880]).

⁶ Ernst Stahl sent an offprint of his paper 'Ueber den Einfluss von Richtung und Stärke der Beleuchtung

auf einige Bewegungserscheinungen im Pflanzenreiche' (On the influence of the direction and intensity of illumination on some of the phenomena of movement in the plant world; Stahl 1880a), which had appeared in several parts in *Botanische Zeitung* between 30 April and 11 June 1880. CD cited Stahl 1880a in *Movement in plants*, p. 446.

⁷ Bernard Darwin was Francis's son.

To G. H. Darwin 28 July [1880]¹

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
July 28th

My dear George

It is rather perplexing, but the probability is that no one will apply to you.— I think you cannot refuse & you might in writing to Wrigley express your sense of his kindness & he certainly brought you on well.—² If any one sh^d. apply to you as referee, you might say that you had always heard that W. had been very successful with military pupils.— If any question were asked about discipline of school, you might justly refuse to answer on grounds that you had left it some 15 years ago.—

You c^d. speak of his personal kindness to you, & that you had profited by being at Clapham.— In fact your answer w^d. deceive no one because it w^d. give no information.—

Very many thanks about trypsin. (N.B. I have just looked, & the Frenchman spells it thrypsine)³

I beg you to thank M^r Lea what he proposes to send will do perfectly.—⁴

Your affect. Father | C. Darwin

We are very sorry to hear about Horace.—⁵

I hope that you will join the Thomsons⁶

DAR 210.1: 95

¹ The year is established by the relationship between this letter and the letter from G. H. Darwin, 27 July 1880.

² See letter from G. H. Darwin, 27 July 1880 and nn. 1 and 2. Alfred Wrigley was the headmaster of Clapham Grammar School.

³ See letter from G. H. Darwin, 27 July 1880 and n. 5. The writer who used the French spelling 'thrypsine' has not been identified. The word 'trypsin' had been coined by Wilhelm Friedrich Kühne (see Kühne 1876, p. 195).

⁴ See letter from G. H. Darwin, 27 July 1880 and n. 6. Arthur Sheridan Lea had promised to send a compound with a high percentage of trypsin in it.

⁵ Horace Darwin was ill with toothache and a sore throat (letter from G. H. Darwin, 27 July 1880).

⁶ George had been invited to join Frances Anna and William Thomson on their yacht at Cowes (letter from G. H. Darwin, 27 July 1880 and n. 3).

From O. C. Marsh 28 July 1880

Yale College | New Haven Ct.
July 28th 1880.

Dear Mr. Darwin,

I send you today a copy of my Memoir on the *Odontornithes*, the plates of which I had the pleasure of showing you two years ago.¹ I trust you may find in the volumes

some new facts in confirmation of your own work, which has changed the whole course of scientific investigation.

I remain | with high respect, | very truly yours, | O. C. Marsh.

Charles Darwin Esq F.R.S. &c.

DAR 202: 111

- ¹ Marsh had visited CD on 9 July 1878 (*Correspondence* vol. 26, letter from W. E. Darwin, 10 July [1878]). CD's copy of *Odontornithes: a monograph on the extinct toothed birds of North America* (Marsh 1880) has not been found.

From Asa Gray 29 July [1880]

July 29—

Thanks for your postal¹

Ipomœa jalapa (Conv. I. macrorhizus. Ell.) of S. Carolina, with a large turnip-like root, sometimes 40–50 lbs, makes only moderately long petioles cotyledons & lengthens the caulicle considerably— — has not caught the trick, but has some idea of it.²

A. Gray

ApcS

Postmark: JUL 80

DAR 186: 53

- ¹ CD's postcard has not been found, but see the letter from Asa Gray, 3 July 1880. Gray had written about germination in two species of *Ipomœa*.

- ² *Ipomœa jalapa* is a synonym of *I. purga* (jalap), a species native to parts of Mexico. *Convolvulus macrorhizus* is another synonym of *Ipomœa purga*, but *Ipomœa macrorhiza* is largeroot morning-glory, a species native to South Carolina; Gray may have accidentally conflated the names, but evidently intended *I. macrorhiza*. In Gray's usage, the caulicle was the initial stem in an embryo (A. Gray 1879, p. 401). The 'trick' refers to the method of germination, which, in the case of some other species of *Ipomœa*, involved the caulicle remaining shut while the petioles lengthened and brought up the cotyledons (letter from Asa Gray, 3 July 1880).

To Ernst Krause 29 July 1880

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

July 29th. 1880

My dear Sir

It was very kind of you to send me the two articles; but it so happens that I had seen both. I did not think much of Zockler's as it appeared to me like a mere robbery of your book. The article in the Revue seemed to me very nicely done.—¹

My dear Sir | Yours sincerely | Ch. Darwin

There have been many interesting articles of late in *Kosmos*; & this reminds me to say, what I had intended before to say, that I ought to pay for this Journal. It is not fair to the Publisher that I sh^d go on receiving them gratuitously.—² Will you give him my *cordial* & *sincere* thanks for his kindness, & ask him to send me a memorandum of cost for the *current* & *next* year, & I will send cheque for both.—

C.D

P.S. I have not yet heard from M^r Murray whether there is any profit from your Book, but ‘vehementer dubito’.³

The Huntington Library (HM 36206)

¹ See letter from Ernst Krause, 27 July 1880 and nn. 3–5. Otto Zöckler drew heavily on *Erasmus Darwin* for his account of CD’s grandfather in Zöckler 1880. For the review in *Revue scientifique de la France et de l’étranger*, see Ferrari 1880.

² The publisher of *Kosmos* was Karl Alberts.

³ CD’s publisher, John Murray, had not yet reported profits on *Erasmus Darwin*. *Vehementer dubito*: I doubt very much (Latin).

To Ernst Krause 30 July 1880

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

July 30th 1880

My dear Sir

I received this morning the enclosed account, which be so kind as to return to me. The profit is lamentably small viz 9[£]15^s11^d, which I herewith transmit by cheque¹ You will see 218 copies are still unsold, & whether they ever will be sold, I cannot conjecture. We shall know next year & then the profit of any sale shall be transmitted to you.—

I have heard nothing from the U. States about the sale of the book by M^{ess}. Appleton.—²

My dear Sir | Yours sincerely | Ch. Darwin

[Enclosure: *see following page*]

The Huntington Library (HM 36207); DAR 210.11: 18

¹ The enclosure was a statement of profits from the sale of *Erasmus Darwin*; this statement, along with statements for CD’s other works published by John Murray, is in DAR 210.11: 18. An entry in CD’s Account books—banking account (Down House MS), dated 30 July 1880, records a payment of £9 15s. 5d. under the heading ‘E. Krause profit of Era. Darwin (I have subtracted amt paid from Murray)’. The part in parentheses (square brackets in ms) refers to the fact that CD had received a single payment from Murray for profits on all his books of £152 12s. 9d. from which he subtracted the profit for *Erasmus Darwin*.

² D. Appleton & Co published *Erasmus Darwin* US ed.

³ Stationers’ Hall distributed copies to the copyright libraries of the United Kingdom (Seville 1999, pp. 233, 262).

De	Life of Dr Erasmus Darwin										Cr	
1879				1879								
Dec	To Printing 1000 No	68	9	10	Dec	By 1000 copies						
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	" Autotype C ^o for	—	—				37	Reviews				
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						2000 Portraits		16	5	—		
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1880												
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[Enclosure: from previous page]

To Francis Darwin [before 1 August 1880]¹

[Down.]

My dear F.

I have just been reading a notice (& must refer to several original German papers) on very ancient furrowed fields in Germany & Scandinavia & Scotland—believed to be prehistoric.² They are on mountain sides & apparently at a considerable elevation.—Did you not tell me of some such old furrowed land in N. Wales. I wish you w^d enquire & have a look at any such place. It is a bad time of year, & too soon for many castings. Could you take a light spade & see if you can find worms? If such places are not distant w^d M^{rs} Atkins³ look in Autumn at them & see if there are castings on the old furrowed land? If there are many, it is a most serious difficulty in the way of my belief of the smoothing power of worms.—I see in my notes I c^d see no castings where there is much Heath growing: Is this true? I have been miserably compelled to take up my worm-notes.

C. D.—

Are there more castings at the bottom of the furrows than on the summits or convex part?

Thanks for today's pleasant acct⁴ | ED

DAR 211: 64

¹ The date is established by the relationship between this letter, the letter to Francis Darwin, 28 July [1880], and the letter from Francis Darwin, [1 August 1880].

² CD refers to Edward Burnett Tylor's presidential address to the Anthropological Institute of Great Britain and Ireland, in which Tylor discussed recent archaeological work on German 'high-fields' and their similarity to Scottish 'elf-furrows' and tilled plots in Swedish forests (Tylor 1880, p. 451). CD cited Tylor 1880 in *Earthworms*, p. 293 n.

³ Mary Elizabeth Atkin.

⁴ This sentence was added at the top of the letter by Emma Darwin. The letter containing the pleasant account has not been found; it may have been addressed to Emma.

From Francis Darwin [1 August 1880]¹

Aberdovey
Sunday

Dear Father

Many thanks for your letter, Natures B. Z. &c. I shall write to Stahl & will give you message² I am glad my corrections have been useful.³ I was very glad to find that you & mother approved of Ubbadub being sent away. The other Miss Pedley is unwell now I expect with measles.⁴

I have been up to the furrowed place on the mountain, M^r Ruck⁵ was told by the farmer that his the farmer's grandfather remembered it being ploughed M^r Ruck thinks it was about 60–80 years ago. I dug two places. one about 48 cm deep one an inch or two less (I havn't a foot rule here) & then I got down to debris of slate rock, I saw no worms How deep ought I to dig? In the autumn they will look on these

old fields for worms. Moles certainly go up on the mountain as high as these places; M^r Ruck doesn't know what they go after as he doesn't believe there are earth worms there. There is something among the roots of the grass for the rooks tear up great patches. If Atty's Regt stops on the Peiwar he will write to the D^r who is fond of such things & ask whether there earthworms there; Atty is almost sure there are; it is 9000 ft high there.⁶

I have had some jolly fishing & caught some decent sized sea-trout: the worst of it is you suffer such agony over those you miss. I lost one about 3 pounds yesterday through trying to land him in a bad place & I might have taken him down the rapid to a gravelly shore & landed him if I had known it was there.

I have found a good many sea shore plants new to me on the sand hills here.

I like Ubbadubs cannon joke: please give him my love & say I will write to him—
| Yrs affec | F. D.

DAR 274.1: 63

- ¹ The date is established by the relationship between this letter and the letter to Francis Darwin, 28 July [1880]. In 1880, the Sunday following 28 July was 1 August.
- ² CD had sent Francis a paper by Ernst Stahl (Stahl 1880; see letter to Francis Darwin, 28 July [1880] and n. 6). CD evidently also sent issues of *Botanische Zeitung* and *Nature*.
- ³ Francis was correcting proof-sheets for *Movement in plants*.
- ⁴ Ubbadub was Francis's son, Bernard Darwin. Mary Eliza and Eve Eleanor Annie Pedley were step-daughters of Francis's brother-in-law Richard Mathews Ruck.
- ⁵ Lawrence Ruck. CD had previously received information on worm activity on steep slopes that had once been ploughed near Pantlludw, Wales (see *Correspondence* vol. 20, letter from Amy Ruck to Horace Darwin, [20 January 1872]). See also letter to Francis Darwin, [before 1 August 1880].
- ⁶ Atty (Arthur Ashley Ruck) was a captain in the Eighth Infantry, King's Regiment (*Hart's army list* 1881). Peiwar Kotal is a mountain pass on the border between Afghanistan and Pakistan; it was seized by the British in November 1878 in the second Anglo-Afghan war (Richards 1990, pp. 79–81). The 'Doctor' has not been identified.

To ? 3 August 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
August 3^d 1880

Dear Sir

I write one line to thank you very much for your second note. I now understand what is to be done, & as soon as my son Francis returns home we will endeavour to make the solution according to your instructions.¹

Believe me Dear Sir | yours faithfully & obliged | Charles Darwin

American Philosophical Society (Mss.B.D25.)

- ¹ The notes have not been found. Francis Darwin was in Wales (see letter from Francis Darwin, [1 August 1880]); he had been doing experiments that involved chemical analysis, and CD had asked Edward Frankland to recommend a trustworthy analyst to assist with the work (see letter to Edward Frankland, 20 April 1880).

To J. M. F. Ludlow 4 August 1880

[Down.]
Aug 4th 1880

Registrar F. S.—
Sir

I have been told that I ought to send to you this year a statement of the assets of the D. F. S. of which I am the Treasurer;¹ & I have been further told that you will recommend an actuary for the purpose, for which advice I sh^d be greatly obliged. The Down F⟨ ⟩ ⟨S⟩ consists of only 51 members, who object much to paying for such an object so that I wish to hear of an Actuary whose charges are not high. I have further been instructed by the Soc. to consult an Actuary, whether our Funds w^d allow an increase in rate of payment during sickness.

Hoping that you will oblige me with an answer | I remain Sir | Your obed. ser | Ch. Darwin | (Treasurer to Down F. Soc.)

ADraftS
DAR 202: 53

¹ CD was treasurer of the Down Friendly Society (or Club) from its creation in 1850 until his death in April 1882.

To Francis Darwin 5 August [1880]¹

Down—
Aug 5.—

My dear F.—

Your criticism all excellent.—²

I had remembered Lotus & sent correction to Printers.³ Further on I have given the reference to Sachs & his comparison of movements of plants to those of animals.—⁴

I have added reference to Stahl; but please remember to tell me, where later numbers of Bot. Zeitung are kept, that I may add page to Stahl.⁵ I hunted yesterday & could not find any but the old bound volumes— I despatched Ch. IX to you this morning & am now at Ch. X— There are only 12 Chapters— I am very tired so will write no more—

Thank God we can see daylight through the work— I am so sorry that your holidays sh^d be spoiled by this accursed work.—

C. D.—

Averrhoa cut all right, but leaflets sometimes sink lower & I have added this.—⁶

DAR 211: 66

¹ The year is established by the reference to corrections to *Movement in plants*.

² Francis was correcting proof-sheets of *Movement in plants*.

³ *Lotus* is the genus of trefoil or deervetch; several species are discussed in *Movement in plants*. The printers were William Clowes & Sons.

- ⁴ CD quoted remarks by Julius Sachs on the resemblance between plant and animal movements in *Movement in plants*, pp. 571–2 n.
- ⁵ CD had received an offprint of an article by Ernst Stahl from *Botanische Zeitung* (Stahl 1880; see letter from Francis Darwin, [1 August 1880] and n. 2). The article is cited in *Movement in plants*, p. 446.
- ⁶ A woodcut and diagram of *Averrhoa bilimbi* (bilimbi or cucumber tree), showing the angular movement of leaves going to sleep, appeared in *Movement in plants*, pp. 330–1; CD explained in the text that part of the falling movement (from eighty-five to seventy-five degrees) had not been included in order to fit the diagram on to the page (*ibid.*, p. 332).

From S. T. Preston 5 August 1880

25 Reedworth Street | Kennington Road SE. | London.

Aug. 5th. 1880

Dear Sir

I hesitated to send you the accompanying copy of a paper on *Natural Science & Morality* before, being doubtful from a letter received from you on May 22nd whether you would be disposed at present to enter upon the subject.¹ But it has occurred to me that the perusal of the paper might possibly not be without interest (waiving perhaps any discussion).

You will at least I am sure kindly notice the few facts I have to mention regarding it.

The subject was entered upon some three years back, and some of the main groundwork of the present paper was published in cooperation with a friend (for private circulation) as part of a pamphlet dated June 1879.² This, curiously enough is the same date as that on the preface of Mr Herbert Spencer's "Data of Ethics".³ Although the subject is elaborated much further in relation to Evolution in the "Data of Ethics", I find that the present paper contains some conclusions in accordance with those in Mr Spencer's work—so as to look almost like a popular exposition of parts of it. I am rather confirmed in this view, inasmuch as a paper by a Canadian Le Sueur, "*A Vindication of Scientific Ethics*" published in New York, in the "Popular Science Monthly",⁴ and which is actually a popular exposition of Mr Spencer's views—resembles still more closely the paper sent herewith (and was published on the same day; July 1st).

One cause for my not reading the "Data of Ethics" before, was perhaps the fact of having published part of the present paper in the private pamphlet mentioned. I am only gratified to find that some of the conclusions are analogous—though the accompanying paper contains but a mere sketch of the relation of the subject to Evolution. I think however that the section on *Responsibility and Physical Causation* (which I developed myself in 1877) may perhaps contain some novelty—the incentive to this having been Prof Tyndall's well known address at the Midland Inst. of Birmingham of that year on the qu(es)ti(on) of the Responsibility of criminals in relation to natural causation (re-published, *Fortnightly Review* Nov. 1877).⁵

I am inclined to the opinion that the paper as a whole might be capable of contributing something towards popularising the new Natural System of Ethics.⁶ But this being the first Essay in this line, in which I had any part, I have naturally

encountered difficulties in the way of introduction, and I fear the paper is practically sunk in its present locality, on account of the extremely limited circulation and repute of the periodical where it appears.

Do you think perhaps Mr Spencer would find any interest in the paper; or I should be very grateful for any assistance or word of advice you might like to give me. I cannot but believe that the subject is one which it would do good to follow up, and I think I could do much work with a little encouragement—but find it hard to progress with a dead wall (as it were) of apathy or indifference, even with the best intentions. The subject is one of so special a kind that I have ventured to send you the paper and mention the facts—and letters received from you before, and from the late Prof. Clerk Maxwell & Sir W Thomson (in relation to physical questions)⁷ induce me to hope that my work may not be entirely undeserving of encouragement. At the same time I should not like it to be overlooked that in the present case a friend (who wishes to remain anonymous for the present) had a large—perhaps greater—share in the development of the main branch of the subject. It is extremely difficult at first in a matter of this kind to obtain even that degree of attention which is necessary to form a judgment on any Essay.

Trusting I shall not be thought to be trespassing too far on your kindness. | Yours truly | S Tolver Preston

Charles Darwin Esq^r FRS &c.

P.S. I should mention perhaps that the private pamphlet on “Physics & Ethics”, which included a small part of the present paper (but the main subject of which was principally the work of my friend), was rejected by “The Fortnightly Review”.

DAR 174: 62

CD ANNOTATIONS

6.1 or I should ... to give me. 6.3] *double scored red crayon*

6.6 The subject ... of encouragement. 6.9] *scored red crayon*

¹ See letter to S. T. Preston, 22 May 1880 and n. 3. Preston's paper was published in the *Journal of Science* for July 1880 (Preston 1880a). It has not been found in the Darwin Archive–CUL.

² The pamphlet was *Physics and ethics* ([Seaton] 1879); Preston's friend was William Sharpey Seaton.

³ CD had referred Preston to Herbert Spencer's *The data of ethics* (Spencer 1879; see letter to S. T. Preston, 22 May 1880 and n. 4).

⁴ William Dawson LeSueur and LeSueur 1880.

⁵ John Tyndall's address ‘Science and man’ was delivered at the Birmingham and Midland Institute on 1 October 1877 (Tyndall 1877). For ‘Responsibility and physical causation’, see Preston 1880a, pp. 457–60.

⁶ Preston argued that ethical behaviour was based on rationally calculated self-interest, rather than on instinctive sympathy for others, as CD had argued in *Descent* 1: 70–104; Preston distinguished self-interest from mere selfishness, and claimed that personal happiness was dependent on the goodwill, friendship, and sociability of others. He was strongly critical of ‘clericalism’, which based morality on the fear of eternal punishment (see Preston 1880a, pp. 447–9, 453–5).

⁷ James Clerk Maxwell and William Thomson. For correspondence between Preston and Maxwell, see Harmon ed. 1990–2002, 3: 551–2.

To R. P. Hardy 6 August [1880]¹

[Down.]
Aug 6th

To Rolfe Price Hardy Esq | Actuary
Sir

The members of the D.F.C. have asked me, as the Treasurer, to find out whether the belief is well grounded that the funds w^d allow of an increase of their benefit ²

The Club was registered in according to the [schedule] of registration by Mr [illeg]—³ There have been 3 distributions of surplus cash by permission of an Actuary.

Each member received 10^s per week for sickness during first 6 months & afterwards 5^s— Burial fee 5 \mathcal{L} .

They wish to receive 12^s weekly during sickness for the first 26 week & afterwards 6^s instead of as at present 10^s & 5^s; Also to receive 10 \mathcal{L} for burial fee instead of 5 \mathcal{L} , with the power of drawing half the 10 \mathcal{L} (i.e. 5 \mathcal{L}) in the case of the death of a wife. I enclose the balance-sheet for the last year,⁴ (with some appended facts) which I hope will give you sufficient information to judge by; but if not any other information will be supplied by me.— As the T & member of this small Club (51 members) may I request that before considering your answer you will have the kindness to inform me what your fee will be for officially informing me whether any increment to the benefits can be safely granted.

I beg leave to remain, Sir | your obliged & obedient serv | Charles Darwin
Return accounts

ADraftS
DAR 202: 62

¹ The year is established by the relationship between this letter and the letter to J. M. F. Ludlow, 4 August 1880.

² CD had been advised to consult an actuary on the matter of increasing sickness benefits for members of the Down Friendly Club (see letter to J. M. F. Ludlow, 4 August 1880).

³ The Friendly Club was established in 1850 (see *Correspondence* vol. 4, letter to J. S. Henslow, 17 January [1850] and n. 6).

⁴ The enclosure has not been found.

From Adolf Ernst 7 August 1880

Caracas
August 7th 1880

Dear Sir,

Allow me to thank you most sincerely for your “Geological Observations”, which I have studied with great interest.¹

In “Nature” you will have noticed an article of mine on the fecundation of *Cobaea penduliflora*, which I hope is not void of interest.² I have to add two observations.

In none of the many fruits of my plant, both the ovules were developed in all ovary-cells; whilst on plants growing in the forest I found many with fully developed six seeds. Should the former be a consequence of geitogamy?³ I know of no plant of *Cobaea* cultivated in Carácas; the nearest specimens are those of a country-garden, called El Paraiso, which is about 1600 meters from my house, as the crow flies; so that a cross-fecundation between specimens at both places appears next to impossible.— A very luxuriant specimen of *Cobaea* which I found lately in the forest, had almost overgrown a species of *Byrsonima*, so that the conspicuous yellow inflorescences of the tree appeared to belong to the climber, which was in flower too. May be the moths are attracted by the bright flowers of the *Byrsonima*, so that the tree would be a little more than the mere hold of the climber.⁴

I inclose some seeds of the *Cobaea*, as I think you might possibly feel interest in growing the plant.

I inclose likewise a poor sketch of the flower of one of our few *Gentianeae*: *Lisianthus vasculosus* Griseb.—⁵ This half shrub grows in our higher mountains, from 5000 feet upwards. The drawing is natural size, but as I am unfortunately a very bad drawer, or none at all, I fear the sketch will scarcely give a good idea of the flower and its interesting structure.

The flowers grow in short cymes at the end of the branches, are rather fleshy, and have short and strong peduncles. They have generally a nearly upright position. The most peculiar part in their interior is a large vessel inclosing the ovary, and I think Grisebach gave the plant on this account the specific name *vasculosus*, though he does not even mention this structure in his description *Linnaea* XXII, 37, 38.⁶ This vessel is formed by the inferior part of the corollatube, whilst the brim is formed by a supra-staminal annular appendix of the same. It is about 15–16 millim. deep, at the mouth it is 5–6mm. wide, but at the bottom it is a little wider. The ovary is just in the middle of it, leaving a clear space of 2mm. on every side. This vessel is full of nectar. It is always open, the mouth shows no hairs, nor is there any other contrivance by which the nectar might be protected against unbidden guests. There is no fear of a natural outflow, as the flowers never have a nodding position.

The large funnel-shaped corolla is of a straw-yellow colour, with some very faint red stripes in the interior. The style rises in a somewhat curved line, and the *unripe* stigma touches closely the upper corolla-lobes, which are a little smaller than the lower ones. The former are at the same time reflexed at their apices, whilst the under ones remain straight, affording, as it were, a most commodious landing to any insect.

The stamens rise from the annular ditch between the corolla and the brim of the nectary, are curved, and bear also curved anthers, the convex side of which (where dehiscence takes place) turns towards the lower corolla lobes; but, as you will see from the sketch, a great part of the mouth is left free.

The flowers are most decidedly proterandrous, for the anthers burst long before the stigma opens; but flowers of all degrees of development are to be met with at

the same time. An insect entering the corolla in the direction of the arrow will get at the nectar, but will also brush off a quantity of pollen and carry it away on its back.⁷

As soon as the anthers have done their work, the stamina withdraw close to the superior lobes, whilst the style with the opened stigma comes a little down, so that the latter lays now in the place where formerly the anthers were, as I try to show in figure 3. It is evident that an insect visiting a flower in this state, must leave some pollen on the stigma, which it cannot avoid to touch with its back.

About a fortnight ago I had the opportunity to witness the visit of these flowers by a large moth (*Chaerocampa trilineata* Walk), and I saw very distinctly that everything went on in the manner described. I secured the animal, and found its back indeed almost covered with pollen, which under the microscope proved to be that of *Lisianthus*. I likewise convinced myself that two of the flowers visited by the moth, had really more or less pollen on their stigmas, these two being the only ones which were in condition to be fecundated.—

The whole process has certainly nothing particular; but it appeared to me worth while to describe it at some length, as it is so extremely simple. Is there anything like in other species of *Lisianthus*? Müller says nothing in his “Befruchtung der Blumen”,⁸ nor do I find anything referring to it in other books which I am able to consult. Some time ago I sent seed of *Lisianthus vasculosus* to Kew;⁹ I hope they will grow; the plant is rather showy and deserves a good plate.

I forgot to say that the flowers have no smell, however they are very conspicuous on account of their yellowish-white colour, which contrasts with the dark green foliage. The nectar is very sweet. It exists already in the flowers at the time of their opening, but not before, nor did I find any in flowers which exhibited some signs of having done their task; so that it certainly only serves indirectly the plant. I think thousands of cases of this character may be found against Bonnier’s views.—¹⁰

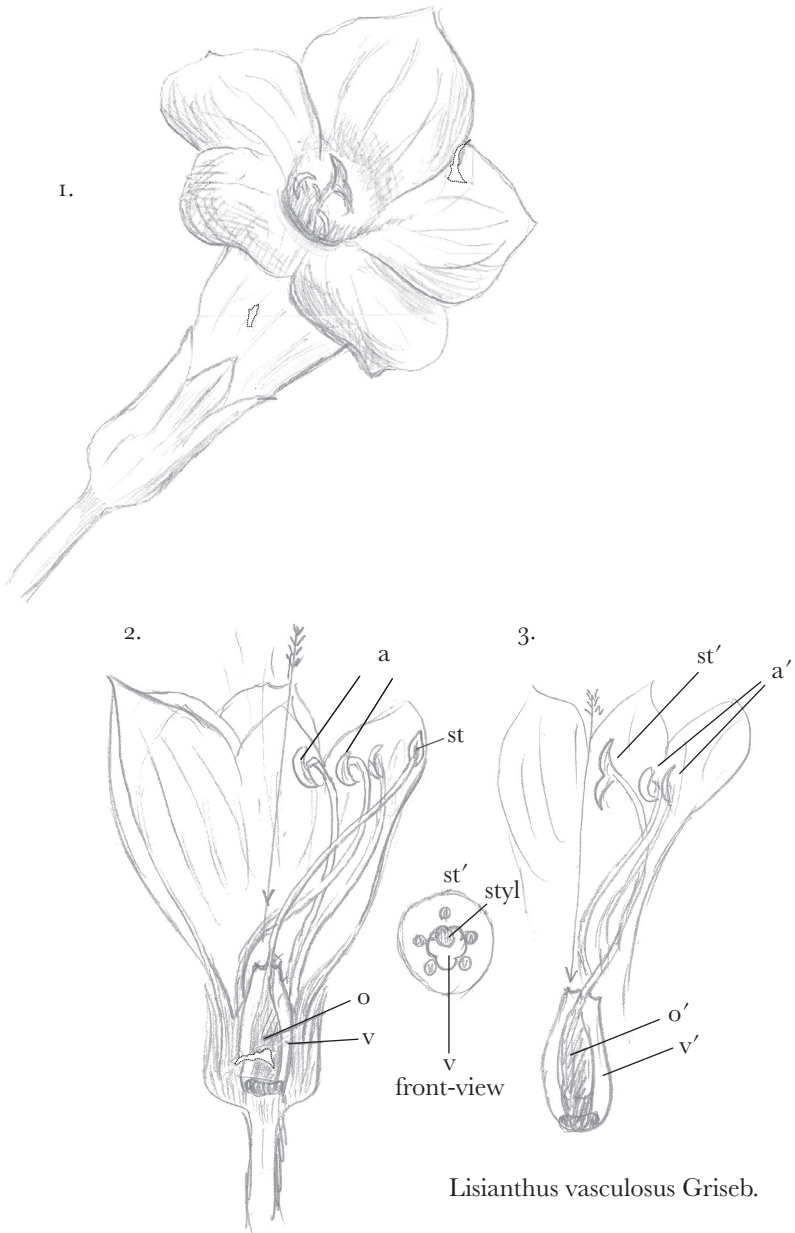
A friend of mine has in his garden a *Physianthus* (Arauja), which I believe to be the species *albens*, though there is no red at all on the pure white corolla.¹¹ I recommended careful observations with respect to insects caught by this plant, reading to him the article in a late number of the *American Naturalist*.—Yesterday he brought me one flower, in which a honey-bee had been caught exactly in the manner as it has been described several times. The observations are continued with much care, as I should like to see whether there be any thing like the reported carnivorous habit of bees.¹²

I hope you will pardon my keeping up so much of your time with my long letters, especially as my observations may be of however small interest to you.

Allow me to continue my communications from time to time, and believe me one of your sincerest admirers and enthusiastic followers.

A Ernst

[Enclosure]¹³



Lisianthus vasculosus Griseb.

DAR 163: 22

- ¹ CD had sent a copy of *Geological observations* 2d ed. (see letter to Adolf Ernst, 4 April 1880 and n. 2).
- ² Ernst's paper 'On the fertilisation of *Cobaea penduliflora*' was published in *Nature*, 17 June 1880 (Ernst 1880). *Cobaea* is a genus of vines and lianas native to montane and upland forests from Mexico to Venezuela; Ernst had concluded that self-fertilisation in *C. penduliflora* was not possible.
- ³ Ernst intended 'geitonogamy', a term coined by Anton Kerner in 1876 to denote pollination of a flower by pollen from a different flower on the same plant (Kerner 1876, p. 192).
- ⁴ *Byrsonima* (locustberries) is a genus in the family Malpighiaceae, and is native to tropical America. Ernst had noted that *Cobaea penduliflora* flowers were dull green and that the plant was visited by several large moths (Ernst 1880, p. 149).
- ⁵ *Lisianthus vasculosus* is a synonym of *Symbolanthus vasculosus*, a species of ring-gentians.
- ⁶ August Heinrich Rudolf Grisebach and Grisebach 1849, pp. 37–8.
- ⁷ See diagrams 2 and 3.
- ⁸ Hermann Müller and H. Müller 1873.
- ⁹ The Royal Botanic Gardens, Kew.
- ¹⁰ CD and Ernst were both critical of Gaston Bonnier's work on nectaries (see letter to Adolf Ernst, 4 April 1880 and n. 6, and Ernst 1880, p. 149).
- ¹¹ *Physianthus albens* is a synonym of *Araujia sericifera*.
- ¹² See the note 'Moths entrapped by an Asclepiad plant (*Physianthus*) and killed by honey bees' in *American Naturalist* 14 (1880): 48–50. CD had written to the editor of the journal, querying whether the bees could have tried to suck nectar from the bodies of the moths (see *Correspondence* vol. 27, letter to A. S. Packard, 23 November 1879 and n. 2).
- ¹³ The diagrams are reproduced here at 75 per cent of their original size.

From S. T. Preston 8 August 1880

25 Reedworth Street | Kennington Road SE | London.

Aug 8th 1880

Dear Sir

I thank you for your frank and open expression of views in your last letter.¹ I can appreciate that there may be an appearance to justify a prejudice as to writing on several subjects in my case. But I think it will be—so far—admitted that as science progresses, it becomes all the more difficult to separate into distinct branches—that Ethics (for instance) cannot in the present day be treated apart from Physics: and minds are so differently constituted that each must (I think) try to do his *best* & hope to be judged by results, irrespective of connecting circumstances. But I venture to think (if I may be allowed the opinion) that a spirit of exclusiveness may tend sometimes to be carried too far. Thus (as an example), the modern Vortex-Atom theory might be thought at first sight an exclusively mathematical subject. But it has its *physical* side.² Mental capacities (as is known) tend to develop in grooves. A man therefore (possibly) might be competent to do something on the *physical* side of this theory, *because* his capacity did not run high in the mathematical direction. Yet I can well imagine that a non-mathematician who attempted to suggest anything in regard to this theory (however carefully he might have thought it out) would run the risk of being considered *à priori* presumptuous. Yet I think he should not withhold what he has to suggest on that ground. The essential fact of the borders of all subjects encroaching on each other, gives (as it seems to me) an occasional chance for an outsider to step in and say a word with advantage—and may it not

be beneficial sometimes for minds of extremely diverse capacities all to be brought to bear on one and the same subject: their sum being then equivalent to (the rare instance of) one mind developed in a large number of directions at once.

As regards experimental work, I quite appreciate what you say as to the value of this, & have already (under drawbacks) given some time to it. From the exceptional nature of my career and the great disadvantages I have been under through the effects of religious dogma (which I can well imagine it may be difficult to realize), I do not expect to achieve much; but what satisfaction I have will be that of doing original work to the best of my capacity. I hope you will excuse these particulars and not think that I wish to draw you into a correspondence on personal matters which can be of very little interest to you.

Your's truly | S Tolver Preston

P.S. I do not know if you may have seen (through Prof Tyndall perhaps) the pamphlet *Physics & Ethics*.³ If so, I should merely like to say that the strong *materialistic* stand-point taken up there is more apparent than real. But, as a fact, I was not so much concerned in the preparation, excepting in the *Ethical* portion of it. Those who are unaware of the time and thought expended by my friend⁴ on this pamphlet, might perhaps think the critical tone adopted in places somewhat bold.

Charles Darwin Esqr FRS &c.

DAR 174: 63

¹ CD's letter has not been found. Preston had sent CD a copy of his paper 'Natural science and morality' (Preston 1880a; see letter from S. T. Preston, 5 August 1880 and n. 6).

² The theory that atoms were vortices in a perfect fluid or ether was first developed by William Thomson (see W. Thomson 1867 and W. Thomson 1880); it stimulated research on fluid dynamics as well as abstract mathematical models of matter and motion (see Preston 1880b and Kragh 2002).

³ John Tyndall. The pamphlet was [Seaton] 1879).

⁴ Preston's friend was William Sharpey Seaton.

To Eduard Strasburger 8 August 1880

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

August 8th 1880

Dear Sir

I thank you cordially for your extreme kindness in having sent me your grand work on the formation & division of cells.¹ I never saw anything so beautiful as the Plates, & with what wonderful profusion your work is illustrated!—

With much respect, I remain, Dear Sir | Yours faithfully & obliged | Charles Darwin

Universitäts- und Landesbibliothek Bonn, Handschriftenabteilung (NL Strasburger I)

¹ Strasburger sent the third edition of *Über Zellbildung und Zelltheilung* (On cell formation and cell division; Strasburger 1880); he had sent the second edition and a French translation of the first edition in 1876

(Strasburger 1876a and Strasburger 1876b; see *Correspondence* vol. 24, letter to Eduard Strasburger, 9 December [1876] and n. 2). CD's copies of the books are in the Darwin Library–Down.

From W. C. Williamson 10 August 1880

Fallowfield | Manchester
Aug 10/80

My Dear Sir

Since I last wrote to you I have been pursuing my observations on the growth of the *Droseræ*.¹

A plant of *D. Capensis* flowered last summer and I sowed the seeds in the autumn as soon as they were gathered. As usual all came up in the form of minute examples of the *D. rotundifolia* like those of *D. spatulata* which I sent you on a previous occasion.² They were densely crowded together in two seed pans. Towards the end of April last I pricked out in separate pots many of those grown in one of the seed-pots,—and these are now about three inches high, and their leaves, as you will see by the enclosed leaf, have all attained the normal form of *D. Capensis*. The plants in the second pot were left in their crowded condition, alive & healthy,—but battling with one another and with the *Sphagnum* amongst which they grew—. You will find an illustration of the results in the enclosed box. Nearly all the plants are still in the state of *D. rotundifolia*. A few (one of which is in the box) lifted their heads above the rest, and have reached the state of the English *D. Inte(rmedia)* but not one in the pot has yet developed into the true form of *D. Capensis*³—yet the plants have all been grown side by side and under exactly similar conditions with the single exception that those in separate pots have had space for their free development and the others have not.

I trust that you are having a favorable summer in the matter of health

I am My Dear Sir | Ever yours | W. C. Williamson

DAR 181: 108

¹ See letter from W. C. Williamson, 13 January 1880.

² *Drosera capensis* is the Cape sundew; *D. spatulata* is the spoon-leaved sundew (*spathulata* is a common misspelling); *D. rotundifolia* is the common or round-leaved sundew. Williamson had noted the similarity of seedlings of *Drosera capensis* and *D. spatulata* to *D. rotundifolia* in his letter of 13 January 1880.

³ *Drosera intermedia* is the spoonleaf sundew. While *Drosera rotundifolia* has leaves arranged in a basal rosette, those of *D. intermedia* are semi-erect and those of *D. capensis* have tall stems.

To Francis Darwin 11 August [1880]¹

Down.
Aug 11th.

My dear F.—

I despatch today another & not so long a Chapt. as the last.—² You need not hurry yourself, for we go early on Saturday to Cambridge & I shall do no slips

there.—³ *Perhaps* I may leave a set of slips on my table ready for you, if you return home before we do.— I sh^d like to hear your plans as soon as you have settled them. What a horrid bore it is that you have sprained your ankle; I fear that it will almost spoil your visit. Bernard is looking as jolly as possible—⁴ what funny things amuse a child— he was in repeated fits of laughter, because I could not remember Jessop⁵ name & said “oh dear what is his name”— Do it again, was said many times & I had to re-act my forgetfulness.—

I have now slips of only 2 last & not very long Chapters to correct, but almost all the revises, which require more work than I had expected. In the last set of slips from you the corrections were mainly erasures of sentences which were all adopted.—

Pray give my very kind remembrances to M^r & M^{rs} M.⁶ Be sure walk from Monk Coniston by lane to the Ewe-land?⁷ road for the sake of view $\frac{1}{4}$ of mile before you get into road.— | C. D.

DAR 211: 67

¹ The year is established by the reference to corrections to CD's visit to Cambridge (see n. 3, below).

² Francis was correcting proof-sheets of *Movement in plants* (see letter to Francis Darwin, 5 August [1880]).

³ CD was in Cambridge visiting Horace and Ida Darwin from 14 to 19 August (CD's 'Journal' (Appendix II)).

⁴ Bernard Darwin. The letter from Francis reporting a sprained ankle has not been found.

⁵ Edward Jessup farmed at Petley's Farm, near Down House.

⁶ CD and Emma had stayed at a hotel on Victor and Victoria Alberta Alexandrina Marshall's Monk Coniston estate in 1879 (see *Correspondence* vol. 27, letter to Victor Marshall, 25 August 1879).

⁷ Yewdale Road.

To R. P. Hardy 11 August [1880]¹

[Down.]

Aug 11th

To R. P. Hardy E^{sq}

Dear Sir—

I am much obliged for your letter of the 10th & for your offer to undertake the valuation.² I sh^d be much glad if you would send me the paper or form about the ages of the individual members, which I will have soon filled up; but there will be a little delay as I shall leave home on the 14th for a week.³

I fear, however, that it may be impossible for you to make a valuation, owing to the scandalous neglect of the members of this small Club.— Ever since its organisation in 1850, I have drawn up balance sheets & sent them annually to the Club, & have repeatedly charged the members to take care of them. Now I find that the box, belonging to the Club, has been left open & *all* the annual Balance sheet since 1867 have been destroyed, except those for 1875, 1877, 1878, & 1879.— I could with much labour pick out of my ledger how much has been paid for sickness & burial fees, during each year subsequently to 1870; & how much was received by the monthly payments; but I have no means of ascertaining what interest was received each

year.⁴ Will you have the kindness to advise me.— I strongly suspect that the fund will not allow of any increase of benefit to the members, but they are convinced that this can be done, & nothing will satisfy them except the assurance of an Actuary, & I am not sure that they have sense enough to be even then convinced.—

Apologising for the length of this note, | I remain dear Sir, Your obliged & obedient servant | Charles Darwin

ADraftS

DAR 202: 63

¹ The year is established by the relationship between this letter and the letter to R. P. Hardy, 6 August [1880].

² Hardy's letter has not been found; see, however, the letter to R. P. Hardy, 6 August [1880].

³ CD was in Cambridge visiting Horace and Ida Darwin from 14 to 19 August, and then in London with his brother, Erasmus, until 21 August (CD's 'Journal' (Appendix II)).

⁴ No accounts for the Down Friendly Club have been found in the Darwin Archive—CUL or at Down House.

To R. P. Hardy [after 11 August 1880]¹

[Down.]

Dear Sir

I hope that the enclosed Tables are properly filled up.— Owing to the loss of the annual accounts as before explained It is impossible for me to fill up the Abstract of the accounts.— You have already the annual Balance sheet for 1879 which give our assets; & I enclose the only other balance-sheets that have been preserved, viz for years 1875, 77 & 78; & be so kind as to preserve & return them to me.²

I beg here to recall to your mind that the members wish to receive 12^s weekly during sickness & 10[£] as burial fee; half to go for wife's funeral if she dies first.

If you grant any such permission, I presume your letter w^d have to be forwarded to the Registrar,³ as the rules of the Club would have therefore to be altered.

Dear Sir | yours faithfully | C. D.

P. S Please see M.S. note in ft of Title Page of Rules⁴

(add up totals From)

ADraftS

DAR 202: 67v

¹ The date is established by the relationship between this letter and the letter to R. P. Hardy, 11 August [1880].

² Hardy was reviewing the accounts of the Down Friendly Club to determine whether sickness benefits could be increased (see letter to J. M. F. Ludlow, 4 August 1880, and letter to R. P. Hardy, 6 August [1880]). The enclosures have not been found; the accounts for the club prior to 1875 had been lost (see letter to R. P. Hardy, 11 August [1880]).

³ The chief registrar of friendly societies was John Malcolm Forbes Ludlow.

⁴ The note has not been found; the Rules of the Down Friendly Society are in the National Archives, FS1/232.

From Hermann Müller 14 August 1880

Lippstadt,
Aug. 14. 1880.

My dear Sir,

My heartiest thanks for your obligingness in having offered to my son to visit you.¹ It will be the greatest honour and pleasure to him to make your acquaintance and he will thankfully make use of your kind offer as soon as he will be placed at London.

I would have answered your kind letter long ago; but until to these days it was most doubtful, whether my son's going to England was not to be delayed for a longer time, Prussian teachers being obliged to give notice six months before quitting their posts. Just now my son has found a successor in his post, and it is almost certain that he will be dismissed from his present place with the beginning of October. Then he will go to London even without having obtained a post there, hoping that after having thoroughly mastered the English language he will succeed in gaining any situation.

I have been greatly satisfied by your approval of my judgement upon G. Bonnier's paper on nectaries. Although fully convinced of the correctness of all that I had stated in my *Kosmos*-article, I was anxious, to have—by being lively interested in the subject—been hurried to too violent expressions.²

My following articles in the *Kosmos* (Jul. Aug. Sept) need to be judged very indulgently, being only abstracts from my work on Alpine flowers (now under the press), which will give the facts my statements rely upon.³

I remain, my dear Sir, with sincere admiration, | yours | H. Müller.

DAR 171: 315

¹ No letter from CD about the proposed visit of Wilhelm Hermann Müller has been found.

² See letter to Hermann Müller, 13 July 1880 and n. 1. Müller's highly critical review of Gaston Bonnier's work on nectaries (Bonnier 1879a) was published in *Kosmos* (H. Müller 1880b).

³ H. Müller 1880a, 1880c, and 1880d, and H. Müller 1881.

To J. W. Judd [15 August 1880]¹

17 Botolph Lane | Cambridge.

My dear Mr Judd.

If you are in London & disengaged on Friday the 20th: would you come & lunch with me at 1 o'clock at my Brothers House 6 Queen Anne S^t Cavendish Square—² I should very much like to have $\frac{1}{2}$ an hour geological talk with you—I ought to come to you but it is a rather long journey for me in a rough Cab to S. Kensington.³ If you cannot come to lunch & are inclined to call at any other hour on Friday—I would stay in if you would inform me when you would call.— We return home early on Saturday morning.

I remain. | Yours very faithfully— | Charles Darwin.

Since poor dear Lyell's death⁴ I rarely have the pleasure of any geological talk with any one.—

Copy
DAR 146: 9

¹ The date is established by a postmark of '15 August 1880' recorded on the copy.

² CD visited Horace and Ida Darwin in Cambridge from 14 to 19 August 1880, and then stayed in London with Erasmus Alvey Darwin, returning home on 21 August (CD's 'Journal' (Appendix II)).

³ Judd was professor of geology at the Royal School of Mines in South Kensington, London (*ODNB*).

⁴ Charles Lyell had died in 1875.

To A. B. Buckley 16 August [1880]¹

Cambridge
Aug. 16th

My dear Miss Buckley

It is an entire blunder of M^r Packard. I have described briefly in *Origin* the slave making process, as seen by myself.—² I have, however, remarked (speaking from memory) that apparently *F. sanguinea* does not attend so much to Aphides in England as on the continent.—³

My dear Miss Buckley | Yours sincerely | Ch. Darwin

University of Chicago Library, Special Collections Research Center (Joseph Halle Schaffner collection, box 1, folder 2)

¹ The year is established by the address; CD stayed in Cambridge with Horace and Ida Darwin from 14 to 19 August 1880 (CD's 'Journal' (Appendix II)).

² Buckley's letter to CD has not been found. CD had discussed the slave-making instincts of *Formica sanguinea* and *F. rufescens* in *Origin* 6th ed., pp. 216–20; from his observations of *F. sanguinea*, he concluded that the species was less dependent on its slaves (*F. fusca*) than was *F. rufescens*, noting that in England *F. sanguinea* collected building materials and food for themselves. Alpheus Spring Packard Jr wrote: 'Darwin states that in England, *F. sanguinea* does not enslave other species' (Packard 1880, p. 183).

³ In *Origin* 6th ed., p. 219, CD remarked:

in Switzerland the slaves and masters work together, making and bringing materials for the nest: both, but chiefly the slaves, tend, and milk as it may be called, their aphides; and thus both collect food for the community. In England the masters alone usually leave the nest to collect building materials and food for themselves, their slaves and larvæ.

Buckley referred to CD's description of slave-making ants in *Life and her children* (Buckley 1880, pp. 291–2); she also described ants protecting aphids and feeding on their secretions (*ibid.*, p. 278).

From W. E. A. Axon 17 August 1880

Fern Bank, Higher Broughton, Manchester,
17 Aug 1880

My dear Sir

The enclosed cutting appears in the current no. of the "Herald of Health", a popular American magazine edited by D^r M. L. Holbrook. From my interest in the subject I am led to ask whether the letter correctly represents your views on the question.¹

As I am now writing a memoir on the “Food of the Poor”² I am anxious to obtain the widest and most correct information. Such facts as those relating to the Chilian miners are in the highest degree of interest for such an investigation.³ Personally I am a Vegetarian chiefly on humanitarian grounds. From a passage at p. 80 of *Life of Erasmus Darwin* I should suppose you were not in favour of that diet.⁴ As a matter of fact I do not find Vegetarians such large eaters, though they have as a rule a keen enjoyment of food.

Apologizing for thus troubling you | I am | Yours truly | William E. A. Axon

[Enclosure]

DARWIN'S REPLY TO A VEGETARIAN. —The following letter was received from Charles Darwin in answer to one written to him by a person who saw in the theory of evolution, as set forth by this great naturalist, evidence in favor of vegetarianism. We find it in a German vegetarian journal, and translate: DEAR SIR.—I have so many letters to answer that mine to you must be brief. Nevertheless, this has not the significance it would have if I had given the subject of vegetarian diet special attention. The only evidence in my opinion which would be of any value, would be the statistics in regard of the amount of labor performed in countries where the population lived on a different diet. I have always been astonished at the fact that the most extraordinary workers I ever saw, viz., the laborers in the mines of Chili, live exclusively on vegetable food, which includes many seeds of the leguminous plants. On the other hand, the Gauchos are a very active people, and live almost entirely on flesh. Further, it appears to me to be good evidence that in tropical Africa an extraordinary craving exists, which increases to a necessity at times, to eat flesh, though I presume that the seeds of leguminous plants abound there, for the earth nut is extensively cultivated.

DAR 202: 12; *Herald of Health*, August 1880, p. 180

¹ The original enclosure has not been found, but the text has been reproduced from the journal in which it was published. For the original letter from CD, see *Correspondence* vol. 27, letter to Karl Höchberg, 25 February 1879. *Herald of Health* was edited by Martin Luther Holbrook.

² The publication has not been identified; Axon published a number of works on diet, especially vegetarianism (for example, Axon 1891).

³ CD had described the diet of Chilean miners as almost exclusively vegetarian in *Journal of researches*, p. 317.

⁴ In *Erasmus Darwin*, p. 80, CD remarked: ‘as Dr. Darwin was a tall, bulky man, who lived much on milk, fruit, and vegetables, it is probable that he ate largely, as every man must do who works hard and lives on such a diet.’

To H. H. Higgins 18 August [1880]¹

Cambridge
August 18th

Dear Sir

On my return home, I shall no doubt find your essay on Nassa, & which from what you say I have no doubt that I shall find very interesting.²

With respect to Cirripedes I gave to the late or present M^r Sowerby a large collection of Duplicates, as after making the British Museum collection as perfect as I could, I had no further use for the Duplicates, & it is probable that these may have been transferred to M^r. Taylor's collection. If this is the case I shall be heartily glad that they have got into such good hands.—³

I remain | Dear Sir | Yours faithfully | Charles Darwin

Bedfordshire Archives and Records Service (HG12/8/3)

¹ The year is established by the address; CD was in Cambridge from 14 to 19 August (CD's 'Journal' (Appendix II)).

² The letter from Higgins on *Nassa*, a genus of sea snails, has not been found. The essay on *Nassa* is probably Marrat 1880; a copy is in the Darwin Pamphlet Collection–CUL. It was published in the *Proceedings of the Literary and Philosophical Society of Liverpool*, of which Higgins was vice-president.

³ CD had given duplicates of his Cirripedia type-specimens to George Brettingham Sowerby Jr; these were later transferred to John Ellor Taylor's conchological collection in Norwich, and then purchased by Higgins for the Liverpool Museum. When Higgins examined the specimens, he noted their close agreement with species described and figured in CD's monographs *Living Cirripedia* (1851) and *Living Cirripedia* (1854), and concluded that the specimens must have belonged to CD. See *Proceedings of the Literary and Philosophical Society of Liverpool* 35 (1881): xlv–xlvi.

From Ernst Krause¹ 18 August 1880

Ragatz in der Schweiz
den 18.8.80.

Hochverehrter Herr!

Ihre beiden freundlichen Schreiben vom 29 u. 30 Juli habe ich erst gestern nach-gesendet erhalten,² da ich ohne vorausbestimmbaren Aufenthalt seit zwei Wochen auf der Reise war. Mit bestem Danke für dieselben bitte ich Sie, gütigst entschuldigen zu wollen, dass ich dieselben erst heute beantworte.

Ihre Wünsche in Betreff der Bezahlung des Kosmos werde ich dem Verleger gleich nach meiner Heimkunft melden. Er wird, glaube ich, sehr in Betrübniss sein, Ihnen das Exemplar nicht wie bisher senden zu dürfen.³

Es geht mir nicht anders, in Betreff der Honorar-Anweisung, die Sie mir aus dem Ertrage der englischen Ausgabe gesandt haben.⁴ Meinem Gefühle nach habe ich nicht den geringsten Anspruch auf diese Summe, denn meine Arbeit daran ist im Vergleich zu dem Vergnügen und zu der Auszeichnung, die mir daraus erwachsen sind, weniger als keine, und Alles was dem Buche Werth und Verkäuflichkeit giebt, geht von Ihnen aus. Um indessen Ihrem Willen gemäss zu handeln, will ich den Betrag erhebe(n). Es wird sich eine Verwendung dafür finden lassen, welche mich der Bedenken überhebt, die ich gegen die Annahme dieser Summe verspüre. Ich bitte Sie aber dringend, irgend welchen ferneren Betrag der aus der englischen oder amerikanischen Ausgabe erwachsen könnte,⁵ anderweitig gütigst verwenden zu wollen, z. B. zur Deckung der Übersetzungskosten, und im Nothfalle zu irgend welchen wohlthätigen Zwecken. Was mich betrifft, so habe ich ein reichliches Einkommen und unabhängige Stellung u. hätte am liebsten auf *jeden* pekuniären Gewinn aus dieser Unternehmung verzichtet, die mir lediglich Herzenssache gewesen ist.

Über den Erfolg der deutschen Ausgabe habe ich nichts erfahren.⁶ Der Verleger⁷ sagt mir, er selbst könne den Absatz erst zur Ostermesse des nächsten Jahres übersehen. Ich fürchte für ihn dass er, wie das in der Natur der Sache liegt, nicht gross sein wird.

Mit dem besten Danke die Buchhändler Nota wieder beilegend,⁸ zeichne ich, hochverehrter Herr, mit dem herzlichsten Wunsche, dass diese Zeilen Sie wohl und munter treffen mögen | Ihr | aufrichtig ergebener | Ernst Krause.

DAR 169: 108

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

² See letters to Ernst Krause, 29 July 1880 and 30 July 1880.

³ CD had requested a statement of costs, so that he could pay for copies of *Kosmos* for the current and following year (see letter to Ernst Krause, 29 July 1880). *Kosmos* was published by the firm Ernst Günther, whose director was Karl Alberts.

⁴ CD enclosed a cheque for the profits from *Erasmus Darwin* with his letter to Krause of 30 July 1880.

⁵ *Erasmus Darwin* US ed.

⁶ The German edition of *Erasmus Darwin* was published in April 1880 (letter from Ernst Krause, 19 April 1880; Krause 1880).

⁷ Karl Alberts.

⁸ See enclosure to letter to Ernst Krause, 30 July 1880.

To W. C. Williamson 18 August [1880]¹

Cambridge
Aug. 18th

From Mr. C. Darwin,

Your specimens, which have been forwarded to me here; are interesting; but I think the slowness of the change might have been expected under the circumstances, as in the case of tadpoles.—²

C. D.

ApcS

DAR 221.4: 246 (photocopy)

¹ The year is established by the relationship between this letter and the letter from W. C. Williamson, 10 August 1880.

² Williamson had sent seedlings of *Drosera capensis* (Cape sundew), whose habit of growth when several plants were grown in close proximity resembled that of *D. rotundifolia*, the common or round-leaved sundew, and *D. intermedia*, the spoonleaf sundew. See letter from W. C. Williamson, 10 August 1880 and nn. 2 and 3. CD alludes to the fact that tadpoles will not develop into frogs in overcrowded conditions.

From J. B. Innes 19 August 1880

Lochcarron, | Ross-shire, N.B.
19 Augst 80.

Dear Darwin—

When ignorant folk fancy they have observed something, most likely it is a mare's nest—¹

Today shooting on the forest here, and being more than half way up the hill of Ben y. Hatt² I found some barnacles on a rock. 1st. it struck me as curious that barnacles should be so high up. 2nd. that as they were easily separated from the rock with a pen knife that they had not long since been separated by weather. The piece of rock they were on must have weighed from $\frac{1}{2}$ ton to a ton or more I looked at a good many places about without finding any more.— I send you the specimens.

I hope you are all well. The reports of M^{rs}. Hoole I lament to say seem very unfavourable. She appears to be even worse than she was when we were at Downe.³

My little home party would join me in kind regards to you all, but I have forsaken them for a run on grouse moor and forest. I am however off home again tomorrow.

Believe me | Faithfully Yours | J Brodie Innes

DAR 167: 36

¹ Mare's nest: 'an illusory discovery, esp. one that is much vaunted and betrays foolish credulity' (*OED*).

² Probably Liathach, a mountain in the Torridon Hills about twenty miles north of Lochcarron.

³ Alice Mary Hoole, Innes's niece, lived at Downe Lodge. Innes had visited Down in May 1880 (letters from Emma Darwin to H. E. Litchfield, [11 May 1880] and [12 May 1880] (DAR 219.9: 236–7)).

From W. E. Darwin 20 August 1880

Bank, Southampton,

Aug^t— 20th 1880

My dear Father,

I telegraphed to you to say that I had asked W^m James to call on you at Q. A. St.¹ He sails for America on Wednesday; it is cruel to worry you directly you get home, but in case he did not see you, and you are fairly well and have a boy at home,² could you ask him down to lunch or sleep on Sunday or Monday? He is very modest about going, but is very anxious to do so and is extremely pleasant.

His address is

3 Bolton Street
Piccadilly

I am very sorry I cannot come to meet him, if you do ask him let me know when in case I possibly could come.

I am very glad you had so pleasant a visit at Cambridge, and am sorry poor Mother was not quite brisk.³

Sara⁴ comes tomorrow I am glad to say, I hope she saw you in London

Your affect son W. E. Darwin

Cornford Family Papers (DAR 275: 77)

¹ William James had met William Erasmus Darwin in 1877 (E. Taylor 1990, p. 10). CD stayed at Erasmus Alvey Darwin's house at 6 Queen Anne Street, London, from 19 to 21 August 1880 (CD's 'Journal' (Appendix II)).

² William probably meant one of CD's other sons.

³ CD and Emma were in Cambridge visiting Horace and Ida Darwin from 14 to 19 August (CD's 'Journal' (Appendix II)).

⁴ Sara Darwin.

To G. H. Darwin 23 [August 1880]¹

Down

23^d.

My dear George

You are a dead hand at decyphering: will you try & make out something of enclosed letter.—² Frank has utterly failed.³ The bothering man has sent me a German book on "The Bible & Science".—⁴

I enjoyed my stay at Cambridge extremely—more than anything for a very long time.—⁵

Your affect Father | C. Darwin

DAR 210.1: 96

¹ The month and year are established by the mention of CD's trip to Cambridge (see n. 5, below).

² The letter has not been found; it was probably written in *Kurrentschrift*, a form of cursive writing that is the written counterpart of typefaces such as Fraktur. In the past CD had asked some of his German correspondents to write in 'Italian character' (see *Correspondence* vol. 27, letter to Wilhelm Pfeffer, 23 March 1879).

³ Francis Darwin.

⁴ The German correspondent and the book have not been identified.

⁵ CD was in Cambridge from 14 to 19 August (CD's 'Journal' (Appendix II)).

To T. M. Hughes 23 August 1880

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

August 23^d 1880

My dear Sir

It would be very ungrateful in me to hesitate for a moment about accepting the honour, which the Chester Nat. Hist. Soc^y is willing to confer on me.— If, however, it is expected that I sh^d attend to receive the Medal, I fear that I must with much regret decline the honour, for I suffer from the fatigue of travelling & more especially from any unusual excitement.¹ Will you, therefore, be so good as to decide for me in whatever way you think best.

With my sincere thanks for your extremely kind note, I remain | my dear Sir | Yours very faithfully | Charles Darwin

Natural History Museum (General Special Collections MSS DAR 43)

¹ No previous letter about the award has been found. Hughes was president of the Chester Society for Natural Science and Woodwardian Professor of geology at Cambridge. The medal was always awarded at the annual conversazione, held in September at the Town Hall in Chester (Siddall 1911, p. 53).

To J. B. Innes 23 August [1880]¹

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

Aug 23^d

My dear Innes

The discovery of Barnacles on the rocks on the mountains of Scotland w^d have been an extraordinary & very interesting one, but I am sorry to say that the objects sent are not barnacles but very hard Lichens.² I do not remember to have seen any of the same kind, but I have never studied lichens, & they are very perplexing bodies.— We returned on Saturday from Cambridge, where we staid a week with Horace & his charming little wife;³ & we enjoyed ourselves much, admiring the grandeur of Kings Coll. Chapel & the other old scenes of my early life.—

My wife has not seen poor M^r Hoole, since her return; but I daresay you have had late news of her, as M^r Hoole was telling us what a comfort to him your letters were, & that he often wrote to you.—⁴

We have just had a curious scene on our lawn, viz 67 half-reformed criminals & vagabond boys who have come down here for a holiday, & to each of whom I gave sixpence.⁵ Some of them had very good faces & some as atrociously bad faces.

Did you see in papers an account of a burglary at High-Elms;⁶ it was a bad one, as the burglars tried to force their way into the Butler's pantry; he being within with no arms.— I wish I had got your rockets for this house—⁷

Ever yours very truly | Ch. Darwin

Cleveland Health Sciences Library (Robert M. Stecher collection)

¹ The year is established by the relationship between this letter and the letter from J. B. Innes, 19 August 1880.

² See letter from J. B. Innes, 19 August 1880.

³ CD and Emma visited Cambridge, staying with Horace and Ida Darwin, from 14 to 19 August 1880 (CD's 'Journal' (Appendix II)).

⁴ Alice Mary Hoole and Stanley Hoole.

⁵ The event was organised by the Nonconformist pastor James William Condell Fegan; Emma Darwin's diary (DAR 242) records a visit from 'Fegan Mr & boys' on 23 August 1880.

⁶ High Elms was the home of John Lubbock. A report of the burglary was in the *Pall Mall Gazette*, 19 August 1880, p. 6.

⁷ See letter from J. B. Innes, 24 August 1880 and n. 4.

From J. B. Innes 24 August 1880

Milton Brodie

24 Aug^s 1880.

Dear Darwin,

You would have seen that I was not unprepared for the upset of my idea that there were barnacles on a rock high on a Scotch Hill.¹ The articles looked to me so much like barnacles that I was deceived. I could not guess how they could have got up there.

I had not heard of the burglary at High Elms.² It is a pity the butler had not means of winging (or rather legging) one or two of the birds. In case any of your 67 visitors³ should have borne away an outline of your house, and the readiest means of nocturnal access, I should think the alarms I had, and still use on occasion, would be serviceable. They can be got, either with cases of fireworks or with maroons, from Wilkinson Gunmaker Pall Mall—⁴

You must have much enjoyed your visit to Cambridge, and your son. I am glad to hear his marriage has been so happy.⁵

I have not had any late accounts of poor Alice.⁶ I fear there is no improvement to report, or it would have come. I know she used to be much cheered and comforted by M^{rs} Darwins frequent visits to her.

With all our kindest regards | Believe me | Faithfully yours | J Brodie Innes

DAR 167: 37

¹ The barnacles that Innes thought he had found were in fact lichens (see letter to J. B. Innes, 23 August [1880]).

² High Elms was the home of John Lubbock (see letter to J. B. Innes, 23 August [1880]).

³ See letter to J. B. Innes, 23 August [1880] and n. 5.

⁴ Wilkinson & Son, gun, sword and rifle manufacturers, 27 Pall Mall, London (*Post Office London directory*). A maroon was a firework designed to make a single loud noise, used especially as a warning or signal (*OED*).

⁵ CD and Emma had stayed in Cambridge with Horace and Ida Darwin from 14 to 19 August 1880 (CD's 'Journal' (Appendix II)). Horace and Ida had married on 3 January 1880 (Emma Darwin's diary (DAR 242)).

⁶ Alice Mary Hoole.

From J. W. Judd 24 August 1880

Science Schools,
S. Kensington. | S.W.
24th August 1880.

My dear Sir,

I have only just returned from the North of Ireland, where I have been doing some geological work during the last few weeks. I greatly regret that I was not in town to receive your note, as nothing would have given me greater pleasure than to have called upon you in the manner you desired.¹

I shall, however, be in London during the remainder of this week and, if it will not be putting you to any inconvenience, should esteem it a great privilege to be permitted to run over to Down for the conversation which I should so greatly like to have with you, on geological matters.

For a field-geologist in training (as I am just now) the walk across from Bromley or Orpington to Down will be most pleasant at this time of year.² And I shall be most happy to come either on Friday or Saturday at any hour which you may name, as likely to put you to the least inconvenience.

Please to address a line to me at
 14 Auriol Road,
 West Kensington
 —W.

and I will gladly come at any hour on either day you may name,
 Believe me to remain, | Yours very faithfully | John W. Judd

DAR 168: 86


CD ANNOTATION

Top of letter: '11°.25' Charing Cross— send for. Go a little before 4 oclock.—' *pencil*

¹ See letter to J. W. Judd, [15 August 1880].

² Bromley and Orpington railway stations were close to Down.

To J. W. Judd 25 August [1880]¹

Down, | Beckenham, Kent. |  (Railway Station | Orpington. S.E.R.)
 Aug: 25. Wednesday

My dear Sir,

I shall be delighted to see you here on Friday—or Saturday but the former day would be a mere **shade** more convenient.—²

Our trains to *Orpington* are inconvenient in the morning, Your plan will be to come by the train which leaves Charing X at 11°.25'a.m. & we will have dog cart for you at Orpington & that will bring you here in time for luncheon at 1 o'clock.

There is an afternoon train which will require your leaving this house to walk back (4 miles) a little before 4 oclock, But I am bound to tell you that I shall not be able to talk with you or any one else for this length of time. however much I should like to do so But you can read newspaper or take a stroll during part of the time—

I am very glad that you will come though it is a long & troublesome expedition for you—

Yours sincerely. | Ch. Darwin.

Copy

DAR 146: 10

¹ The year is established by the relationship between this letter and the letter from J. W. Judd, 24 August 1880.

² Judd had suggested visiting CD on Friday or Saturday, and walking from the railway station to Down House (see letter from J. W. Judd, 24 August 1880).

To T. M. Hughes 26 August 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
 Aug 26th 1880

My dear Sir

I am very sorry for all the trouble which you have had in this affair. When I first received the announcement of the intended honour, I remarked to my son that it

was a pity that the medal was not confined to local workers, so that you will see how fully I approve of the rule.¹ As you truly say, the knowledge of the wish of the Council of the Chester Nat. Hist. Soc^y to honour me, & not the mere reception of the medal, would have been the real gratification to me; & this I *fully* possess, & shall never forget.—

Believe me, my dear Sir | Yours sincerely | Charles Darwin

Natural History Museum (General Special Collections MSS DAR 8)

¹ See letter to T. M. Hughes, 23 August 1880 and n. 1. The rule evidently required the recipient of the medal to attend the annual conversazione in Chester. The medal for 1880 was awarded to Hughes himself, for his work in geology and his service to the society (Siddall 1911, p. 53).

From H. W. Monk 27 August 1880

16 Sydney Street | Onslow Square
Chelsea | London S.W.
27th August 1880—

Professor Darwin

Dear Sir

I have been lecturing in Canada all last winter, and only arrived in England last week—

My purpose is to direct some attention to a great truth which seems to have been hitherto overlooked by the Scientific and Philosophic world; consequently, I naturally wish much to consult with a few men who are the most distinguished for their scientific attainments, before I begin to publish my ideas in England by lecturing or otherwise.¹ Would you kindly mention time and place that I could see you— If I could meet any of your friends at the same time, it would of course be so much the better—

Faithfully your's | Henry Wentworth Monk

DAR 201: 27

¹ While in Canada, Monk had campaigned for the restoration of Palestine as part of a scheme for a world government based in Jerusalem; he resided in England from 1880 to 1883 trying to gain support for his scheme (*DCB*). No record of Monk's meeting CD has been found.

From J. W. Judd 28 August 1880

14 Auriol Road, | West Kensington | W.
28th August 1880

Dear Mr Darwin,

I send you by book-post a copy of Whitaker's paper on sea-cliffs & Escarpments.¹ As I have a duplicate copy, please not to trouble to return it. I hope the fatigue of conversation yesterday has not left any ill-effects.² To me it is such a pleasure to listen

to the words of suggestion and sympathy from a father in Science, that I fear I may have forgotten the danger in my eagerness at the time.

Yours very faithfully, | John W. Judd

DAR 168: 87

¹ CD's copy of William Whitaker's paper 'On subaërial denudation, and on cliffs and escarpments of the chalk and lower Tertiary beds' (Whitaker 1867) is in the Darwin Pamphlet Collection–GUL.

² Judd had visited CD on 27 August; CD had warned him that he could only talk for a limited time (see letter to J. W. Judd, 25 August [1880]).

To O. C. Marsh 31 August 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Aug 31 1880

My dear Prof. Marsh

I received some time ago your very kind note of July 28th, and yesterday the magnificent volume. I have looked with renewed admiration at the plates, and will soon read the text.¹ Your work on these old birds, & on the many fossil animals of N. America has afforded the best support to the theory of evolution, which has appeared within the last 20 years. The general appearance of the copy which you have sent me is worthy of its contents, and I can say nothing stronger than this.

With cordial thanks, believe me yours very sincerely | Charles Darwin

LS

Yale Peabody Museum of Natural History

¹ See letter from O. C. Marsh, 28 July 1880. Marsh had showed CD the plates for *Odontornithes: a monograph on the extinct toothed birds of North America* (Marsh 1880) on his visit to Down in 1878. CD's copy has not been found.

From R. F. Cooke 2 September 1880

50^A, Albemarle St. | W.
Sept: 2 1880

My dear Sir

Have you arranged anything with Mess^{rs}. Appletons about your new work?¹ The *Index* we are having made by the same hand as did the last & your *written instructions* have gone to be attended to & Mess^{rs}. Clowes send on the sheets.²

Yours faithfully | Rob^t. Cooke

Cha^s. Darwin Esq

DAR 171: 508

¹ In his letter to Cooke of 16 July 1880, CD said that he had told his US publisher, D. Appleton & Co., that he did not know whether it was worth their while to reprint *Movement in plants* from the stereotyped plates of the English edition.

² CD had asked for an indexer to be found for *Movement in plants* and had sent some instructions for making the index; see letter to John Murray, 10 July 1880. The indexer was Matilda Smith; William Clowes & Sons were printing *Movement in plants*.

From W. C. Williamson to Emma Darwin 2 September 1880

Owens College | Manchester
Sept 2nd/80

Private

Dear Mrs Darwin

I am troubling you by the advice of our mutual friend Sir John Lubbock, under the following circumstances. There exists in Yorkshire a large confederation of smaller societies entitled “*The Yorkshire Naturalists Union*”, of which I happen to be President.¹ The members of that union some time ago determi⟨ned⟩ to address to your Dis⟨tinguished⟩ husband their congratul⟨ations⟩ on his having lived to see his great doctrines attain their majority—or to use Huxleys admirable expression in reference to them, “Come of Age”.² The appropriate document designed to convey to your husband their sense of the greatness of his work is now ready—and since the “*Union*” consists largely of working Naturalists and equally largely of men of the operative class.—I have a conviction that Dr Darwin will not despise the tribute of respect co⟨ming⟩ from such a source.

At the same time, knowin⟨g⟩ his dislike to all display, I have felt it my duty to guard them against taking any steps calculated to be inconvenient to him. They seem anxious to send a small deputation from themselves to bring the document to Down personally.—Having the notion that to *send* it in any less dignified way would fail to convey to his mind a right sense of their high estimation of his life’s work.—Nevertheless they place themselves in my hands to act as I may wish.

Knowing Sir J Lubbocks friendship wi⟨th⟩ ⟨your⟩ family I asked his advice whether su⟨ch⟩ ⟨a⟩ deputation would or would not inconvenience your husband, because if so, the document shall be **sent** to him.³ I shall be much obliged therefore if you will give me a private hint as to the best course to be adopted in this matter and I will guide my friends accordingly.

May I venture Madam, whilst I am writing, to congratulate you upon having such a husband, and also, like the Mother of the Gracchii, such sons.⁴

I am most sincerely yours | Wm. C. Williamson

⟨Charles⟩ Darwin

DAR 181: 109

¹ Williamson was president of the Yorkshire Naturalists’ Union from 1880 to 1881 (https://www.ynu.org.uk/YNU_Presidents, accessed 2 April 2019).

² Thomas Henry Huxley’s lecture to the Royal Institution of Great Britain on 19 March 1880, ‘The coming of age of the Origin of Species’, was published in *Nature*, 6 May 1880, pp. 1–4.

³ John Lubbock was CD’s neighbour, living at High Elms, near Down.

- ⁴ Cornelia, mother of the Gracchi, devoted herself to the education of her two sons, who became famous Roman tribunes (*Oxford classical dictionary*). Emma Darwin's sons were William Erasmus, George Howard, Francis, Leonard, and Horace Darwin.

To Williams & Norgate [before 4 September 1880]¹

[Down.]

(Comparative Embryology by F. M. Balfour) (Macmillan & Co)

(The Brain as an Organ of Mind by D^r. Bastian (Kegan Paul & Co))²

When the Translation of Semper's book on Biology in the International Series comes out, please send it³

C. Darwin

Incomplete?

Sotheby's (dealers) (11 July 2017)

¹ This note was sold as part of a collection of items from CD to Williams & Norgate; CD regularly purchased books from the firm. The date is established by the relationship between this letter and the letter to F. M. Balfour, 4 September 1880.

² The first volume of Balfour 1880–1 was published between 1 and 14 June 1880 (*Publishers' Circular* (1880): 453). Bastian 1880, by Henry Charlton Bastian, was published between 16 and 31 May 1880 (*ibid.*, p. 415). CD's copies of both works are in the Darwin Library–Down.

³ Semper 1881, by Carl Gottfried Semper, was published by C. Kegan Paul & Co. as volume 31 of the International Scientific Series; CD's copy is the Darwin Library–CUL. CD had read proof-sheets of the German edition (Semper 1880; see *Correspondence* vol. 27, letter to C. G. Semper, 2 October 1879).

To F. M. Balfour 4 September 1880

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

Sept 4th 1880

My dear Balfour

I hope that you will not think me a great bore, but I have this minute finished reading your address at the B. Assocⁿ; & it has interested me so much that I cannot resist thanking you heartily for the pleasure derived from it, not to mention the honour which you have done me. The recent progress of embryology is indeed splendid.¹ I have been very stupid not to have hitherto read your book, but I have had of late no spare time; I have now ordered it, & your address will make it the more interesting to read, though I fear that my want of knowledge will make parts unintelligible to me.—² In my recent work on plants I have been astonished to find to how many very different stimuli the same small part, viz the tip of the radicle, is sensitive & has the power of transmitting some influence to the adjoining part of the radicle, exciting it to bend to or from the source of irritation according to the needs of the plant; & all this takes place without any nervous system! I think that such facts sh^d be kept in mind, when speculating on the genesis of the nervous system.³ I always feel a malicious pleasure when *a priori* conclusions are knocked on the head; & therefore I felt somewhat like a Devil, when I read your remarks on

Herbert Spencer.—⁴ (I hope that you will soon start for the Alps (& cross the glacier to Horace & Ida), for I am sure that you must much need rest.)— Our recent visit to Cambridge was a brilliant success to us all, & will ever be remembered by me with much pleasure.—⁵

Believe me | Yours very sincerely | Charles Darwin

National Records of Scotland (GD433/2/103C/2)

- ¹ Balfour had given his address on 27 August 1880 as chairman of the department of anatomy and physiology at the British Association for the Advancement of Science meeting. It was published in *Nature*, 2 September 1880, pp. 417–20, and in the *Report of the 50th Meeting of the British Association for the Advancement of Science, held at Swansea* (1880), Transactions of the sections, pp. 636–44. The entire address was devoted to a review of the role of Darwinian theory in the growth of embryology.
- ² CD refers to volume 1 of *A treatise on comparative embryology* (Balfour 1880–1); Balfour had also published *A monograph on the development of elasmobranch fishes* (Balfour 1878). Copies of both books are in the Darwin Library–CUL.
- ³ For CD's conclusions about the sensitivity of plants, which have no nervous system, see *Movement in plants*, pp. 571–3.
- ⁴ In his address (see n. 1, above), Balfour criticised Herbert Spencer's theory of nerve formation (see *Nature*, 2 September 1880, p. 420). For Spencer's work on the genesis of nerves, see Spencer 1870–2, 1: 511–20.
- ⁵ Balfour was a member of the Alpine Club; he met Horace and Ida Darwin in Zermatt, Switzerland, while they were on their honeymoon tour (see letter from Emma Darwin to Sara Darwin, [3 September 1880] (DAR 219.1: 138), and letter from F. M. Balfour, 13 September 1880). CD had visited Horace and Ida in Cambridge from 14 to 19 August 1880 (CD's 'Journal' (Appendix II)). The trip included a visit to Balfour's laboratory (letter from Emma Darwin to H. E. Litchfield, [15 August 1880] (DAR 219.9: 243)).

From Horace Darwin 4 September 1880

Zermatt
Sep 4th 1880

Worms.

In a bare place in fir woods rather higher up the valley than Zermatt (Zermatt is 5300 ft. above the sea) I found worm castings.¹ Owing to the dryness of the ground and the powderiness of the castings it was difficult to be sure that they were worm-castings, but in some cases I was sure, but I should like to have seen some recent ones. They seemed to be in large quantities, but of this I could not be sure, as I could recognise them only in a few cases.

This was the only place in which I found any traces. I have looked carefully in the fields. I will observe more & especially after rain. The guide evidently knew earth worms & said they existed up as high as this²

DAR 64.1: 34–5

- ¹ Zermatt is a town in the Swiss Alps at the foot of the Matterhorn. Horace and Ida Darwin were on their honeymoon tour; see letter to F. M. Balfour, 4 September 1880 and n. 5.
- ² CD described worm-castings in the Alps in *Earthworms*, pp. 12 and 279.

To R. P. Hardy 7 September 1880

[Down.]

Dear Sir

I am much obliged for the great trouble which you have taken for the D. F Club, & which is by no means remunerated by the enclosed Fee.—¹

I will communicate the result to the Club, & hope that the members will have the sense to abide by your careful consideration of their affairs.— You are so good as to offer to fill up the Valuation Return for the Registrar, which I herewith enclose; but for the loss of our annual balance-sheet, the Secretary will not be able to fill up the details.²

Be so kind as to acknowledge the receipt of the cheque.

I remain | Dear Sir | Yours faithfully & obliged | C. D.

R. P. Hardy Es^q | Sept 7th | 1880.

ADraftS

DAR 202: 64

¹ See letter to R. P. Hardy, [after 11 August 1880]. CD had sought Hardy's advice on whether the sickness benefits of the Down Friendly Club could be increased. An entry in CD's Account books—banking account (Down House MS), dated 7 September 1880, records a payment of £3 3s. marked 'Hardy (for valuation of Down club)'.

² The chief registrar of friendly societies was John Malcolm Forbes Ludlow; see letter to J. M. F. Ludlow, 4 August 1880. The enclosure has not been found. The balance sheets for the club from 1868 until 1874 had been lost (see letter to R. P. Hardy, 11 August [1880]). Stephen Whitehead and Thomas Lewis were the last known secretaries (or clerks) of the Down Friendly Club; CD was treasurer.

From Horace Darwin 9 and 12 September 1880

Macugnaga
Sep 9th. 1880

Worms.

Macugnaga is 5115 ft. above the sea.¹ It rained heavily last night. I found one small worm casting near here in an open place in a larch wood. Higher up the valley, 300 ft. I should say above this, I found several small castings. The castings are evidently made by a very small worm, & were very small. I saw the tail of one of them. The hole through which the castings are made must have been about the size of a pin. I only saw one fresh casting, & altogether I found very few. Old ones are difficult to recognise, as they are very powdery.

Baveno Sep. 12th.

For the first two hours of our way down here from Macugnaga I looked out for worms but saw none.

The fields & grass about Zermatt and Macugnaga so far as I have seen, have not that peculiar hummocky appearance, which is seen at home in fields which are habitually grazed by sheep.²

DAR 64.1: 36–7

¹ Macugnaga is a mountain village in northern Italy at the base of Monte Rosa.

² Baveno is a town in northern Italy on the western shore of Lake Maggiore. For Horace's observations from Zermatt, a town in the Swiss Alps at the foot of the Matterhorn, see the letter from Horace Darwin, 4 September 1880.

To W. E. Darwin 10 September [1880]¹

[Down.]

My dear W.

Do not trouble yourself about Beaulieu Abbey; for I have found your packet of washed castings & under a high power the bits of brick do not appear rounded.— If I hereafter find that there are *generally* little stones in the gizzards of worms, then I sh^d be very glad to examine some castings from Beaulieu, but not otherwise. So do nothing unless you hear from me again.²

yours affect. | C. Darwin

Sept 10th.— | W.E. Darwin Esq

DAR 210.6: 162

¹ The year is established by the relationship between this letter and the letter to W. E. Darwin, 18 June 1880.

² In his letter of 18 June 1880, CD had asked William to visit Beaulieu Abbey in Hampshire to collect a packet of worm-castings so that CD could investigate whether fragments of brick in the castings were rounded by the action of the worm's gizzard. For CD's report on the fragments in castings collected from the nave at Beaulieu, see *Earthworms*, pp. 254–5.

To Adolf Ernst 11 September 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Sept. 11th. 1880

Dear Sir

I thank you for your very kind letter & for the seed of the *Cobæa*, which I will have sown.— I was much interested by your article on this plant, & especially by the evidence against M. Bonnier.— But you hardly put your case clearly enough; & I have seen your article (in the *American Naturalist*) quoted as supporting M. Bonnier's view about nectar! Hermann Müller has reviewed Bonnier in *Kosmos* with extreme severity.—¹

I sh^d. think what you say about the diminished fertility of the plant, which you artificially fertilised, probable; but then there always remains the doubt whether changed conditions under culture have not slightly decreased the fertility of the plant, as so often happens. The case of *Lisianthus* is an excellent one of structure & the movements of the parts, adapted for cross-fertilisation; but I do not see any special novelty.—²

With all good wishes I remain | Dear Sir | Yours faithfully | Charles Darwin

PS It is possible that you might give me information on a *little* point, about which I am interested. Near Caracas do earth-worms throw up during the damp season vermiform castings of fine earth, as is so common in Europe. If they do, I sh^d be very glad of any facts *soon*,— whether many castings are thrown up?²— on the plains or in the woods?²— their size?²— to what depths do the worms burrow?²—

&c &c.—

State Darwin Museum, Moscow (GDM KP OF 8974)

- ¹ See letter from Adolf Ernst, 7 August 1880. In his paper 'On the fertilisation of *Cobæa penduliflora*', published in *Nature*, 17 June 1880, pp. 148–9 (Ernst 1880), Ernst argued against Gaston Bonnier's thesis that nectar was of no direct advantage to the plant (Bonnier 1879b, p. 206). See also letter from Adolf Ernst, 29 February 1880 and n. 7. For the statement that Ernst's article supported Bonnier, see *American Naturalist* 14 (1880): 669. For Hermann Müller's critique of Bonnier, see 'Gaston Bonniers angebliche Widerlegung der modernen Blumentheorie' (Gaston Bonnier's supposed refutation of modern floral theory; H. Müller 1880b) in the July 1880 issue of *Kosmos*.
- ² For Ernst's description of his fertilisation experiments with *Cobæa penduliflora*, see Ernst 1880, p. 149. Ernst enclosed a sketch and described *Lisianthus vasculosus* (a synonym of *Symbolanthus vasculosus*; ringentian) in his letter of 7 August 1880.

From F. M. Balfour 13 September 1880

Zermatt
Sept 13. '80

My dear Mr Darwin

I do not know how to thank you for the very kind letter w^h was forwarded to me here.¹ When I accepted the position of Vice President of Section and it was I confess with anything but satisfaction that I looked forward to the prospect of having to write my address; & it was with still less pleasure that I accomplished the task during my last visit here; but to receive such a letter from you more than compensates me for all the trouble & indeed I almost feel it to be one of the events of my life.²

You have probably heard by this time that my brother & I found Horace & his wife here. They left much to our regret soon after we came, crossing over one of the glacier passes! I could not persuade them to come back by another pass.³

My brother & I have been most vigorous since we came out. We have slept or tried to sleep more often in some hut than in bed, & have already been up the Matterhorn & the Weiss horn⁴ For the moment I have almost forgotten all Embryology but hope to come back with renewed vigour in the course of a week or two

I am | yours very sincerely | F. M. Balfour

DAR 160: 29

¹ See letter to F. M. Balfour, 4 September 1880.

² Balfour was a vice-president of section D (biology) and chairman of the department of anatomy and physiology of the British Association for the Advancement of Science for their annual meeting at Swansea. His chairman's address was given on 27 August 1880 and published in *Nature*, 2 September 1880, pp. 417–20.

³ Balfour's brother, Gerald William Balfour, accompanied him on a trip to the Alps (see M. Foster and Sedgwick eds. 1885, pp. 16–17). Horace and Ida Darwin were on their honeymoon tour; see letter from Emma Darwin to Sara Darwin, [3 September 1880] (DAR 219.1: 138).

⁴ The Matterhorn and the Weisshorn are mountain peaks near the town of Zermatt in the Swiss Alps.

To J. V. Carus 14 September 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Sept. 14th 1880

My dear Sir

I send by this Post the 2 first sheets of my book—"The Power of Movement in Plants"—for translation, if you think fit.¹

As I believe I said before, the work appears to me to possess some value & novelty; but it is very dull.— You must not, however, judge of it by the first long chapter, which contains mere descriptions of cases of nearly the same nature; but it seemed to me necessary to establish securely my first point.—

There are 195 wood-cuts, & if, as I hope, you decide to translate I will get them stereotyped at as little cost as I can.—²

Please let me hear what you decide; but perhaps you would wish first to see more of the sheets.—

I most truly hope that your health keeps moderately good | Believe me, my dear Sir | yours sincerely | Ch. Darwin

Staatsbibliothek zu Berlin – Preußischer Kulturbesitz (Slg. Darmstaedter Lc 1859: Darwin, Charles, Bl. 179–180)

¹ CD had promised to send the proof-sheets of *Movement in plants* to Carus (see letter to J. V. Carus, 28 April 1880).

² The first chapter was 56 pages long and described the circumnutating movements of seedling plants; *Movement in plants* contained 196 woodcuts.

To W. E. Darwin [before 16 September 1880]¹

[Down.]

My dear W.

The Indian-rubber bands, which you got for me, have done my wrist a great deal of good, but it is not as strong as it was, so that I wish to continue wearing them for some months more.—² I have only one left, for after a time they burst in an odd manner.— Will you therefore send me by Post 4 or 6 more— I forget price, but will endeavour to ask you what I owe you, as you are such a beggar you will never remind a gentleman what he owes you.—

Your affect Father | C. D.

DAR 210.6: 168

¹ The date is established by the relationship between this letter and the letter from W. E. Darwin, 16 September 1880.

- ² No other references to CD's wrist complaint have been found. For advice on putting the bands on and their cost, see the letter from W. E. Darwin, 16 September 1880. For the use of rubber bands fixed along the hand and forearm to relieve distortion of the digits and stiffness of the joints resulting from injuries to the nerves, see Annandale 1865, p. 212.

From B. D. Wrangham [before 16 September 1880]¹

[M^r Darwin

In reading the life of Kepler, I came to the following passage, which I copied, & sent to M^r Darwin. I had the gratification to receive a letter of thanks from him, in his own writing.]²

“Comentarios on the motions of Mars

I beseech my reader, that, not unmindful of the Divine goodness bestowed on man, he do with me praise & celebrate the wisdom & greatness of the Creator, which I open to him from a more inward explanation of the form of the world, from a searching of causes, from a detection of the errors of vision; & that thus, not only in the firmness & stability of the earth, he receive with gratitude, the preservation of all living things as the gift of God, but also that in its motion, so recondite, so admirable, he acknowledge the wisdom of the Creator. But him who is too dull to receive this Science, or too weak to believe the Copernican system without harm to his piety,—let him, I say, I advise that leaving the school of astronomy, & condemning, if he please, any doctrines of the philosophers, he follow his own path, & desist from this wandering through the universe; & lifting up his natural eyes, with which he alone can see, pour himself out in his own heart, in praise of God the Creator; being certain that he gives no less worship to God than the astronomer, to whom God has given to see more clearly with his inward eye, & who, for what he has himself discovered, both can & will glorify God—

ACopy incomplete

The Huntington Library (HM 72756)

- ¹ The date and the author are established by the relationship between this letter and the letter to B. D. Wrangham, 16 September 1880.
- ² Given the context of Johann Kepler's life, the source of the quotation was probably Olmsted 1841, pp. 311–12; the passage also appeared in Whewell 1833, pp. 314–15. For the original passage from the introduction to Kepler's *Astronomia nova*, see Kepler 1609, pp. [viii–ix].

From W. E. Darwin 16 September 1880

Bank, Southampton,
Sept 16 1880

My dear Father,

I send you 4 wrist bands, one of a smaller size which if too small I can change.¹

They say at the shop that if they are put on by putting the finger through the

whole width, you avoid the nail splitting them up; but the advice does not seem worth much, as I fancy one cannot put them without taking hold of the edge.

$1^s/4$ is the dim total.

I am glad to say George² is uncommonly well, I have not seen him so brisk for years; he went a 12 mile ride yesterday on the pink mare. If you do want any Beaulieu worm castings I can easily get them.³ Please tell Mother that Lilly came all right last night & looks very well & happy, & was perfectly well all the passage; and poor M^{lc}: Wild the french Lady arrived this morning from Havre in an equinoctial gale.⁴ George is giving us a long visit, I am villain enough to think it is partly to air his French with Mademoiselle.

Sara is pretty well, but a little tired by the two arrivals.

Goodbye, dear Father, I hope Mother is well | Your affect son | W. E. Darwin

I have just been much amused at reading old Sedgwick's ferocious letter about the "Vestiges" and the female author in M. Napier's correspondence⁵

Cornford Family Papers (DAR 275: 78)

¹ In his letter to William of [before 16 September 1880], CD said that Indian-rubber bands had done his wrist good and requested more.

² George Howard Darwin.

³ Beaulieu Abbey, Hampshire; see letter to W. E. Darwin, 10 September [1880] and n. 2.

⁴ Sara Darwin's niece, Lily Norton, was visiting from America; Henriette Wild was probably employed as her governess. Le Havre is on the French coast, across the English Channel from Southampton.

⁵ Adam Sedgwick (1785–1873) had published a scathing attack on *Vestiges of the natural history of creation* ([Chambers] 1844) in the *Edinburgh Review*, which was edited by Macvey Napier ([Sedgwick] 1845). For Sedgwick's letters about his article in Macvey Napier's correspondence, see Napier 1879, pp. 489–95. In a letter on p. 493, Sedgwick says he believes the author of [Chambers] 1844 to be a woman.

To Wilhelm Viëtor 16 September 1880

DOWN, | BECKENHAM, KENT. | (RAILWAY STATION | ORPINGTON. S.E.R.)

Sept. 16th 1880.

Dear Sir

As I think any improvement in orthography would be a national benefit, I shall be happy if you will enter my name as a supporter & well-wisher; but I cannot promise to be a "contributor", if by this is meant a writer of articles or letters to your paper.—¹

With all good wishes | I remain Dear Sir | yours faithfully | Charles Darwin.

Copy

DAR 148: 194a

¹ The letter from Viëtor has not been found. Viëtor edited the journal *Zeitschrift für Orthographie: Unparteiisches Centralorgan für die orthographische Bewegung im In- und Ausland* (Journal of orthography: Impartial central organ for the orthographic movement at home and abroad) from 1880 to 1886. In the first issue (October 1880), p. 2, CD's name appears in the list of supporters.

To B. D. Wrangham 16 September 1880

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

Sept. 16. 1880

Dear Sir

I am much obliged for your kindness in having copied & sent me the long & striking passage from Kepler.—¹

I remain Dear Sir | Yours faithfully | Ch. Darwin

The Huntington Library (HM 72756)

¹ The recipient is identified from the accompanying envelope, though CD has addressed it to B. D. Wrangband in error. Wrangham sent a quotation of Johann Kepler probably from Olmsted 1841, pp. 311–12, with his letter of [before 16 September 1880].

From D. Appleton & Co. 17 September 1880

D. Appleton & Co. | 1, 3 & 5 Bond Street | New York.

Sept 17th. 1880

Dear Sir:

We have just received your letter of the 3rd ult.¹ We also received your former letter & replied to the effect that we should be glad to have a set of plates of your new book “The Power of Movement in Plants” & would pay copyright as with your other books.²

Regretting that you should have had any annoyance through the failure to receive our former letter, we remain | very truly yours | D. Appleton & Co

Chas Darwin Esq

DAR 159: 107

¹ CD's letter has not been found.

² The earlier correspondence between CD and Appleton has not been found; see, however, the letter to R. F. Cooke, 16 July 1880.

From J. V. Carus 18 September 1880

Leipzig

Sept 18th. 1880.

My dear Sir,

I am very much obliged to you for sending me the two first sheets of your new book and for the kind words with which you accompany them. Of course I shall be most happy to translate the book and I wrote at once to Mr Koch, who will again publish the translation. He will I think directly apply to you about the stereotypes of the wood cuts.¹

Before I take up my regular winter work I shall for a few days go to Hamburgh. In the mean time some more sheets of your book will be struck off, so that I can begin

translating soon after my return. Would you perhaps be so kind as to tell me how large the book will be, of course some sheets more or less would not matter.

I was at Ems again for four weeks and feel again all the better for it.² Although I drank the water there and made a regular “cure”, yet I consider my going there principally as a time of rest and slackening the tension of the ropes and springs of my hard working mental and bodily machine.

I enjoy just now the visit of my dear old friend D^r Acland from Oxford, who was the first to set an example of true staunch trustworthy English friendship.³ And so many followed him! You may believe that I am really happy to be able to thank you also amongst them all for great kindness and sympathy.

Believe me | My dear Sir, | Yours ever sincerely | J. Victor Carus

DAR 161: 113

¹ CD had sent the first two proof-sheets of *Movement in plants* with his letter to Carus of 14 September 1880 and said that he would get the woodcuts stereotyped if Carus wished to translate the work. Eduard Koch ran E. Schweizerbart'sche Verlagsbuchhandlung, CD's German publisher.

² Bad Ems is a spa resort on the river Lahn, a tributary of the Rhine; Carus suffered from bronchial problems (see *Correspondence* vol. 24, letter from J. V. Carus, 19 March 1876 and n. 4).

³ From 1849 to 1851, Carus had worked as a conservator at the Museum of Comparative Anatomy in Oxford (*Complete dictionary of scientific biography*); Henry Wentworth Acland was Lee's Reader in anatomy at Christ Church, Oxford, during that period.

From Horace Darwin to Emma Darwin [18 September 1880]

Turin.
Sat.

This is only a line to show you that we are alive. We get back to Cam. on the 23rd¹ We have a delightful day today We went up to the top of a hill 2555 ft. above the sea & found lots of worm castings, & had a delightful panarama of the town.² We tramwayed to the bottom of the hill & walked up & trammed back. Then we saw a steam tram—imagine my excitement—and we went a long way in it, it was v. nice. Now we are going to dine at a cafe

H. D.

To Emma Darwin

ApcS

Postmark: 18 9 80

DAR 162: 73

CD ANNOTATIONS

1.1 This ... today 1.2] *crossed pencil*

1.2 a hill] 'probably Superga' *added above pencil*

1.3 worm castings] *closing square bracket pencil*

1.3 & had ... cafe 1.6] *crossed pencil*

- ¹ Horace and Ida were on their honeymoon tour in the Alps; see letter from Emma Darwin to Sara Darwin, [3 September 1880] (DAR 219.1: 138), and letter from F. M. Balfour, 13 September 1880. Cam.: Cambridge.
- ² CD discussed worm-castings at high altitude, including hills near Turin, in *Earthworms*, p. 12.

To Wilhelm Breitenbach 21 September 1880

Down. | Beckenham Kent (&c).
Sept^r 21. 1880.

Dear Sir.

I am obliged to you for the copy of your paper which I read with interest when it appeared in the *Bot. Zeitung*.— I came to the conclusion that I was probably wrong about the length of the pistil in the Ancestor of the Primulaceæ & that you were right—¹ But my opinion goes for very little, for I have lately been working on other subjects, & it is a considerable exertion to me now that I am old to recall to mind what I happen to know on any subject. I was then too much engaged to undertake this labour.

Should you visit Brazil you will have a splendid field for new observations & wishing you all success² | I remain Dear Sir | Yours faithfully | Ch. Darwin.

Copy

DAR 143: 144

- ¹ Breitenbach had sent his paper ‘Über Variabilitäts-Erscheinungen an den Blüten von *Primula elatior* und eine Anwendung des “biogenetischen Grundgesetzes”’ (On the phenomena of variability in the flowers of *Primula elatior* and an application of the ‘fundamental biogenetic law’; Breitenbach 1880). On p. 580, Breitenbach concluded that because the young plant was homostyled, then, according to the biogenetic law (ontogeny recapitulates phylogeny), the ancestral form must have been homostyled. In contrast, CD discussed the emergence of homostyled from heterostyled forms in species of *Primula* in *Forms of flowers*, pp. 272–4.
- ² Breitenbach travelled to Brazil in 1881; see *Correspondence* vol. 29, letter from Wilhelm Breitenbach, [before 20 June 1881].

To J. V. Carus 21 September 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Sept. 21st 1880

My dear Sir

I thought that I had told you the size of the book.— I am ashamed of its length, but do not know what part could be omitted with propriety.— With Index it will be very nearly 600 pages.— You will be awfully sick of the first chapter.—¹

I hope that you have by this time received a large additional number of sheets, which were despatched a day or two ago.—

I am delighted that you are able to give a fairly good account of your health.²

Believe me my dear Sir | Yours sincerely | Ch. Darwin

P.S. In the intervals of correcting the Proofs, I am writing a very little book & have



Wilhelm Breitenbach.
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Image by Cambridge University Library.

done nearly half of it.— Its title will be (as at present designed) “The Formation of Vegetable Mould, through the action of Worms”— As far as I can judge it will be a curious little book.—³

Staatsbibliothek zu Berlin – Preußischer Kulturbesitz (Slg. Darmstaedter Lc 1859; Darwin, Charles, Bl. 181–182)

¹ See letter from J. V. Carus, 18 September 1880. CD was sending the proof-sheets of *Movement in plants* for Carus to translate into German. The work was 592 pages long including the index and excluding preliminaries.

² See letter from J. V. Carus, 18 September 1880 and n. 2.

³ *Earthworms* was published in October 1881 (Freeman 1977).

From T. M. Reade 21 September 1880

Canning Chambers, 4 South John Street, | Liverpool,
Sept 21st 1880

My dear Sir—

I send you a short paper on “Oceans & Continents” which may interest you as bearing upon some of the problems dealt with in your researches on Coral Atolls &c—¹

When in Edinburgh last Autumn I had a conversation with Mr Murray who explained to me his theories of the formation of coral reefs &c as since set forth in this paper republished in *Nature* but read before the Royal Soc of Edin—² It appears to me that he goes out of his way very much to account for everything relating to coral reef growth without subsidence & elevation— It all arises from a fixed belief in the permanence of oceans & continents & a consequent desire to limit such movements as much as possible— I think the theory he sets forth that the cones or peaks on which he considers atolls have been formed have been levelled up by pelagic deposits & thus brought within the limits of reef building coral growth, a very far fetched idea—

I remain Dear Sir | Yours faithfully | T. Mellard Reade

D: Chas. Darwin—

DAR 176: 30

¹ Reade 1880; CD’s latest published work, on coral reefs was *Coral reefs* 2d ed.

² John Murray (1841–1914). An abstract of Murray’s paper ‘On the structure and origin of coral reefs and islands’ was published in the *Proceedings of the Royal Society of Edinburgh* (Murray 1880); it was reprinted in *Nature*, 12 August 1880, pp. 351–5.

From Josiah Mason [before 22 September 1880]¹

The Bailiff & Trustees of
Sir Josiah Mason’s Science College,
request the pleasure of the Company of
M. Charles Darwin and Lady
at the
opening Address by Professor Huxley, F.R.S.

*in the Town Hall, Birmingham, at noon,
on the 1st of October 1880,
and at a Soiree at the College
at 8 o'clock in the evening.²
An early answer on the accompanying form is
requested, when tickets of admission will be sent.³*

D
DAR 64.1: 49

¹ The date is established by the relationship between this invitation and the letter to Josiah Mason, 22 September 1880. The invitation was presumably accompanied by a now missing letter also inviting CD to a luncheon on the same day; see letter to Josiah Mason, 22 September 1880.

² Thomas Henry Huxley's opening address, 'Science and culture', was printed in *Nature*, 7 October 1880, pp. 545–8.

³ The form has not been found.

To Josiah Mason 22 September 1880

*Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Sept 22^d 1880*

M^r. Ch. Darwin regrets much that it is impossible for him to have the honour & pleasure of accepting Sir J. Mason's invitation to Luncheon on October 1st or for him to attend Prof. Huxleys opening Address.—¹

AL

The New York Public Library. Astor, Lenox and Tilden Foundations. Manuscripts and Archives Division.
(Montague Collection of historical autographs: Series 1, box 2, Darwin folder)

¹ For CD's invitation to the opening of Mason Science College, Birmingham, on 1 October 1880, see the letter from Josiah Mason, [before 22 September 1880]. The opening was marked by an address by Thomas Henry Huxley titled 'Science and culture', and printed in *Nature*, 7 October 1880, pp. 545–8.

To T. M. Reade 22 September 1880

*Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Sept. 22^d 1880*

My dear Sir

I am much obliged for your note & paper, which I shall be very glad to read.¹ I am not a fair judge, but I agree with you exactly that M^r. Murray's view is 'far-fetched'. It is astonishing that there sh^d. be rapid dissolution of C. of Lime at great depths & near the surface, but not at intermediate depths, where he places his mountain-peaks.—²

Dear Sir. Yours faithfully | Ch. Darwin

University of Liverpool Library (TMR1.D.7.6)

¹ Reade 1880; see letter from T. M. Reade, 21 September 1880.

- ² See letter from T. M. Reade, 21 September 1880 and n. 2. John Murray (1841–1914) argued, contrary to CD's theory in *Coral reefs*, that reef formation could be explained without subsidence or elevation; his conclusions were based on samples and measurements taken during the *Challenger* expedition (Murray 1880).

From Édouard Heckel¹ 23 September 1880

*Chaire de Botanique | Université de France | Faculté des Sciences de Marseille. | Marseille,
le 23 9^{bre} 1880*

Monsieur et très illustre Maître,

Je traduis votre nouveau livre ou mieux je la fais traduire sous ma direction.² Le second chapitre est fini et j'ai lu tout le livre par fragments avant votre envoi de ce jour dont je vous remercie du fond du coeur.— J'ai été surpris de ne pas trouver dans ce grand et magnifique travail le mouvement des diverses parties de la fleur, comme *Sommeil des corolles, mouvement spontané et mouvement provoqué des étamines et des pistils*. Je désirerai bien savoir si vous le réservez pour une autre publication et je me demande si vous n'aurez pas pu rattacher ces forms du mouvement à celles que vous étudiez si bien dans votre nouveau livre.

Je fais exception pour le *mouvement provoqué* qui est d'une essence différente, mais évidemment le mouvement spontané répond aux mêmes causes (circumnutation) Je me permets de vous donner sur le point mon opinion parce que l'examen de ces faits a fait l'objet de mon étude spéciale: et j'ajoute encore que je serais bien heureux de vous voir traiter cette grande question du mouvement dans les diverses parties de la fleur, question que J'ai effleuré il y 6 ans.³

Veillez croire, Monsieur et très illustre Maître, à mes sentiments de profonde admiration | D^r Ed. Heckel

Je vous adresserai sois peu un travail sur le *convolvulus arvensis* (monstrosité) dans lequel vous reconnaîtrez l'influence de vos travaux.⁴ Avec mon faible bagage je serai bientôt candidat à l'Institut de France, quelles seraient les conditions à remplir pour arriver comme correspondant à la *Royal Society*? Ce serait un grand honneur pour moi que d'appartenir à la Societe Savante qui est la plus haute expression de la Science en Angleterre et qui la première a sa rendre justice à l'immense valeur de vos travaux.⁵

DAR 166: 129

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

² Heckel was supervising the French translation of *Movement in plants* (Heckel trans. 1882).

³ In 1875, Heckel had published *Du mouvement végétale; nouvelles recherches anatomiques et physiologiques sur la motilité dans quelques organes reproducteurs des phanérogames* (Vegetable movement; new anatomical and physiological research on motility in some reproductive organs of phanerogams; Heckel 1875). Heckel discussed the omission of 'le mouvement provoqué' (provoked movement) in CD's work in the preface of Heckel trans. 1882, pp. xxiv–xxv.

⁴ *Convolvulus arvensis* is field bindweed. A copy of Heckel's 'Recherches de morphologie, de tératologie et de tératogénie végétales. Pétalodie staminale et polymorphisme floral dans le Convolvulus arvensis L.; création artificielle de cette monstruosité. — Multiplication et pétalodie staminales du Viburnum Tinus L.; conditions de formation de cette monstruosité' (Researches on plant morphology, teratology and teratogeny. Staminal petalody and floral polymorphism in *Convolvulus arvensis* L.; artificial

creation of this monstrosity. — Stamina multiplication and petalody of *Viburnum Tinus* L.; conditions of formation of this monstrosity; Heckel 1880a) is in the Darwin Pamphlet Collection—CUL. Heckel also published a shorter note: ‘Dimorphisme floral et pétalodie staminale, observés sur le *Convolvulus arvensis* L.; création artificielle de cette dernière monstruosité’ (Floral dimorphism and stamen petalody, observed in *Convolvulus arvensis* L.; artificial creation of this last monstrosity; Heckel 1880b).

- ⁵ Heckel eventually became a corresponding member of the rural economy section of the Académie des sciences, a component academy of the Institut de France, in 1907 (*Complete dictionary of scientific biography*). He did not become a foreign corresponding member of the Royal Society of London; for the conditions of election, see *Record of the Royal Society of London*, pp. 95–6.

From J. D. Hooker 24 September 1880

Royal Gardens Kew
Sept. 24/80.

Dear Darwin

The De Candolles ♂ & ♀ are here & the ♂ wants very much to visit you & see your Laboratory!—What am I to say to him?—They came here today & we have arranged dinner parties for them on Saturday & Tuesday—so I suppose Monday would be a possible day for him, or Wednesday.¹ I know he goes back next week.

The Grays stay into next week then go to Spain, then to Paris for A. G. to work for a fortnight— we shall join them late in December for a trip to Rome & Naples.²

Ever aff Yrs | J D Hooker

DAR 104: 140–1

¹ Alphonse de Candolle and Jeanne-Victoire-Laure de Candolle; Alphonse de Candolle visited Down on Monday 27 September 1880 (see letter from Asa Gray, 30 September 18[80]).

² Asa Gray planned to visit herbaria in Europe to research volume 2 of his synoptical flora of North America (A. Gray 1878–84). He and his wife, Jane Loring Gray, spent the autumn of 1880 in Spain and France, winter at the Royal Botanic Gardens, Kew, spring 1881 in Italy, and summer at Kew, returning home in October 1881. Hooker and his wife Hyacinth Hooker joined them for a tour of Italy in early March 1881. (J. L. Gray ed. 1893, 2: 701 and 714.)

From J. S. Keltie 24 September 1880

Nature | *Advertisements and business letters to be addressed to the Publishers*
Editorial Communications to the Editor.
Publishing Office: | Bedford Street, Strand, | London.
Sept. 24/80

Dear Sir

Perhaps you would not object to our prefacing Prof. Wilder’s proposal with the statement that you have been good enough to forward it to us and think that it ought to be republished in the country?¹

I am | Yours truly | J. S. Keltie

Charles Darwin Esq.

DAR 202: 105

¹ CD had forwarded to *Nature* Burt Green Wilder's letter, 'The two kinds of vivisection—sentisection and callisection', from the *Medical Record: a Weekly Journal of Medicine and Surgery*, 21 August 1880, pp. 219–20.

From Charles Layton 24 September 1880

D. Appleton & Co. | *Publishers* | *Booksellers & Importers.* | 549 & 551, Broadway, | *New York.* | *Foreign Agency* | 16, Little Britain, E.C. | *London*
Sep! 24 1880

Charles Darwin Esq

Dear Sir

I enclose a/c Sales of your Books from New York, with cheque on Union Bank for £24.4.11 in *payment*— a receipt will oblige¹

Yours Respectfully | Charles Layton | Agent D. Appleton & Co

[Enclosure]

Statement of Sales of Origi(n) of Species²
to Aug 1st, 1880 by D. APPLETON & CO.
(for account of) Cha^s Darwin

<i>On hand last account,</i>	523	<i>On hand this day</i>	(290)
<i>Pri(n)ted since,</i>		<i>(Gi)ven away,</i>	
Ed over	(1)		
		<i>Sold to date</i>	(234)
		Est	524
s(o)l(d) 234. c(opies)			()

Climbing plants

F(e)by 1/80	On han(d)	366	
Aug 1		(3)23	
	(sold under cost)	10	
	(Sold)	33 10%	()

O(rchids)

Feby 1/80	(on hand)	47(6)	
Aug 1—		456	
	(sol)d under cost	10	
	Sold	(10)	()

Different forms of Flowers

Feby 1/79	on hand	504	
Aug 1. —		48(2)	
	sold	(22) ()	(23.59)
		Forwd	\$32.58
<Cross Fer>tilization		Forwd	\$32.58
<Feby 1/80>	On hand	258	
<Aug 1/80>	"	217	
	sold under cost	15	
	Sold	26. 10% of \$2	\$5.20
<Expressio>n of Emotions			
<Feby 1/80>	at hand	944	
<Aug 1/80>		897	
	<Sol>d	47 10% of \$3(.50)	16.45
<De>scend <of> Man			
<Feby 1/80>	at hand	944	
<Aug 1/80>		897	
	<Sol>d	47 10% of \$3(.50)	16.45
<Insect>ivorous Pla<nts>			
< >			

DAR 159: 106, 108

¹ CD's Account books—banking account (Down House MS) records the receipt of £24 14s. 11d. under the heading 'Appleton & Co Profits of Books' on 25 September 1880.

² Where figures are supplied to this badly damaged enclosure it is by reference to the preceding and following statements (see letter from D. Appleton & Co, 11 March 1880, and *Correspondence* vol. 29, letter from Charles Layton, 17 March 1881). The publications mentioned are *Origin* 3d US ed., *Climbing plants* US ed., *Orchids* 2d US ed., *Forms of flowers* US ed., *Cross and self fertilisation* US ed., *Expression* US ed., *Descent* 2d US ed., and *Insectivorous plants* US ed.

To J. S. Keltie [after 24 September 1880]¹

[Down.]

Dear Sir

I have no objection to your proposal if a single sentence is used,— so as not to appear as if I was patronizing Prof. Wilder.— Something like the following one would perhaps do.—

M^r Darwin has forwarded to us the following article, as he thinks that the suggestion there contained deserves consideration in this country.²

ADraft

DAR 202: 105v

- ¹ The date is established by the relationship between this letter and the letter from J. S. Keltie, 24 September 1880.
- ² See letter from J. S. Keltie, 24 September 1880 and n. 1. CD had forwarded a letter on vivisection by Burt Green Wilder to *Nature*; it had originally been published in the *Medical Record: a Weekly Journal of Medicine and Surgery*, 21 August 1880, pp. 219–20. An extract from the letter with a preface similar to the one suggested by CD was reprinted in *Nature*, 30 September 1880, pp. 517–18. Wilder's suggestion was to distinguish painless (under anaesthetic) from painful vivisection, noting that the vast majority of operations (including all those for teaching) were painless; but he argued that painful operations might still be performed under special conditions for research purposes. For more on CD's involvement with vivisection debates, see *Correspondence* vol. 23, Appendix VI.

From James Torbitt 26 September 1880

*J. Torbitt, | Wine Merchant. | 58, North Street, | Belfast,
26 Sept 1880*

Charles Darwin Esqr. | Down.

My dear Sir,

I am careful of your time, and this is no acknowledgment of your kind and valued letter of 16th June last, but I cannot let a day pass without giving you the results of yesterdays work.¹

yesterday I had raised about 500 varieties, out of one cross of the second generation, comprising about 1,500 varieties. These are this years seedlings and among the tubers (some 10,000) only three small ones were found diseased. Many of the plants produced from two to three lbs of tubers, all globular I have had two reports from growers of new varieties of 1875—one is, “yield prodigious and no disease” the other is “immense yield and no disease”.

I have not raised the six acres of my own (or yours rather) 1877^s and '78^s but in them I am informed there is some disease, and the process of selection must be carried out in them as it was in the 1875.²

But given a variety which has been during four or five years free from disease, will it continue to be so forever? I have no doubt that it will not, and I have no doubt that it will not live forever. New varieties must be continually coming into existence, in my opinion, if the maximum capacity of the plant is to be made available.

(To be continued)

I remain my dear Sir | most respectfully | and faithfully yours | James Torbitt

DAR 178: 168

CD ANNOTATIONS

- 1.1 I ... work 1.3] *crossed pencil*
 2.1 yesterday] *open square bracket red and blue crayon*
 2.1 of one] *after 'out' interl pencil*

- ¹ See letter to James Torbitt, 16 June 1880. Torbitt was conducting crossing experiments to grow disease-resistant varieties of potato.
- ² For details of the six acres Torbitt had planted using the funds that CD had helped to raise, and the landowners he had induced to grow fourteen acres between them, see the letter from James Torbitt, 1 April 1880 and n. 4, and the letter from James Torbitt, 13 May 1880.

From George King 28 September 1880

c/o Messrs Grindlay & Co | 55 Parliament St | London

28 Sept 1880.

Dear Sir,

Before leaving Calcutta for England last April, M^r John Scott (of whose premature death you may have heard) put up in spirits two curious specimens of imperfectly developed young pigs.¹ These he took with him with the intention of giving them to you. Being in this country on a short holiday I went the other day to see the relative in whose house M^r Scott died & I there found the jar containing these specimens. I fear M^r Scott was too ill after his arrival in this country to write to you about them, but being anxious to carry out his intentions about them I have had them sent to you—² The specimens will explain themselves. Probably what will interest you most about them is the fact that the sow that gave birth to these has given birth to many litters of the same kind.

His friends have made over M^r Scotts papers to me & if I find amongst them any notes about these pigs I shall send them to you.

Believe me to be | Yrs faithfully | George King

C. Darwin Esq

DAR 169: 21

¹ In 1879, John Scott was forced to return to Britain from India on two years' sick leave after developing a spleen complaint. He died at his sister's house at Garvald, East Lothian, on 11 June 1880 (*ODNB*). King was superintendent and Scott had been curator at the Royal Botanic Garden, Calcutta.

² The specimens have not been found. CD had been sent specimens of imperfectly developed animals in response to his discussion of polydactylism and inheritance in *Variation* 2: 12–17; for an instance of a pig's foot, see *Correspondence* vol. 25, letter to Otto Zacharias, 26 April 1877. CD also discussed semi-monstrous breeds of pigs in *Variation* 1: 75.

To George King 29 September 1880

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

Sept. 29th 1880

Dear Sir

I am much obliged for your letter & for the trouble about the specimens.¹ I was truly grieved to hear of John Scott's death: he did not write to me.— It was very kind of you to visit his poor relations.— I did what I could by getting them an introduction to a neighbouring clergyman, who I thought might aid them in the disposal of Scotts property.— They wanted me to go down there; but I have very little strength & the journey was much too long for me.—²

I had not heard before receiving your note that you were in England. I sh^d very much like to have the pleasure of making your personal acquaintance. Can you spare the time to come here to dinner & sleep any day soon, which would suit you? If so, you had better leave Charing Cross for Orpington St. by the 4^o5' train or 5^o5' if more convenient. I sh^d almost certainly be able to send you a carriage to meet you

at the Station & take you back next morning. Please observe that the Trains may possibly be changed on Oct 1st.—³

I hope that you may feel inclined to come.— & if so be so kind as to let me hear.

I am now writing a little essay on the action of worms, and the information which you formerly gave me has proved *invaluable*.—⁴ I had forgotten, until carefully going over all your notes, what immense trouble you had so kindly taken for me

My dear Sir | Yours sincerely | Ch. Darwin

DAR 249: 89

¹ See letter from George King, 28 September 1880 and n. 1.

² John Scott had died at his sister's house at Garvald, East Lothian, on 11 June 1880 (*ODNB*). No previous correspondence about Scott's death has been found; the clergyman has not been identified.

³ King probably visited Down on 4 October 1880; see letter to George King, 2 October 1880.

⁴ King had previously supplied CD with information about earthworms; see *Correspondence* vol. 20, letters to George King, 28 October 1872 and November 1872; *Correspondence* vol. 21, letter to George King, 18 February 1873. CD discussed King's observations and specimens of worms and casts in *Earthworms*, pp. 5, 106–8, 117, 126–8, 161–3, 168, 274–8, 281, 285. *Earthworms* was published in October 1881 (Freeman 1977).

To James Torbitt 29 September 1880

Down,
Sept. 29, 1880.

My dear Sir

I heartily rejoice at your good news.¹ With such success there will soon be a good demand for your new varieties, and all the anxiety which you have so nobly borne will be over. I will let Mr. Farrer hear the result, and ask him to forward it to Mr. Caird.² I cannot see why some of your new varieties should not endure for a good many years fungus-proof; though I fully subscribe to what you say about the probability of their not enduring for ever.

Again I congratulate you and remain, My dear Sir | Yours sincerely Ch. Darwin

Copy

DAR 148: 122

¹ See letter from James Torbitt, 26 September 1880.

² Thomas Henry Farrer and James Caird had helped CD raise a subscription for Torbitt to continue his potato experiments; see letter to James Caird, 24 March 1880.

From Asa Gray 30 September 18[80]¹

Kew.
Sept. 30. 18(80)

My Dear Mr. Darwin

I had hoped to see you during this fortnight, but it has been completely filled by pressing work here. Hooker could not spare me on Monday; and I was sure that one *foreigner* was enough for you to see and talk with in one day, and so I spared you from a sense of duty as well as of necessity.²

We are off tomorrow morning, for 3 or 4 months. (We) shall then settle at Kew.

Mrs. Darwin and yourself will be glad to know that Mrs. Gray³ bore the voyage very well, and is wonderfully recruited since.

I long to see your Circumnutating book, and must write one, if not two notices of it in U.S. Perhaps you could get it over to me—to Decaisne's care, at the Jardin des Plantes, as soon as it is out.⁴

Sincerely Yours | Asa Gray

DAR 165: 166

¹ The year is established by the reference to *Movement in plants*; see n. 4, below.

² Gray had sailed for England in early September 1880 and was visiting Joseph Dalton Hooker at the Royal Botanic Gardens, Kew, as part of his research at herbaria in Europe. Alphonse de Candolle and his wife Jeanne-Victoire-Laure de Candolle were also staying at Kew; Alphonse de Candolle visited Down on 27 September 1880 (see letter from J. D. Hooker, 24 September 1880).

³ Jane Loring Gray.

⁴ *Movement in plants* was published on 6 November 1880 (Freeman 1977). Joseph Decaisne was based at the Jardin des plantes in Paris; Gray visited Decaisne at his house in November 1880, and had read nearly all of *Movement in plants* by 26 December 1880 (J. L. Gray ed. 1893, 2: 709 and 714). Gray's reviews of *Movement in plants* were in the *American Journal of Science* 3d ser. 21 (1881): 245–9 and *Nation* 32 (1881): 17–18.

To T. H. Farrer 1 October 1880

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

October 1. 1880

My dear Farrer

I have been writing an account of the worms at Abinger & **most** useful I have found your notes.¹ When my M.S. has been well copied, I will ask you to look it over, as you may detect some inaccuracies; it is only about a dozen pages.²

I write now to ask 2 or 3 questions: is the concrete floor so well protected that it is quite dry? if not so, I sh^d much like to hear whether castings are still ejected on it.—³

Secondly, in the Times it is said that M^r: Wright w^d publish an account of the ruins *with a plan*: has this been done & could you lend me the article & plan?⁴

Thirdly you refer incidentally in one of your notes to some walk, the foundations of which consist of broken bricks & mortar: now will you be so kind as to look at this walk, & if the worms have thrown up any castings on it, to send me a few. I want to see whether the particles of brick have been at all tri-turated by the action of their muscular gizzards.—⁵

Lastly I enclose extract from letter from M^r: Torbitt, giving a grand account of his success.⁶ Will you give me address of “J. ??? Caird Esq C.B”, as I sh^d like also to send him copy of Torbitt's letter, telling him, of course, that no answer was required.⁷

You will have heard that Elizabeth Wedgwood is very unwell, & I cannot but think very seriously.⁸ My sister Caroline has also been unwell, & had it not been for these two causes we sh^d now have been at Leith Hill Place.⁹

Ever yours very sincerely | Ch. Darwin

Linnean Society of London (LS Ms 299/34)

- ¹ Farrer had kept a 'worm journal' from 25 August to 23 September 1877 to record earthworm activity at the excavation site of a Roman villa on his estate at Abinger, Surrey, and had sent it to CD (see *Correspondence* vol. 25, letter from T. H. Farrer, 23 September 1877).
- ² CD published Farrer's observations in *Earthworms*, pp. 186–8, and included further brief observations made on 25 September and 13 October 1877.
- ³ The excavation of the Roman villa had revealed the presence of a concrete floor still partly covered with small red tiles. CD at first doubted whether worms could have penetrated this floor; he assumed that the earth above it had been washed down from higher land, until he observed worm burrows forming on the freshly cleared surface of the floor (*Earthworms*, pp. 184–5).
- ⁴ An article titled 'Roman Villa at Abinger', published in *The Times*, 2 January 1878, p. 7, stated that plans of the villa and the paper by George Robert Nicol Wright had been presented at a meeting of the British Archaeological Association and would be published at a later date. A report, 'Recent discovery of the remains of a Roman villa at Abinger, Surrey', was published in the *Builder*, 5 January 1878, pp. 19–20, with some plans of the site on p. 20 and a statement that the paper by Wright would appear in the journal of the British Archaeological Association. Wright's paper was not published.
- ⁵ Farrer had mentioned that plenty of wormcasts appeared on gravel paths laid with 'beds of brick bat & lime rubbish' in his letter of 23 September 1877 (*Correspondence* vol. 25). CD reported in *Earthworms*, p. 18, that little stones swallowed by worms served, like millstones, to grind their food.
- ⁶ See letter from James Torbitt, 26 September 1880. Torbitt was carrying out large-scale experiments in the hope of producing blight-resistant potatoes.
- ⁷ Earlier in the year, James Caird had raised funds from subscribers for Torbitt's project (see letter from T. H. Farrer, 8 March 1880 and enclosure).
- ⁸ Elizabeth Wedgwood, Emma Darwin's sister, lived in Down village; Farrer's wife, Katherine Euphemia, was Elizabeth Wedgwood's niece.
- ⁹ Caroline Sarah Wedgwood lived at Leith Hill Place, Surrey.

From George King 1 October 1880

Alfred House | Gipsy Hill | Upper Norwood | S.E.
1 Oct 1880.

Dear Sir,

I am very much obliged to you for your kind letter of 29 which reached me last night.¹

I shall be most happy to dine with you in accordance with your kind invitation if you will kindly allow me to come in ordinary dress, my evening clothes having been already sent on with my heavy baggage to Brindisi.²

It is very good of you to offer to put me up for the night, but as I am living so near to Beckenham I can easily return here after dinner.³ I came home on a very short holiday indeed & have been in London only since Sunday last. I must leave again in about a week & I regret to say I have only two evenings left free, namely Sunday & Monday next, on either of which I should, as may be convenient for you, be most happy to dine with you.⁴

I am glad that my notes on worm casts have proved of some use⁵

Believe me to be | Yrs sincerely | George King

C. Darwin Esq

DAR 169: 22

¹ See letter to George King, 29 September 1880.

- ² From 1871, the Italian port of Brindisi became the terminus for P&O ships sailing to India. Passengers and cargoes travelled to Brindisi by train, from where ships left for India every Sunday at 2PM ('The Old Peninsula & Oriental Steam Navigation Company c 1835–1972', <http://www.pandosnco.co.uk/indianmail.html> (accessed 16 May 2019); see also Howarth and Howarth 1994, p. 69). King was superintendent of the Royal Botanic Garden in Calcutta.
- ³ Upper Norwood was just over three miles from Beckenham.
- ⁴ King probably dined with CD on Monday 4 October; see letter to George King, 2 October 1880.
- ⁵ See letter to George King, 29 September 1880 and n. 4.

To George King 2 October 1880

Down, | Beckenham, Kent | (Railway Station | Orpington. S.E.R.)
October 2nd. 1880

My dear Sir,

I am very glad that you will come here, but am sorry that you cannot keep.—¹ Monday will be better than tomorrow, for we shall tomorrow have a very large party of my children & relations here, most of whom will be gone on Monday.—² I do not know how you will be able best to get here.— Orpington station is 4 miles—Bromley 6 miles from Down. You can get a fly at either place, & keep it here.— We dine at 7 $\frac{1}{2}$, but if the trains suit, will you not come about an hour before dinner?³— I must tell you one thing with much regret viz that I cannot talk long to any one, as the excitement of much conversation brings on giddiness. Therefore I am sure that you will allow me to leave you for a time after dinner.— It is very good of you to come such a distance to make my acquaintance.—

My dear Sir | Your sincerely | Ch. Darwin

P.S. Beckenham is full 7 miles from Down.— I fear from your note that you think that Down is close to Beckenham.—³

Copy
DAR 146: 18

- ¹ CD had invited King to dine and stay overnight at Down House (see letter to George King, 29 September 1880. The copyist probably misread 'sleep' as 'keep').
- ² According to Emma Darwin's diary (DAR 242), on Sunday 3 October, Henrietta and Richard Buckley Litchfield and Horace and Ida Darwin were at Down House.
- ³ See letter from George King, 1 October 1880 and n. 3.

From Frederick Parsons 3 October 1880

Willow Vale, | Frome.
Oct 3. 80

Dear Sir,

A case occurred in my practice a short time ago of sudden death in a girl aged 16 years—

She had been apparently well, till a minute before death when she suddenly became collapsed with violent pain in the abdomen, & was dead before her companion could fetch help— At the P.M.¹ I found a large abscess (entirely unsuspected during

life) had burst into the peritoneal cavity, in the centre of it was a cherry stone which had worked through a ragged hole in the vermiform appendage of the cæcum— In re-reading your *Descent of Man* (Edit 2. p. 20–21) I thought this short record might be interesting to you as bearing on a statement there made.²

I trust you will forgive the presumption of an entire stranger writing to you on so small a point and believe me to be | Yours respectfully | Frederick Parsons

DAR 174: 26

¹ 'P.M.': post-mortem.

² In *Descent* 2d ed., pp. 20–1, CD had discussed the vermiform appendage of the caecum (the appendix), and how small hard bodies such as seeds could cause death by entering it and causing inflammation.

From Ernst Krause¹ 4 October 1880

Berlin N.O. Friedenstrasse 11. 3 Tr.
den 4.10.80

Hochverehrter Herr!

Wenn ich mir gestattete, Ihnen gestern die zweite Auflage meines Buches "Werden und Vergehen" zu übersenden und Sie bitte, dieselbe als Zeichen meiner herzlichen Verehrung in Ihre Bibliothek aufzunehmen, so hege ich nicht die Anmassung, zu denken, dass Sie das Buch lesen würden.² Sie würden auch wenig Neues darin finden. Es ist eine populäre Darstellung der gesammten durch Sie in's Leben gerufenen modernen Weltanschauung, zu der ich durch einen Auftrag des "Vereins für deutsche Literatur"³ vor vier Jahren angeregt wurde, und der wegen seiner versöhnlichen Sprache sich einer guten Aufnahme zu erfreuen hatte. Im vorigen Jahre was das Buch zum Gegenstand mehrtägiger Debatten im preussischen Abgeordnetenhanse gemacht worden, weil Dr Hermann Müller in Lippstadt das Buch den Schülern der obersten Klassen als Lectüre empfohlen hatte. Die Ultramontanen machten das Buch deshalb zum Gegenstand heftiger Angriffe, die nicht nur darauf hinausliefen, Dr. Herman Müller, sondern den Cultusminister, der solche Lehrer anstelle u. dulde, zu stürzen.⁴ Glücklicher Weise erwies sich das Buch bei näherer Betrachtung als so harmlos und in einem so concilianten Sinne geschrieben, dass diese fürchterlichen Anklagen in nichts zusammenfielen.

In Betreff des Kosmos lässt Sie der Verleger herzlich bitten, Ihnen das Journal wie vordem zusenden zu dürfen, wir hoffen, dass es mit der Zeit an Gehalt zunehmen wird. Bisher hatte es allzusehr mit dem Vorurtheil einzelner Kreise zu kämpfen.⁵

In der Hoffnung, dass diese Zeilen Sie in erwünschtem Wohlsein treffen, zeichne ich, hochverehrter Herr | Ihr | herzlichst ergebener | Ernst Krause

DAR 169: 109

CD ANNOTATION

End of letter: 'Kosmos m(essa)ge to Editor— | D: D.' pencil; 'I have been very glad to hear about H. Müller'⁶ blue ink

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

- ² Krause published *Werden und Vergehen: eine Entwicklungsgeschichte des Naturganzen in gemeinverständlicher Fassung* (Genesis and decline: a popular account of the developmental history of nature) under his pseudonym Carus Sterne; he had sent CD a copy of the first edition, which is in the Darwin Library–CUL (Sterne 1876; see *Correspondence* vol. 25, letter from Ernst Krause, 11 March 1877 and n. 4). The second edition, Sterne 1880, is in the Darwin Library–Down.
- ³ Society for German literature.
- ⁴ The Ultramontanists were Catholics who wanted to integrate church and state, with ultimate authority resting with the church. Criticising Hermann Müller was a way for the Ultramontanists in Germany to attack the liberal minister of education, Adalbert Falk. CD had heard about this incident from Müller himself the previous year (see *Correspondence* vol. 27, letter from Hermann Müller, 14 February 1879).
- ⁵ Krause was the editor of the journal *Kösmos*, founded in 1877; it was published by Karl Alberts in Leipzig. In 1879, Francis Darwin had reported that *Kösmos* was widely regarded in Germany as the ‘organ of “uncultivated materialism”’ (*Correspondence* vol. 27, letter from Francis Darwin, [after 2 June 1879]). In July, CD had offered to pay two years’ subscription to the journal rather than continue to receive issues gratis from the publisher (see letter to Ernst Krause, 29 July 1880).
- ⁶ CD’s annotations relate to his reply to this letter. See letter to Ernst Krause, 7 October 1880.

From W. S. Dallas 6 October 1880

Geological Society
6th. October 1880.

Dear M^r. Darwin

I don’t know whether you are aware that owing to poor Kippist’s (re)signation, there is to be an election of a new Librarian at the Linnean Society shortly.— Murie will of course be a candidate, but owing to his position, (he having already served the Society in nearly the same capacity for several years) he can hardly be expected to get up Testimonials as if the whole business was a new thing.—¹

Under these circumstances it has occurred to some of us, both on & off the Council, that a joint Testimonial in the form of that enclosed would be a good thing, & if you approve of his candidature, or rather of his appointment, I shall be much obliged if you will sign & return it to me.— Perhaps M^r. Francis Darwin will also be good enough to add his signature.—²

Should you prefer to give D^r. Murie a personal testimonial, rather than to take part in a joint expression of opinion, I am sure he would be highly gratified,—but in that case be so kind as to send it to me.—

I hope that you are well & strong for the coming winter— It is to be hoped that it may be a rather more favourable one than the last, but we are already suffering from fogs here in London.—

With kind regards, | Believe me | Yours very truly | W. S. Dallas

DAR 162: 32

¹ Richard Kippist had been librarian of the Linnean Society since 1842; by 1880 he was in poor physical and mental health, but still had to be persuaded by the society to retire on a full salary pension (*ODNB*). James Murie had been assistant librarian since 1876.

² There is no testimonial in the archives of the Linnean Society. The nineteen applications for the librarian’s post were discussed at a special council meeting on 18 October 1880, and resulted in Murie and one other candidate being selected and balloted; the Council Minutes record that Murie received eleven votes and the other candidate four (CM/5 (1873–1880)).

From T. H. Farrer 6 October 1880

Abinger Hall, | Dorking.
(Gomshall S.E.R. | Station & Telegraph).
6 Oct/80

My dear Mr Darwin

Your note followed us on our travels where, inter alia, we had been seeing Silchester, and talked there of you & “worms”—¹

Now for your questions.

1. The concrete floor is not protected or dry. It has perished much with wet & frost, & is also in parts grown over with moss & grass & weeds. This morning we have had such heavy rain that the worm casts are much washed away. But I will look at it the first calm rainless morning and tell you about the worms. I have little doubt that they are at work there & think I trace them this morning.²

2. I send you Mr Wrights article with plan— also a plan I made myself.³

3. All our walks near the house are underlaid with some 6 inches of brick rubbish: and some of them are now on still mornings covered with worm casts; to my gardening feelings, a great nuisance, for they spoil the gravel surface. This morning the rain has washed them into indistinguishable lumps—but you shall have some worm casts on the first favourable day.⁴ Under the fir trees the fallen leaves wash into the holes making a little crown of spines.

4. As to Torbitt— the address is

James Caird Esq C.B
Inclosure Comm^{rs} Office
St James Square.

I was very sorry that Torbitts case did not come before the recent Irish Committee on Potatoes—but I did not know there was such a Committee till it was over, or I would have suggested their calling him—⁵ He will I trust advertize his potatoes. Put a price on a thing—says Sir W Thompson and you make people think it worth notice.⁶

We found here a rather better account of “Aunt Eliz”: but we can scarcely look for a much better one.⁷

Effie & I have had a charming little excursion to our friends the Maskelynes in Wiltshire—driving back with our own horses & seeing Abury & many other remains.⁸ They are very curious—but still more curious is the infinite quantity written and the infinitesimal quantity known about them

Very sincerely yours | T H Farrer

Ida & Horace we hear are busy furnishing. It is delightful to see her turned into a “Martha” & cumbered with much serving⁹

DAR 164: 100

CD ANNOTATION

5,5 Under ... spines. 5,6] *double scored blue ink*

- ¹ See letter to T. H. Farrer, 1 October 1880. Silchester in Hampshire was the site of a Roman town; it was first excavated by James Gerald Joyce between 1864 and 1878. CD was interested in earthworm activity at the site of a Roman villa found on Farrer's estate at Abinger, Surrey.
- ² CD's questions were listed in his letter to T. H. Farrer, 1 October 1880. CD wondered whether the floor of the Roman villa found on Farrer's estate had been protected since its excavation in August 1877.
- ³ An account of the excavation at Abinger, with a plan, was published in the *Builder*, 5 January 1878, pp. 19–20. It was not by George Robert Nicol Wright, but mentioned the paper he had presented at a meeting of the British Archaeological Association. Farrer's plan is possibly that in DAR 64.2: 38.
- ⁴ CD had asked for wormcasts from these walks to see whether the small fragments of stone swallowed by worms acted like millstones to grind their food (letter to T. H. Farrer, 1 October 1880 and n. 5).
- ⁵ CD had enlisted Farrer's help in raising funds for James Torbitt, who was attempting to breed blight-resistant potatoes (see letter from T. H. Farrer, 6 March 1880 and n. 1). Bad weather in 1879 depressed Irish potato yields and many Irish farmers and labourers were facing destitution; in May 1880, a parliamentary select committee had been formed to investigate how best to produce disease-resistant varieties. Torbitt's work was not considered by the committee (DeArce 2008, pp. 216–17).
- ⁶ William Thomson. Torbitt had advertised in 1876 although CD had discouraged him (see *Correspondence* vol. 24, letter to James Torbitt, 21 April 1876, and letter from James Torbitt, 22 April 1876). Torbitt advertised again in the 1890s, well after CD's death (DeArce 2008, pp. 217–18).
- ⁷ Elizabeth Wedgwood was seriously ill; Farrer was married to her niece.
- ⁸ Farrer and his wife, Katherine Euphemia (Effie) Farrer, were visiting Thereza Mary and Nevil Story-Maskelyne. Nevil Story-Maskelyne had inherited Basset Down House, near Wroughton, Wiltshire, in 1879 (*ODNB*). Abury was an alternative name for Avebury, a Neolithic site in Wiltshire with three stone circles; from the seventeenth century various ideas had been proposed about their origin and use (Ucko *et al.* 1991). In 1871, John Lubbock had purchased part of the Avebury site to prevent destruction of the stone circle (*ibid.*, p. 257).
- ⁹ Ida (Farrer's daughter by his first wife) and Horace Darwin were settling into their new house at 66 Hills Road, Cambridge. Martha: a reference to Luke 10:38–42. On the arrival of Jesus at the house of two sisters, one (Martha) became preoccupied with preparations in the home while the other (Mary) sat at the Lord's feet to hear his words.

From Horace Darwin 7 October 1880

66 Hills Road Cambridge
Oct. 7th 1880

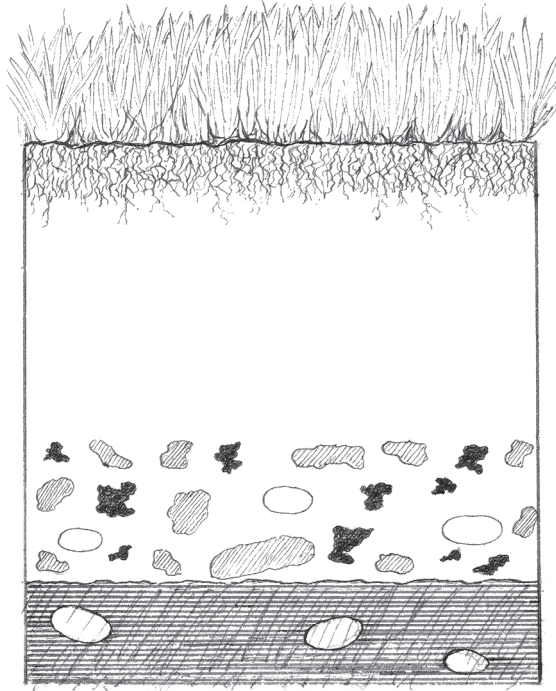
Dear Father,

I enclose the drawing, and a couple of samples of grass and shading, which perhaps you might like to see.¹ I am quite sure there is no mistake in the drawing this time. I could do the shading more carefully if you wished it done. I also send the Gravetye notes, but I don't think they will be much good.² If you want me to ask Mr. Easton anything, I shall be in London early next week & could do so.³ He was away when I took the notes. This morning I went to the place I told you about where I had seen the worm castings. It is a little back street called Saint Tibbs Row.⁴ On one side there is a wall which comes down to the pavement, leaving a crack between the flags and the bricks of the wall from $\frac{1}{2}$ in. to $1\frac{1}{2}$ in. wide. It is on the earth in this crack where I found the castings. There are some trees on the other side of the wall, but I saw no leaves about, but there were plenty of bits of straw, & grass lying near the castings. Also several small grasses were growing near the castings and on the wall there was moss. I suppose from this that this is of no use to you, shall I observe anything more about it.

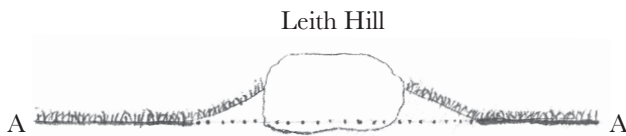
We are getting a little more settled here, but things are very much upside down still, & there is plenty to do.⁵ George is back but I have not seen him yet, when I do I hope to hear a better account of Mother.⁶

Your affec son | Horace Darwin

[Enclosure 1]

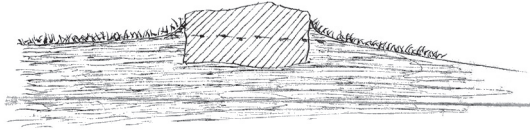


[Enclosure 2]



Scale $\frac{1}{2}$ inch to the foot.

Trial of grass and shading
to see how you like it.

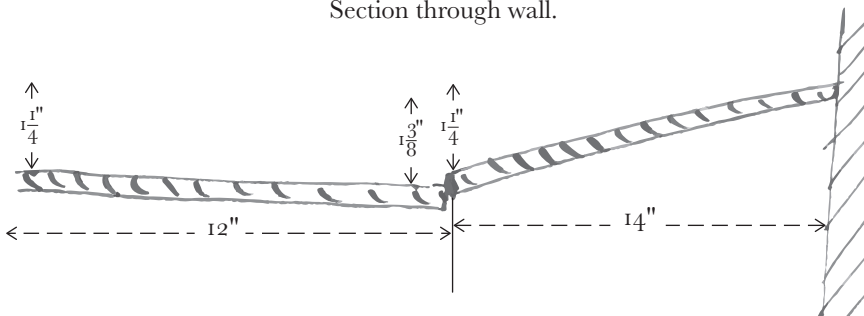


[Enclosure 3]

Gravetye Manor.
Sep 10th. 1878

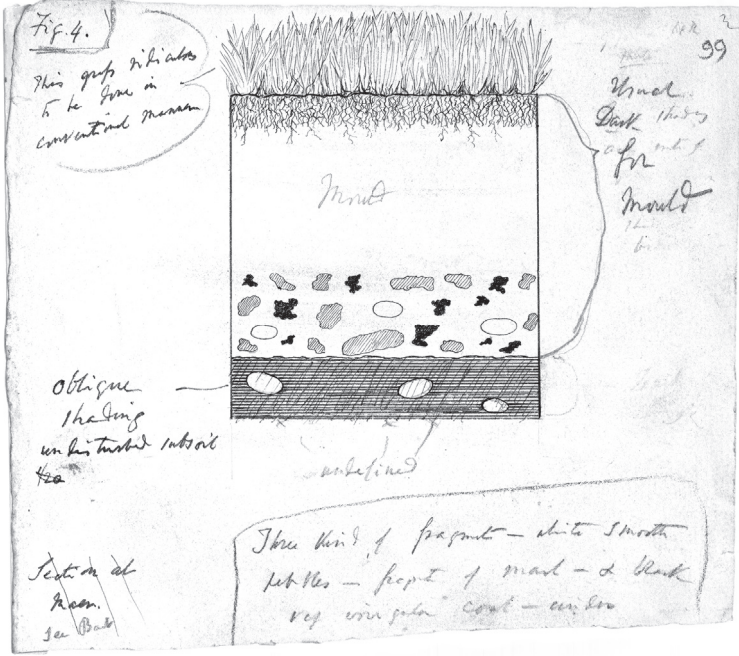
I was told that the S.W. corner of the house had settled, & I went into the cellar to see if I could see any worms at work. I found a good many at work between the stones they were small castings. The floor was damp. & I believe water lodges sometimes or rather used to lodge there, there are drains made in the floor. I only found castings near the S.W. corner, the floor having been repaved over a good piece of the cellar. There was a root between the stones in one place. In some other parts of the pavement it was possible to force anything down between the stones for a few inches, there being nothing between them or only very loose earth. The ground had been opened at the S.W. corner outside the house to a depth of about 4 feet to see if a thing could be found out about the sinking but with what result I do not know.

Section through wall.



This section⁷ shows the start of a paving stone next to the wall, which I thought at first might be due to the worms, but I think it most likely it is not so, because the floor has gutters in it, & there is one running parallel to this wall about 14" from it, and a little beyond the point where the section is taken, hence this stone might have been placed in the position in which I found it, so as to form the beginning of the gutter.

DAR 65: 99, 100; DAR 162: 72, 73



Description

Section in field of near lake

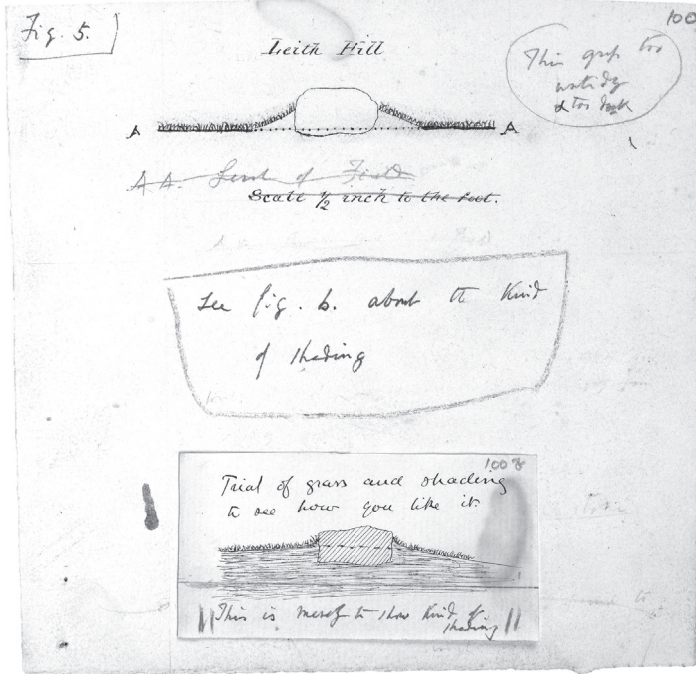
Reduced to 1/2 natural scale

A. Surf 1/2 inch

B. Fine white mud 2 1/2 inches

C. Mud with fragment (benches) 2 1/2

D. Substrate of dark platy soil with grey pebbles



Letter from Horace Darwin, 7 October 1880, showing CD's annotations.

Left: enclosure 1 (recto, verso).

Above: enclosure 2.

Reproduced at 60 per cent of their original size.

DAR 65: 99-100.

By permission of the Syndics of Cambridge University Library.

CD ANNOTATIONS

*Enclosure 1:**Top of page:* 'Fig. 4.' *ink circled ink and red crayon**Top of diagram:* 'This grass ridiculous to be done in conventional manner' *ink circled red crayon**Middle of diagram:* 'Mould | Usual Dark for mould' *pencil over erased illeg**Bottom of diagram:* 'Oblique shading undisturbed subsoil' *ink; '1/20' ink del ink**Below diagram:* 'undefined' *pencil*; 'Three kinds of fragments—white smooth pebbles—fragments of marl—& black very irregular coal-cinder' *pencil circled red crayon**Bottom of page:* 'Section at Maer' *ink del ink*; 'see Back' *pencil del ink**Enclosure 2:**Top of page:* 'Fig. 5.' *ink circled ink**Upper diagram:* 'This grass too untidy & too dark' *pencil circled pencil*; 'A A. Level of Field' *pencil del pencil*; 'See fig. 6. About the kind of shading' *ink circled red crayon**Lower diagram:* 'This is merely to show kind of shading' *ink, double scored red crayon**CD note:**Enclosure 1 verso:**Description*

Section in field at Maer Hall

Reduced to $\frac{1}{2}$ natural scaleA. Turf $\frac{1}{2}$ inchB. fine vegetable mould $2\frac{1}{2}$ inchesC. Mould with fragments (describe) $21\frac{1}{2}$

D. Substratum of black peaty soil with grey pebbles

¹ Enclosure 1 was reproduced as figure 5 in *Earthworms*, p. 133, and used to show the amount by which a layer of burnt marl and cinders that had been strewn on newly drained land fifteen years previously had sunk by 1837 owing to the action of worms. The two other drawings (enclosure 2) portray a boulder near Leith Hill Place in Surrey, illustrating how large stones sink in the ground owing to worm activity; these drawings form the basis of figure 6 in *Earthworms*, p. 151. See also plates on pp. 322–3.

² See enclosure 3. Horace's observations at Gravetye Manor in Sussex were included in *Earthworms*, pp. 105–6, although CD does not give the name of the house.

³ Edward Easton was the owner of Gravetye Manor, West Hoathly (*Post Office directory of the six home counties* 1878). He was a partner in the engineering firm Easton and Anderson, where Horace Darwin had served an apprenticeship from 1874 to 1877 (*ODNB*).

⁴ St Tibbs Row is in Cambridge.

⁵ Horace and Ida Darwin had recently moved to their new home at 66 Hills Road, Cambridge.

⁶ George Howard Darwin, Horace's brother, lived in Trinity College, Cambridge (F. Darwin 1916). Emma Darwin had been ill on 3 and 4 October 1880; she was ill again from 9 to 13 October (Emma Darwin's diary (DAR 242)).

⁷ The diagram has been reproduced here at 90 per cent of its original size.

To Ernst Krause 7 October 1880

*Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)*Oct 7th 1880

My dear Sir

I rejoice at the new Edit. of your book, & am much obliged to you for having sent it to me.—¹ I will certainly read some or the whole, if I have time, but I cannot improve in my German & read at a snail's pace.— I am particularly obliged to you for having told me about Hermann's Müller's affair with the Government; I had heard a vague account, & wished to know the truth.²

I request that you will give to the Editor of *Kosmos* my *especial* thanks for his kindness. The numbers always interest me, though I am able to read only a part: I

see that there is a curious article in the one just received by the Prince of observers, Fritz Müller.—³

Pray permit me to add one word about the life of D^r E. Darwin: in your previous letter you write as if you had no claim to the miserably small profits from the English Edition; but I must differ from you, for I sh^d never have dreamed of writing what I did, had it not been for your Essay.⁴ This Essay is the really valuable part of the little book, requiring much labour & thought in its composition; whereas my part is chiefly what we in England call gossip.⁵ I shall publish in a month or two a book on the “Movements of Plants”: I will send you a copy, but I fear it is *much too special*, for anyone but a physiological botanist to care about.⁶ I have long thought that old men, like myself, ought to write only on confined & easy subjects.

Believe me, Yours sincerely | Ch. Darwin

The Huntington Library (HM 36208)

¹ Krause had published a second edition of *Werden und Vergehen* (Genesis and decline; Sterne 1880). See letter from Ernst Krause, 4 October 1880.

² See letter from Ernst Krause, 4 October 1880 and n. 4.

³ In the October 1880 issue of *Kosmos*, there is an article by Fritz Müller on dimorphic females in a species of midge (F. Müller 1880). CD had offered to pay for the issues of *Kosmos*, but the publisher begged to be allowed to continue to send them to him for free (see letter from Ernst Krause, 4 October 1880 and n. 5).

⁴ See letter from Ernst Krause, 18 August 1880. CD and Krause's book *Erasmus Darwin* had been prompted by CD's wish to publish a translation of an essay by Krause on Erasmus Darwin's evolutionary ideas (Krause 1879a).

⁵ CD had written a biographical account of Erasmus Darwin based on family materials (see *Correspondence* vol. 27, letter to Ernst Krause, 19 March 1879).

⁶ *Movement in plants* was published on 6 November 1880 (Freeman 1977).

From C.-F. Reinwald¹ 7 October 1880

15, Rue des Saints-Pères | Paris Paris

7 Oct 1880

Mon cher Monsieur

J'ai reçu votre lettre confidentielle du 6 Oct.²

La mort du regrettable Ed. Barbier laisse réellement sa dame dans une position précaire.³ Elle s'est adressée également à moi pour venir à son secours. Quoique M. Barbier ait toujours été fortement en avance chez moi financièrement et qu'il me devait une assez forte somme, je lui ai payé pourtant une pension régulière pendant les derniers mois de sa maladie. Malgré cela je serai tout disposé à faire encore quelque chose pour sa famille si cela était nécessaire.

M. Barbier n'a pas d'autre enfant qu'une fille née pendant son exil en Angleterre, qui est mariée depuis 2 ans et qui me semble hors de tout besoin.⁴ Dans cette position j'ai conseillé à Madame Barbier de s'adresser à Ms About, Sarcey etc au Journal le XIX^e Siècle, dont il a été le gérant, et que pourront seuls prendre l'initiative d'un appel général avec amis du défunt avec quelque succès.⁵

J'attends l'effet de mon conseil, qui date seulement d'hier, et je suis décidé de joindre alors aussi mon obole à celles du collègue du défunt.

Quant à Mme Barbier je ne l'ai vu qu'une seule fois pendant la dernière maladie de son mari; je ne puis donc prétendre connaître ni son caractère d'épouse, ni de mère, ou de veuve, car le défunt était surtout lié avec mon neveu qui était en plus de conformité d'âge avec lui que moi-même.⁶

Veillez donc excuser le peu de renseignements que je puis vous donner à ce sujet et me permettre de finir par le proverbe français:

qu' "Un bienfait n'est jamais perdu".

Agréé, cher Monsieur, l'assurance de mes sentiments les plus dévoués | C Reinwald

P.S. J'ai la satisfaction de pouvoir vous annoncer que la nouvelle traduction de la Descendance a été achevée par Barbier avant sa dernière maladie, et qu'il ne me reste maintenant qu'à soigner l'impression de la fin et la confection des tables. Nous pourrions certainement paraître avant le fin de l'année.⁷ | CR

DAR 176: 111

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

² CD's letter to Reinwald has not been found.

³ In his letter of 16 June 1880, Reinwald mentioned that Edmond Barbier, CD's French translator, had been gravely ill for several months. Barbier died on 30 September 1880 (*Paris & vicinity, France, death notices, 1860–1902* (Ancestry.com, accessed 24 May 2019)). Barbier's wife was Arthémise Barbier.

⁴ Barbier's daughter, Marguerite Barbier, was 3 years old and living in Brighton with her parents at the time of the 1861 census; Edmond Barbier was listed as 'Professor of the French Language' on the census form (Census returns of England and Wales 1861 (The National Archives: Public Record Office RG9/602/56/42)). Banns for the marriage of Marguerite Barbier were read in Paris in 1876 (*Paris, France & vicinity marriage banns, 1860–1902* (Ancestry.com, accessed 24 May 2019)).

⁵ Edmond François Valentin About had founded the newspaper *Le XIXe siècle* in 1872; Francisque Sarcey was a regular contributor. The paper espoused republican and anti-clerical views (*EB* s.v. About, Edmond François Valentin).

⁶ Reinwald probably refers to his nephew Frédéric Buhlmeier, who had died in June 1879 (see letter from C.-F. Reinwald, 16 June 1880 and n. 7).

⁷ The third French edition of *Descent* was published in 1881 (Barbier trans. 1881).

To T. H. Farrer 8 October 1880

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

Oct 8th 1880

My dear Farrer

I am sorry to trouble you again, but I enclose envelope ready addressed to give as little trouble as possible.— I am quite perplexed by the extension of the excavations. You will remember which was the first room that was cleared (with a concrete floor, & tesserae over a part) & will you be so kind as mark this room in both plans in pencil as "First Room," & return the plans to me, & I will afterwards return them to you.¹ You had 2 trenches dug for me; one I call in my notes "the upper or N.W. trench" & the other "the lower or S.W. trench"— "Upper" & "lower" referring, I suppose, to the slope of the field.² The upper trench was close to the upper bounding wall of the "first room," which seems to have been the atrium.—

If you can remember places, will you put pencil crosses (X) where trenches were dug.

I am astonished at my own perplexity about the plans— The points of the compass do not agree in the 2 plans.—³

Yours very sincerely | Ch. Darwin

The fir-leaves are drawn by the worms into the mouths of their burrows, & if you will look, you will see that they are all drawn in by the basal end which shows more sense or instinct, in worms than I sh^d have given them credit for.—⁴

Many thanks for address of M^r Caird & for promise of castings.⁵

(Morning 8th)⁶

P.S. Elizabeth Wedgwood was in exactly the same state yesterday as on previous days.⁷ She suffers a good deal from her breathing, but is wonderfully patient. She does not eat nearly enough, & I think if she does not rally soon, she must sink. If it is to be, I wish it may be soon for her sake.—

Horace & Ida were in wonderfully good spirits when here;⁸ it did one good to see people so happy & how marvellously strong Ida is. She could ascend Mont Blanc!

Linnean Society of London (LS Ms 299/35); DAR 185: 38

¹ Farrer had sent a published plan and another drawn by himself of the Roman villa excavated at Abinger in 1877 (see letter from T. H. Farrer, 6 October 1880).

² CD had been visiting the Farrers at Abinger Hall in August 1877 when the villa began to be excavated; two trenches had been dug so that CD could examine the nature of the soil near the remains to ascertain the extent of worm activity (CD's 'Journal' (DAR 158); *Earthworms*, pp. 179–83).

³ Farrer confirmed that the compass bearings on the printed map were incorrect (see letter from T. H. Farrer, 9 October 1880 and n. 2).

⁴ Farrer had supposed that the fallen leaves of fir trees had washed into the holes and made little crowns of spines (letter from T. H. Farrer, 6 October 1880). CD's experiments to test the notion that worms could determine the most efficient way to draw in material to plug the mouths of their burrows were discussed in *Earthworms*, pp. 64–98.

⁵ James Caird. Farrer had promised to send wormcasts from his gravel paths (see letter from T. H. Farrer, 6 October 1880).

⁶ Given that this postscript is labelled 'morning', CD may have mistakenly dated it 8th instead of 9th.

⁷ Elizabeth Wedgwood, Emma Darwin's sister, was seriously ill; on 6 October, Farrer had written to say that he had heard a slightly better account of her health (see letter from T. H. Farrer, 6 October 1880 and n. 7).

⁸ Horace and Ida Darwin had stayed at Down on 3 and 4 October (Emma Darwin's diary (DAR 242)). Ida was Farrer's daughter.

To Sophy Wedgwood 8 October [1880]¹

[Down.]

Oct 8th

My dear Sophy.

Will you be so kind in any of your walks as to observe whether there () any or many worm castings in the midst of Heath. It would be best to look where any grass-covered path crosses Heath, for if there are castings on the grass-covered paths or road & not amongst the Heath, it would show that heath is somehow unfavourable for worms. I ask, because I find a memorandum in my notes, that "there does not appear to be any worms amongst the Heath on Hayes common".—² If Lucy is with you, I know that she would readily look from her well-known affection for

worms—³ I am also becoming deeply attached to worms.— Can Lucy remember what sort of lantern she used when she looked at the worms. We find that the light frightens them.⁴

Give my best love to your Mother; I do hope that she is better⁵ | Yours affectionately | C. Darwin

Cambridge University Library (MS Add 4251: 335)

¹ The year is established by the references to CD's work on worms.

² CD's memorandum has not been found, but there are notes dated 15 October 1880 noting the lack of worms on the patch of heath on Keston Common near Hayes (DAR 64.1: 51). In *Earthworms*, p. 10, CD stated that hardly any worms were found in heath. Hayes Common was near Bromley, Kent.

³ Lucy Caroline Harrison was Sophy's sister. In 1872 she had helped CD ascertain the angle of worm burrows on slopes using a knitting needle; she also observed wormcasts on Leith Hill common (see *Correspondence* vol. 20, letters to L. C. Wedgwood, 5 January [1872] and 21 January [1872]). Her detailed results were given in the letters from L. C. Wedgwood, [8 February 1872] and [15 June 1872?] (*ibid.*).

⁴ CD discussed worms' responses to light in *Earthworms*, pp. 19–25.

⁵ Caroline Sarah Wedgwood, CD's sister, had been ill at the start of October (see letter to T. H. Farrer, 1 October 1880 and n. 9).

To G. H. Darwin [before 9 October 1880]¹

[Down.]

My dear George.

There was heavy rain last night & this morning here, & the walk under the Limes was flooded & water flowed down it. The fruit of Lime-trees formed 12 transverse rows, like a ripple.— You will see by enclosed that each has a little stalk. Would such objects arrange themselves transversely to stream. At the end of each ripple, I think, but dare not say so positively, that the majority of the stalks projected transversely to the line of walk.²

Yours affectionately | C. Darwin

I was dismayed when I first saw them on the walk, for I did not see that each had a stalk.

DAR 210.1: 97

¹ The date is established by the relationship between this letter and the letter from G. H. Darwin, 9 October 1880.

² George later published on ripple marks (G. H. Darwin 1883; see letter from G. H. Darwin, 19 November 1880 and n. 4).

From G. H. Darwin 9 October 1880

Trin. Coll
Sat. Oct. 9. 80

Dear Father,

I am a little surprized at the lime fruits making such good ripple marks.¹ I am having a trough made to try experiments with & then hope I shall find out something.

Young Sedgwick will be elected Nat. Sci. fellow today tho' it is not generally known yet.² No time for more

G H Darwin

I've sent off my 1st. revise & the thing will be published within a month I think³

DAR 210.2: 86

¹ See letter to G. H. Darwin, [before 9 October 1880].

² Adam Sedgwick, great-nephew of the Cambridge professor of geology Adam Sedgwick. The younger Sedgwick was a zoologist.

³ George refers to his lengthy paper titled 'On the secular changes in the elements of the orbit of a satellite revolving about a tidally distorted planet', which had been read at the Royal Society of London in December 1879 (G. H. Darwin 1879b). The part of the *Philosophical Transactions of the Royal Society of London* in which this paper was published was available on 6 November according to a notice in the *Athenaeum*, 6 November 1880, p. 618.

From T. H. Farrer 9 October 1880

Abinger Hall, | Dorking. | (Gomshall S.E.R. | Station & Telegraph.)

9 Oct/80

My dear Mr Darwin,

I return the plans marked as well as I can.¹ But what a treacherous thing is memory. I should have put the Upper Trench at the Eastern boundary of the 4th room. But that cannot be: it was not excavated till October, and you were here in August. The one trench must have been where I have put the X X. at the eastern end of the first room. The other trench I cannot remember: If parallel to the first it must have been about ⊕ to ⊕: if at right angles probably next to the wall O to O.

The compass bearing in the printed plan is wrong. Allowing for magnetic pole the walls of the building run nearly due N & S & E & W— the hedge running E & W.²

The ground slopes from E to W

I will send you my rough notes of what was done at the time in a copy book.³

What is history & what are records when we are so much puzzled by what we did ourselves three years ago?

I have been astounded lately by the quantity which antiquarians manage to write about Avebury Sibbury and Silchester,⁴ as compared to the next to nothing which they know.

Effie has Mrs. Ds kind note this morning: and feels sure that if she can be of use or comfort she will be sent for— but the answer is what we looked for.⁵ Indeed there is little to be done, and what we have to hope is as little suffering as possible.

Ida & Horace come this ev^g which is very nice as Tom will be here too.⁶

Ever yrs sincerely | T H Farrer

No worm casts on the concrete this morning. Wise worms to keep indoors this weather!

I return your note as you may like to see your own questions—⁷ Please send it back to me.

DAR 164: 101

CD ANNOTATION

5.1 What ... Farrer 9.1] *crossed pencil*

- ¹ CD had returned two plans of the excavation of the Roman villa at Abinger so that Farrer could mark the position of the trenches dug for CD during his stay at Abinger Hall from 20 to 25 August 1877 (letter to T. H. Farrer, 8 October 1880 and n. 1).
- ² The printed plan appeared in the *Builder*, 5 January 1878, p. 20; the compass bearing shows the walls running north-west, south-west, north-east, and south-east, not due north, south, east, and west.
- ³ These notes have not been found.
- ⁴ Avebury henge and stone circle is a Neolithic site in Wiltshire; Silbury, near Avebury, the largest prehistoric artificial mound in northern Europe, had been supposed to be a burial site, observatory, or sundial; and Silchester in Hampshire was the site of a Roman town.
- ⁵ Emma Darwin probably wrote to Katherine Euphemia (Effie) Farrer about Elizabeth Wedgwood's ill health (see letter to T. H. Farrer, 1 October 1880 and n. 8).
- ⁶ Ida and Horace Darwin and Thomas Cecil Farrer, Farrer's eldest son from his first marriage.
- ⁷ CD had asked Farrer whether wormcasts still formed on the exposed concrete floor of the Roman villa. CD's questions were in his letter to T. H. Farrer, 1 October 1880.

To T. H. Farrer 10 October [1880]¹

From Mr. C. Darwin, Down, Beckenham.
Oct. 10th

Many thanks: as you say you will send copy of notes made at time, I will keep all the documents & return all together.² From the section, which I made with measurements, it is certain that the eastern trench was **outside** the E. boundary wall of the Atrium, & therefore *within* the next room to the East, marked "never completely excavated:" the other trench was parallel & at W. end of Atrium.³

C. D.

I sh^d be very grateful for *more* castings from walk.

Castings have come, but alas contained only one particle of brick, [nor any] of mortar.⁴

ApcS

Linnean Society of London (LS Ms 299/36)

- ¹ The year is established by the relationship between this letter and the letter from T. H. Farrer, 9 October 1880.
- ² Farrer had promised to send a copy of his notes made at the time of the initial excavations of a Roman villa on his estate at Abinger (see letter from T. H. Farrer, 9 October 1880).
- ³ CD was determining the location of the two trenches that were cut for him at the time of the initial excavation. He had asked Farrer to mark the position of the trenches on both plans of the excavation site he had sent to CD (see letter to T. H. Farrer, 8 October 1880). A diagram of a section through the foundations of the villa, indicating the location of one of these trenches, was published in *Earthworms*, p. 180.
- ⁴ CD had asked to be sent wormcasts from a brick and mortar path at Abinger to see whether the small fragments of stone swallowed by worms acted like millstones to grind their food (letter to T. H. Farrer, 1 October 1880 and n. 5).

From T. H. Farrer 10 October 1880

Abinger Hall, | Dorking. | (Gomshall S.E.R. | Station & Telegraph.)

10 Oct/80

My dear Mr Darwin

Horace, Payne, Ida & I have been examining the ruins again today—and I think we can answer your questions.¹

1. The worms are still working through the concrete floor of Room No 1.—²

2. The two trenches we made for you were at the East & west ends of room No 1. the trench at the East end I feel quite sure of: it was outside the wall of that room. About the trench at the west end I cannot be quite so sure— At the west end there is no outer wall but the concrete was broken away and it ended in a mass of broken stones & rubbish.³ The compass bearings are as I told you. The hedge runs east & west, the east end being the higher—up the hill. The walls of the ruin run as nearly as possible north & south & east & west—⁴

I inclose a sketch Horace made on the spot.

Some splendid red worm castings collected by Horace on a road made of brick rubbish shall come tomorrow.⁵

We are much enjoying a sight of them both—robust from Switzerland—both in excellent health & spirits & Horace keen for work⁶

Ever yours sincerely | T H Farrer

We examined the walk round the rough which is on fibrous peat and could find no worms or their doings: there were plenty on the neighbouring open rough where the soil is sand with a little mould above it⁷

DAR 164: 102

CD ANNOTATIONS

2.1 1. ... 1.—] *double scored red crayon*; 'Use' *blue ink circled blue ink*

5.1 Some ... tomorrow. 5.2] *scored red crayon*

6.1 We ... both—] *crossed blue crayon*

8.1 We ... doings: 8.2] *double scored red crayon*; 'Sand **not** Boggy' *blue crayon*

First page: 'Used' *pencil circled pencil*

Top of letter: 'very glad to see your Memo Book.—' *pencil*; 'Keep Habits'⁸ *red crayon*

¹ Horace Darwin, George Payne, Farrer's gardener, Ida Darwin, and Farrer were examining the ruins of a Roman villa that had been partially excavated on the Abinger estate. For CD's questions, see letters to T. H. Farrer, 1 October 1880 and 8 October 1880.

² CD had asked Farrer to check whether worm-castings were still ejected onto the concrete floor in one of the rooms of the excavated Roman villa (letter to T. H. Farrer, 1 October 1880).

³ CD wished to locate the two trenches that were dug at the time the excavations began (letter to T. H. Farrer, 8 October 1880).

⁴ Farrer had informed CD that the bearings given on a printed plan of the excavation site were incorrect (letter from T. H. Farrer, 9 October 1880).

⁵ CD had asked for more castings from paths made from brick rubbish in order to see whether the small fragments of stone swallowed by worms acted like millstones to crush their food (letter to T. H. Farrer, 10 October [1880]).

- ⁶ Horace and Ida Darwin had been on a honeymoon tour in Switzerland in September (see letter from Emma Darwin to Sara Darwin, [3 September 1880] (DAR 219.1: 138)).
- ⁷ The Abinger Roughs are areas of ancient woodland and grassland above the village of Abinger Hammer in Surrey.
- ⁸ 'Memo Book': see letter from T. H. Farrer, 9 October 1880. 'Keep Habits': possibly a note CD wrote to remind himself to keep this letter in a portfolio relating to the habits of earthworms. CD might also be referring to earthworms continuing to penetrate the concrete floor of the Roman villa after it had been excavated (see n. 2, above).

To Hyacinth Hooker 10 October [1880]
Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
 Oct 10th

My dear Lady Hooker

I am very much obliged for your kind present of the Bananas.—¹ The orange-brown ones are the most splendid, aristocratic specimens, I have ever seen.

Believe me | yours truly obliged | Charles Darwin

Endorsement: '780'

Christie's (dealers) (8 June 2005, lot 56)

- ¹ CD enjoyed eating bananas and regularly received samples of the varieties grown at the Royal Botanic Gardens, Kew, as gifts from Hyacinth and Joseph Dalton Hooker (see *Correspondence* vol. 25, letter to Hyacinth Hooker, [18 November 1877]). At this time, bananas were rare and expensive (Endersby 2007, pp. 170–1).

To Romain Moniez 10 October 1880
Down, | Beckenham, Kent. | (Railway Station | Orpington S.E.R.)
 October 10th 1880

Dear Sir

I am very much obliged to you for your great kindness in having sent me a copy of your work on parasitic worms.¹ The subject has always interested me, & I am sure that I shall profit by reading your work.²

Believe me Dear Sir | Yours faithfully & obliged | Charles Darwin

Pierre Bergé (dealers) (22 November 2010, lot 63)

- ¹ Moniez probably sent CD a copy of his study of the larvae of tapeworms, *Essai monographique sur les cysticerques* (Moniez 1880).
- ² CD had collected parasitic worms during the *Beagle* voyage (see *Correspondence* vol. 17, letter to T. S. Cobbold, 9 August [1869]). In *Origin*, p. 70, he noted that when animals of the same species were crowded into a small space epidemics ensued, caused in some cases by parasitic worms, which flourished in such situations because of their facility of diffusion among crowded animals.

From James Caird 11 October 1880
Cassencary | Creetown | N.B.
 Oct 11. 80

My dear Sir

I am much obliged for your letter & its enclosure.¹ Mr. Torbitt seems to be getting into an extensive and robust family of Tubers, and his expenses must be considerable.

If he could only make the grand discovery of a disease proof variety his fortune would be in his hands. I need not say that insofar as our small contribution goes it is left entirely to your discretion as to disposal.²

In this part of the country³ the potato has not been so prolific as this year since the first appearance of disease in 1845. Before that time 10 tons an acre was a common crop,— five to 6 or 7—was the best since. But there are this season 10 & even 11 & 12 ton crops—with but a small proportion diseased. And I expect that the Irish general crop will be in something like the same proportion good.

I expect to return to town about the 25th inst.

Believe me My dear Sir | Yours very faithfully | James Caird

Charles Darwin Esq F.R.S.

DAR 161: 5

¹ CD had sent Caird a copy of an extract from the letter from James Torbitt, 26 September 1880 (see letter to T. H. Farrer, 1 October 1880). Neither CD's letter nor the enclosure has been found.

² Caird had helped raise funds for James Torbitt, who was carrying out large-scale experiments aimed at producing blight-resistant potatoes. For Caird's contribution, see the enclosure in the second letter from T. H. Farrer, 8 March 1880. He had been an enthusiastic supporter of the scheme from 1878 (see *Correspondence* vol. 26, letter from T. H. Farrer, 28 February 1878).

³ Caird was writing from his home in Galloway in the south-west of Scotland.

From A. R. Wallace 11 October 1880

Pen-y-bryn, St. Peter's Road, | Croydon.
Oct^r 11th. 1880

My dear Darwin

I hope you will have received a copy of my last book "Island Life"¹ as I shall be very glad of your opinion on certain points in it. The first five chapters *you* need not read as they contain nothing fresh to you, but are necessary to make the work complete in itself. The next five chapters however (VI to X) I think will interest you, as I *think*, in Chapters VIII. and IX., I have found the true explanation of Geological Climates,—and on this I shall be very glad of your candid opinion as it is the very foundation stone of the book.

The rest will not contain much that is fresh to you except the three chapters on New Zealand. Sir Joseph Hooker thinks my theory of the Australian & N. Zealand floras a decided advance on any thing that has been done before.² In connection with this the chapter on the Azores should be read.³

Chap. XVI. on the British Fauna may also interest you.

I mention these points merely that you may not trouble yourself to read the whole book unless you like.

Hoping that you are well | Believe me | Yours very faithfully | Alfred R. Wallace

C. Darwin Esq.

DAR 106: B144

¹ Wallace 1880a.

² Wallace posited that the difference between the flora of south-western Australia and the rest of Australia could be explained geologically; during the formation of the south-western flora, the eastern parts of the continent were either widely separated from the western part or not yet risen from the ocean. The form of the sea bottom provided evidence that before the joining of eastern and western Australia, New Zealand had been in close connection with eastern Australia. Wallace thus explained the presence of tropical Australian plants in the New Zealand flora. Temperate Australian species from the western regions that were present in the New Zealand flora, he argued, would have been transmitted by sea after eastern and western Australia had formed one continent. See Wallace 1880a, pp. 464–76. This hypothesis, he claimed, fulfilled Joseph Dalton Hooker's prediction that the anomalous floras of Australia and New Zealand would 'present the least difficulties to whatever theory may explain the whole case' (*ibid.*, p. 475). Hooker was an expert on the New Zealand flora (see J. D. Hooker 1853 and J. D. Hooker 1864–7).

³ Wallace pointed out that because the Azores had never been connected to a continent, their flora and fauna contained only species that had been able to reach the islands across many hundreds of miles of ocean (Wallace 1880a, pp. 248–53).

From E. B. Aveling 12 October 1880

Royal Polytechnic. | W.

12.10.80

Dear Sir,

Many months ago I ventured to send you the earlier numbers of a series of articles on your works. Of these you were good enough to express your approval.¹ The Magazine wherein they appeared came to an untimely end and I have since its decease rewritten the articles & published them together with many others, their successors in the National Reformer.² The works hitherto dealt with are the Voyage, Volcanic Islands, Geology of S. America, Orchids, Climbing Plants, Insectivorous Plants. I purpose after a study of the Forms of Flowers & Cross & self-fertⁿ dealing with the Cirripedia & finally with the series commencing with the Origin & ending at present with the Emotions.³

My friends M^{rs}. Annie Besant and Charles Bradlaugh, M.P. contemplate publishing under the title of the International Library of Science & Freethought a series of works either by great scientific and freethinking men or upon their labors. The first of the series will be a translation of D^r L. Büchner's "An dem Geistes leben der Thiere" by M^{rs}. Besant.⁴ To this translatⁿ. Dr. Büchner has given full assent. A translatⁿ. of some work from the pen of Ernst Hæckel by myself is also designed⁵ and other arrangements in regard to French & Italian works are pending. We desire to make the second volume of the series my work upon your writings and teachings.⁶ To you, Sir, therefore I again write to know if such a plan will meet with your approval and have the distinct advantage of your personal sanction. We desire from you as from D^r. Büchner and Professor Hæckel the illustrious support of your consent. As it is long since I last wrote, I remind you that the volume we desire to produce is designed (1) to give students of your writings a condensed analysis thereof (2) to give those who have not time to read your productions a brief account of your discoveries and ideas.



Annie Besant.
Wellcome Collection (CC BY 4.0).

Further I purpose, again subject to your approval, to honor my work and myself by dedicating the former to you. If you approve of this my wish & of the general plan of our second publicatⁿ, I need hardly say to you how honored we should be were you to see fit to give us the immense support of a few words stating that approval. This would without doubt aid us very greatly in our endeavor to reach large numbers of those who are yet but little acquainted with the thought-work of the 19th century, work with which your name must be for ever associated so closely. I forward herewith a little pamphlet of D^r Büchner's already translated into English by M^{rs} Besant⁷ & if it will not be troubling you too greatly I should be very glad to send to you the proof-sheets of my work as they are issued.

With the hope that the help of your approval may be ours | I am | yours faithfully
| Edward B. Aveling | D.Sc.Lond.

DAR 159: 133

- ¹ See *Correspondence* vol. 26, letter from E. B. Aveling, 23 September 1878, and letter to E. B. Aveling, [after 23 September 1878].
- ² Aveling's early articles were published in the *Student's Magazine and Science and Art* (Aveling 1878–9). The *National Reformer* was a periodical owned and edited by the freethinker Charles Bradlaugh.
- ³ Aveling's series of articles, 'Darwin and his works', appeared in twenty-eight instalments in the *National Reformer* between 16 November 1878 and 19 September 1880.
- ⁴ Besant and Bradlaugh had founded the Freethought Publishing Company. The translation of Ludwig Büchner's *Aus dem Geistesleben der Thiere* (The mental life of animals; Büchner 1876) was published under the title *Mind in animals* in 1880 (Büchner 1880b).
- ⁵ Aveling's translation of Ernst Haeckel's work was published in 1883 under the title *The pedigree of man: and other essays* (Haeckel 1883).
- ⁶ This work was published in 1881 under the title *The student's Darwin* (Aveling 1881).
- ⁷ Besant's translation of Büchner's pamphlet was published under the title *The influence of heredity on free will* (Büchner 1880a). This work has not been found in the Darwin Archive–CUL.

To E. B. Aveling¹ 13 October 1880

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

Oct. 13th 1880

Private

Dear Sir

I am much obliged for your kind letter & the enclosure.—² The publication in any form of your remarks on my writings really requires no consent on my part, & it would be ridiculous in me to give consent to what requires none.— I sh^d prefer the Part or Volume not to be dedicated to me (though I thank you for the intended honour) as this implies to a certain extent my approval of the general publication, about which I know nothing.—³ Moreover though I am a strong advocate for free thought on all subjects, yet it appears to me (whether rightly or wrongly) that direct arguments against christianity & theism produce hardly any effect on the public; & freedom of thought is best promoted by the gradual illumination of men's minds, which follows from the advance of science. It has, therefore, been always my object to avoid writing on religion, & I have confined myself to science. I may, however,

have been unduly biased by the pain which it would give some members of my family, if I aided in any way direct attacks on religion.—

I am sorry to refuse you any request, but I am old & have very little strength, & looking over proof-sheets (as I know by present experience) fatigues me much.—⁴

I remain Dear Sir | Yours faithfully | Ch. Darwin

International Institute of Social History, Amsterdam (Karl Marx / Friedrich Engels Papers D. 1014)

- ¹ A Russian translation of this letter in 1931 mistakenly claimed Karl Marx as the recipient, leading to the assumption among scholars that Marx had wished to dedicate the second volume of *Das Kapital* (Marx 1867–94) to CD. The second volume was published in 1885. It was not until the 1970s that Aveling was reinstated as the recipient of this letter following the realisation that Aveling's later association with Marx's daughter had led to his and Marx's papers being combined. See Colp 1982.
- ² The enclosure was an English translation of a pamphlet by Ludwig Büchner (see letter from E. B. Aveling, 12 October 1880 and n. 7).
- ³ Aveling had requested CD's approval of his plan to discuss CD's works in a book titled *The student's Darwin* (Aveling 1881). He had also asked whether he might dedicate the book to CD. See letter from E. B. Aveling, 12 October 1880.
- ⁴ Aveling had offered to send CD the page-proofs of *The student's Darwin* (letter from E. B. Aveling, 12 October 1880).

To T. H. Farrer 13 October 1880

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

Oct 13. 1880

My dear Farrer,

I have been glad to see your copy-book, and all is now clear to me.¹ I have been able to correct some doubtful points, so that I need not trouble you with my M.S. I have been examining under the microscope the castings received this morning, and they are quite invaluable to me.² I shall send the bits of brick to Mr Sorby, who is the great authority on rock-structure, to see if my impression is correct that they have undergone attrition.³ But before sending them I much want answers on a few points. The queries are written on a separate paper, with open spaces, as this will save you trouble.⁴ These are the last of my questions.

My dear Farrer, | Yours truly obliged. | Charles Darwin

LS

Linnean Society of London (LS Ms 299/37)

- ¹ Farrer had sent a copy of the notes he had made when the Roman villa on the Abinger estate was first excavated (see letter from T. H. Farrer, 9 October 1880).
- ² Horace Darwin had collected worm-castings from a road made of brick rubbish (see letter from T. H. Farrer, 10 October 1880).
- ³ CD wanted castings from paths made from brick and mortar rubble in order to see whether the small fragments swallowed by worms acted like millstones to crush their food (letter to T. H. Farrer, 10 October [1880] and n. 4). Henry Clifton Sorby had developed techniques to make thin sections of rocks that allowed him to determine the linear dimensions of the small bodies of which the rock was composed (*ODNB*).
- ⁴ For CD's questions and Farrer's answers, see letter from T. H. Farrer, 16 October 1880.

To T. H. Farrer 13 October [1880]¹

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

Oct 13th

My dear Farrer

Since I wrote this morning to you a horrid fear has crossed my mind.² It is possible, though not probable, that the rounded particles of brick in the castings, may have been rolled about by wind & rain, & afterwards been swallowed by the worms.—³ This possible source of error would be avoided if you could send me some castings from a gravel-walk with **underlying** brick rubbish; for the rubbish will thus have been protected from being rolled or rounded.

For Heaven sake forgive me if you can & believe me | yours very sincerely | Ch. Darwin

Linnean Society of London (LS Ms 299/38)

¹ The year is established by the relationship between this letter and the letter to T. H. Farrer, 13 October 1880.

² See letter to T. H. Farrer, 13 October 1880.

³ CD was trying to determine whether the fragments of brick rubble or similar material swallowed by worms acted like millstones to crush their food; he had assumed that this would be confirmed if the small particles found in wormcasts were smooth and rounded (letter to T. H. Farrer, 1 October 1880 and n. 5).

To Baxter, Payne, and Lepper 14 October 1880

[Down.]

Oct 14th 1880

to M^{rs} Baxter, Payne & Co

Dear Sir,

I have received three notices signed by your firm requesting payment of a half years commutation Rent Charge. The notices do not state upon whose behalf you apply, but I suppose the claim is for Vicarial Tithe.¹

I do not, however, perceive how the total agrees with what I have before paid & I shall be obliged if you will let me know the numbers of the fields referred to.²

These tithes, I believe, have hitherto always been paid in the Village & it appears to me hardly reasonable to ask the tithe payers to go to Bromley to make the payment.³

I hear that this is the general opinion, & I should therefore suggest that you name some place in the village at which the tithes can be paid at an appointed time.

I am, Dear Sir, | yours faithfully | Ch Darwin

Draft(A)

DAR 202: 29

¹ The 1836 Tithe Commutation Act had established a procedure whereby tithes (the traditional requirement that one tenth of local produce be given to the church) could be converted into money payments. The amount due varied, depending on the current valuation of land and resources. The charge on land, instead of on produce, was called the rent charge. Vicarial or small tithes were originally payable to the vicar, while the great tithe was payable to the rector.

- ² CD's Classed account book (Down House MS) records a payment of £1 4s. for 'Vicarial tithes' on 18 October 1880. He had already paid vicarial tithes of £4 10s. 7d. on 6 April 1880, a considerably higher sum than most of his previous vicarial or small tithe payments and closer to the amount he usually paid for the great tithe. CD evidently wished to check that all the property on which his total was based belonged to him, and that there had been no error in attributing land ownership. However, he evidently changed his mind and crossed out this paragraph in his draft.
- ³ Baxter, Payne, and Lepper were auctioneers, valuers, land agents, and surveyors located in Bromley, Kent (*Post Office London suburban directory* 1880).

From C.-F. Reinwald¹ 14 October 1880

15, Rue des Saints-Pères | Paris Paris

14 Oct 1880

Mon cher Monsieur

Je reviens aujourd'hui à votre offre du 24 Septembre, concernant la traduction française de votre nouveau volume

Movement of Plants

dont vous m'avez déjà envoyé les 208 premières pages.²

J'accepte l'offre que vous avez eu la bonté de me faire et je me charge de publier cette traduction pour laquelle j'ai traité avec le Prof. E. Heckel de Marseille.³

Je compte maintenant sur votre amabilité pour décider M. Murray à me céder les clichés de cet ouvrage à un prix raisonnable.⁴

Je vous prie donc cher Monsieur de faire passer l'incluse à cet éditeur et d'y ajouter un mot de recommandation si vous le jugez utile.

Veillez compter par mon entière reconnaissance et agréée | cher Monsieur | l'expression de mes sentiments les plus distingués | C Reinwald

To Ch: Darwin Esq Down

DAR 176: 112

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

² No letter to Reinwald dated 24 September 1880 has been found. CD had begun sending out pages of the English edition of *Movement in plants* to translators in September 1880 (see, for example, letter to J. V. Carus, 14 September 1880).

³ Édouard Heckel had already been in touch with CD about the translation (see letter from Édouard Heckel, 23 September 1880). CD's usual French translator, Edmond Barbier, had died on 30 September 1880 (*Paris & vicinity, France, death notices, 1860–1902* (Ancestry.co.uk, accessed 24 May 2019)).

⁴ John Murray was CD's publisher.

From Sophy Wedgwood 15 October [1880]¹

LHP.

Oct. 15th

Dear Uncle Charles

I have been up on the common today after the worms.

I could not find anything in the middle of the heath, away from the paths, but there were some worm castings on the edge of the grass covered road, resti(ng) on

sprigs of heath, & in one case or more, with the sprays of heath pushed up through them.² But there was grass or other plants all growing among the heath, in fact it is hardly pure anywhere.

I should have thought it a very unlikely place for worm casts, among a thick tangled mass, whether heath or not?²

Also, from my experience, I shd hardly have expected to meet with any worms in that stony sand, (or in peat either.) unless in such a case as grass roots, weeds, in a gravel path. I dug a little among the heath, but found none, but this does not shew much, as I only in one case found any in digging the grass beneath actual wormcasts.

I don't know whether Lucy has written to you, she went up one day, and found nothing particular I believe, and meant to have gone again, but had to go home rather suddenly on account of one of the children not being well.³ She could not remember what light she took, unless she has been able to tell you since—⁴ I am sorry to have written you such a long winded statement about so little.

As my mother has been able to write herself, I will not about anything else.⁵ I do hope aunt Emma is better.⁶ yr affect. niece KESW.⁷

DAR 181: 69

¹ The year is established by the reference to CD's research on earthworms.

² CD had asked Sophy Wedgwood to observe whether there were any wormcasts on the heath near her home at Leith Hill Place, Surrey (see letter to Sophy Wedgwood, 8 October [1880]).

³ No letter of this date from Lucy Caroline Harrison, Sophy's sister, has been found. In 1880, Lucy had three children, Geoffrey Richard Harrison, Anne Dorothea Harrison, and Thomas Edmund Harrison.

⁴ See letter to Sophy Wedgwood, 8 October [1880] and n. 4.

⁵ Caroline Sarah Wedgwood, CD's sister, had been ill at the start of October (see letter to T. H. Farrer, 1 October 1880 and n. 9).

⁶ Emma Darwin had been ill on 3 and 4 October 1880; she fell ill again from 9 to 13 October (Emma Darwin's diary (DAR 242)).

⁷ Sophy, although always known by this name, signed with the initials of her full name, Katherine Elizabeth Sophy Wedgwood.

To R. F. Cooke 16 October 1880

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

Oct. 16th 1880

My dear Sir

You will see by the enclosed that M. Reinwald wants to publish a French Translation of my book, but is a little doubtful, & very naturally so, on account of expense.—¹ I wish, therefore, to supply him with stereotypes of the 195 cuts at **prime** cost. Will be so good as to find out from M^{ess} Clowes² what the cost is, & inform me, & I will write to M. Reinwald.

My dear Sir | Yours sincerely | Ch. Darwin

P.S. | Please make memorandum that I shall require 60 copies with edges cut for presentation.—³

National Library of Scotland (John Murray Archive) (Ms. 42152 f. 375)

- ¹ Charles-Ferdinand Reinwald had asked CD to pass on a letter to CD's publisher, John Murray, concerning the sole rights to the French translation of *Movement in plants* (Heckel trans. 1882). Reinwald's letter to Murray was enclosed in the letter from C.-F. Reinwald, 14 October 1880.
- ² William Clowes & Sons was the printing company used by Murray.
- ³ See Appendix IV for CD's presentation list for *Movement in plants*. Books were usually sold with their pages uncut. For CD's plea to publishers to sell all books with cut pages, see *Correspondence* vol. 15, letter to *Athenæum*, 1 January 1867.

From T. H. Farrer 16 October 1880¹

(1) Was the brick rubbish from a new building, or from an old building whilst being pulled down?

From the old house at Abinger²

(2) How many years ago was the rubbish laid down on the road?

About seven years ago

(3) Is the subsoil of a red colour? For if not the colour of the castings must be due to brick-dust.

No the subsoil is sand or sandy mould not red

(4) Was care taken that the castings alone were picked up, and not particles lying loose in the road?

Yes carefully picked up by Horace Darwin in my presence³

(5) Were there many castings on the road?

Yes a good many especially at the edges—not in the middle.

(6) Could you ascertain (as I much wish to know) how thick the layer of brick rubbish is on the road?

Originally 4 to 6 inches. Now worn in the centre to 2 or 3; but the castings were taken from the sides, where it is still 4 to 6 inches

I do not think the particles of brick could have been rolled about by the wind.⁴ The rubbish was laid seven years ago on a much used farm road: on a sandy bottom: about 6 inches thick. It soon became a compact mass; the centre of the road being much used by carts. But did the cart wheels break up and round the particles? I think not: they would squeeze but scarcely rub them— The former castings were taken from the side of the road. Today—16 October I have collected some more from the grass on the margin 18 inches or more from where the cart wheels now go: & where the brick rubbish is covered with grass & a little mould. The brick rubbish here is 5 or 6 inches deep with turf at the top.

I send by parcel four specimens of castings⁵

1. Those from the margin of the brick road above mentioned. These are full of brick particles.

2. A large quantity collected from the walks near the house in a great no of places. In all these walks there is about 6 inches of brick rubbish—sometimes less

under 4 to 6 inches of gravel—partly chalk gravel—partly sandstone gravel from Wotton.— I can see no brick particles in this. The bricks were put down as they came from the old house—in large bits—& so I think they lie now

3. A small quantity from the top of a heap of brick rubbish shot into a hole in this field.— It is now covered at the top with grass

4 A small quantity from the concrete— Room No 1. of the Roman villa. The concrete is much broken by frost & covered with vegetation, & the worm workings are not so easy to see as they were

T H Farrer

16 Oct/ 80

DAR 63: 42

CD ANNOTATIONS

1.1 (1) ... 6 inches 12.2] 'These queries all relate to the first lot of castings from road' *added blue ink*

13.6 Today— ... the top. 13.9] *triple scored blue ink*

15.1 1. ... they were 18.3] *scored red crayon*

End of letter: '[Intention]' red crayon

¹ Farrer was responding to questions sent by CD with his first letter of 13 October 1880; the questions were written out by Francis Darwin with spaces left for Farrer's answers.

² After purchasing the Abinger estate, Farrer had the eighteenth-century hall demolished and replaced it with a new hall designed by Alfred Waterhouse (*ODNB*).

³ See letter from T. H. Farrer, 10 October 1880.

⁴ This paragraph started a second sheet of paper that Farrer sent to CD along with the sheet of questions. Farrer was here responding to CD's follow-up letter of 13 October [1880], in which CD expressed his fear that the small fragments of brick might be rounded by causes other than the muscular gizzards of worms.

⁵ CD had asked for castings from areas where the brick and mortar rubble was covered by other material in order to rule out the possibility that the small particles of brick in the casts had been rounded and worn smooth by the action of cartwheels (see letter to T. H. Farrer, 13 October [1880]).

From Adolf Ernst 17 October 1880

Caracas
Oct 17/1880

Dear Sir,

In answer to your last letter I hasten to tell you that earth-worms behave here precisely in the same manner as in Europe.¹ I have noticed their castings every where in gardens and fields, but not in the forest. I had an excellent opportunity of observing these animals in the back-yard of my own house, a space measuring 20 yards by ten. This morning I counted on these 200 square yards one hundred and fifty six (156) holes and castings. These were of rather unequal size from half a cubic centimetre to 5 cubic centimeters. I collected as carefully as possible the whole quantity of upcast earth, and found it to be close to half a litre, so that 3 cubic centimetres is the mean quantity of a cast.²

The whole depth I was however unable to ascertain satisfactorily; I can only say that at 75 centimetres the hole was still visible, but I lost there its trace further on.

I will send you as soon as possible some of the earth worms, in order that you may get from some one the systematic name.³

The depth of the burrows must be pretty different, as I saw from the different nature of the up cast earth; sometimes it is a yellow loam (which is generally our subsoil, the result of desintegration of gneissic rocks), sometimes a fine black mould, the occurrence of which has nothing strange in the soil of a city:

I had overlooked in the *American Naturalist* the strange misstatement to which you are good enough to call my attention.⁴ It is singular, though I may be partly guilty by not having worded my phrases more clearly. I shall send a note to the Editors, claiming a true statement of my observations.—⁵

I shall be most happy to write to you now and then, and remain respectfully your obedient servant | A Ernst.

DAR 163: 23

CD ANNOTATIONS

1.6 half a cubic 1.7] *underl pencil*

1.7 5 cubic] *underl pencil*

1.8 cubic centimetres 1.9] *underl pencil*

2.2 further on.] ‘— nearly 30 inches’ *added pencil*

Top of third page: This morning ... cast.]² $5\frac{1}{2}$ ’ *pencil del pencil*;

‘ 156 | 500cc (32’ *pencil*

468	2.2
320	

Top of letter: ‘This will work in ch I & III only [briefly] in’ *pencil*

End of letter: ‘(send my book)’⁶ *pencil*

¹ See letter to Adolf Ernst, 11 September 1880.

² Ernst’s observations were reported in *Earthworms*, pp. 121–2.

³ Ernst did not send a specimen of the common Venezuelan earthworm until after he had received his presentation copy of *Earthworms* (see *Correspondence* vol. 30, letter from Adolf Ernst, 2 March 1882).

⁴ See letter to Adolf Ernst, 11 September 1880 and n. 1.

⁵ The *American Naturalist* was edited by Alpheus Spring Packard and Edward Drinker Cope; no correction by Ernst appears to have been published.

⁶ Ernst’s name is on the presentation list for *Earthworms* (DAR 210.11: 30).

From G. E. Mengozzi¹ 17 October 1880

Londres | 56 Brompton Square S. W.
17 Oct. 1880

Illustre Monsieur,

Envoyé a Londres par la Royal et Imperial Accademie “La Scuola Italica” de Rome, pour étudier et savoir plus de prés la Théorie de l’évolution sur la formation naturelle des existences, par Vous doctement annoncé et einsegné.

C'est presque un an que avec satisfaction de mon esprit j'ai méditée sur la même, et aujourd'hui je suis dans la nécessité de Vous demander ci pour la Votre Théorie réellement Vous intendez trouver vraies les deux opinions des positivistes à la Compte, c'est à dire; l'*athéisme* et le *materialisme* d'où la *matière*, être absolu, non celui des ontologes au principe de causalité substituée celui des causes à l'infini, c'est à dire, que la série des causes ne finissant jamais ons à pas lieux à se tranquilliser dans une *cause première*.² L'accademie ci-dessu, à pour son but de soutenir la sapience primitive Italique d'où le *Teo* distingué de l'*Ile*. Dieu, Être absolu, principe incréée et nécessairement créateur; cause première des causes secondes.³

Et par la Théorie de l'évolution et par la lutte pour l'existence, justement vos sublimes idées; l'Accademie intende de trouver l'existence d'une intelligence ordonnatrice et prévidente, au lieux de la niér comme les proselytes de Votre Théorie, positivistes fanatiques qui n'entendaient que a faire la guerre au absolu à Dieu, défendu par la saine philosophie que eut le berceau en Italie d'où son primat en ordre au pensée et à la vraie science.

Comme ça les honorables savants de la R. I. Accademie, croyent faire honneur à Vos études, et par mon moyen aujourd'hui il se présentaient à Vous désireux d'avoir une reponse au propos pour avoir raison de en faire partécipe le monde scientifique, par Rome. L'avoir nous comprise Votre opinion externée dans Vos derniers ouvrages attribuant aux animeaux les sens non seulement esthétique, mai aussi le moral jusque au religieux, des simples acts psychologiques rationals dans les formes rudimentales, pouvons nous voir en Vous le plus docte défenseur de *l'animation general de la matière*, et encore on peut comprendre en Vous le savant qui voit dans la conception de la matière des moderne positivistes, qu'il n'est pas licite de dévorcier par Dieu, par l'ésprit, et non pas admettre l'iminent force qui faite passer toutes les attitude de la substance mondiale par la *puissance à l'act*, faisant le parcourir sur le chemin ascendant qu'il s'appelle progres cosmique avec intelligence et intentionnalité. En conclusion, illustre Monsieur, pour Votre Théorie glorieusement objectivée avec Votre nom insigne, par tous les savants de l'Europe, la cause de Dieu et celle de la science, est une!

Veuille Dieu Vous garder longuement en honneur des sciences, que moi avec toute mon esprit je ne cèsserai jamais d'être | Votre admirateur obsequieux |
Comm. Prof: G. E. Mengozzi M.D. | Presidente Eff. Reale ed Imper. Accademia |
La Scuola Italica in Roma.

À l'illustre Monsieur | Charles Darwin M.A., F.R.S. ect. | Down Beckenham Kent

DAR 171: 153

¹ For a translation of this letter, see Appendix I. Mengozzi's French (not his native tongue) was extremely poor.

² Auguste Comte founded the philosophical and political movement known as positivism. He argued that humanity passed through three stages: the theological, the metaphysical, and the positive or scientific (*DSB*).

³ Mengozzi belonged to a group that aimed to restore the ancient wisdom of Italy and develop a new nationalistic philosophy based on the teaching of Pythagoras, whom he believed to be Tyrrhenian not Greek. To this end, he had founded the Accademia Nazionale, la Scuola Italica in 1860 (Giudice 2016, pp. 90–3). Francis Darwin later wrote on this letter ‘Mengozzi (fool)’.

From R. F. Cooke 19 October 1880

50^A. *Albemarle St. | W.*
Oct 19. 1880

My dear Sir

I return you Reinwald’s letter & beg to inform you that the actual cost of making a set of Electros from the woodcuts in yr new book will be £10.¹

But really you should make these foreign publishers pay more for at the usual rate of charging per inch &c they w^d. be over £40.

Suppose you mentioned £20.

We have a set now ready to go to Stuttgart & sh^d. like to know your feelings as to the German Translation also.

We thought of charging them £25 or £30.

This sum would help to repay your own expenses on the work.

Your wishes as to 60 copies with cut edges shall be attended to.²

The work is to be bound up in green as usual I suppose? What shall the lettering on the back

The
Movement
in
Plants

—————
Darwin.

Mess^{rs}. Clowes³ tell us they have just received the last sheets from you,

Yours faithfully | Rob^t Cooke

Cha^s. Darwin Esq

DAR 171: 509

CD ANNOTATIONS

8.4 Movement] *altered to ‘Movements’ pencil*

8.5 in] *altered to ‘of’ pencil*

End of letter: ‘Review | List of Copies | 25 Koch⁴ pencil

¹ See letter to R. F. Cooke, 16 October 1880 and n. 1. The electrotypes were for the French translation of *Movement in plants* (Heckel trans. 1882), published by Charles-Ferdinand Reinwald.

² CD had requested sixty presentation copies of *Movement in plants* with the pages already cut (see letter to R. F. Cooke, 16 October 1880 and n. 3).

³ William Clowes & Sons were the printers used by John Murray publishers.

⁴ CD agreed that his German publisher, Eduard Koch, should pay £25 for the use of the electrotypes (see letter to R. F. Cooke, 20 October 1880).

To R. F. Cooke 20 October 1880

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

Oct. 20— 1880

My dear Sir

I am much obliged for your note & kind regard for my interests.¹ As M. Reinwald felt doubt about paying for Translator & bringing out my book, I promised him that he sh^d have clichés at cost price, so I must keep my word & please let him have them at this price, viz 10£—, *if he finally decides to have translation.*—²

As my Books sell well in Germany it will, from what you say, be fair to charge Herr Koch 25£;; but he must not know that I have let the Frenchman have them for 10£.—³

The volume had better be in green to match the others & lettered

“The
Movements
of
Plants

—————
Darwin”

I hope that the index-maker has completed his job.—⁴

Will you be so good as to send copies to whatever Reviews you think desirable: I w^d suggest *Nature*, *Gardener’s Chronicle*, & *Journal of Botany*.⁵

Before long I will send a list of copies to be distributed in England, & will despatch myself those for abroad.—⁶

My dear Sir | Yours faithfully & obliged | Ch. Darwin

National Library of Scotland (John Murray Archive) (Ms. 42152 ff. 376–7)

¹ See letter from R. F. Cooke, 19 October 1880.

² See letter from C.-F. Reinwald, 14 October 1880. Charles-Ferdinand Reinwald published the French translations of CD’s works.

³ See letter from R. F. Cooke, 19 October 1880 and n. 4. Eduard Koch published the German translations of CD’s works.

⁴ The indexer was Matilda Smith (see letter to John Murray, 10 July 1880 and n. 2).

⁵ Reviews of *Movement in plants* appeared in *Gardeners’ Chronicle*, 27 November 1880, pp. 692–3, and *Journal of Botany* 19 (1881): 375–81. No review of *Movement in plants* has been found in *Nature*.

⁶ For the presentation list for *Movement in plants*, see Appendix IV.

To T. H. Farrer 20 October 1880

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

Oct 20th 1880

My dear Farrer

What a man you are to do thoroughly whatever you undertake to do! The supply of specimens has been magnificent, & I have worked at them for a day & a half.¹ I found a very few well rounded grains of brick in the castings from over gravel

walk, & plenty over the hole in field & over Roman floor. You have done me the greatest possible service by making me more cautious than I sh^d otherwise have been, viz by sending me the rubbish from the road itself; in this rubbish I find very many particles, rounded (I suppose) by having been crushed, angles knocked off, & somewhat rolled about. But not a few of the particles may have passed through the bodies of worms during the years since the road was laid down.— I still think that the fragments are ground in the gizzards of worms, which always contain bits of stone; but I must try & get more evidence. I have today started a pot with worms in very fine soil, with sharp fragments of hard tiles laid on the surface, & hope to see in the course of time whether any of these become rounded.² I do not think that more specimens from Abinger would aid me.

With hearty thanks for all your most kind assistance, I am | My dear Farrer | Yours sincerely | Charles Darwin

Eliz. Wedgwoods progress is, I fear, decidedly downwards, but she had a rather better night.—³ She suffers greatly from extreme restlessness & short-breathing. Her life, I think, is a misery to her.

Linnean Society of London (LS Ms 299/31)

¹ See letter from T. H. Farrer, 16 October 1880 and n. 5.

² In *Earthworms*, p. 18, CD stated that it was probable that the little stones and similar material swallowed by worms were used like millstones to grind their food.

³ Elizabeth Wedgwood was seriously ill (letter from T. H. Farrer, 9 October 1880 and n. 5); Farrer was married to her niece Katherine Euphemia (Effie) Farrer.

To C.-F. Reinwald 20 October 1880

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

Oct 20th. 1880

Dear Sir

I have just heard from M^r Murray that the actual cost of the 195 woodblocks is 10£; & though M^r Murray tells me it is very unusual to allow anyone to have the clichés at prime cost, I have arranged that you shall have them at this price so as to aid you in bringing out a French Translation.—¹ As soon as you have decided, pray write to M^r Murray, asking him to have the clichés made & reminding him that I have stated that the cost to you is to be only 10£.—

Dear Sir | Yours faithfully | Ch. Darwin

P.S. | I send Mad. Barbier 10£, & I am very much obliged for your kind letter on the subject.—²

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

¹ See letter from R. F. Cooke, 19 October 1880. Cooke was a partner in the John Murray publishing company; he was responding to CD's letter of 16 October 1880. The clichés were the stereotype plates of the woodcuts in *Movement in plants*.

- ² See letter from C.-F. Reinwald, 7 October 1880 and n. 3. CD's payment to Arthémise Barbier (the widow of Edmond Barbier, CD's French translator) was recorded as 'charity' in his Account books—cash account (Down House MS).

From Arthur de Souza Corrêa 20 October 1880

Brazilian Legation. | London | 2^a Granville Place | W.
20th October 1880

Sir,

The Baron de Villa Franca, a great proprietor and cultivator in Brazil, has written the enclosed short memorial in French on the subject of culture and propagation of Sugar Cane, and his great ambition is to procure your appreciation of his facts and opinion of his deduction.¹ For that purpose he has asked me to submit his work to you. Should you be disposed to gratify him by perusing it and giving your opinion upon its merits, I would be extremely obliged for your kindness.²

The only excuse I can make for this intrusion on you is the anxiety of my—countryman to submit his investigations to the highest scientific authority and my own inability to ascertain how to approach you.

Should my application be inadmissible, I will ask you to regard it as another consequence of that fame which has made your name as renowned in the new as in the old world.

Believe me with profound respect | Your faithful servant | A. de Souza Corrêa | Secretary of the Brazilian Legation.

Chas. Darwin Esq^{re}. M.A, F.R.S.

DAR 160: 281

¹ Ignacio Francisco Silveira da Motta, baron de Vila Franca, was a Brazilian politician and farmer.

² The baron de Vila Franca's memorial on the culture of sugar cane has not been found. After further correspondence in 1881 and 1882, CD and George John Romanes communicated baron de Vila Franca's work on new varieties of sugar cane to the Linnean Society on 6 April 1882; the paper was read on 4 May 1882, but not published (*Proceedings of the Linnean Society of London* (1880–2): 30–1; Linnean Society, SP register, reference 2371). The paper was also mentioned in *Journal of Botany: British and Foreign* 20 (1882): 192. CD's draft of his and Romanes's commentary on the paper is in DAR 207.4.

To T. H. Thomas 22 October 1880

Down, | *Beckenham, Kent.* | (*Railway Station* | *Orpington. S.E.R.*)
Oct 22^d 1880

Dear Sir

I am much obliged to you & to Prof. Schaafhausen for the photographs, which are very interesting.—¹ You were indeed fortunate to find such fine foot-marks, which seem to me excellently engraved.²

Dear Sir | Yours faithfully | Ch. Darwin

The National Library of Wales (NLW MS. 3127C no.12)

*Series of five three-toed footprints, Dolomitic Conglomerate, Newton Nottage, Glamorganshire: $\frac{1}{18}$ th nat. size.
Drawn by T. H. Thomas Esq.*



Quarterly Journal of the Geological Society of London 35 (1879): 512.
(The plate is reproduced here at the same size as in the original publication.)
By permission of the Syndics of Cambridge University Library.

- ¹ The photographs sent by Thomas and Hermann Schaaffhausen have not been found.
- ² Thomas had discovered fossil footprints in the Triassic rocks of Glamorgan, Wales; his article in the *Transactions of the Cardiff Naturalists' Society* describing the discovery contained two lithograph plates reproduced from drawings he had made (Thomas 1879). The discovery was also communicated to the Geological Society of London in early 1879 together with a lithograph of one of Thomas's drawings (Sollas 1879). See plate on p. 349.

From Arthur de Souza Corrêa 23 October 1880

Brazilian Legation. | *London*
23^d October 1880

Sir,

I have to express my own gratification and by anticipation the thanks of the Baron de Villa Franca for the gracious and instructive manner in which you have replied to the advances I made to you in some apprehension that it might be troubling and disturbing you.¹ But it is one of the advantages of science I find in you to at once welcome strangers as to teach mankind.

I will forward your precious letter to the Baron de Villa Franca, and it will no doubt urge him to fresh efforts to add to that collection of facts which it is for you to generalise.²

I have the honor to be with profound respect | Your faithful Servant | A. de Souza Corrêa.

Charles Darwin Esq^{re}. F.R.S.

DAR 160: 282

- ¹ The memorial of Ignacio Francisco Silveira da Motta, baron de Vila Franca, on the culture and propagation of sugar cane had been sent to CD for comment (see letter from Arthur de Souza Corrêa, 20 October 1880).
- ² CD's letter has not been found.

To ? 23 October 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
October 23 1880

Dear Sir

I am much obliged for your courteous letter of Oct 8th.—¹ I have no difficulty in answering your questions; but I cannot see how my answers can be of interest to anyone— I was born on Feb. 12th 1809.— On my return home after the voyage of the *Beagle*, I opened my first note-book for facts bearing on the Origin of species in July 1837.² In June 1842 I wrote a brief sketch of the notions then arrived at; & this was enlarged in 1844 into a sketch of 230 M.S. pages.³ The Origin of Species was published near the close of 1859.—

Dear Sir | Yours faithfully | Charles Darwin

Copy

Cleveland Health Sciences Library (Robert M. Stecher Collection)

¹ No letter to CD dated 8 October 1880 to which this might be a reply has been found.

² CD's 'Notebook B' is DAR 121; for a transcription, see *Notebooks*, pp. 167–236.

³ There are various rough drafts and fair copies of CD's species sketches of 1842 and 1844 (see DAR 6: 1–14, 16–50 for the 1842 sketch and DAR 7 and DAR 113 for versions of the 1844 essay).

From S. T. Preston 24 October 1880

25 Reedworth Street | Kennington Road SE. | London.

October 24th 1880

Dear Sir

In perusing my letter of August 5th again, it strikes me that there is an appearance of pretentiousness in the mode of my allusion to Mr Herbert Spencer's "Data of Ethics" in relation to the Essay on "Natural Science & Morality" by myself and friend.¹ I can only say that this was not my intention, and my object was to express a certain amount of gratification at the apparent agreement of principles *as far as they went* in the comparatively very brief Essay by self and friend. As however one cannot in correspondence have the great advantage of knowing the impression one has created and of ascertaining whether the intended meaning has been conveyed (which is possible only in an *oral* communication): and as I should be sorry that the wording of my letter should convey an impression of the above kind (which I think it legitimately might do)—I am therefore induced to write these few lines, and as no correspondence is called for, I hope I shall not be thought to be unduly troubling you in alluding briefly to this matter.

Your's truly | S Tolver Preston

Charles Darwin Esqr FRS &c—

DAR 174: 64

¹ In his letter of 5 August 1880, Preston had stated that some of the conclusions reached in his essay on natural science and morality (Preston 1880a) read like a popular exposition of parts of Spencer 1879. Preston's essay was based on the privately circulated pamphlet he had written with William Sharpey Seaton on physics and ethics ([Seaton] 1879).

To S. T. Preston 25 October 1880

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

Oct. 25th., 1880.

Dear Sir

I did not at all take the impression which you thought I might have done from your note.¹

I read you article on Woman with much interest, though I differ on some points,—as when you speak of inheritance as draining qualities from man—and

about antiquity of mental differences in man and woman.² But I really have not strength or time for correspondence. I wish you success in all your investigations.

Dear Sir | Yours faithfully | Ch. Darwin

Copy

DAR 147: 251

¹ See letter from S. T. Preston, 24 October 1880.

² Preston's article 'Evolution and female education' argued in favour of the intellectual training of women on the grounds that this would also advance the brain development of men through the equal transmission of characters to children of both sexes (Preston 1880c)

From W. D. Roebuck to G. H. Darwin 25 October 1880

Yorkshire Nat. Union | Sunny Bank, | Leeds,

Oct. 25th 1880

Dear Sir/

Prof. Williamson has handed to me your letter with regard to the visit of a small deputation to Down for the purpose of presenting a memorial address to your distinguished father—¹

The Committee have now finally completed their arrangements, and, in order to avoid the delay which would arise from my writing to Prof. Williamson & asking him to inform you, I hasten to communicate them to you at once, hoping they will meet the convenience of M^r: Darwin & family.

It has been arranged that the deputation shall proceed from London by the 11 a.m. S.E. train for Orpington on Wednesday the 3rd of November. The deputation wish to put M^r: Darwin to as little fatigue & inconvenience as possible and will consequently leave Down after the presentation in time to take an early afternoon train back to London.

The deputation will be headed by H. C. Sorby Esq LL.D., F.R.S., late president of the Union (Prof Williamson's University duties preclude his being able to take his position as President for the present year); he will be accompanied by Messrs. Tho^s: Hick BA., B.Sc., J. W. Davis FSA. &c. William Cash F.G.S., possibly also by D^r: H. F. Parsons FGS., and a gentleman from Huddersfield whose name I will send you when I learn it.² We hope there will not be too many for your convenience, but the difficulty has been to limit the number of our members who wish to do honour to the greatest biologist of our time—

Any further particulars I shall be glad to supply.

Believe me, Yours truly | W^m: Denison Roebuck.

DAR 176: 191

¹ William Crawford Williamson, president of the Yorkshire Naturalists' Union, had written to Emma Darwin to enquire whether CD would accept a visit from a small deputation of members who wished

to congratulate him on the ‘coming of age’ of *Origin* (letter from W. C. Williamson to Emma Darwin, 2 September 1880). The reply stating this would be acceptable, evidently written by George Howard Darwin rather than Emma Darwin, has not been found.

- ² The gentleman from Huddersfield was George Brook (see letter from W. D. Roebuck to G. H. Darwin, 26 October 1880).

From S. T. Preston 26 October 1880

25 Reedworth Street | Kennington Road SE. | London.
October 26th 1880

Dear Sir

I thank you much for your kind reply to my letter, which it was very satisfactory to me to receive.¹ I would merely say that in my article on “Evolution & Female Education”, I took the reference from your work “The Descent of Man” to imply that had it not been for the “law of equal transmission of characters to both sexes”, woman would have fallen behind man in mental endowment to a very large extent (as illustrated by the comparison employed in the quotation).² I wished to convey the idea therefore that from the fact that woman has *not* fallen behind to this extent, she must (in effect) have *gained* somehow this amount, i.e. the amount which she would have lost, had not this “law of equal transmission” come to the rescue— in other words, that the total loss attendant on woman’s inaction (of brain) has been distributed with (approximate) equality on both sexes by the law of equal transmission, instead of being thrown entirely *on one side* so as to produce the marked and palpable inequality which would otherwise have resulted. This is the interpretation which (to the best of my ability) I attached to the passage quoted from your book, [*vz.*] this is substantially the meaning I intended to convey in speaking of “inheritance as draining qualities from man”.

I trust that in this sense I may be substantially correct (and that any points of difference that may exist in regard to my paper may be *minor* ones)—not wishing to lead to any correspondence.³ But I thought the cause of Female Education &c was a good one that might be worthy of any additional encouragement, provided the means be legitimate.

Again thanking you for your last letter. | Yours truly | S Tolver Preston

Charles Darwin Esqr FRS &c—

DAR 174: 65

¹ See letter to S. T. Preston, 25 October 1880.

² In Preston 1880c, Preston quoted CD’s statement from *Descent* 2d ed., p. 565: ‘It is, indeed, fortunate that the law of the equal transmission of characters to both sexes prevails with mammals; otherwise it is probable that man would have become as superior in mental endowment to woman, as the peacock is in ornamental plumage to the peahen.’

³ In his letter to S. T. Preston, 25 October 1880, CD mentioned that he had no strength or time for correspondence.

From Anthony Rich 26 October 1880

Chappell Croft, | Heene, Worthing.
Oct^{ber}. 26. 1880.

My dear M^r Darwin

I received a friendly letter from you in the early part of last spring just before you were about to settle down to several months slavery under the imperious rule of Murray and his printer's devils; and as discretion is the better part of many other virtues besides valour, I thought it my duty not to add the worry of an idle correspondent to the irritations of incessant calls from the printers boy for "more copy".—¹ So that letter has gone unacknowledged up to the present moment. But the spring has long passed, the summer is gone, and autumn is well on the move; the "Movement of Plants" figures conspicuously amongst M^s Announcements; and the severe weather which we have lately experienced will probably have driven you home again, if you had gone out to refresh yourself by sea side or on mountain tops as a "reward of merit" for past exertions. Thus my time has come round when I think that I may fairly write and ask how it has fared with you and yours during all that time—and now. For myself I have got through the season without any special checks or ailments; the summer on the whole I have found to be a pleasant one, warm and sunshiny without being over hot, and, with the exception of a single fortnight about the time which is supposed to obey the rules of S^t. Swithin, pleasantly dry underfoot and overhead so as to allow of sitting in the open air without discomfort.—²

The *Philadelphus* you so kindly sent to me flourished and flourishes.³ It has made shoots six feet long direct from the ground, and their wood has thoroughly ripened. I suppose that it will be proper to shorten them by and by, if the plant is to be kept under control. It bore three magnificent bunches of flowers, which seemed to have special attraction for a particular kind of fly with a black body and sharp pointed tail nearly twice the length of the common house fly. There were always four or five of them about the flowers with their heads deep into the bottom of the cups, like bees. I never observed these flies about any other of the flowers in the garden, or at any other time or any where else.—

I possess likewise a fine plant of *Berberis Darwinii*,⁴ which has flourished proudly with its long sprays of orange coloured blossoms in my plantation for ten goodly years, but is now getting out of all bounds, with a ragged, stragling, and dissipated air. Should it not be cut down, or shortened? or is it, as some plants I understand are given to be, recalcitrant to surgical operations? Do pray enlighten my darkness.—

Sometime during the summer I saw an announcement in one of the Papers of the marriage of a M^r. Huxley, of Penge, I think, and fancied that it might be a son of the Professor.⁵ The name is not a common one, as far as I know. If I could have satisfied myself that it was so, I am not sure that I would not have made it an excuse for writing a note to him, trusting that congratulation upon such an event, in itself an act of civility, would not be liable to the charge of officiousness

Yes, truly must Sir J. Lubbock laugh in his sleeve whenever he passes through that glorious borough—incorruptible of course—which dismissed him from its bosom to be embraced by the intelligent constituency of the London University—a seat

for life, I imagine, and no “honest independent *woters*” to study.⁶ And I thoroughly agree with you that science is a more useful, a much better friend, to all of us than law. The worst members that a constituency can choose for a seat in parliament have always appeared to me to be those of the legal profession. It is so obvious that they can only seek the seat for their own private interests; and the political arena forms but an indifferent school for a seat on the bench of Justice.—

Now then it is time for me to relieve you from the trouble of reading these lucubrations; and how can I do that better than by asking you to call me to the remembrance of M^{rs}. Darwin, and to present her with my compliments and respects?— I hope that she thoroughly approves the change that has taken place in the *personel* of our Government. It is so pleasant to talk politics to ladies. They are such excellent politicians—when they are on our sides. Though I cannot expect that M^{rs}. Darwin will approve of all my wilful radicalism, I feel sure that she will feel not less satisfaction than I do in the thought that we are not to be Orientalized—at least for the present, or, let us hope, for evermore;⁷ for which time I intend to remain |
Very truly yours | Anthony Rich

DAR 176: 143

CD ANNOTATION

Verso of last page. ‘[illeg] | Huxley | [2 words illeg] blue ink

- ¹ CD’s letter has not been found. He spent the spring finishing *Movement in plants*, and then correcting the proof sheets (CD’s ‘Journal’ (Appendix II)). His publisher was John Murray; ‘printer’s devil’ was the name given to a young assistant in a printing house.
- ² According to folklore, whatever the weather is like on St Swithin’s day (15 July) it will continue the same for the next forty days.
- ³ The *Philadelphus* (a genus of mock-orange) had been growing in Rich’s garden since at least autumn 1879 (see *Correspondence* vol. 27, letter from Anthony Rich, 28 December 1879).
- ⁴ This species of *Berberis* was collected by CD on Chiloe Island, Chile, during the *Beagle* voyage; it was described in 1844 by William Jackson Hooker, who named it *Berberis darwinii* (W. J. Hooker 1844; see also *Correspondence* vol. 2, letter from J. D. Hooker, [12 December 1843 – 11 January 1844] and n. 8).
- ⁵ According to the *Standard*, 23 July 1880, p. 1, George Thomas Scott Huxley was married at Holy Trinity Church, Penge, Kent. He was not Thomas Henry Huxley’s son.
- ⁶ John Lubbock had been the MP for Maidstone, Kent, from 1870, but after losing the seat in 1880, he was elected MP for the University of London (*ODNB*).
- ⁷ The Tory prime minister Benjamin Disraeli had been defeated in the April 1880 election by the Liberal William Ewart Gladstone. Disraeli, who was of Jewish descent, looked to the ‘Orient’ and eastern philosophy as a source of wisdom (Kalmar 2005). Rich had commented on Disraeli’s ‘orientalism’ in his letter of 7 March 1880. Emma Darwin had been particularly critical of Disraeli’s proclamation of Queen Victoria as empress of India in 1876 (letter from Emma Darwin to Leonard Darwin, 26 March [1876?] (DAR 239.23: 1.41)). This act had led to Disraeli’s being lampooned as an eastern potentate, corrupting the British monarchy (M. Taylor 2004).

From W. D. Roebuck to G. H. Darwin 26 October 1880

Yorkshire Nat. Union | Sunny Bank, Leeds
Oct. 26th, 1880

Dear Sir/

I believe in my letter of yesterday I omitted to ask you whether the date provisionally arranged by our deputation was convenient— If not we will arrange to suit you—¹

I am now able to give you exact information as to the composition of the deputation. The gentleman from Huddersfield will be M^r. George Brook FLS., my colleague in the secretaryship of the Union. In other respects the names stand as I first wrote you, except that official duties prevent D^r. Parsons² from taking part.

I think it as well to send you, which I do by this post, one of the copies of the Address which will be given to a few of our members.³ The wording of the address is repeated in the copy & the signatures are traced from the original—

Trusting you will not scruple to alter our arrangements if necessary | Believe me
| meanwhile | Yours truly | W^m. Denison Roebuck | Sec

Geo. Darwin Esq

DAR 176: 192

¹ See letter from W. D. Roebuck to G. H. Darwin, 25 October 1880.

² Henry Franklin Parsons.

³ CD's copy has not been found. The address was later published in *Nature*, 18 November 1880, p. 57, in an article titled 'Homage to Mr. Darwin', which described the visit to Down House of the delegation from the Yorkshire Naturalists' Union and CD's response to the address. For the text of the address, see Appendix III.

To G. E. Mengozzi¹ [before 28] October 1880²

Londra, [Down.]

Ottobre 1880.

Caro signore,

Vi ringrazio per le vostre estremamente cortesi lettere.³ Il tentare una risposta alle questioni che Voi mi avete fatto l'onore d'indirizzarmi (per quanto io le comprenda) sarebbe una lunga impresa, e io sono in debole salute e il lavoro mi affaticherebbe molto. Ma avendo con l'ultima vostra compreso più chiaramente la questione, io volentieri risponderò ad essa come meglio potrò.— Io non credo che nessun essere organico dimostri evidenza di disegno. Se Voi vi date la pena di leggere le ultime due pagine della mia *Variazione degli Animali e delle Piante sotto la domesticazione*, Voi in parte rinverrete le mie ragioni. Ma sebbene nessun organismo può mostrare disegno, ciò in *nessun modo* esclude la credenza nell'esistenza di un amoroso Creatore di tutte le cose. L'evidenza di un tale Creatore bisogna che sia indagata, come a me sembra, ancora fuori dei limiti della Scienza Fisica. Il problema è uno dei più difficili. Dall'altro lato io so che molti uomini, le cui menti sono incomparabilmente più chiare e profonde della mia (ed io non ho mai atteso abbastanza alle questioni metafisiche e religiose) sono convinti che l'evidenza dell'esistenza di Dio è quasi evidente per se stessa.

Mi fo premura accusarvi ricevimento e ringraziarvi per il dono del vostro magnifico volume sulla Filosofia della Medicina.⁴ Di più vi prego ad essere così buono da portare alla vostra Società, *La Scuola Italica*, residente in Roma, i molti miei cordiali ringraziamenti pel grande onore che in sì distinta maniera mi conferiscono.⁵

Pregovi di accettare i miei migliori ringraziamenti per le vostre molto amabili espressioni inverso di me, mentre io rimango, caro Signore, con molto rispetto, | *Professor Mengozzi M. D.* | *Vostro fedelmente e molto obbligato* | CARLO DARWIN.

Roma Etrusca 2 (1881): 10

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

² The date is established by the relationship between this letter and the letter from G. E. Mengozzi, 28 October 1880.

³ The only known letter from Mengozzi prior to 28 October 1880 is dated 17 October 1880.

⁴ CD's copy of Mengozzi 1869, bearing an inscription in Italian by Mengozzi, is now in the Linnean Society.

⁵ The *Scuola Italica*, of which Mengozzi was founder and president, aimed to 'honour' CD's studies by showing that his scientific conclusions were not incompatible with a belief in a creator (see letter from G. E. Mengozzi, 17 October 1880).

From G. E. Mengozzi¹ 28 October 1880

56 Brompton Square S.W.
28 October 1880

Illustre Monsieur,

Samedi ou dimanche prochaine je vais à Rome avec le pensée entièrement tourné à Vous, beaucoup honorée des Vos chères lettres les quelles j'ai lues avec attention et avec profit que s'extrait par chaque parole inspiré par un homme eminent savant.²

À présent je ne puis ni je doive m'étendre sur la question que je Vous avait faite en profitant de la bonte et courtoisie que Vous avez eu pour mes pensées.

Toute la question par nous agitée, pour moi elle se résoudre dans l'harmonie dialectique parmi le savoir des sciences du monde extérieur et celles du intérieur, trouvant le positif et excluant le leur négatif; parce que isolément prises elles n'ont pas, le pouvoir de démontrée clairement l'existence de Dieu. Pour cela les arguments qu'il ce fondaient sur les seules notions du monde extérieure ne suffisaient comme Vous même dites, et en particulier le cosmologique et le théologique, ce dernier s'attendent à Votre doctrine évolutionniste la quelle a tante de matière à discuter.

Moi—sans Dieu je ne comprend ni l'Univers ni la vie, ni la société, et je ne puisse admettre que le monde puisse vivre heureux sans la santification de la religion qui à sa foi dans l'Unique Dieu Créateur! Le livre éternel de Dieu il n'est pas fermé, et la nouvelle page divine, par l'oeuvre de Votre grand esprit et de Vos études profonds, elle sera tournée.

Moi, je crois ainsi toute les fois que je prend à méditer les ouvrages qui Vous avez donnée au monde des penseurs.

Je voudrais bien que tous les hommes avancées dans les sciences physiques et métaphysiques puissent penser, comme moi je pense et j'espère. Quelque ce soient mes pensées et mes desirs, je Vous prie de mes croire avec fraternelle affection, et profonde veneration toujours à Vous | Devoué | Comm. Prof. G. E. Mengozzi M.D.

P.S. Pardonnez-moi la liberté que je me prend, de Vous envoyer Votre portrait au fin de Vous bien voulevoir le orner de Votre Nom august par Votre propre main signé.

Je vais glorieux de le porter dans le sol, que à dire avec Dante, chaque pierre mérite révérence ... !³

DAR 171: 154

¹ For a translation of this letter, see Appendix I. Mengozzi's French (not his native tongue) was extremely poor.

² The only known letter from CD to Mengozzi prior to 28 October is the letter to G. E. Mengozzi, [before 28] October 1880.

³ The reference is to Dante Alighieri's *Il convivio*, book 4, end of chapter 5: 'Certo di ferma sono opinione che le pietre che nelle mura sue stanno siano degne di reverenza' (I am certainly of the firm opinion that the stones of her walls are worthy of reverence). This work was written between 1304 and 1307.

From Florence Dixie 29 October [1880]¹

Glen Stuart, | Annan. | N. B.

"October 29th. Friday.—"

Dear Sir.—

Whilst reading the other day your very interesting account of "A Naturalist's Voyage round the world"—I came across a passage descriptive at Maldonado of the subterranean habits of the tucutuco in which you express the belief that this animal never comes to the surface of the ground.—² I am sure it will be interesting to you to know that tho' this *may* be the usual habits of the tucutuco that there are exceptions. In 1879, I spent 6. months on the Pampas and in the Cordillera Mountains of *Southern Patagonia* and during my wanderings over the plains I have had occasion to notice in places tenanted by the tucutuco, as many as five or six of these little animals at a time outside their burrows. This was on moonlight nights, and I c^d not possibly be mistaken as they w^d frequently come within a yard of the spot on which I lying.— On *two* other occasions I have seen the tucotuco in broad daylight come out of its burrow and shuffle awkwardly along some 20 or 30. yards ere it took refuge in another of the hundreds of holes with which the ground appeared undermined.³ On one of these occasions an indian who was sitting near threw an unfinished stone ball of a bolas which he was fashioning at the animal and killed it.— A dog immediately carried the body off so I was unable to examine it and see whether its eyes appeared blind or not.—⁴ The other one which I caught could see well enough & when I let it go shuffled quickly away.— I feel sure you will forgive me writing what I have done but I felt that what I personally saw w^d be interesting to prove that on some occasions the tucutuco does come to the surface of the ground.—

Trusting you will forgive the seeming presumption on my part I beg to remain | very faithfully yours. | Florence Dixie.

From | Lady Florence Dixie.

DAR 162: 182

- ¹ The year is established by the reference to Dixie's trip to Patagonia in 1879, and by the day in the heading. In 1880, 29 October fell on a Friday.
- ² CD had discussed the tuco-tuco (genus *Ctenomys*) in *Journal of researches* (1860), pp. 50–2, the first edition to have 'Naturalist's Voyage Round the World' as the title on the spine of the book. CD had observed the Brazilian species, *Ctenomys brasiliensis*, in Maldonado (Uruguay); he noted that although numerous, the small burrowing rodents never came out of the ground (*ibid.*, p. 50).
- ³ Dixie was evidently unaware that she had observed a different species of tuco-tuco. She may be referring to Haig's tuco-tuco (*Ctenomys haigi*), which comes above ground between midnight and sunrise to make brief forays to collect the grassy vegetation on which it feeds; she may also have seen the colonial tuco-tuco (*Ctenomys sociabilis*), which is strictly diurnal. Dixie did not mention the tuco-tuco in her published account of her travels in Patagonia (Dixie 1880).
- ⁴ In *Journal of researches* (1860), pp. 51–2, CD had noted that blindness was common in subterranean Brazilian tuco-tucos.

To James Torbitt 29 October 1880

Down, [4 Bryanston Street, London.]
Oct. 29, 1880.

My dear Sir

I am writing in London, as not having been very well of late, I have come here for 2 or 3 days rest.¹

Your secret shall be inviolable, and to make sure in case of my illness or death I have now burnt your letter.² I kindly? sincerely?³ wish you success in your projects, but it is not the least use consulting me, as I never have and never shall attend to any commercial affair.

I suppose the success of your projects would depend on the contingent expenses of the process, though it is obviously a gain for the public to save waste food. I wish you success but pray do not be rash, and remember that though enthusiasm is a virtue it is a dangerous one.

Believe me my dear Sir | Yours sincerely | Ch. Darwin.

Copy
DAR 148: 123

- ¹ CD stayed with Henrietta Emma Litchfield from 28 October to 2 November 1880 (CD's 'Journal' (Appendix II)).
- ² It is unclear whether Torbitt's 'secret' related to his attempts to breed blight-resistant potatoes or to some other process he had developed for saving waste food (A. Evans *et al.* 1996, p. 8). For Torbitt's potato breeding experiments, see the letter from James Torbitt, 26 September 1880.
- ³ These suggestions were probably made by the copyist, who was evidently unable to read the original.

From Bartholomäus Carneri 30 October 1880

Wildhaus.
30 Oct. 1880

My highly honoured Sir!

I hope you will not think that I have taken a liberty in describing in the preface to my "Foundation of Ethics" (which you will soon receive through Braumüller) the appearance of that book as an act of homage due to you.¹

The date of my preface will prove to you that I hoped, not without reason, to have placed the book in your hands by Oct. 1 of this year. A provoking delay in the publication has deprived me of this pleasure. It is not on this account less true that I have to thank you for the oneness of my theory of the world, and therewith for making my Ethics possible. Judge then from this of the sincerity and warmth of the good wishes which I offer you from afar. It is however the kindness with which you have received my former writings which has encouraged me to this act of homage.²

I cannot, my highly honoured Sir, expect from you that you will read this book, but if the case of a cat being suckled by a female dog should be new to you, I beg you will look at page 130.

With a veneration such as one gives only to the greatest men of science, I am &c
| B. Carneri

DAR 161: 49

¹ Carneri's *Grundlegung der Ethik*, dedicated to CD, was published by Wilhelm von Braumüller in 1881 (Carneri 1881).

² Carneri had already sent CD his book on morality and Darwinism as well as his psychological study of emotion, consciousness, and will (Carneri 1871 and Carneri 1876; see *Correspondence* vol. 19, letter to Bartholomäus von Carneri, 17 April [1871], and *Correspondence* vol. 24, letter to Bartholomäus von Carneri, 22 April 1876).

To Francis Galton 30 [October 1880]¹

4. Bryanston St
Sat 30th

My dear Galton

I hear from Litchfield that you have returned.—² If you have nothing special to do, will you come to luncheon here at 1 o'clock, as we sh^d very much like to see you.

Ever yours | Ch. Darwin

UCL Library Services, Special Collections (GALTON/1/1/9/5/7/30)

¹ The month and year are established by the dates of CD's visit to the Litchfields in London (see n. 2, below). In 1880, 30 October fell on a Saturday.

² CD stayed with Henrietta Emma and Richard Buckley Litchfield from 28 October to 2 November 1880 (CD's 'Journal' (Appendix II)); Francis Galton may have been away on a recuperative trip following illness over the summer (see letter from Francis Galton, 5 July 1880).

To James Murie 30 October 1880

4. Bryanston St | Portman Sqe¹
Oct. 30th 1880

M^r Ch. Darwin w^d be much obliged to the Librarian,² if he w^d give Bearer the Vol. of the "Zoologist" which has an article dated "October 1849".³ This is the sole reference given.—

Linnean Society of London (LL/4)

¹ This was the London address of Henrietta and Richard Buckley Litchfield; CD stayed with them from 28 October to 2 November 1880 (CD's 'Journal' (Appendix II)).

² Murie had been appointed librarian of the Linnean Society on 18 October 1880 (see letter from W. S. Dallas, 6 October 1880, n. 2).

³ 'On leaves adhering to the casts of worms' by William Kencely Bridgman was published in the *Zoologist* 7 (1849): 2576–7. CD later referred to this article in *Earthworms*, p. 20 n. and p. 33 n.

To A. B. Buckley 31 October [1880]¹

4 Bryanston St. Portman Sq.
Sunday Oct. 31.

Home tomorrow or early on Tuesday²

Private

My dear Miss Buckley

Some time ago I spoke to Sir J. Lubbock about Wallace and a Government pension, and this morning I produced a decided effect on Huxley.—³ He has asked me to draw up a full, but *condensed* statement of Wallace's claims; and he will then endeavour to talk over Hooker and Spottiswoode.—⁴ Therefore I think there is a fair *chance* of getting up a memorial to Government.— When I began to think over the case, I found myself very deficient in knowledge, and bethought me that you with your generous spirit would aid me. I have written down some questions, which will serve me as memoranda when I get home, and when I will lose no time.— If I were to ask Wallace any of these questions he would think me mad or impertinent.— (He perhaps would think that you intended writing sketch of his life in some Journal.) Perhaps you can answer some, or get answers by some indirect manner from him.— Any hints or advice of any kind would be of greatest value.— Especially about his present circumstances. You will understand these materials are solely for Huxley, Hooker and perhaps 2 or 3 others' consideration.— The Government Memorial will be a separate consideration. I do *most earnestly* hope that we may succeed.

I know well, busy as you are, that you will help me as far as lies in your power.

Believe me, My dear Miss Buckley | Yours sincerely | Charles Darwin

Huxley feared that even if we could get a memorial signed by a few first-rate men, yet it might be extremely difficult to get a pension on account of the scandalous manner in which these pensions are jobbed.—

Therefore it seems very desirable that Wallace should hear nothing about it

Copy

DAR 143: 182

¹ The year is established by the reference to a government pension for Alfred Russel Wallace. In 1880, 31 October was a Sunday.

² CD was staying with his daughter Henrietta Emma Litchfield in London; he returned home on 2 November 1880 (CD's 'Journal' (Appendix II)).

³ John Lubbock and Thomas Henry Huxley. CD had first considered petitioning for a government pension for Wallace in December 1879 after being alerted by Buckley to Wallace's financial difficulties

(*Correspondence* vol. 27, letter from A. B. Buckley, 16 December 1879, and letter to A. B. Buckley, 17 December 1879).

- ⁴ In December 1879, Joseph Dalton Hooker had discouraged CD from attempting to obtain a government pension for Wallace (*Correspondence* vol. 27, letter from J. D. Hooker, 18 December 1879). Hooker had served as president of the Royal Society of London from 1873 to 1878; William Spottiswoode was the current president (*ODNB*).

To John Crier November 1880

Down. | Beckenham

Nov 1880

J. Crier Esq. | Registration Office | G^t Wⁿ. R^y. Station | Paddington, W.

Sir

I write as to my £3400 Monmouthshire R^y Co. Stock in answer to y^r. printed circular of 4th. inst as to the issue of a new Certificate on my giving an Indemnity.¹

I have no reason to believe that I have ever had in my possession any formal “certificate” of this Stock, and I have always regarded the papers I sent to you on the 3rd. instant as a sufficient evidence of my ownership.² Those papers are all the documents I have had in reference to the stock.

If under these circes and after looking at the papers sent on the 3rd. inst. (w^h. I shall be obliged by y^r. returning if *not* of any use for the purpose) you think it necess^y. for me to give an Indemnity to the Co.^y I will do so and in this case I name as a “Surety”

Richard Buckley L^d. Esquire, Barrister at Law, of the Inner Temple and of N^o4 Bryanston Street Portman Square London.³

I hold and can forward to you the upper half of the latest Div^d. warrant of the Monmouthshire R. & C. Comp^y—6 Sept. 1880⁴ and I have not in any way alienated or dealt with the s^d £3400 Stock

I am Sir | Y^rs. Faithf^l | C R D

Draft

DAR 202: 37

¹ CD had owned shares in the Monmouthshire Canal and Railway Company since 1846; he evidently planned to convert this holding into Great Western Railway shares. An entry in his Investment book for 15 November reads: ‘I now hold 3400 stock. This has been converted into 4420, 5 per cent Consolidated Guaranteed Stock of the Gt Western Railway’ (CD’s Investment book (Down House MS)).

² No other papers or correspondence with Crier have been found.

³ Richard Buckley Litchfield was CD’s son-in-law; this draft is in his hand.

⁴ A dividend of £125 18s. 6d. is recorded in CD’s Investment book for September 1880.

From W. D. Roebuck 1 November 1880

Yorkshire Nat. Union | Sunny Bank, Leeds,

Nov. 1st 1880

Dear Sir,

I venture to take the liberty of forwarding to you by this post copies of such parts of our publications as have been issued, together with a set of our excursion

circulars, in anticipation of anything the deputation may have to say with respect to our objects and the means adopted for attaining them—¹

Apologizing for troubling you so many times | Believe me | Yours truly |
W^m. Denison Roebuck | Sec. Y.N.U

Charles Darwin Esq

DAR 202: 130

¹ Roebuck was arranging for a deputation from the Yorkshire Naturalists' Union to present a memorial address to CD at Down (see letter from W. C. Williamson to Emma Darwin, 2 September 1880, and letter from W. D. Roebuck to G. H. Darwin, 25 October 1880). The publications and circulars have not been found in the Darwin Archive-CUL; Roebuck sent issues of the *Transactions of the Yorkshire Naturalists' Union*, which had commenced publication in 1877 (letter to W. D. Roebuck, 3 November 1880).

To Volney Rattan 3 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
November 3^d 80

Dear Sir

I am very much obliged for the seeds of the *Megarrhiza* & for the information which I derived through your letter to Dr A. Gray.¹

I will despatch to you in 2 or 3 weeks a book, in which I give the results of my observations on this interesting plant, but I had not specimens enough.—²

Dear Sir | Yours very faithfully | Ch. Darwin

University of California Berkeley, Bancroft Library (BANC MSS 74/78 z)

¹ Asa Gray had enclosed a letter from Rattan with observations on the germination of *Megarrhiza californica* (a synonym of *Marah fabacea*, California manroot); Rattan had also sent seeds of the plant to Gray (see letter from Asa Gray, 4 April 1880 and enclosure).

² See *Movement in plants*, pp. 81–3. Rattan's name appears on CD's presentation list for the book (Appendix IV).

To W. D. Roebuck 3 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 3rd 1880

Dear Sir

I am much obliged for the Transactions which I was glad to look at before the arrival of the Deputation.¹

The Address which was presented to me is certainly one of the greatest honours ever paid to a scientific man.— It is admirably expressed, & the engrossing seems to me an exquisite work of art.²

I fear that I by no means deserve all that is said of me in the address; but it shows the great kindness & sympathy of the senders.

Pray accept my best thanks for all the kind interest which you have shown in the affair & believe me | Dear Sir | Yours faithfully | Charles Darwin

Leeds University Library Special Collections (SC MS 429/89)

- ¹ Roebuck had sent issues of the *Transactions of the Yorkshire Naturalists' Union* (see letter from W. D. Roebuck, 1 November 1880 and n. 1). He had arranged for a deputation from the union to present CD with a memorial address on 3 November (see letter from W. D. Roebuck to G. H. Darwin, 25 October 1880).
- ² Engrossing; decorative writing in large letters (*OED*). The address was published in *Nature*, 18 November 1880, p. 57 (see Appendix III); the original has not been found in the Darwin Archive-CUL.

To A. R. Wallace 3 November 1880

Down, | Beckenham, Kent. | (*Railway Station | Orpington. S.E.R.*)
November 3^d. 1880

My dear Wallace

I have now read your book, & it has interested me deeply. It is quite excellent, & seems to me the best book which you have ever published; but this may be merely because I have read it last.— As I went on, I made a few notes, chiefly where I differed slightly from you; but God knows whether they are worth your reading.¹ You will be disappointed with many of them; but they will show that I had the will, though I did not know the way to do what you wanted.

I have said nothing on the infinitely many passages & views, which I admired & which were new to me. My notes are badly expressed; but I thought that you w^d excuse my taking any pains with my style. I wish that my confounded hand writing was better.—

I had a note the other day from Hooker, & I can see that he is *much* pleased with the Dedication.²

With all good wishes | Believe me | Yours sincerely | Ch. Darwin

In 2 or 3 weeks you will receive a book from me;³ if you care to know what it is about, read paragraph in Introduction about new terms & then the last chapter & you will know whole contents of book.—

[Enclosure]

p. 46.— I am sure that I have read of a *Mus* from Viti Is^d, but this may have been introduced. I am nearly sure that Günther has described Mammals from New Hebrides, & French-man from New Caledonia, but perhaps you w^d hardly call latter oceanic Is^d

p. 68 I most heartily concur about separated genera of same family: I cautioned Günther on this account before he published his Tortoise paper.⁵

p. 72 You probably know more than I do about distribution of Land Mollusca over Pacific, but I think there must be some **far** *more effective* means of dispersal than rafts, or floating trees. D^f Gould showed how every islet in the Pacific has land-shells.⁶

p. 157 I heartily agree about N. Zealand. When Hutton speaking of the extinction of all temperate forms during a glacial period, he overlooks probability (as it seems to me) of former land (or approximate island^s) communication to the North, whence, as I suspect, N. Zealand was formerly stocked.—⁷

p. 172 Is it not rather rash to refer paucity of fossils to coldness of waters, seeing how wonderfully rich the bottom of sea has just proved off the N. coast of Siberia,—not to mention the abyssal regions of the great oceans. May not paucity be due to the stirring up of the bottom by icebergs?⁸

With respect to your Glacial Chapt^r, my opinion is worth very little, as the subject is so difficult. But as far as I can judge, your view seems the most probable ever suggested.⁹ Until reading your book, I had quite rejected the Lyellian doctrine, but joined to the influence of the form of the land on sea-currents the case has a very different aspect.¹⁰ I had also felt a good deal of difficulty in Croll's views, as far as I could follow them.¹¹ I think that you have rendered improbable any great number of true glacial periods.¹² I still feel much difficulty about the plants & great Saurians &c of the Arctic regions.¹³ If much warm water was poured into the Arctic basin & got chilled, would not the return currents lower the temp^{re} of whole tropical seas (or are these too extensive) & so lessen your source of heat. It seems to me a serious omission that you do not explain what geograph. change coincided with or caused the *cessation* of the last glacial period; for if it was caused by coincident excentricity & geograph. changes, its cessation would equally require geograph. changes. From my son George,¹⁴ who read these chapters with much interest, & admired the clearness & vigour of the discussion, I could not extract any judgment, on account of the many doubtful meteorological points. He demurs to your use of term "epoch", & says that in astronomy it is used for a definite point of time & not for a period.—One speaks of an epoch in history.—

Chapter X. I cannot feel content with your 28 million years, but solely on geological grounds (*see addendum)—viz when I think of the Chalk—successive coal-beds—nummulitic rocks, & a wide-spread of conglomerate in Andes, which I estimated at least of 10,000 ft in thickness.— But my chief difficulty lies in the cases where one side of fault in solid rock has been raised above 10,000 ft. & yet the surface betrays nothing & resembles that of the whole surrounding country. This amt of denudation at your rate of 1 ft per 3000 years would require 30 million of years; & during many a long period the surface must have been submerged & saved from wear & tear.¹⁵

It might be argued that you overestimate the importance of climatal changes & migration in the modification of species, unless you guard yourself by saying that it applies only where there is no retreat for them. For how little the marine molluscs have changed since before glacial period! I accounted for this fact by their having slowly migrated all in a body together, as I believe that the interaction of organisms is much more important than climatal changes.

Azores. The discussion seems to me *excellent*.¹⁶ I formerly came to same conclusion with you, but believe that I attributed a little more to stranded icebergs & coast-ice,

for I have a vague remembrance of some glacial deposit on northern shores.—¹⁷ God knows where the reference is. I have, also, somewhere a M.S. on the straggling birds, sent to me in answer to a letter on subject, by a scientific consul there, many years ago.—¹⁸

Galapagos.— I regret that you have not discussed plants. Perhaps I overvalue these Isl^s, for how they did interest me & how they have influenced my life, as one main element of my attending to origin of species.

You see that I have gone on writing as I read, & on almost next page there comes discussion of Galapagos Flora!¹⁹

(p. 295. No doubt preoccupation with plants is very important; but if a new form has any considerable advantage it tells, as I believe, very little. I have read several accounts of European plants occupying ground, in New Zealand, which had never been touched by the hand of man.— So with guava bushes in Tahiti.— But the Pampas offers the most flagrant instance against what you say.—²⁰

Ch. *Gr. Britain*. This seems to me **first rate** & includes very much matter quite new to me.— How curious about the Irish F.W. fishes! As your book will be sure to run through several editions, I advise you to look to changes in trout (due to direct action of conditions) in different rivers in N. Zealand in course of some 10 years.— See “Arthur in Transact N. Zealand Institute Vol XI 1878 p. 284.”—²¹

p. You might possibly like to hear that it is said in the “Voyage a l’isle de France par un Officier du Roi” who visited the island in 1870 that a fresh-water fish the Gourami had been introduced from Batavia & had multiplied (as well as Gold-fishes) in Mauritius. He also says (p. 170) “On a essayé, mais sans succès, d’y transporter des grenouilles, qui mangent les œufs que les moustiques déposent sur les eaux stagnantes”. It thus *appears* that there then were no frogs on island.—²²

This Madagascan Ch: seems to me one of best in book. How well you show here & elsewhere the importance of changes in the inhabitants of the adjoining continent. I hope that you have destroyed Lemuria for ever: I never believed in it for a minute.²³

I am quite inclined to believe in your Australian views; they are wonderfully ingenious, but almost too audacious for me. My old brain, perhaps, is too weak to grasp so many new ideas.— I was quite prepared for the former northern & southern extension of N. Zealand, I used to think with New Caledonia. The most startling of all your views is that of stocking the former Antarctic continent, viâ Tierra del Fuego, with northern forms, & thence N. Zealand & S. Australia.²⁴ This gives me a shudder from its boldness. With respect to absence of Australian trees, I remember that A. De Candolle shows that they from some cause spread less than herbs.²⁵

Ch XXIII. is rather too speculative for my old noddle.— I must think that you overrate importance of new surfaces on mountains & dispersal from mountain to mountain.— I still believe in alpine plants having lived on the lowlands & in the northern tropical regions having been cooled during glacial period, & thus only can I understand character of floras on the isolated African mountains. It appears to me

that you are not justified in arguing from dispersal to oceanic islands to mountains. Not only in latter cases currents of sea are absent, but what is there to make birds fly direct from one alpine summit to another? There is left only storms of wind, & if it probable or possible that seeds may thus be carried for great distances, I do not believe that there is at present any evidence of their being thus carried more than a few miles.—²⁶

* Addendum to p.3

It seems to me (not that I have been able to think out the whole case) that the problem (as far as age is judged of by the thickness of our formations) is the rate of deposition over *areas of subsidence*, & not near the coast over the world; for beneath the Tertiary beds most of the formations appear to have been deposited during subsidence. I must confess, however, that I have never succeeded in realising what the conditions were & whence all the sediment came, during the deposition of the enormous Carboniferous formation.— During elevation I believe that the shore deposits are raised up & distributed again & again; & that near the mouths of great rivers the land is added to; but I doubt whether our Secondary & palæozoic formations (except the Neocomian) were deposited as estuaries & growing low land.

British Library (Add MS 46434 ff. 292–3); Natural History Museum (Wallace Papers WP/6/4/1)

¹ Wallace had sent CD a copy of *Island life* (Wallace 1880a), and had asked for comments (see letter from A. R. Wallace, 11 October 1880).

² The dedication in Wallace 1880a reads: ‘To Sir Joseph Dalton Hooker, who, more than any other writer, has advanced our knowledge of the geographical distribution of plants, and especially of insular floras, I dedicate this volume, on a kindred subject, as a token of admiration and regard’. Hooker’s note has not been found.

³ Wallace’s name appears on the presentation list for *Movement in plants* (Appendix IV).

⁴ Wallace remarked on the total absence of mammals in the Pacific islands. Viti Levu is one of the Fijian islands. The New Hebrides was the name for an island group in the South Pacific; it is now the independent Republic of Vanuatu. New Caledonia is an island in the Coral Sea, colonised by France and still a French territory; it was visited by the French naturalists Eugène Viellard and Émile Deplanche, who described two species of flying fox and a small bat, as well as noting the presence of two types of rat or mouse that they believed to be indigenous (see Viellard and Deplanche 1863, pp. 131–2). The publication by Albert Günther has not been identified.

⁵ In *Island life*, p. 68, Wallace wrote that many families now represented by disconnected genera would have had almost continuous distribution in Tertiary times. Günther’s monograph on giant land tortoises was based on several papers given at the Royal Society of London (see Günther 1877, p. iii). In a letter to J. D. Hooker, 28 January 1877 (*Correspondence* vol. 25), CD remarked: ‘I said to Günther many months ago that the most probable view about his tortoises seemed to me to be that various closely allied forms had once been distributed over almost the whole world.’

⁶ Augustus Addison Gould had described the Mollusca and shells collected by the United States Exploring Expedition to the Pacific, 1838–42 (Gould 1852–6). In a letter to A. R. Wallace, 22 December 1857 (*Correspondence* vol. 6), CD had remarked, ‘Gould has conclusively shown that many land-shells have there been distributed over the Pacific by man’s agency.’

⁷ In *Island life*, pp. 156–7, Wallace discussed glaciation patterns in New Zealand. He later assessed Frederick Wollaston Hutton’s geographical explanations for the distribution of animals in New Zealand (Wallace 1880a, pp. 449–50).

⁸ Wallace explained the lack of fossils in deposits of great thickness in the Alps by suggesting that glaciers had descended from mountains and cooled the water of an immense inland sea, destroying

the Mollusca and other organisms (see Wallace 1880a, pp. 171–3). CD alludes to the discoveries made by the *Véga* expedition (1878–80), a Swedish research expedition that explored the polar sea above Siberia and was the first to navigate through the North-East Passage. For a contemporary report of the expedition, see Nordenskiöld 1881.

- ⁹ For a summary of Wallace's views on the causes of glacial epochs, see Wallace 1880a, pp. 197–202.
- ¹⁰ In *Principles of geology*, Charles Lyell had argued that major climate change was chiefly caused by the relative positions of land and sea (see C. Lyell 1867–8, chapters 12 and 13). For CD's doubts about the theory, see *Correspondence* vol. 14, letter to Charles Lyell, 8 March [1866]; see also *Origin* 6th ed., p. 336. Wallace suggested that if Lyell's theory was modified 'so as to allow a freer passage of currents in the tropics', this would show a condition of the earth that would bring about 'a perpetual summer or an almost universal winter' (Wallace 1880a, p. 144).
- ¹¹ James Croll had proposed that glacial epochs occurred in alternate hemispheres during prolonged periods of high eccentricity of the earth's orbit (Croll 1868 and Croll 1875). CD had used Croll's theory to explain the survival of tropical species during glacial periods (see *Origin* 6th ed., pp. 335–42). Wallace discussed Croll's theory extensively; see especially Wallace 1880a, pp. 164–8, 197, and letter from A. R. Wallace, 9 January 1880.
- ¹² See Wallace 1880a, pp. 199–201.
- ¹³ On fossil plants and Saurians found in the Arctic and their implications for climate change, see Nordenskiöld 1875.
- ¹⁴ George Howard Darwin.
- ¹⁵ Based on the average rate of denudation and deposition, Wallace estimated the time that had elapsed since the Cambrian period to be twenty-eight million years; he noted that this figure was broadly in agreement with that of physicists, and argued that larger amounts of geological time (hundreds of millions of years) were unnecessary for the developments that had produced existing organic forms (see Wallace 1880a, pp. 227–9).
- ¹⁶ On the Azores, see Wallace 1880a, pp. 238–53. Wallace argued that the prevalence of violent storms in the Azores, combined with the proximity of Europe and North Africa, explained the character of its flora and fauna.
- ¹⁷ On plants carried by icebergs to the Azores in the glacial epoch, see *Origin* 6th ed., p. 328.
- ¹⁸ Thomas Carew Hunt had written to CD about birds in the Azores (see *Correspondence* vol. 5, letter from T. C. Hunt, 2 July 1855). The manuscript has not been identified, but CD referred to Hunt's published list of birds visiting the Azores in his posthumously published 'big book on species' (*Natural selection*, p. 493 n. 1).
- ¹⁹ On the Galápagos flora, see Wallace 1880a, pp. 276–9.
- ²⁰ In discussing the native vegetation of St Helena, Wallace remarked: 'plants, when once established in a suitable climate and soil, soon take possession of a country and occupy it almost to the complete exclusion of later immigrants' (Wallace 1880a, pp. 294–5). In *Origin* 6th ed., p. 51, CD had described the cardoon and thistle, introduced from Europe, covering the plains of La Plata, 'almost to the exclusion of every other plant' (see also *Journal of researches*, pp. 138, 143, 172).
- ²¹ On freshwater fish in Ireland, see Wallace 1880a, pp. 321–4. On variations in trout from different rivers in New Zealand, see Arthur 1878.
- ²² Gouramis are freshwater fish native to Asia. The passage may be translated as: 'We tried, but without success, to transport the frogs that eat the mosquito eggs deposited on stagnant water' (Bernardin de Saint Pierre 1773, 1: 242).
- ²³ Lemuria was a hypothetical continent, thought to date from the pre-Miocene period, and extend from Madagascar to Ceylon (Sri Lanka) and the Malay islands; it was used to explain the distribution of land birds in the region (see Wallace 1880a, pp. 394–9, 409).
- ²⁴ To explain the peculiarities of New Zealand flora and fauna, Wallace proposed that the continent of Australia had been divided in the Cretaceous period; that New Zealand had been joined with the north-eastern, more tropical part of Australia, and later with the Antarctic continent, allowing for the migration of species from South America (see Wallace 1880a, pp. 465–66, 443–44, 455). Wallace included CD's comment in Wallace 1905, 2: 12–13.
- ²⁵ CD mentioned Alphonse de Candolle's view on the restricted geographical range of trees in *Origin* 6th ed., p. 350; see A. de Candolle 1855, 1: 527–32.

²⁶ Wallace proposed that seeds borne by wind across mountain chains were a major means of migration for plants from the northern to the southern hemisphere (see Wallace 1880a, pp. 480–6). For CD's theory, see *Origin* 6th ed., pp. 338–40.

From R. F. Cooke 4 November 1880

50A, Albemarle Street, London. W.
Nov 4 1880

My dear Sir

We have seen a copy of your new book & fixed the price at 15/- but I am sorry to say, that if every copy was sold (1000) it would leave you in a loss of about £50.¹

Yours faithfully | Rob^t Cooke

Cha^s Darwin Esq

DAR 171: 510

¹ In his letter to R. F. Cooke, 21 July 1880, CD had agreed to the terms of sale, and to cover for any loss, for *Movement in plants*.

From Florence Dixie 4 November [1880]¹

Bosworth Park. | Hinckley. | Leicestershire.
Nov. 4th

Dear Mr. Darwin.—

I must write a line to thank you for your kind letter in reply to mine.—² The books you recommend I shall certainly procure & read with interest;— I have myself written a short description of my wanderings in Patagonia which appears this month in print and if you will do me the honour of accepting a copy I shall feel very proud to send you one.³ The work does not comprise the extent of my whole expedition which on leaving Patagonia I carried on up the Rivers Plate Uruguay & Parana.⁴

From Patagonia I brought home some ostriches a gunaco, & from the Rivers Plate, Uruguay, & Parana, a great many animals, comprising some ostriches, a Capybara & a little jaguar.⁵ The mother attacked me & followed me up a tree, in self defence I was obliged to shoot her but saved one of the cubs from the gauchos.— Since then he has been my almost constant companion following me ab^t like a dog altho' of an enormous size being now 2. years old. I only yesterday took him to the Zoological Gardens, much to my regret, but he was growing so big that it was not safe keeping him longer at large. I have mentioned this fact to prove how these animals can be tamed by kindness as completely as a dog.—

With many apologies for thus troubling you | I beg to remain | very faithfully y^{rs}
| Florence Dixie

DAR 162: 183

- ¹ The year is established by the reference to Dixie 1880 (see n. 3, below).
² CD's reply to the letter from Florence Dixie, 29 October [1880], has not been found.
³ CD's copy of *Across Patagonia* (Dixie 1880) is in the Darwin Library–CUL.
⁴ The Río Paraná runs through Brazil, Paraguay, and Argentina, merging with the Río Uruguay to form the Río de la Plata estuary, which flows into the Atlantic Ocean near Buenos Aires and Montevideo.
⁵ The guanaco (*Lama guanicoe*) is a South American camelid, related to the llama. Two species of rhea (an ostrich-like flightless bird) are extant in South America, *Rhea americana* and *R. penata*. The capybara (*Hydrochoeris hydrochaeris*) is the largest extant rodent, related to guinea pigs. The jaguar is *Panthera onca*.

From R. F. Cooke 5 November 1880

Albemarle St

Nov 5. 1880

There were about 600 *Movements of Plants* counted rapidly this evening, which will start us well¹

R Cooke

ApcS

DAR 171: 511

CD ANNOTATION

On back of postcard: '50£ | Index | Cut. Copies | 250 Copies | M. | 700 gone | (Separate notices in Nature)² pencil; square brackets in ms

- ¹ The copies of *Movement in plants* were sold at John Murray's annual dinner for booksellers on 6 November 1880 (CD's 'Journal' (Appendix II)).
² CD's annotations are notes for his reply of 8 November [1880].

To T. H. Huxley 5 November 1880

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

Nov. 5th 80

My dear Huxley

On reading over your excellent review with the sentence quoted from Sir W. Thomson, it seemed to me adviseable, considering the nature of the publication, to notice "extreme variation" & another point.—¹ Now will you read the enclosed, & if you approve, post it soon. If you disapprove, throw it in the fire, & thus add one more to the 1000 kindnesses which you have done me.— Do not write; I shall see result in next week's *Nature*.—² Please observe that in the foul copy I had added a final sentence which I did not at first copy, as it seemed to me inferentially too contemptuous; but I have now pinned it to back, & you can send it or not,—as you think best,—that is if you think any part worth sending.³ My request will not cost you much trouble, i.e to read two pages, for I know that you can decide at once.—

I heartily enjoyed my talk with you on Sunday Morning⁴

Ever yours | Ch. Darwin

If my M.S. appears too flat, too contemptuous, too spiteful, or too anything, I earnestly beseech you to throw it into the fire

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 344)

¹ Charles Wyville Thomson had claimed that the results of the *Challenger* expedition failed 'to give the least support to the theory which refers the evolution of species to extreme variation guided only by natural selection' (C. W. Thomson 1880, p. 50). Huxley's review of C. W. Thomson 1880 was published in *Nature*, 4 November 1880, pp. 1–3.

² The enclosure has not been found; see, however, letter to *Nature*, 5 November 1880.

³ The additional sentence was omitted from CD's letter to *Nature* by Huxley (see letter from T. H. Huxley, 14 November 1880); it was published, however, in *ML* 1: 389:

Perhaps it would have been wiser on my part to have remained quite silent like the breeder; for as Prof. Sedgwick remarked many years ago, in reference to the poor old Dean of York who was never weary of inveighing against geologists, a man who talks about what he does not in the least understand, is invulnerable.

CD alludes to a controversy in 1844 between Adam Sedgwick and William Cockburn, the dean of York (see M. Roberts 2009, pp. 164–5).

⁴ CD was in London from 29 October to 2 November 1881 ('Journal' (Appendix II)); in 1881, the Sunday before 5 November was 31 October.

To *Nature* 5 November [1880]¹

Sir Wyville Thomson and Natural Selection

I AM sorry to find that Sir Wyville Thomson does not understand the principle of natural selection, as explained by Mr. Wallace and myself.² If he had done so, he could not have written the following sentence in the Introduction to the Voyage of the *Challenger*.—"The character of the abyssal fauna refuses to give the least support to the theory which refers the evolution of species to extreme variation guided only by natural selection."³ This is a standard of criticism not uncommonly reached by theologians and metaphysicians, when they write on scientific subjects, but is something new as coming from a naturalist. Prof. Huxley demurs to it in the last number of *NATURE*; but he does not touch on the expression of *extreme variation*, nor on that of evolution being guided *only* by natural selection.⁴ Can Sir Wyville Thomson name any one who has said that the evolution of species depends only on natural selection? As far as concerns myself, I believe that no one has brought forward so many observations on the effects of the use and disuse of parts, as I have done in my "Variation of Animals and Plants under Domestication"; and these observations were made for this special object. I have likewise there adduced a considerable body of facts, showing the direct action of external conditions on organisms;⁵ though no doubt since my books were published much has been learnt on this head. If Sir Wyville Thomson were to visit the yard of a breeder, and saw all his cattle or sheep almost absolutely true, that is, closely similar, he would exclaim: "Sir, I see here no extreme variation; nor can I find any support to the belief that you have followed the principle of selection in the breeding of your animals". From what I formerly saw of

breeders, I have no doubt that the man thus rebuked would have smiled and said not a word. If he had afterwards told the story to other breeders, I greatly fear that they would have used emphatic but irreverent language about naturalists.

CHARLES DARWIN | Down, Beckenham, Kent, November 5

Nature, 11 November 1880, p. 32

¹ The year is established by the publication date of the letter in *Nature*.

² Charles Wyville Thomson and Alfred Russel Wallace.

³ Thomson's remarks on natural selection appeared in his 'General introduction to the zoological series of reports' of the *Challenger* expedition (C. W. Thomson 1880, p. 50).

⁴ Thomas Henry Huxley's review of C. W. Thomson 1880 was published in *Nature*, 4 November 1880, pp. 1–3.

⁵ On the effects of use and disuse, see *Variation* 2: 295–303 and 418–19; on the direct action of external conditions, see *ibid.*, pp. 271–92.

From G. J. Romanes 5 November 1880

November 5, 1880.

I was sorry to hear on my return from Scotland that I had missed the pleasure of a call from you,¹ and also to hear from Mr. Teesdale to-day that you had returned to Down, owing, he fears, to the alarming condition of Miss Wedgwood.² I trust, however, that her state of health may not be so serious as he apprehends.

On my way South I stayed for a couple of days at Newcastle, to give two lectures on Mental Evolution, and hence my absence when you called.³ I stayed with Mr. Newall, who has the monster telescope, and 'as good luck would have it, Providence was on my side,' in the matter of giving us a clear sky for observing, rather a rare thing at Newcastle.⁴

You will be glad to hear that our season's work at the 'Zoological station' has been very successful. A really interesting research has been conducted by Ewart and myself jointly on the locomotor system of Echinoderms, he taking the morphological and I the physiological part.⁵ When next I see you I shall tell you the principal points, but to do so in a letter would be tedious.

I think it is probable that Mivart and I shall have a magazine battle some day on Mental Evolution, as I think it is better to draw him in this way before finally discussing the whole subject in my book.⁶

E. D. Romanes 1896, pp. 99–100

¹ CD was in London from 29 October to 2 November 1881 ('Journal' (Appendix II)).

² John Marmaduke Teesdale was a neighbour of CD; Elizabeth Wedgwood, who lived at Tromer Lodge in Down, was seriously ill (see letter to T. H. Farrer, 8 October 1880).

³ Romanes's lectures were delivered at the Newcastle Literary and Philosophical Society on 2 and 3 November 1880 (*Newcastle Journal*, 2 November 1880, p. 1).

⁴ Robert Stirling Newall's telescope was erected at his house near Newcastle in 1871 (*ODNB*). The quotation is from *Erewhon, or, Over the range* ([Butler] 1872, p. 28).

- ⁵ A small zoological station was opened in Cowie, Aberdeenshire, Scotland, in August 1879 (*Nature*, 14 August 1879, pp. 372–3). A joint paper on echinoderms was later published by Romanes and James Cossar Ewart (Romanes and Ewart 1881; see letter from G. J. Romanes, 14 December 1880).
- ⁶ Romanes's next book was *Animal intelligence* (G. J. Romanes 1882); this was followed by *Mental evolution in animals*, in which he criticised St George Jackson Mivart's view that reason was uniquely human (see G. J. Romanes 1883, pp. 335–40).

To ? 5 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 5./80

Dear Sir

I am so much engaged I cannot write at length; but if you will consult my “Descent of Man” p. 590, 591. Ch. XX (in one of the later editions) you will find all that I know about their marriage arrangements, with some references.—¹ If you will look to index, under monkeys, you will find somewhat on their mental & moral nature.—

Heartily wishing you all success | I remain, dear Sir | Yours faithfully |
Ch Darwin

Historical Society of Pennsylvania

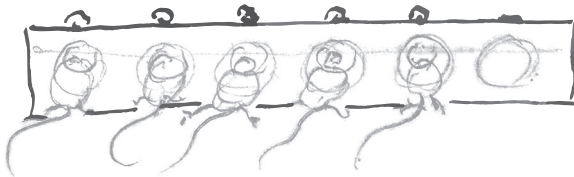
¹ The letter to which this is a reply has not been found. In *Descent* 2d ed., pp. 590–1, CD discussed marriage practices among ‘savages’ and monogamy and polygamy among primates. He referred principally to the work of Lewis Henry Morgan, John Ferguson McLennan, and John Lubbock.

From George Maw 6 November 1880

Benthall Hall, | nr Broseley.
Nov 6. 80

Dear Sir,

I have lately come across rather a curious fact which I do not think has been before observed in which you may perhaps be interested.



In the death of vertebrate animals as far as I have observed the tail becomes deflected to the left hand side. I first

observed this in a mouse-trap in which the row of dead mice had their tails all turned to the left I have noticed this several times & I believe it is invariable.

Again in passing through Paris last Monday all the sheep (without any exception) hanging up in the Butchers Shops had their tails turned to the left *in rigor mortis*. In France most of the sheep have their tails uncut so that it was more noticeable there than at an English butchers. Would you kindly tell me whether this fact is generally known. & has it any connection with another curious fact—the tendency

of a person walking in the dark or in a fog to deviate to the right from an intended straight line. & the use of the right hand in preference to the left hand in a young child, a sort of want of exact symmetry in the action of the muscles on the two sides of the body.

I venture to send you by to days post a specimen of my monograph on the genus *Crocus* at which I have been hard a work during the last two years. When I commenced there were but 50 species known & I have been able to bring the number up to 70 nearly the whole of which I have in cultivation.¹ I am drawing all the plates myself mostly from living examples. It is a genus full of interest in every detail & I shall have to record some very curious facts in connection with the distribution of species & the character & structure of the fossil parts in relation thereto.

Believe me I remain | Y^{rs} very sincerely | George Maw

C. Darwin Esq

DAR 171: 106

¹ The specimen has not been found; Maw's *Monograph of the genus Crocus* was published in 1886 (Maw 1886).

From A. B. Buckley 7 November 1880

1 St Mary's Terrace | Paddington W.
Nov 7. 1880.

Dear Mr Darwin,

It is all right as far as regards Mr Wallace himself—

I told him that you & Mr Huxley thought him entitled to a Gov^t pension *if* it could be got—¹ At first he hesitated but when I represented that such men as Joule & Faraday had received it² he said “I confess it would be a *very great relief* to me and if such men as Darwin & Huxley think I may accept it I suppose I may”—adding “I really have some claim, for most naturalists & travellers on their return from a foreign country have been given some post, & I have tried for one in vain”.

It seems some friend suggested it to him some time ago but he rejected the idea; but now that it comes from men like yourself & Huxley who can appreciate his work it makes a difference—

I could not get the memorial lists but when I said that you would have only a few good names & suggested the Duke of Argyll,³ Mr Wallace said he is just the man who would probably give his name with pleasure—

He quite understands that the result is very doubtful & indeed he said very little about it, for when I had once ascertained his views I did not want to lead him to dwell upon it—

I have nothing I think to add to the notes I gave you.⁴ I enclose a very brief statement which may be of some use, though of the real value of his work you can speak best—

From my short conversation yesterday I am more than ever sure that your generous efforts if they succeed will really confer a great boon on M^r. Wallace & relieve him of anxiety—

If I can look out anything more for you please let me know—

With kind remembrances to M^{rs}. Darwin | Yours very sincerely | Arabella B
Buckley

[Enclosure]

He gave up his profession of surveyor & architect from pure love of Natural History & depended entirely on his collections for his remuneration— The loss of these collections sent him to the Malay (archipelago) & on his return from there in 1862 he began at once to try for some post as naturalist & curator—⁵ The East London Museum was as nearly promised to him as was possible before the building was concluded, & the loan of Sir R Wallace's pictures giving a new turn to the use of the building threw him out—⁶ His failure with regard to Epping Forest⁷ & other appointments has led him to give up all idea at his age of obtaining a post— Want of success in some investments has diminished his income & he is dependent now *chiefly* on his writings which are (not very) remunerative in an inverse ratio to their true value—

His chief claims seem to be

That he has opened out a knowledge of the whole flora & fauna of the Malay Archipelago of which specimens are in the British & other museums—

His share in the question of the Origin of Species—

And above all his application of this theory to the Geographical distribution of Animals⁸

DAR 160: 370

CD ANNOTATIONS

Top of letter: 'Tropical | Nature | 370' *pencil*

End of enclosure: 'His views of the [*illeg*] of animals—which illustrates together with colouration [*'in' del*] originality of his mind' *pencil*

¹ CD had been asked by Thomas Henry Huxley to prepare a statement of Alfred Russel Wallace's claims for a government pension (see letter to A. B. Buckley, 31 October [1880]).

² Michael Faraday received a civil list pension in 1835; James Prescott Joule received one in 1878 (*ODNB*). On the history of civil list pensions, see MacLeod 1970.

³ George Douglas Campbell.

⁴ For Buckley's notes on Wallace, see Appendix VI.

⁵ On Wallace's work as a surveyor, the loss of his South American collections, and his activities after his return to England in 1862, see Raby 2001, pp. 15–19, 81, and 163–83.

⁶ Wallace had hoped to be appointed director of a new branch of the South Kensington Museum at Bethnal Green in east London (see Raby 2001, pp. 199, 206, 211, and Fichman 2004, p. 60). When the museum opened in 1872, it displayed Richard Wallace's large collection of paintings and French decorative arts (see Lasic 2014).

⁷ On Wallace's failure to obtain the post of superintendent of Epping Forest, see *Correspondence* vol. 27, letter from A. B. Buckley, 16 December 1879 and n. 1, and Raby 2001, pp. 218–21).

⁸ See Darwin and Wallace 1858 and Wallace 1876.

From William Clowes & Sons [before 8 November 1880]¹

William Clowes & Sons. | Limited.
Duke Street, Stamford Street, | London, S.E.

Dear Sir,

We are very much vexed that you should imagine for one moment that we should put aside your book² for others: having always been treated with great courtesy by you it has been our strong wish to push forward your works with the greatest rapidity—the delay in this case has been with the Index-maker:³ we received the *copy* only this morning and before we got your letter⁴ had arranged *for the whole of the proof* to be out tonight: Hoping that this explanation will convince you that we are not to blame.

We are Sir | Yours faithfully | W^m Clowes & Sons Id.: | per E. [A] Clowes:⁵

Ch: Darwin Esq^{re}

DAR 161: 180

¹ The date is established by the year in which the firm William Clowes and Sons became 'William Clowes and Sons Ltd.', and by the relationship between this letter and the letter to R. F. Cooke, 8 November [1880].

² *Movement in plants*.

³ The indexer was Matilda Smith.

⁴ CD's letter has not been found.

⁵ Edward Arnott Clowes.

To R. F. Cooke 8 November [1880]¹

Down, | Beckenham, Kent. | 🚉 (Railway Station | Orpington. S.E.R.)
Nov. 8th

My dear Sir

It was **very** kind of you to tell me so soon of the 600 copies: I expected about 200, so that the loss will not be so heavy as I expected.² Will you be so good as to advise me whether I had not better have 250 copies struck off: for reviews Libraries & 65 presentation copies will dispose of nearly 100 more. What would it cost to have type kept up say for 2 months?

I enclose list of 24 copies to be distributed & please send other 41 copies for foreigners (for N.B I shall want 65 copies *cut*)³ to me, addressed "Orpington St. S.E R".—

Please to see about Reviews.— Please insert advertisement by itself twice in Nature.⁴

Lastly I am sorry to trouble you, but it is right that you should know that the index-maker⁵ has made the worst index, I believe, ever published, notwithstanding

that my son⁶ & I spent 2 days in correcting it, & this has caused delay. We had to look to fully $\frac{1}{3}$ of references to conjecture what reference meant. The miserable work is chiefly due to ignorance of the matter, but not wholly so, for he scamped his work. For instance under names Lynch & De Vries, instead of looking to see what they had written about the general heading on top of page was copied.⁷ In no book published by you was there ever so bad an index.—

Once again thanking you cordially for all the trouble which you have taken for me I remain, My dear Sir | Yours sincerely | Ch. Darwin

P.S | *Sometime* I sh^d like to hear how my other books sold.—

National Library of Scotland (John Murray Archive) (Ms. 42152 ff. 378–9)

- ¹ The year is established by the relationship between this letter and the letter from R. F. Cooke, 5 November 1880.
- ² Around 600 copies of *Movement in plants* had been printed before John Murray's annual dinner for booksellers (see letter from R. F. Cooke, 5 November 1880). CD had agreed to pay for costs not covered by sales of the book (letter to R. F. Cooke, 21 July 1880).
- ³ For CD's presentation list for *Movement in plants*, see Appendix IV. He had originally requested sixty copies with the edges cut (see letter to R. F. Cooke, 16 October 1880).
- ⁴ An advertisement for *Movement in plants* appeared in *Nature*, 11 November 1880, p. xii.
- ⁵ The indexer was Matilda Smith.
- ⁶ Francis Darwin.
- ⁷ Richard Irwin Lynch and Hugo de Vries (see for example, *Movement in plants*, pp. 330 and 340).

To George Maw 8 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 8th 1880

My dear Sir

Your case is curious & altogether new to me.¹ You will remember Linnæus' specific character of *Canis familiaris* "Cauda (*sinistrorsum* recurrata)".² The flexure in your dead animals must, I suppose, depend on the greater strength of the muscles on the left side, & this seems very odd. My son, George of Trin: Coll: Cambridge many years ago tested a lot of boys blindfolded to see which way they would turn; he, also, if I can trust my memory, tested the strength of their two legs by making them hop.—³ If you are really interested in the result, I would write to him to learn whether he w^d find his notes.

I am very much obliged for the present of a specimen of your monograph on *Crocus*, which will probably arrive tomorrow.⁴

Pray believe me | My dear Sir | Yours sincerely | Ch. Darwin

Royal Horticultural Society, Lindley Library (MAW/1/18)

- ¹ See letter from George Maw, 6 November 1880.
- ² The description of *Canis familiaris* appears in *Systema naturæ* by Carolus Linnaeus: 'cauda (*sinistrorsum*) recurvata' ('recurved tail turned towards the left'; Linnaeus 1758–9, 1: 38). *Canis familiaris* is a synonym of *Canis lupus familiaris*, the domestic dog.

³ George Howard Darwin's experiments were designed to study directional instinct; his notes have not been found (see *Correspondence* vol. 22, letter to G. H. Darwin, 27 May [1874]).

⁴ The specimen of Maw's *Monograph of the genus Crocus* (Maw 1886) has not been found.

From A. R. Wallace 8 November 1880

Pen-y-bryn, St Peter's Road, | Croydon.

Nov. 8th. 1880

My dear Darwin

Many thanks for your kind remarks & notes on my book.¹ Several of the latter will be of use to me *if* I have to prepare a second Edition, which I am not so sure of as you seem to be.²

1. In your remark as to the doubtfulness of paucity of fossils being due to coldness of water, I think you overlook that I am speaking *only* of waters in the latitude of the Alps, in Miocene & Eocene times, when icebergs and glaciers *temporarily* descended into an otherwise warm sea;— my theory being that there was no *glacial epoch* at that time but merely a *local* and temporary descent of the snow line & glaciers owing to high excentricity & winter in *aphelion*³

2. I cannot see the difficulty about the *cessation* of the glacial period.

Between the Miocene and the Pleistocene periods geographical changes occurred which rendered a true glacial period possible *with high excentricity*. When the high excentricity passed away the glacial epoch also passed away in the *temperate zone*;— but it *persists* in the *arctic zone* where, during the Miocene there were mild climates, & *this* is due to the persistence of the changed geographical conditions. The present arctic climate is itself a comparatively new and abnormal state of things due to geographical modification.

As to "epoch" & "period" I use them as synonyms to avoid repeating the same word.

3. Rate of deposition & geological time. There no doubt I may have gone to an extreme, but my "28 millions years" may be anything under 100 millions, as I state. There is an enormous difference between *mean* and *maximum* denudation & deposition. In the case of the great faults the upheaval along a given line would itself facilitate the denudation (whether subaerial or marine) of the upheaved portion at a rate perhaps a hundred times above the average, just as *valleys* have been denuded perhaps a hundred times faster than *plains* and *plateaux*.⁴ So, local subsidence might itself lead to very rapid deposition— Suppose a portion of the Gulf of Mexico near the mouths of the Mississippi were to subside for a few thousand years, it might receive the greater part of the sediment from the whole Mississippi valley & thus form strata at a very rapid rate.

4. You quote the Pampas thistles &c. against my statement of the importance of preoccupation. But I am referring especially to *St. Helena*. and to plants naturally introduced from the adjacent continents. Surely if a certain number of African plants reached the island and became modified into a complete adaption to its climatic conditions they would hardly be expelled by *other* African plants arriving subsequently. They might be so, conceivably, but it does not seem probable. The cases of the Pampas, New Zealand, Tahiti &c. are very different where highly

developed *aggressive* plants have been artificially introduced—⁵ Under nature it is these very aggressive species that would *first* reach any island in their vicinity, & being adapted to the island and colonising it thoroughly would then hold their own against other plants from the *same* country, mostly less aggressive in character.

I have not explained this so fully as I sh^d have done in the book. Your criticism is therefore useful.

5. My Chap. XXIII. is no doubt very speculative and I cannot wonder at your hesitating at accepting my views. To me however *your* theory of hosts of *existing species* migrating over the tropical lowlands from the N. temperate to the S. temperate zone appears *more* speculative & *more* improbable.⁶ For, where *could* the rich lowland *equatorial* flora have existed during a period of general refrigeration sufficient for this? and what became of the wonderfully rich Cape Flora which, if the temperature of Tropical Africa had been so recently lowered would certainly have spread northwards & on the return of the heat could hardly have been driven back into the sharply defined and *very restricted area* in which it now exists.

As to the migration of plants from mountain to mountain not being so probable as to remote islands, I think that is fully counterbalanced by two considerations:

- a. The area and abundance of the mountain stations along such a range as the Andes are immensely greater than those of the islands in the N. Atlantic for example.
- b. The *temporary* occupation of mountain stations by migrating plants (which I think I have shown to be probable) renders *time* a much more important element in increasing the number & variety of the plants so dispersed than in the case of islands, where the flora soon acquires a fixed and endemic character, & where the number of species is necessarily limited.

No doubt direct evidence of seeds being carried great distances through the air is wanted but I am afraid can hardly be obtained. Yet I feel the greatest confidence that they *are* so carried.⁷ Take for instance the two peculiar orchids of the Azores (*Habenaria* sp.), what other mode of transit is conceivable?⁸ The whole subject is one of great difficulty, but I hope my chapter may call attention to a hitherto neglected factor in the distribution of plants.

Your references to the *Mauritius* literature are very interesting, & will be useful to me,⁹ & again thanking you for your valuable remarks | Believe me | Yours very faithfully | Alfred R. Wallace

DAR 106: B145-8

¹ CD had sent notes on *Island life* (Wallace 1880a); see letter to A. R. Wallace, 3 November 1880 and enclosure.

² A second edition of *Island life* was published in 1892 (Wallace 1892).

³ See letter to A. R. Wallace, 3 November 1880, enclosure and n. 8.

⁴ See letter to A. R. Wallace, 3 November 1880, enclosure and n. 15.

⁵ See letter to A. R. Wallace, 3 November 1880, enclosure and n. 20.

⁶ See letter to A. R. Wallace, 3 November 1880, enclosure and n. 26.

- ⁷ On wind as a means of seed dispersal to the Azores, see Wallace 1880a, pp. 248–9. CD had emphasised ocean currents and birds as a means of transport (see *Origin* 6th ed., pp. 325–8).
- ⁸ Two species of orchid found on the Azores and previously identified as *Habenaria* (rein or bog orchids) are now classed as *Platanthera* (butterfly orchids) and thought to have migrated as airborne seed from south-west Europe (see Bateman *et al.* 2013).
- ⁹ See letter to A. R. Wallace, 3 November 1880, enclosure and n. 22. In the second edition of *Island life*, Wallace added a reference on Mauritius that he had obtained from CD (see Wallace 1892, p. 435).

To A. B. Buckley 9 November 1880

Down Beckenham
Nov. 9th. 1880

My dear Miss Buckley

Your last letter and the M.S. notes are all as clear and full as could possibly be desired.— I quite agree with your summary of Wallace's more important results;¹ but I have added a sentence about the colouring of animals.² I fear that it would never do to ask a Cabinet Minister (the Duke of Argyll) to sign a memorial to the Prime Minister.—³ This morning I drew up a long and full statement of Wallace's claims, position &c., and have sent it to be well copied.—⁴ Immediately on its return it shall be despatched to Huxley.⁵ I hardly ever wished for anything more than I do for the success of our efforts.

Yours very sincerely | Ch. Darwin

Copy

DAR 143: 183

- ¹ Buckley had sent a summary of Alfred Russel Wallace's claims for a government pension (see letter from A. B. Buckley, 7 November 1880 and enclosure).
- ² For Wallace's theory of protective coloration, see Wallace 1878, pp. 158–220; he had corresponded at length with CD on the topic (see, for example, *Correspondence* vol. 19, letter to A. R. Wallace, 30 January [1871]).
- ³ George Douglas Campbell was appointed lord privy seal in April 1880 (*ODNB*); the prime minister was William Ewart Gladstone.
- ⁴ There is a draft of the memorial in DAR 196: 3; for a transcription, see Appendix VI.
- ⁵ Thomas Henry Huxley.

From R. F. Cooke 10 November 1880

50A, Albemarle Street, London. W.
Nov 10 1880

My dear Sir

We are only waiting for the printer to finish off the Index & end of yr book & I understood from M^r Clowes¹ who was here yesterday that you had not *then* ret^d. it for press.

What you say of the Index maker has distressed us as it is by the same hand that has done your previous ones with which you have been well pleased.²

Take no trouble as to the Type of the work.³ It will be kept standing for 5 or 6 months certain without charge & we had better wait for the hostile criticisms !!! before we print any more off.

We have now sold *800 Copies*
Yours faithfully | Rob^l Cooke

Ch^{as} Darwin Esq

We will not forget *Nature*⁴

DAR 171: 512

¹ Edward Arnott Clowes.

² Matilda Smith had prepared the indexes for *Forms of flowers* and *Cross and self fertilisation*. For CD's complaints about the index to *Movement in plants*, see letter to R. F. Cooke, 8 November [1880].

³ CD had asked the cost of keeping the type up for two months (letter to R. F. Cooke, 8 November [1880]).

⁴ CD had asked for advertisements for *Movement in plants* to be placed in *Nature* (see letter to R. F. Cooke, 8 November [1880] and n. 4). A short notice, 'New work by Mr. Darwin next week, with woodcuts', appeared in *Nature*, 11 November 1880, p. xii; the book was also in the list published by John Murray in *Nature*, 25 November 1880, p. xxxii, and 23 December 1880, p. lx.

From Francis Darwin [11 or 12 November 1880]¹

66, Hills Road, | Cambridge.

Dear Father

I am very sorry that I forgot the Gardener's address.² I hope you found it all right.

I am having a very nice time here, & have all but done the bramble paper—³

The drawing room is all upside down so we live in H's working room and the dining room I think it will make a nice house, & they seem very happy over it.⁴ They have at last finished Abney's⁵ camera which is a blessing Also Fulcher has come round to going in a peaceable manner & remains friends with Dew H looks on it as certain that he shall join Dew but it is still a state secret⁶

The Greek question was lost, 185 to 145.⁷ The Senate house was crammed with MAs, & it was funny to see the mass of grey heads on the opposite side to us, it was all fogies & country parsons You have to sit down to vote, & one saw people crouching down just to touch the edge of a seat till the Proctor had taken the vote.

Last night G dined here & seems pretty well Tonight we all three dine at F Balfours—⁸

On Sat Jimmy & I dine at Downing in Hall with Crawley,⁹ & on Sunday in Trinity also in Hall so we have lots of dissipation—

I has turned muddy so it isnt worth while bicycling

I have got some stuff for spectacles— Horace says he asked Dew who answered at once but said he should like to have Michael's authority and so asked him— It ∴ comes from Michael— I think his book is in shelf 30¹⁰

Tell Ubbadub I have been going in a tramway just like his tin one.¹¹ I hope the officer has come home again | Your affect son | Frank Darwin

DAR 274.1: 64

¹ The date is established by the reference to the vote in the Senate House on Thursday 11 November 1880 (see n. 7, below).

- ² Probably *Gardeners' Chronicle*, CD had asked for a review copy of *Movement in plants* to be sent to the newspaper (see letter to R. F. Cooke, 20 October 1880).
- ³ Francis Darwin 1880b. The bramble studied by Francis was *Rubus fruticosus*, the common blackberry.
- ⁴ Francis was staying with Horace and Ida Darwin in Cambridge.
- ⁵ William de Wiveleslie Abney.
- ⁶ Robert Fulcher built scientific instruments with Albert George Dew-Smith; their partnership was dissolved in December 1880, and in January 1881 Dew-Smith entered into partnership with Horace Darwin in the Cambridge Scientific Instrument Company (Cattermole and Wolfe 1987, pp. 12–22).
- ⁷ On 11 November 1880, the Senate rejected a proposal to modify the entrance requirements for proficiency in Greek and Latin (*Cambridge University Reporter*, 16 November 1880, p. 140). A memorial calling for the substitution of French or German or both for either or both of the classical languages had been submitted in December 1878, and a syndicate had been appointed to consider the issue in March 1879 (see *ibid.*, 19 October 1880, pp. 48–9, and 2 November 1880, pp. 103–6).
- ⁸ George Howard Darwin and Francis Maitland Balfour.
- ⁹ Jimmy was a nickname for Horace Darwin. Charles Crawley was a friend of Francis's. Formal dinners ('halls') of three or more courses are regularly held at Cambridge colleges.
- ¹⁰ Many of the instruments of the Cambridge Scientific Instrument Company in its early years were made in consultation with Michael Foster for use in his physiological laboratory (see Cattermole and Wolfe 1987, pp. 9–10, 18–20). The book by Foster has not been identified.
- ¹¹ Ubbadub was Bernard Darwin. Cambridge Street Tramways had opened a horse-drawn tramway service in Cambridge on 28 October 1880 (K. Turner 1996).

From Daniel Mackintosh 11 November 1880

36 Whitford Road, | Tranmere, | Birkenhead
11th Nov. 1880.

Dear Sir,—

I have lately been discovering beds of rounded gravel and sand with shells on the *eastern* Slopes of the Welsh mountains at about the same elevation above the sea as the Moel Tryfan deposits in Caernarvonshire, and after a considerable amount of observation, I have arrived at the conclusion that there are three zones on the outer slopes of the Welsh & Pennine hills—a lower zone of rounded stones and sand with shells—then a zone of angular or subangular stones—3rd the Moel Tryfan zone of rounded gravel and sand with shells, and above it a zone of angular stones.

I am writing a paper for the Geol. Society about it, in which I have ventured to refer the angular stone zones to a comparatively rapid subsidence or elevation of the land while under the sea, or to earthquakes; and the well rounded stone zones to the sea lingering for a long time at nearly the same level.¹

You would very much oblige by letting me know if you think that in historical times the elevation or subsidence of the land has been *mainly* caused by earthquakes or by slow and gradual movements.

I believe that the *transverse* horizontality of many of the so-called raised beaches on the western coasts of Britain could only have resulted from earthquakes, and this I find is the opinion of Norwegian geologists concerning the raised beaches of their country.²

If you have written anything about the Moel Tryfan deposits would you kindly let me know.

Apologizing for troubling you with these queries, | I am Dear Sir, | Your very faithful and | obliged Servant, | D. Mackintosh.

DAR 171: 10

¹ Mackintosh had previously sent CD an outline of his proposed paper on the Moel Tryfan deposits in Wales (see letter from Daniel Mackintosh, 15 January 1880). The paper was eventually published in the *Quarterly Journal of the Geological Society of London* (Mackintosh 1881; on the arrangement of the deposits into three vertical zones, see pp. 364–5).

² On raised beaches in Norway and Sweden, see Nordlund 2001.

From J. J. Eyre 12 November 1880

15 Edwards Terrace | Cardiff
Nov 12th/80

Sir,

Just now I happen to be reading your very observant & entertaining work “The Expression of the Emotions”. Last evening on reading the paragraph on the action of the depressores anguli oris muscles in grief, it directly recalled a painful personal episode.

Some time ago I received a telegram to the effect that my only sister¹ to whom I was much attached was dying & that I was to go home at once.

On telling the painful news to some people with whom I was then staying & did not know very intimately I felt very much moved & it was only by the most extreme effort of the will that I controlled myself from giving way altogether. However in spite of that effort I felt the depressores anguli oris act markedly & I found for a few minutes that I could not prevent their acting. I trouble you with this personal experience as I believe it bears out your inference that the depressor anguli oris is one of the muscles of the face least under the control of the will.²

I have the honour to be, Sir, | Yours faithfully | John J. Eyre | Physician

Charles Darwin Esq

DAR 163: 39

¹ Mary Teresa Eyre died in 1876 (Rootsireland.ie, accessed 13 November 2019).

² In *Expression*, pp. 193–5, CD had described the contraction of the *depressores anguli oris* muscles in movements of the mouth, upper lip, and nostrils during grief; on the difficulty of controlling these muscles by the will, see *Expression*, p. 195.

From James Paget 12 November 1880

1. Harewood Place. | Hanover Square. | W.
Nov^r. 12. 1880.

My dear Darwin

I venture to send you an Address I lately gave; but I am anxious to explain to you that it was not intended for the teaching of any natural history— It was only to point to things in which members of my profession, especially the younger and less busy, may study pathology.¹ And it had to be talked for an hour.

Sincerely your's | James Paget.

Skinner (dealers) (Auction 3103T, 6 August 2018)

¹ Paget sent an offprint of his presidential address, 'On elemental pathology', delivered to the pathological section of the British Medical Association meeting in Cambridge in August 1880 (Paget 1880). The address discussed the physiological responses of plants to injury and disease. CD's lightly annotated copy is in the Darwin Pamphlet Collection–CUL.

From B. J. Sullivan 12 November 1880

Bournemouth
Nov^r 12/80

My dear Darwin

I find the year has gone round since our payment for J. FitzRoy Button's support in the orphanage at Oshawia I have sent the 10£ and must now collect the subscriptions.¹

M^r. Langton told me today of Miss Wedgwood's death.² I had not seen it in the papers, having hardly seen a "Times" this six weeks not having time to go to the Club through finishing & preparing to open a Coffee Tavern by a Limited Comp^y. of whose Directors I am chairmen.³

You will all I am sure miss your relative sadly she was so bright and cheerful—but she has been spared to a good old age. M^r. Langton keeps pretty well, but he certainly gets thinner and shows his age more than he did not long since.

I have heard lately from King, Mellersh & Usborne⁴ Mellersh after long illness has got much better, but has had great anxiety about his wife.⁵ a few weeks since she had to be operated on for cataract, soon after a second operation was necessary to save the eye. & the sight is still doubtful. When well enough she has to go through another removal of cataract from the *other* eye.

With our united kind regards to M^rs Darwin and yourself | Believe me | very sincerely your's | B. J. Sullivan

On Thursday next D.V.⁶ I shall complete my 70 years.

DAR 177: 312

¹ Sullivan had proposed that former members of the *Beagle* crew support one of Jemmy Button's grandsons, James FitzRoy Button, at the orphanage at Ushuaia in the Beagle Channel (see *Correspondence* vol. 26, letter from B. J. Sullivan, [14–20] April [1878], and *Correspondence* vol. 27, letter from B. J. Sullivan, 13 October 1879). Jemmy's Yahgan name was Orundellico and his grandson's name was Cooshaipunjiz.

² Elizabeth Wedgwood had died on 8 November 1880 (CD's 'Journal' (Appendix II)). Charles Langton lived in Bournemouth, as did Sullivan.

³ Sullivan was a member of the United Service Club, London (Sullivan ed. 1896, p. 376). The Bournemouth Coffee Tavern Company laid the foundation stone for a new coffee tavern in April 1880 (*Coffee Public-House News*, 1 June 1880, p. 363). Sullivan was an active supporter of the temperance cause, which promoted eating and drinking establishments where alcohol was not served (see Sullivan ed. 1896, p. 392, and Harrison 1994, pp. 295–6).

⁴ Philip Gidley King, Alexander Burns Usborne, and Arthur Mellersh had all served on HMS *Beagle* during CD's voyage.

⁵ On Mellersh's health problems, see letter from B. J. Sullivan, 2 January [1880]; his wife was Henrietta Frances Mellersh.

⁶ 'D.V.': *Deo volente* or God willing (Latin).

To T. H. Huxley 13 November 1880

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

Nov. 13th 1880

My dear Huxley,

At last I have collected sufficient facts about Wallace, chiefly from Miss Buckley. The enclosed paper is too long, but I could not well make it shorter.¹ The more I hear about Wallace, the more I think that you will do a really kind action if you can start with a few good signatures for a memorial. If there is any prospect of success I would send the memorial with notes to all the men on whom we fix, and would do anything else which I *possibly* could. But what form the memorial ought to take, I have no idea; and I hope that you will aid in this. Nor do I know how the memorial ought to be presented; if by a deputation of two or three, I would gladly come to London for the purpose. I have scribbled down the names of the persons whom to ask to sign, **that is if you approve**. I could approach Lord Aberdare Pres. Geograph. Soc. through Bates.² Miss Buckley suggested the Duke of Argyll; and I have reason to think that he would wish to aid Wallace; but I suppose it would never do to ask a Minister to sign a memorial to the Premier.³

I have seldom wished for anything so much, as to succeed in getting some provision for Wallace.

My dear Huxley, | Yours very sincerely | Charles Darwin

I am so anxious about this affair, that ask M^r. Huxley to send me a Post-card with word "received".⁴

P.S Very many thanks about letter to Nature.—

P.S. 2^d Your note about "dried" forms will give many a person a jolly laugh.⁵

[Enclosure]

Spottiswood	Pres.	R. Soc.
Huxley	Sec.	— do
Allman	Pres	Linnean Soc.
Flower	Pres.	Zoological Soc
Sclater	Sec.	— do
L ^d Aberdare	Pres.	Geograph. Soc.
Bates	Sec	— do
	Duke of Argyll???	
	Lubbock	
	Hooker	
	Self ⁶	

Perhaps (what do you think?)

Günther—Curator(?)	British Museum	(I could ask Gunther to sound Owen)
Newton	Professor of Zoology,	Cambridge
Rolleston	—	Oxford ⁷

This *altogether* makes 13 names with one or two additional doubtful men

M^r Smiles got a pension for Edwards, the Scotch Naturalist, & when he came down here for my signature, he told me that he felt sure that it was the best plan to have very few signers.⁸ He said this when I suggested some good men to him.—

LS(A)

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 346)

- ¹ Arabella Burton Buckley had sent a summary of Alfred Russel Wallace's claims for a government pension (see letter from A. B. Buckley, 7 November 1880 and enclosure, and Appendix VI). There is a draft of the memorial in DAR 196: 3; for a transcription, see Appendix VI.
- ² Henry Austin Bruce, first Baron Aberdare, became president of the Royal Geographical Society in 1880 (*Proceedings of the Royal Geographical Society and Monthly Record of Geography* 2 (1880): 772). Henry Walter Bates was assistant secretary of the society.
- ³ See letter from A. B. Buckley, 7 November 1880. George Douglas Campbell was lord privy seal under the prime minister, William Ewart Gladstone (*ODNB*).
- ⁴ Henrietta Anne Huxley; no postcard has been found, but see the letter from T. H. Huxley, 14 November 1880.
- ⁵ CD had asked for advice on his letter to *Nature*, 5 November [1880] (see letter to T. H. Huxley, 5 November 1880 and nn. 1 and 3). Huxley's reply and the note on 'dried' forms have not been found.
- ⁶ William Spottiswoode was president of the Royal Society of London and Huxley was secretary; George James Allman was president of the Linnean Society; William Henry Flower was president of the Zoological Society of London and Philip Lutley Sclater was secretary (*ODNB*). John Lubbock and Joseph Dalton Hooker.
- ⁷ Albert Günther was keeper of the zoological department at the British Museum and Richard Owen was superintendent of the natural history departments; Alfred Newton was professor of zoology and comparative anatomy at Cambridge University; George Rolleston was Linacre Professor of anatomy and physiology at Oxford University (*ODNB*).
- ⁸ Samuel Smiles had started a memorial to obtain a civil list pension for Thomas Edward; CD had agreed to sign (see *Correspondence* vol. 24, letter to Samuel Smiles, 16 December 1876, and Second 2003).

To Daniel Mackintosh 13 November 1880

Down. | Beckenham Kent. &c.
Nov. 13. 1880.

Dear Sir.

Your discovery is a very interesting one & I congratulate you on it.¹ I failed to find Shells on Moel Tryfan, but was interested by finding (*Philosoph Mag.* 3rd Series—Vol XXI p. 184) *shattered rocks* & far distant rounded boulders, which I attributed to the violent impact of icebergs or coast-ice—² I can offer no opinion on whether the more recent changes of level in England were or were not accompanied by earthquakes.— It does not seem to me a correct expression (which you use probably

from haste in in your note) to speak of elevations or depressions as caused by earthquakes;— I suppose that everyone admits that an earthquake is merely the vibration from the fractured crust when it yields to an upward or downward force— I must confess that of late years I have often begun to suspect (Especially when I think of the step-like plains of Patagonia, the heights of which were measured by me). that many of the changes of level in the land are due to changes of level in the sea.³ I suppose that there can be no doubt that when there was much ice piled up in the Arctic Regions the sea would be attracted to them, and the land on the temperate regions would thus appear to have risen— There would also be some lowering of the sea by evaporation and the fixing of the water as ice near the Pole—

I shall read your paper with much interest when published | & remain | Dear Sir.
| Yours faithfully. | Ch. Darwin.

Copy

DAR 146: 334

¹ See letter from Daniel Mackintosh, 11 November 1880 and n. 1.

² CD had described deposits on Moel Tryfan as shattered and rounded by icebergs grating over the surface ('Ancient glaciers of Caernarvonshire', p. 184). See also letter from Daniel Mackintosh, 15 January 1880.

³ Mackintosh cited CD on this point in his published article (Mackintosh 1881, p. 366).

From G. J. Romanes 13 November 1880

18 Cornwall Terrace:
November 13, 1880.

I am grieved to hear from Mr. Teesdale that his fears were only too well founded. Although I had not myself the privilege of Miss Wedgwood's acquaintance, I know, from what I have been told by those who had, how greatly your household must feel her loss.¹

I should not, however, have written only to trouble you with expressions of sympathy. I desire to ask you one or two questions with reference to an article on Hybridism which I have written for the 'Encyclopædia Britannica,' and the corrected proof of which I send.² It is in chief part an epitome of your own chapters upon the subject,³ and therefore you need not trouble to read the whole, unless you care to see whether I have been sufficiently clear and accurate. But there are two points on which I should like to have your opinion, both for my own benefit and for that of my readers. First, I think it is desirable to append a list of the more important works bearing upon the subject, and if I make such a list I should not like to trust to my own information, lest I should do unwitting injustice to some observing writers. If, therefore, you could, *without taking any special trouble*, jot down from memory the works you think most deserving of mention, I think it would be of benefit to the reading public.

E. D. Romanes 1896, pp. 100–1

- ¹ Elizabeth Wedgwood died on 8 November 1880 (CD's 'Journal' (Appendix II)). Romanes had learned of her illness from John Marmaduke Teesdale, a neighbour of CD's (see letter from G. J. Romanes, 5 November 1880).
- ² Romanes's article appeared in *EB* 9th ed. 12: 422–6 (G. J. Romanes 1881).
- ³ Romanes quoted at length from CD's chapter on hybridism in *Origin*, pp. 245–78, and from the extended discussion of the subject in *Variation* 2: 178–91.

To A. B. Buckley 14 November 1880

Down Beckenham
Nov. 14th. 1880

My dear Miss Buckley

I am very much obliged to you for sending me your new book, the appearance of which is most elegant.¹ I have read the two first chapters and shall hereafter read more; but just at present I have a lot of papers to read on account of work in hand.

I think that you have treated evolution with much dexterity and truthfulness; and it will be a very savage heretic-hunter who will persecute you. I daresay that you will escape, and you will not be called a dangerous woman.— Your plan seems to me an excellent one, and who can tell how many naturalists may spring up from the seed sown by you.— I heartily wish your book all success. At p. 4 I think you ought to except utter deserts, for I believe they support nothing.—² I believe that you might make an equally interesting book for the young about Plants.

Pray believe me, my dear Miss Buckley, your's sincerely | Ch. Darwin

I have despatched my paper about Wallace to Huxley and have spoken again to Sir John Lubbock.—³

Copy
DAR 143: 184

- ¹ Buckley's latest book was *Life and her children* (Buckley 1880; see also letter to A. B. Buckley, 16 August [1880] and n. 3).
- ² Buckley had remarked: "There is no spot on the surface of the earth ... which is not filled with life" (Buckley 1880, p. 4).
- ³ CD had sent a draft memorial to Thomas Henry Huxley in support of a civil list pension for Alfred Russel Wallace (see letter to T. H. Huxley, 13 November 1880 and n. 1). CD had previously spoken to John Lubbock about the pension (see letter to A. B. Buckley, 31 October [1880]).

From E. A. Darwin 14 November [1880]¹

6 Q. A. S^t
14 Nov

Dear Charles

Will you please sign my guarantee.— I had not intended to bother you & yesterday asked William² but in the mean time the Reg^r had taken my question if they would accept us as mutually joint Sec^{ies} for a proposal.³

My pride has had a fall I made the Reg^r look up the receipt for the sealed Certificates feeling confident there was none & he did find it signed by Cath Wedgwood of Parkfield.⁴ I never will be cock sure again of anything.

Horace must count Sir W^m Thompson among his lovers for yesterday Dykes, (do you remember him?) came to exhale himself on Horaces charm.⁵ Some time ago Sir W T told Dykes that when he came to Cambridge he would introduce Horace to him & that was the first he knew of his existence. Accordingly Sir W had Cayley Stokes (Adams?)⁶ & Horace to dine in Hall with him & then introduced Dykes who was sitting by Horace— The swells talked mathematics without intermission— he thought Cayley was the very dullest man he had ever met in his life & he afterwards told M^{rs} Adams⁷ that she really ought to educate him & she said she had done all she could in that way by supplying him with an infinite number of novels.

Yours affec | EAD

DAR 105: B114

¹ The year is established by Horace Darwin's move to Cambridge in 1880, his connection with William Thomson (see n. 5, below), and Erasmus's death in August 1881.

² William Erasmus Darwin.

³ Possibly a registrar of wills. The guarantee has not been found.

⁴ Catherine Wedgwood, CD's aunt, had resided at Parkfield in Staffordshire (Freeman 1978).

⁵ Horace had stayed at the home of William Thomson in April 1879 (letter from Horace Darwin to Emma Darwin, [18 April 1879], DAR 258: 825). Lamplugh Brougham Ballantine Dykes was a former schoolfellow of Erasmus's at Shrewsbury.

⁶ Arthur Cayley, George Gabriel Stokes, and John Couch Adams were all mathematicians and professors at Cambridge.

⁷ Adams's wife was Eliza Adams.

From T. H. Huxley 14 November 1880

4 Marlborough Place. | Abbey Road, N.W.
Nov^r. 14th 1880

My dear Darwin

The papers *in re* Wallace have arrived¹ and I lose no time in assuring you that all my 'might, amity & authority' as Essex said when that sneak Bacon asked him for a favour, shall be exercised as you wish²

The best course to pursue will need a little thinking over but there shall be no delay on my part in setting to work—

Your 'pinned-on' paragraph was so good that if I had written it myself, I should have been unable to refrain from sending it to the Printer. But it is much easier to be virtuous on other people's account—and though Thomson deserved it & more—I thought it would be better to refrain.³

If I say a savage thing it is only 'pretty Fanny's way'⁴ but if you do it is not likely to be forgotten.

The letter as it stands will be very useful in more ways than one

The wife⁵ is very much on her back still I am sorry to say

With all our love | Ever | Yours | T H Huxley

DAR 166: 353

¹ CD had sent a draft memorial and a list of potential signatories in support of a civil list pension for Alfred Russel Wallace (see letter to T. H. Huxley, 13 November 1880 and n. 1).

- ² The remarks are attributed to Robert Devereux, second earl of Essex, in reference to Francis Bacon's bid for the post of attorney-general in 1594: 'The Attorneyship for Francis is that I must have; and in that I will spend all my power, might, authority, and amity' (see Macaulay 1843, 2: 308). Bacon served as a prosecutor at Devereux's trial for treason in 1601 (*ODNB* s.v. Devereux, Robert).
- ³ CD had sent Huxley his letter to *Nature*, 5 November 1880; it was a reply to critical remarks on natural selection made by Charles Wyville Thomson in Thomson 1880, including a sentence that Huxley did not pass on to *Nature* (see letter to T. H. Huxley, 5 November 1880 and nn. 1 and 3).
- ⁴ The expression appears in the poem 'An elegy, to an old beauty' by Thomas Parnell: 'And all that's madly wild, or oddly gay, / We call it only pretty Fanny's way' (Parnell 1833, p. 81).
- ⁵ Huxley's wife was Henrietta Anne Huxley.

To Henry Johnson 14 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 14th 1880

My dear Johnson

An overwhelming lot of letters has prevented me from thanking you sooner for your answer about the slope of the ground at Worcester; also your daughter for her very kind note.¹ I hope before very long to hear again about my other bothersome questions. My heart & soul care for worms & nothing else in this world just at present!²

But I write now to say that I have directed a new book by me on the "Power of Movement in Plants" to be sent you,³ (& it will arrive in about a week) & to add that it is unreadable except by a specialist. If you read Introduction & last chapter you will know *whole contents*, except the evidence on each head.— Whilst correcting proofs, I bethought me of an old paper by you, but I had forgotten reference, & as far as my memory served me it was chiefly or exclusively on the movements due to difference in tension in different parts, as when a dandelion peduncle is split & such cases do not concern me.⁴ But if you have treated of other subjects, I shall be punished for my idleness.

I have written today, seven letters & am quite tired, so Farewell | Yours sincerely
| Ch. Darwin

Shrewsbury School, Taylor Library

- ¹ Johnson's letter and the note from his daughter, Mary Elisabeth Johnson, have not been found.
- ² CD's letter has not been found; he was working on *Earthworms*.
- ³ Johnson's name appears on the presentation list for *Movement in plants* (see Appendix IV).
- ⁴ Johnson had observed the movements of plants such as the common dandelion (*Taraxacum officinale*) after their stems had been divided or notched; he attributed the subsequent movements to a 'vital contractile power' analogous to the irritability of animal tissue (Johnson 1835a and Johnson 1835b; see *Correspondence* vol. 27, letter to Henry Johnson, 24 September 1879).

To James Paget 14 November 1880

Down, Beckenham, Kent. | Railway Station | Orpington. S.E.R.
November 14th 1880

My dear Paget

I am very much obliged for your essay which has interested me greatly.¹ What indomitable activity you have! It is a surprising thought that the diseases of plants

sh^d illustrate human pathology. I have the German Encyclopædia & a few weeks ago told my son Francis that the article on the diseases of Plants would be well worth his study, but I did not know that it was written by D^r Frank, for whom I entertain a high respect, as a first-rate observer & experimentiser, though for some unknown reason he has been a good deal snubbed in Germany.²

I can give you one good case of regrowth in plants, recently often observed by me, though only *externally*, as I do not know enough of histology to *follow out details*. It is the tip of the radicle of a germinating common bean. The case is remarkable in some respects, for the tip is sensitive to various stimuli & transmits an order, causing the upper part of the radicle to bend. When the tip (for a length of about 1mm) is cut transversely off the radicle is not acted on by gravitation or other irritants, such as contact &c &c, but a new tip is regenerated in from 2 to 4 days, & then the radicle is again acted on by gravitation & will bend to centre of earth. The tip of the radicle is a kind of brain to the whole growing part of the radicle.³

My observation will be published in about a weeks time, & I w^d have sent you the book, but I do not suppose that there is anything else in the book which would interest you.—⁴

I am delighted that you have drawn attention to galls.⁵ They have always seemed to me profoundly interesting. Many years ago I began (but failed for want of time, strength & health, as on infinitely many other occasions) to experimentise on plants, by injecting into their tissues, some alkaloids. & the poison of wasps, to see if I could make anything like galls. If I remember rightly in a few cases the tissues were thickened & hardened. I began those experiments, because if by different poisons I could have affected slightly & differently the tissues of the same plant, I thought there w^d be no insuperable difficulty in the fittest poisons being developed by insects so as to produce galls adapted for them.⁶ Every character, as far as I can see, is apt to vary. Judging from one of your sentences you will smile at this.—⁷

To anyone believing in my Pangenesis (if such a man exists) there does not seem to me any extreme difficulty in understanding why plants have such little power of regeneration; for there is reason to think that my imaginary gemmules have small power of passing from cell to cell.—⁸

Forgive me for scribbling at such unreasonable length; but you are to blame from having interested me so much.—

My dear Paget | Yours very sincerely | Ch. Darwin

P.S. | Perhaps you may remember that some 2 years ago you asked me to lunch with you & proposed that I sh^d offer myself again, whenever I next come to London, I will do so, & thus have the pleasure of seeing you.—

Wellcome Library (MS 5703/31)

¹ Paget 1880; see letter from James Paget, 12 November 1880 and n. 1.

² Paget had cited an article by Albert Bernhard Frank from *Encyklopädie der Naturwissenschaften* vol. 4, Die Pflanzenkrankheiten (see Paget 1880, pp. 611 n. and 613 n.); CD's copy of the *Encyklopädie* is in the Darwin Library–Down. CD cited Frank 1868 and Frank 1870 in *Movement in plants*. Francis Darwin had remarked on Frank's career difficulties in his letter of 29 May 1879 (*Correspondence* vol. 27).

- ³ CD experimented on the regrowth and bending of the tip or apex of the radical in *Vicia faba* (broad or fava bean) after amputation and the application of caustic (see *Movement in plants*, pp. 524–37). On the tip of the radical as analogous to the ‘brain’ of lower animals, see *ibid.*, p. 573.
- ⁴ Paget’s name does not appear on the presentation list for *Movement in plants* (see Appendix IV).
- ⁵ Paget compared galls to inflammation and morbid growths in animal tissue, such as cysts, tumours, and cancers (see Paget 1880, pp. 649–51).
- ⁶ For CD’s interest in galls and gall-making insects, see *Variation 2*: 282–5. In 1864, he suggested an experiment involving the insertion of poisons from insects and snakes, and other chemical compounds into plant tissues (see *Correspondence* vol. 12, letter to B. D. Walsh, 21 October [1864] and n. 6, and *Correspondence* vol. 13, letter to B. D. Walsh, 27 March [1865]).
- ⁷ While noting the great variety of galls and gall-making insects, Paget also remarked on the specificity and constancy of each form:
- And the whole process of the plant, though it be one of disease and, in a sense, unnatural, is yet so regular, so constant and specific, that the form and other characters of each gall or other morbid product are, usually, as constant and characteristic as are those of the insect itself.
- (Paget 1880, p. 650.)
- ⁸ On CD’s hypothesis of pangenesis and the role of gemmules, see *Variation 2*: 374–7; he cited Paget’s claim that the reparative power in organisms was the same as the power of growth in *ibid.*, p. 359. CD remarked on the possibility of gemmules passing through cell walls in his letter to *Nature*, [before 27 April 1871] (*Correspondence* vol. 19).

To G. J. Romanes 14 November [1880]¹

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 14th

My dear Romanes

Many thanks for your kind sympathy.— My wife’s sister was, I fully believe, as good & generous a woman as ever walked this earth.—²

The proof-sheets have not arrived, but probably will tomorrow.³ I shall like to read them, though I may not be able to do so **very** quickly, as I am bothered with a heap of little jobs which must be done.— I will send by today’s post a large book by Focke received a week or two ago on Hybrids & which I have not had time to look at, but which I see in Table of Contents includes full history of subject.⁴ & much else besides.— It will aid you far better than I can; for I have now been so long attending to other subjects & with old age, I fear I c^d make no suggestions worth anything. Formerly I knew the subject well.—

Kölreuter, Gärtner & Herbert are certainly far the most trustworthy authorities.⁵ There was also a German, whose name I mention in *Origin* who wrote on Hybrid Willows.⁶ Naudin, who is often quoted, I have much less confidence in.—⁷ By the way Nägeli (whom many think the greatest botanist in Germany) wrote a few years ago on Hybridism: I cannot remember title; but I will hunt for it, if you wish.⁸ The title will be sure to be in Focke.—

I quite agree with what you say about *Passiflora*.⁹ Herbert observed an analogous case in *Crinum*.¹⁰

Yours very sincerely | Ch. Darwin

American Philosophical Society (Mss.B.D25.574)

- ¹ The year is established by the relationship between this letter and the letter from G. J. Romanes, 13 November 1880.
- ² See letter from G. J. Romanes, 13 November 1880. Elizabeth Wedgwood had died on 8 November 1880 (CD's 'Journal' (Appendix II)).
- ³ Romanes had sent proof-sheets of his forthcoming article on hybridism (G. J. Romanes 1881).
- ⁴ Wilhelm Olbers Focke's book, *Die Pflanzen-Mischlinge*, included a chapter on the history of plant hybrids from the earliest times to the present (Focke 1881, pp. 429–45). CD's copy is in the Darwin Library–CUL.
- ⁵ Joseph Gottlieb Kölreuter, Karl Friedrich von Gärtner, and William Herbert.
- ⁶ Max Ernst Wichura's work on hybrid willows (Wichura 1865) is mentioned in *Origin* 4th ed., p. 315, and *Variation* 2: 267.
- ⁷ Charles Victor Naudin.
- ⁸ Carl Wilhelm von Nägeli had published a number of articles on hybridism. CD's copy of Nägeli 1866 (Darwin Pamphlet Collection–CUL) is extensively annotated.
- ⁹ Romanes discussed plants of species of *Passiflora* that were completely fertile with other species, and infertile with their own (G. J. Romanes 1881, p. 424); he drew the examples from *Variation* 2: 137–8.
- ¹⁰ Herbert had noted that various species of *Crinum* sent to Calcutta had crossed so freely that pure seed could not be obtained (Herbert 1837, p. 32). CD used this case as an example of pollen from a distinct species over time obliterating the action of a plant's own pollen (see *Cross and self fertilisation*, p. 395 n.). Herbert's successful crossing of *Crinum capense* with *C. revolutum* was also described by CD as a case of perfect fertility in a first cross between two distinct species (see *Origin*, p. 250; this passage is quoted in G. J. Romanes 1881, p. 424).

From A. R. Carrington 15 November 1880

Royal Agricultural College | Cirencester.
15 Nov. 1880—

Sir,

In reading one of your books lately, I came to a passage in which the non-existence of frogs in New Zealand is mentioned—¹ Some ten years ago I was in New Zealand, and attached to a Surveying party under Government.— We were engaged in traversing the bed of a very rocky mountain stream near a new digging settlement called Te Tiki situated on the Coromandel peninsula, not far from Kapanga or Coromandel— We had our camp in a very remote and almost inaccessible part of the range by the side of the stream and one day when I was off duty and engaged in tracing a small vein of quartz across the bed of the stream with the hope of finding gold, I found a small green frog in a crevice— It was about the size of the little green tree frogs found in the south of Europe, only of a darker green & slightly blotched with black or dark brown, and its feet were only partially webbed— I kept it in a match box for some time but it died, and having no spirit I was unable to preserve it— I lived in NZ altogether about 5 years, nearly always in the Bush, and this is the only one I ever saw or heard of—with the exception of some ordinary brown frogs w^h were said to have been found in the settled districts near Auckland, but for these I cannot answer—² I have taken the liberty of writing, as I thought the above facts, for which I can vouch, might be of interest to you—

I am, Sir, | faithfully yours | A. R. Carrington (Lecturer on Field-Engineering &c. R.A.C)

DAR 161: 50

¹ In *Origin* 6th ed., p. 350, CD wrote:

Bory St. Vincent long ago remarked that Batrachians (frogs, toads, newts) have never been found on any of the many islands with which the great oceans are studded. I have taken pains to verify this assertion, and have found it true, with the exception of New Zealand, New Caledonia, the Andaman Islands, and perhaps the Salomon Islands and the Seychelles.

² Four species of frog in the genus *Leiopelma* have been identified as native to New Zealand (see Bell and Bishop 2018, pp. 185–8). The Coromandel Peninsula is part of the native range of two of these species (*L. hochstetteri* and *L. archeyi*).

From R. F. Cooke 15 November 1880

50A, Albemarle Street, London. W.
Nov 15 1880

My dear Sir

We shall to reprint your work on the *Descent of Man*, as we require nearly 200 more copies than we have in stock.¹

Do you wish to make any corrections in the stereotype plates?²

Yours faithfully | Rob^l Cooke

Cha^s Darwin Esq

DAR 210.11: 19

¹ The most recent reprinting of *Descent* 2d ed. was in 1879; an additional thousand copies (the fourteenth thousand) were issued in 1881 (Freeman 1977).

² There were no known changes to the text of the 1881 reprint (Freeman 1977).

To G. J. Romanes 15 November 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Nov. 15th 80

My dear Romanes

I have just read your article.¹ As far as my judgment goes it is excellent & could not be improved.— You have skimmed the cream off the whole subject. It is also very clear.— One or two sentences near the beginning seem rather too strong; as I have marked with pencil, without attending to style.—² I have made one or two small suggestions.— If you can find my account in *Nature*,* (last summer I think) about the hybrid Chinese-gees inter se, it w^d be worth adding & w^d require only 2 or 3 lines.³ I do not suppose you wish to add but in my paper on *Lythrum* & I think requoted in *Var.* under *Dom.* Vol 2. 2^d Edit, bottom of page 167 I have a good sentence about a man finding 2 vars of *Lythrum* & testing them by fertility & coming to egregiously wrong conclusion.—⁴

I think your idea of reference to best books & short history of subject good.—⁵

By the way you have made me quite proud of my chapter on Hybridism; I had utterly forgotten how good it appears when dressed up in your article!!⁶

Yours very sincerely | Ch. Darwin

I have had a hunt & found my little article on Geese, which please hereafter return.—

American Philosophical Society (Mss.B.D25.575)

- ¹ Romanes had sent proof-sheets of his article on hybridism for the *Encyclopaedia Britannica* (G. J. Romanes 1881; see letter from G. J. Romanes, 13 November 1880).
- ² Romanes's article began with the etymology of 'hybrid' and its supposed Greek source, 'hubris', as 'an insult or outrage, with special reference to lust; hence an outrage on nature, a mongrel' (G. J. Romanes 1881, p. 422).
- ³ The article quoted CD's discussion in *Origin*, p. 253, of fertile hybrids produced from crosses between the common and the Chinese goose; Romanes added a note referring to CD's letter to *Nature*, 15 December [1879] (*Correspondence* vol. 27), which contained further details of the case, including experiments by CD himself (see G. J. Romanes 1881, p. 425 and n.). The Chinese goose is a domestic variety of the wild swan goose (*Anser cygnoides*). The common European domestic goose is a variety of the wild greylag goose (*Anser anser*).
- ⁴ For the description of possible mistaken conclusions about *Lythrum salicaria* (purple loosestrife) from a hypothetical botanist, see *Variation* 2d ed. 2: 167–8; CD's earlier paper on the subject was 'Three forms of *Lythrum salicaria*'.
- ⁵ Romanes's article included a long list of botanical works and authorities on hybridism, some of which were suggested by CD (see letter to G. J. Romanes, 14 November [1880], and G. J. Romanes 1881, p. 426).
- ⁶ Romanes quoted at length from CD's chapter on hybridism in *Origin*, pp. 245–78; and from the extended discussion of the subject in *Variation* 2: 178–91.

From B. J. Sullivan 16 November 1880

Bournemouth
Nov^r. 16/80

My dear Darwin

You gave me two Pounds last year The sum was made up by M^{rs}. FitzRoy you & I giving 2£ each. Stokes Mellersh Osborne & Hamond 1£ each.¹

Can you solve a Botanical riddle for me: five years since I got three new vines for greenhouse; a "white Muscat" was planted between a "Black Hambro" and an "Alicante"—(Black) The last year or two I allowed a few bunches to remain on each. and the Muscats were the usual colour. This year there were many fine bunches on all three, but after the Muscat grapes were nearly their full size they began to turn purple at the point of each grape, and it gradually extended to the stem. and they have ripened a darker colour than the Black ones. I enclose the skin of one.

The vines are about 6 feet apart; the grapes retain the exact character and flavour of the Muscat in all but colour.

Gardeners and gentlemen here who know most about vines tell me they have never heard of such a case. and are satisfied that the grapes retain the true Muscat Flavour &c.²

with kind regards | Believe me | very sincerely yours | B. J. Sullivan

DAR 177: 313

- ¹ See letter from B. J. Sullivan, 12 November 1880 and n. 1. No intervening letter from CD to Sullivan has been found. Maria Isabella FitzRoy was the widow of Robert FitzRoy, the former captain of HMS *Beagle*; John Lort Stokes, Arthur Mellersh, Alexander Burns Osborne, and Robert Nicholas Hamond were former officers of HMS *Beagle*.
- ² CD had discussed grape varieties and crossing in *Variation* 1: 332–4. The Alicante, Muscat and Black Hambro or Hamburg are some of the oldest varieties of grape; all are varieties of *Vitis vinifera*.

To Williams & Norgate 16 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 16th 1880

Dear Sir

Though the great Neapolitan work is of no use to me, I feel bound to become a subscriber, as the Zoolog. Station at Naples has done such admirable service to science.—¹ Will you therefore be so kind as to forward the enclosed & pay the charges, & you can send in your account whenever you think fit.—²

Dear Sir | Yours faithfully | Ch. Darwin

Swann Auction Galleries (dealers) (4 November 2010, lot 46)

- ¹ The first two volumes of the monograph series *Fauna und Flora des Golfes von Neapel* appeared in 1880 (Chun 1880 and Emery 1880; see letter from Anton Dohrn, 11 February 1880 and n. 7); CD's copies are in the Darwin Library–CUL. On CD's previous support for the Zoological Station at Naples, see *Correspondence* vol. 22, letter to Anton Dohrn, 7 March 1874.
- ² Dohrn later asked CD to accept the series as a gift (*Correspondence* vol. 29, letter from Anton Dohrn, 18 February 1881).

To B. J. Sullivan 17 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 17th 80

My dear Sullivan

I enclose a cheque for 2£ & am sorry to have troubled you—¹ Your case is a curious one, viz that of the direct action of the pollen of one variety on the mother plant of another variety.— If you care to hear of analogous cases, they are given in full in Vol I Ch XI (p 430 of 2^d Edit) of my *Variation of Animals & Plants under Domestication*.²

I keep fairly well & continue to work as hard as I can.— I was heartily glad to hear in your last note a better account of Mellersh—³

My dear Sullivan | Yours sincerely | Ch. Darwin

Sullivan family (private collection)

- ¹ Sullivan was collecting subscriptions to support Cooshaipunjiz (James FitzRoy Button), a grandson of Orundellico (Jemmy Button; see letters from B. J. Sullivan, 12 November 1880 and n. 1, and 16 November 1880). CD's earlier letter to Sullivan has not been found.
- ² Sullivan had asked about the influence of black grape varieties on white muscat grapes that had turned black after several seasons (see letter from B. J. Sullivan, 16 November 1880). CD discussed the effects of 'foreign pollen' on various parts of the mother-plant in *Variation* 2d ed. 1: 427–35.
- ³ On Arthur Mellersh's improved health, see letter from B. J. Sullivan, 12 November 1880.

From W. C. Williamson 17 November 1880

Fallowfield | Manchester
Nov 17/80

My Dear Dr Darwin

It has been a great satisfaction to me that you have received our Deputation so kindly.¹ I was only afraid lest it should tire you, knowing as I do how much conversation fatigues you when you are not quite well. My little regiment has been delighted beyond measure with your kind reception of them. They certainly were thoroughly & sincerely earnest in their work—

I only regret that I was unable to accompany them owing to home duties— We are worked to (de)ath here with our preparations for floating the Victoria University.² I had no idea that so much labour would be involved in working out the preparatory details. Allow me now however to add my small personal congratulations to those of my Yorkshire Colleagues

You may be interested to know that I *now* begin to see my way towards reconciling our Carboniferous Flora with Evolution. I have introduced a long paragraph to that effect into my memoir Part XI now in the hands of the Royal Society.³ My only fear is that in their craving for the curtailment of all memoirs, the Paper's Committee may cut it out. Anyhow I have thrown the responsibility of doing so on them

With kind regards to your son⁴—& regret that I have lost this opportunity of becoming acquainted with your domestic circle I am My Dear Sir | most sincerely yours | W C Williamson

Dont trouble to acknowledge (this)— I know that correspondence fatigues you & you must have much of it!

DAR 181: 110

¹ A deputation from the Yorkshire Naturalists' Union presented a memorial address to CD at Down on 3 November 1880; Williamson was president of the Union (see letter from W. D. Roebuck to G. H. Darwin, 25 October 1880 and n. 1).

² On the establishment of Victoria University, a federal university for the north of England, see J. Thompson 1886, pp. 536–43; Owen's College, Manchester, became affiliated to the University in 1880.

³ Part XI of Williamson's work on the fossil flora of the Coal Measures (W. C. Williamson 1871–92) was published in the *Philosophical Transactions of the Royal Society of London* 172 (1881): 283–305; on how his findings supported 'the doctrine of evolution', see *ibid.*, pp. 295–7.

⁴ Probably George Howard Darwin.

To Henry Johnson 18 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 18th 1880

My dear Johnson

Nothing can be clearer than your answers to my queries, & I am very much obliged to you for all the great trouble which you have taken.¹ I regret that I sh^d. have sent these questions at a time when you must have suffered from fatigue & distress.

I sh^d. very much like to visit Wroxeter,² but I have very little bodily strength, & such a journey would quite knock me up.—

According to your desire I will sign my name on the next page, but good Lord what geese people are about autographs.

Once again thanking you most truly I remain | yours very sincerely | Ch. Darwin

Charles Darwin | Down, Kent | Nov. 18th 1880.—

Private collection

¹ Johnson's letter and CD's queries have not been found. CD had previously asked Johnson to measure worm activity at Wroxeter in Shropshire (see *Correspondence* vol. 19, letter to Henry Johnson, 23 December 1871).

² In *Earthworms*, pp. 222–8, CD included detailed observations by Johnson of the thickness of vegetable mould in fields above the Roman remains at Wroxeter.

From G. J. Romanes 18 November 1880

18 Cornwall Terrace:
November 18, 1880.

Very many thanks for your kind assistance and expressions of approval.¹ It was stupid of me to forget your article in 'Nature' about the geese.² I now quite well remember reading it when it came out.

Focke's book is just the very thing I wanted, as it supplies such a complete history of the subject.³ If I do not hear from you again, I shall keep it for a few days to refer to when the proof which I have sent to press shall be returned with my historical sketch added.

I have now nearly finished my paper on the physiology of the locomotor system in Echinoderms. The most important result in it is the proof, both morphological and physiological, of a nervous plexus, external to everything, which in Echinus serves to co-ordinate spines, feet, and pedicellariæ in a wonderful manner.⁴ By the way, I remember once talking with you about the function of the latter, and thinking it mysterious. There is no doubt now that this function is to seize bits of seaweed, and hold them steady till the sucking feet have time to establish their adhesions, so assisting locomotion of animal when crawling about seaweed-covered rocks.

E. D. Romanes 1896, pp. 98–9

¹ CD had commented on proof-sheets of Romanes's article on hybridism for *Encyclopaedia Britannica* (G. J. Romanes 1881; see letter to G. J. Romanes, 15 November 1880).

² CD had suggested that Romanes include a reference to his letter to *Nature*, 15 December [1879] (*Correspondence* vol. 27) about hybrid geese.

³ Wilhelm Olbers Focke's book, *Die Pflanzen-Mischlinge*, contained a chapter on the history of plant hybrids (Focke 1881, pp. 429–45).

⁴ On the nervous system of *Echinus* (a genus of sea urchins) and its role in locomotion, see G. J. Romanes and Ewart 1881, pp. 835–6, 842–7. CD had discussed the evolution of pedicellariae in both sea urchins and starfish (sea stars), speculating on the function of the organs at different stages of development (see *Origin* 6th ed., pp. 191–2).

To W. C. Williamson 18 November 1880

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

Nov. 18th. 1880

My dear Professor Williamson

I must write one line to thank you very much for your kind note. Receiving the Deputation gave me real pleasure, and was an extraordinary honour.—¹

I hope that the passage to which you refer will not be omitted from your paper.—²

With many thanks | Yours sincerely | Ch. Darwin

Photocopy

DAR 221.4: 247

¹ See letter from W. C. Williamson, 17 November 1880 and n. 1.

² See letter from W. C. Williamson, 17 November 1880 and n. 3.

From G. H. Darwin 19 November 1880

Trin. Coll. Camb

Nov. 19. 80

My dear Father,

I received the book this morning & have just turned over the pages & looked at the wood-cuts.¹ I am rather disappointed to see how carelessly they are printed; they are not comparable to the proofs I saw at home. I notice a slight mis-statement Fig 149 was not from a photo. And surely 147 was not either² Fig 162 there *was* a photog. but so blurred that I drew the left hand one almost entirely from the plant as I believe. The right hand one was drawn entirely from nature.³ If I am not right in my memory of this I shall be enormously surprized & especially I remember the right hand fig. of 149 because it was the hardest one which was done entirely from nature. However this is all very unimportant.

I have been going on with ripple-marks & can now produce them in a flat bath with almost the regularity of a mathematical figure with wave lengths varying from perhaps 3 or 4 inches to $\frac{1}{2}$ inch.⁴ I can produce *no* ripple mark with currents & I believe that when ripple is produced along with current the current must be slow & wave motion must be going on on the surface I find a rough formula for wave length of ripple-mark as a multiple of the greatest velocity of the water relatively to the sand in the oscillatory motion. Also no ripple is produced if this max: vel: is less than $\frac{1}{2}$ a ft per sec. & again none if greater that 1.2 ft per sec. There is a marked tendency to sort the sand along the crests of the ripples.

I am inclined at present to think it is a complex affair partly depending on the rates at which different sized grains acquire the velocity of the water moving past them & partly on my previous theory of the sand being elongated I fancy I shall be able to reduce my apparatus so as to make absolutely regular ripples. They already are good. I got the other day 66 ripples round the bath with only one partially broken. The more I see the more I think Lyell utterly wrong.⁵

I am in despair about my astronomy. I am in this position that I *must* refer to my theory in its bearing on the solar system as a whole that I can not make any problem which shall reasonably represent the case & that I must write a sort of general discussion which will I fear be very unsatisfactory & will be more speculative than I like, and moreover I fear the outcome of the whole to any one who does not read thro'ly will be more unfavourable to the theory than I am convinced it sh^d. be.⁶

I have been rather better for about a week now & have worked spasmodically. My cold, quà cold, has much subsided, tho' it is at me pretty vigorously in the usual way.

Arthur Balfour has been here for a few days, but he had'nt got very much politics to tell.⁷ I dined last night at Horaces & met Miss Gladstone & afterwards we went to the Amateur Dramatic play.⁸ We went down in the tram-car in an awful crowd with Ida sitting on Miss G's lap, & then walked on thro' the rain. We had a fearful storm of wind & rain last night, all night thro'. & only stayed at the play for two or three acts. It was'nt at all good & I had seen it before—.

A.B. wants Horace to do another little job at Whittinghame but I don't think it will be much—⁹ It is something wh H. had suggested before.

I am dining at F. Balfour's tonight to meet Evans (J. E's son) who is come back from Bosnia & Ragusa for a short visit in England. I think you read his book.¹⁰

Your affec. son | G H Darwin

DAR 210.2: 87

¹ George had done several drawings for *Movement in plants*, which was published on 7 November 1880 (Freeman 1977). CD acknowledged George's work in *Movement in plants*, p. 8.

² See *Movement in plants*, pp. 356, 358. Both captions read, 'Copied from a photograph; figures reduced.' See plate in page 401 and frontispiece.

³ See *Movement in plants*, p. 385; the figure shows *Nicotiana glauca* with leaves expanded during the day and asleep at night. The caption indicates both figures were copied from photographs and reduced.

⁴ George had a special trough made to experiment on producing ripple-marks in sand covered by water (see letter from G. H. Darwin, 9 October 1880). George's experiments are described in his paper 'On the formation of ripple-mark in sand' (G. H. Darwin 1883). His work demonstrated that the formation of irregular ripple marks was due to vortices carrying sand up the lee slope while the direct current moved sand up the weather slope.

⁵ Charles Lyell had discussed the production of ripple-marks in *A manual of elementary geology* (C. Lyell 1851, pp. 19–21). He accounted for ripple-marks in sand (under air) as a result of the direct action of the wind on the weather slope alone (see *ibid.*, p. 20 fig. 9).

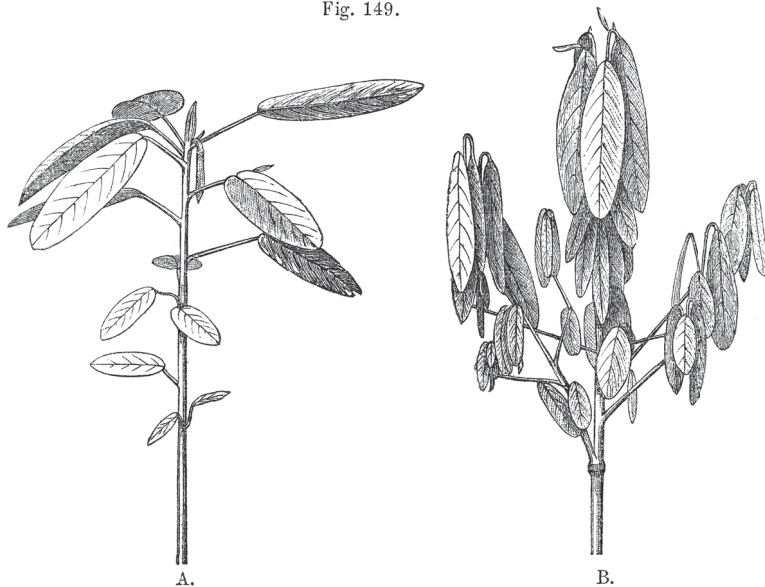
⁶ George had written a series of papers on tidal friction and related subjects (see, for example, G. H. Darwin 1878a, 1878b, 1879a, and 1880). This work related to the underlying problem of cosmic development; George presented a paper titled 'On the tidal friction of a planet attended by several satellites, and on the evolution of the solar system' (G. H. Darwin 1881) in January 1881. He later presented a non-technical account of tides and their connection with astronomy in *The tides and kindred phenomena in the solar system* (G. H. Darwin 1898).

⁷ Arthur James Balfour was an MP; he had been a student at Trinity College, Cambridge, at the same time as George.

⁸ Horace and Ida Darwin also lived in Cambridge. Helen Gladstone was the youngest daughter of William Ewart Gladstone; she was secretary to the vice-principal at Newnham College, Cambridge, at this time.

⁹ Whittingehame House in East Lothian, Scotland, was Balfour's family home (*ODNB*). The nature of the job has not been discovered.

Fig. 149.



Desmodium gyrans: A, stem during the day; B, stem with leaves asleep.
Copied from a photograph; figures reduced.

Desmodium gyrans.

Movement in plants, p. 358, fig. 149.

By permission of the Syndics of Cambridge University Library.

- ¹⁰ Francis Maitland Balfour, Arthur James Balfour's brother, was a lecturer in animal morphology at Cambridge. Arthur John Evans, the son of John Evans, had published a collection of letters sent during 1877, when he travelled in the Balkans for the *Manchester Guardian* (A. J. Evans 1878). He founded Casa San Lazzaro at Ragusa, Sicily, for the study of language, antiquities, and customs.

From B. J. Placzek¹ 19 November 1880

Hochverehrter Herr!

Die hohe Bedeutung, die Sie in Ihrem epochalen Werke "Das Variiren u.s.w." besonders I. cap. 6 der genauen Notirung der kleinsten Abweichungen in Structur und Gewohnheiten der Tauben beilegen,² veranlaßt mich, Ihren genialen Scharfblick auf eine ungewöhnliche Beobachtung hinzulenken. Dieselbe betrifft eine Flugart der Tauben, wie sie meines Wissens jetzt nicht mehr vorkommt und auch in den besten Monographien über Tauben nicht erwähnt erscheint, die aber für das Gesetz der Variabilität, bei dem Umstande, als jede eigenartige Bewegungsform auch eine besondere Entwicklung der entsprechenden Muskel- und Knochenpartien zur Folge haben muß, vom nicht geringem Belang sein mag.

In dem Buche "Bereschith-rabba" (einer Art Glossarium zur Genesis, aus dem 3^{ten} Jahrhundert v. Chr. stammend) ist Cap. 39 eine Bemerkung zu lesen, die in englischer Übersetzung lautet: "All birds rest of flying on a tree or on a rock, while the dove, when tired of flying alternatively holds one wing at rest, swinging herself upwards with the other wing".—³

Die Tauben haben demnach ihre Flugart seither geändert und dadurch eine für den Kampf ums Dasein ungemein vortheilhafte distincte Eigenschaft eingebüßt.—

Durch eine freundliche kurze Notiz über die erwähnte Beobachtung würden Sie sehr verbinden Ihren in aufrichtiger Verehrung | Ihnen ergebenen | D^r. B. Placzek | (Austria) Brünn, Thalgasse, 7.

Brünn 19.11.80

[Contemporary translation]⁴

The high importance which you in your { } work "The variation" &c, especially I ch VI. attack the accurate notice of the smallest variation in the structure and habits of pigeons induces me to direct your wonderful penetration to something hitherto unnoticed. It has to do with the manner of flight in pigeons, which, as far as I know, no longer occurs, & does not seem to me to be mentioned even in the best monographs on pigeons, but wh. may be of considerable importance as regards the law of variation, since every special form of Movement must be followed by a special development of the set of muscles and bones concerned in it. In the book "Bereschith raba" (a sort of glossarium on Genesis of the 3rd Century A.D) there is, in Ch. 39, a remark to the following effect: "All birds rest of {from} flying on a tree or on a rock, while the dove when tired of flying alternately holds one wing at rest,

swinging herself upwards with the other wing.” Pigeons have therefore changed their mode of flight, & have thereby lost a distinct property—specially advantageous in the struggle for existence.

By a kind short notice of the above observation you would much oblige your sincere admirer | D^r. B. Placzek | (Austria) Brünn | Thalgasse 7.

Brünn 19.11.80

DAR 174: 47

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

² CD discussed pigeons in *Variation* 1: 131–224 (chapters 5 and 6); most of the details of physiology and habit are in chapter 5.

³ *Bereshit Rabbah* is a commentary on the book of Genesis (*Jewish encyclopedia*). The passage, a gloss on Psalms 55: 6 (‘And I said: Oh, that I had wings like a dove! Then I would fly away, and be at rest’), may be translated as: ‘all the other birds, when tired, rest on a rock or a tree, but when a dove is tired, she draws in one of her wings and flies on with the other’ (see Placzek 1883 and Freedman and Simon eds. 1939, p. 317).

⁴ Curly brackets in the contemporary translation represent square brackets in the ms.

To B. J. Placzek [after 19 November 1880]¹

[Down.]

It appears to be impossible for a bird to use only one wing, without tumbling over and without losing its balance.²

Placzek 1883, p. 112 n.

¹ The date is established by the relationship between this letter and the letter from B. J. Placzek, 19 November 1880. The letter fragment appears in a note to Placzek 1883, p. 112.

² See letter from B. J. Placzek, 19 November 1880 and n. 3.

From J. V. Carus 20 November 1880

Leipzig
Nov. 20. 1880.

My dear Sir,

I am very sorry to be obliged to trouble you about a few sheets. In going on with the translation of your new book I came to a stoppage, as p. 145–208 are wanting.¹ I am not quite sure, if I have got them shortly before or after my short absence from Leipzig in the end of September, or if I have not received them at all. They may be mislaid amongst the heaps of new literature pouring in for the new Jahresbericht.² So I beg your pardon very heartily, but ask you to have sent these four sheets once again. I received the conclusion with the Index this morning.

I am delighted with the book, as it tells again a most wonderful lesson of methods of observation, patience and thought. The first Chapter will perhaps seem a little long to general readers, but I think it is one which ought to be read most carefully.

With my best wishes for your health (I am almost stiff with a severe rheumatism in my right shoulder) believe me | My dear Sir, | Ever yours sincerely, | J. Victor Carus

DAR 161: 114

¹ CD had been sending the proof-sheets of *Movement in plants* for Carus to translate into German (see letter to J. V. Carus, 21 September 1880).

² Carus was the first editor of *Zoologischer Jahresbericht*, published by the Zoological Station, Naples (see *Correspondence* vol. 29, letter from Anton Dohrn, 18 February 1881).

To R. F. Cooke 20 November 1880

Down, Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 20th. 1880

My dear Sir

The copies arrived all safe last night, & I like the appearance of the volume much.—¹

I wonder who in the world has been glorifying me in the Times—: it ought to sell a few more copies & then I shall not lose.—²

But my object in writing now is to thank M^r. Murray very much for his very handsome present of the three beautiful volumes, which I am extremely glad to possess. If admiration makes a man worthy of possessing St. John's book, I am worthy of it.— My old copy is all in tatters, as I have lent it to so many persons.³ The last person was Miss Balfour(, sister of A. Balfour M.P.) & she was as enthusiastic about the book, as I could have desired—⁴

Believe me, my dear Sir | Yours very sincerely | Ch. Darwin

P.S Please send me here **two** more copies of my book for foreign Botanists

Also a copy to Sir J. Lubbock Bart. at the Bank—Lombard St⁵

Please send soon—

National Library of Scotland (John Murray Archive) (Ms. 42152 ff. 380–1)

¹ *Movement in plants*.

² An anonymous review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9. CD was concerned that the costs of producing the book would not be recovered by sales (see letter to R. F. Cooke, 8 November [1880] and n. 2).

³ John Murray sent three copies of a recent illustrated edition of Charles St John's popular book, *Sketches of the wild sports and natural history of the Highlands* (St John 1878). CD's copy of St John 1878 is in the Darwin Library–Down. He evidently also owned the original 1846 edition, which is cited in *Expression*, pp. 47–8 n. 19.

⁴ Alice Blanche Balfour was Arthur James Balfour's sister.

⁵ John Lubbock was head of the banking firm Robarts, Lubbock & Co., at 15 Lombard Street, London. For CD's presentation list for *Movement in plants*, see Appendix IV.

To G. H. Darwin 20 November [1880]¹

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

Nov. 20th

My dear George

According to my memory the *sleeping* plant of fig. 149 was copied from photograph; but if more copies are struck off, I will look to the originals photographs.— I am almost certain that the sleeping leaf is 147.. So, as I thought, with 162.—²

I am very glad that you have been able to go on with ripple-marks, & I hope that you will publish the results.—³ How about ripples made by the wind, as is said to be the case.— If you make out theory of ripples they might give important information about some of the most ancient deposits.

I grieve to hear about your astronomical difficulties;⁴ but I daresay some light will dawn on you; anyhow it has often done so before.—

We have had Snow here for a week, whilst making drawings of Aunt Elizabeth's house & rooms.—⁵

What damnable weather it has been.

Farewell my poor dear old George. | Yours affect^{ly} | C. Darwin

I have been wonderfully glorified in the Times & I marvel who could have written it—not a physiological Botanist.—⁶

Our dear old mother has been much better of late, owing to her freedom from wearing anxiety.—

DAR 210.1: 98

¹ The year is established by the relationship between this letter and the letter from G. H. Darwin, 19 November 1880.

² George had remarked that several illustrations in *Movement in plants* had been drawn from nature, and not from photographs, as stated in the captions (see letter from G. H. Darwin, 19 November 1880 and nn. 2 and 3).

³ See letter from G. H. Darwin, 19 November 1880 and n. 4.

⁴ See letter from G. H. Darwin, 19 November 1880 and n. 6.

⁵ Snow was Frances Julia Wedgwood. Elizabeth Wedgwood had died on 8 November 1880 (CD's 'Journal' (Appendix II)); her house was advertised for sale in *The Times*, 4 June 1881, p. 15.

⁶ A highly favourable review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9.

From Anthony Rich 20 November [1880]¹

Chappell Croft, | Heene, Worthing.

Nov. 20.

My dear Mr Darwin

When you read the postmark on the envelope of this letter, you will, I doubt not, guess that it comes to thank you at once for a copy of the "Movement of Plants" which Murray sent me yesterday.² The meekest glance at its pages is sufficient to indicate the labour you must have had from first to last with such a book; and makes

me doubt whether my “empty little egg shell of a head” (to appropriate the Slade Professor’s effective definition of his pupil’s cranium)³ will be able to master all the special details of the volume; but I hope to make myself thoroughly acquainted with the main argument they are intended to support, and furnish additional proof of the truth of that grand theory upon which you have spent so many years of your life, and which is to render your name illustrious for generations.—

I saw in the Papers some few weeks ago the death of Miss Wedgwood, at Downe, the lady I conclude about whom you wrote that M^{rs}. Darwin had had so much anxiety.⁴ Any thing which gives pain or grief to M^{rs}. Darwin would be a source of sorrow to me. To say more than that would be a sort of officious impertinence on my part.—

A source of sorrow there was likewise in the account you gave of my “friend George” (if he will consent to accept that title). I had persuaded myself that a summer’s yachting with relaxation from labour either mental or bodily, would have procured him a sufficient stock of robust health to confront the coming winter with a bold face. The wish it seems must have been father to the thought.⁵ You say that he can not make up his mind exactly where to go for the winter months. Has he ever tried Rome? In my youth I spent six consecutive winters there, having caught a serious cold soon after leaving Cambridge, that from neglect or other causes and frequent relapses seemed determined to settle itself upon my lungs, and not to quit its hold until it had settled me. At the end of those years I returned home free from all delicacy in my chest and have remained sound in that respect ever since. I spent the summers as well as the winters in Italy, because the journey in those days to England and back was a long, trying, and expensive one, before railways were known, and steam carriage by water only in its infancy. But now for a traveller like him such a journey would be little more than a pleasant excursion.—

I can read your writing without any difficulty; and accept your compliments upon mine with pleasure for the sake of my correspondents, who are, fortunately for them in other respects but few—

Very truly yours, Anthony Rich

DAR 176: 144

¹ The year is established by the reference to *Movement in plants*, which was published in 1880.

² *Movement in plants* was published by John Murray; Rich’s name appears on CD’s presentation list for the book (see Appendix IV).

³ The quotation is from a letter attributed to John Ruskin, who had been Slade Professor of art at Oxford; the letter was widely circulated in the press in November 1880, although Ruskin denied having written it (see Hamilton 1882, pp. 14–15).

⁴ Elizabeth Wedgwood, Emma Darwin’s sister, had died on 8 November 1880 (CD’s ‘Journal’ (Appendix II)). Her death was reported in *The Times*, 9 November 1880, p. 1.

⁵ See letter from Anthony Rich, 4 June 1880. CD’s letter to Rich has not been found; on George Howard Darwin’s continuing health problems, see the letter from G. H. Darwin, 19 November 1880. George accompanied Frances Anna and William Thomson on their yacht (see letter from G. H. Darwin, 27 July 1880 and n. 3).

From S. H. Haliburton 21 November [1880]¹

Bridge House | Richmond | S.W.
Nov^r 21st

Dear Charles Darwin

(For I really cannot address you in any other way)²

Yesterday I read, in a leading Article of the Times, “Of all our living Men of Science, none have laboured longer, or to more splendid purpose than M^r Darwin”,³ & it recalled to my mind, your boyish assertion made many many years ago, that “if ever Eddowes’s Newspaper alluded to you, as “our deserving Fellow Townsman”, your ambition would have been amply gratified”—⁴

So you may believe with what sincere gratification, I see your fondest hopes, more than gratified, & realized— You have hosts of friends, but few older, or more sincere than myself, for you are associated with the happiest memories of my youth, & I have the most affectionate recollections of the name of Darwin, as connected with all that was good & pleasant— How my poor Father⁵ would have rejoiced in your “splendid success”, & I can fancy his carrying that Newspaper about, & reading it to every body!—

It is a long time since I have heard any thing of you, but I hope you are tolerably well, as I see you are able to receive “Deputations”—⁶

Let me hope we may live to meet again, meanwhile believe me always | Your’s very affectionately | S. H. Haliburton

DAR 99: 211–12

CD ANNOTATION

Top of letter: ‘done’ ink

¹ The year is established by the reference to the review of *Movement in plants* (see n. 3, below).

² Haliburton had addressed CD by his first name in their youth (see *Correspondence* vol. 1, letter from Sarah Owen, 18 February [1828]).

³ Haliburton quotes the first sentence of the review of *Movement in plants* in *The Times*, 20 November 1880, p. 9.

⁴ *Eddowes’s Journal* was a weekly newspaper published in Shrewsbury (North 1997, s.v. *Salopian Journal and Courier of Wales*). Haliburton had recalled the same ‘boyish assertion’ in her letter of 3 November [1872] (*Correspondence* vol. 20).

⁵ William Mostyn Owen.

⁶ CD had received a deputation from the Yorkshire Naturalists’ Union at Down on 3 November 1880 (see letter from W. C. Williamson, 17 November 1880); the visit was reported in *The Times*, 19 November 1880, p. 4.

From Daniel Mackintosh 21 November 1880

36 Whitford Road | Tranmere, | Birkenhead,
21st Nov. 1880.

Dear Sir,

I am much obliged by the receipt of your kind letter of the 13th. I admit I was writing rather carelessly when I spoke of earthquakes as a geological cause in my last letter.¹

With regard to changes in the level of the sea, the existence of shelly beach deposits at between about 1,100 and 1350 f. in mountain districts at a considerable distance from each other (as for instance, the Wicklow hills, Moel Tryfan, S. of Minera (Denbyshire) and above Macclesfield) would certainly seem to be better explained by the surface of the sea being higher than by the land being lower; but I have found it better not to complicate the subject by entering much into this question.²

I have not read of any geologist having noticed the shattered rocks under the drift of Moel Tryfan excepting yourself.³ I have seen them twice, at different levels in the new quarries, and you must have seen them in the old at a lower level. I look upon them as the most remarkable phenomon connected with Moel Tryfan, excepting the discovery of sea-shells.

With many thanks, | I am Dear Sir, | Your obliged & faithful Servant, |
D. Mackintosh.

P.S. In the new or upper quarry sections, there is clear evidence of the floating ice having come from the N.W. or from the N. of Ireland.

DAR 171: 11

¹ See letter to Daniel Mackintosh, 13 November 1880.

² See letter to Daniel Mackintosh, 13 November 1880 and n. 3.

³ Mackintosh cited CD's paper on the drift deposits on Moel Tryfan ('Ancient glaciers of Caernarvonshire') in Mackintosh 1881, p. 352.

From A. R. Wallace 21 November 1880

Pen-y-bryn, St. Peter's Road, | Croydon.
Nov: 21st. 1880

My dear Darwin

Many thanks for your new book containing your wonderful series of experiments & observations on the movements of plants.¹ I have read the introduction and conclusion, which shows me the importance of the research as indicating the common basis of the infinitely varied habits and mode of growth of plants. The whole subject becomes thus much simplified, though the nature of the basic vitality which leads to such wonderful results remains as mysterious as ever.

Yours very faithfully | Alfred R. Wallace

Charles Darwin F. R. S.

DAR 106: B149

¹ Wallace's name appears on the presentation list for *Movement in plants* (see Appendix IV).

From V. O. Kovalevsky [after 21 November 1880]¹

14, Jermyn Street St. James

Dear Sir

After nearly nine years absence I am again for a few days in London and should be very happy to see You for an hour or so.² My life was not a very quiet one, but

now I have settled in Moscow, where they proposed me the chair of Geology at the University.³ I have heard this Sunday from M^{rs} Huxley⁴ You are going well, have had a glance of Your new book⁵ and shall be glad to see You before leaving London, if possible Thursday.

Yours truly | W. K(owalevsky)

DAR 169: 98

¹ The year is established by the appointment of Kovalevsky at Moscow University (see n. 3, below); the day and month are established by the relationship between this letter and the letter to V. O. Kovalevsky, 25 November [1880]; in 1880, the Sunday before 25 November was 21 November.

² Kovalevsky may have visited Down on 29 November (letter from Emma Darwin to H. E. Litchfield, [28 November 1880] (DAR 219.9: 253)); he and his wife, Sofia Vasilyevna Kovalevskaya, later met CD for lunch at Queen Anne Street, London (see letter to G. H. Darwin, 9 December [1880]).

³ Kovalevsky was appointed associate professor at Moscow University in 1880 (*DSB*).

⁴ Henrietta Anne Huxley.

⁵ *Movement in plants*.

From F. M. Balfour [22 November 1880]¹

Trinity College, | Cambridge.
Monday

My dear Mr Darwin

I have to thank you very much for a copy of your work on the movements of plants.² I have as yet only read the introduction & the last chapter.

To offer any praise of the work would be a form of gross impertinence on my part, but I may say that it has been to me a complete revelation—The remarkable nervous system without nerves, for I do not know what else to call it, the existence of wh you have proved, must have a most important bearing on speculations as to the origin of the nervous system in animals—³ One is almost led to wonder why a nervous system has become developed, when it is possible for so perfect an arrangement can exist without any corresponding structural differentiations

Your's very sincerely | F. M. Balfour

DAR 160: 27

¹ The date is established by the reference to *Movement in plants*; CD received his copies on 19 November 1880 (letter to R. F. Cooke, 20 November 1880). The Monday following 19 November 1880 was 22 November.

² Balfour's name appears on the presentation list for *Movement in plants* (see Appendix IV).

³ CD compared sensitivity and the transmission of movement in plants and animals, and likened the tip of the radical in plants to the brain of lower animals (see *Movement in plants*, pp. 572–3). See also letter to F. M. Balfour, 4 September 1880.

To J. V. Carus 22 November 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 22^d/80

My dear Sir

Your note has pleased me exceedingly, for I fully expected that you would find the book intolerably dull, though the result of much labour.—¹

I have written for the missing sheets & they shall be despatched on arrival.—

I am sure that they were sent to you, & you will some day discover them amongst your papers.

As I remember that you like to keep a perfect copy of my books, I despatched one yesterday by the Post.—²

My dear Sir | Yours very sincerely | Ch. Darwin

I am now at work on a very little book on Worms³

Staatsbibliothek zu Berlin – Preußischer Kulturbesitz (Slg. Darmstaedter Lc 1859; Darwin, Charles, Bl. 183–184)

¹ See letter from J. V. Carus, 20 November 1880. Carus had been receiving proof-sheets of *Movement in plants* to complete his German translation.

² Carus's name appears on CD's presentation list for *Movement in plants* (see Appendix IV).

³ *Earthworms*.

From W. E. Darwin 22 November [1880]¹

Bank
Nov 22

My dear Father,

Thank you for the copy of your book.² I am much interested in reading a good deal of it; it almost makes one dizzy to think of the enormous labour of so many observations. I was delighted to see the article in the Times.³ What a wonderful sign of change of feeling that the old Times dares to write such an article without a sneer or a smile of pity.

It was really interesting and explained well I thought to the world in general the drift of your work—and it certainly is a compliment to have not merely a notice but an article.

I hope soon to go over to Beaulieu to get some worm castings,⁴ but the days are very short, & it is difficult to catch a fine day on which I can get away early enough.

My gardiner⁵ is an odd mixture of theory & sense; he has a great reverence for worms and calls them “our civil engineers”. I promised to tell Frank⁶ that he says that in order to make a shoot bend in any direction he only plucks off a few leaves on that side and he says the sunlight playing on the stem bends it over.

Goodbye dear Father I hope Mother is well. it is very jolly to think of Christmas being so near | Your affect son | W. E Darwin

Cornford Family Papers (DAR 275: 83)

CD ANNOTATION

4.2 I promised ... side 4.4] *double scored red crayon*, ‘I remember analogous case’ *blue crayon*

¹ The year is established by the allusion to *Movement in plants* (see n. 3, below).

² William's name appears on CD's presentation list for *Movement in plants* (see Appendix IV).

³ A highly favourable review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9.

⁴ Beaulieu Abbey was in Hampshire; see letters to W. E. Darwin, 18 June 1880 and 10 September [1880].

⁵ William's gardener has not been identified.

⁶ Francis Darwin.

To S. H. Haliburton 22 November 1880

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

Nov. 22^d 1880

My dear Sarah.

You see how audaciously I begin; but I have always loved & shall ever love this name.—¹ Your letter has done more than please me, for its kindness has touched my heart. I often think of old days & of the delight of my visits to Woodhouse & of the deep debt of gratitude which I owe to your Father.² It was very good of you to write. I had quite forgotten my old ambition about the Shrewsbury newspaper; but I remember the pride which I felt when I saw in a book about beetles the impressive words “captured by C. Darwin”.³ Captured sounded so grand compared with caught. This seemed to me glory enough for any man! I do not know in the least what made the Times glorify me, for it has sometimes pitched into me ferociously.⁴

I should very much like to see you again; but you would find a visit here very dull, for we feel very old & have no amusements & lead a solitary life. But we intend in a few weeks to spend a few days in London;⁵ & then if you have anything else to do in London you would perhaps come & lunch with us.

Believe me my dear Sarah | Yours gratefully & affectionately | Charles Darwin

My health is better than it was & I am able to do daily a good deal of work, but 24 hr^s never pass without some discomfort, & I am easily tired. Nevertheless there is much to make me happy & life is still an enjoyment.—

DAR 185: 24

¹ See letter from S. H. Haliburton, 21 November [1880] and n. 2.

² Haliburton's father, William Mostyn Owen, was the squire of Woodhouse in Shropshire; on CD's friendship with Mostyn Owen, see *Correspondence* vol. 2, letter from William Owen Sr, 21 November 1838).

³ The Shrewsbury newspaper was *Eddowes's Journal* (see letter from S. H. Haliburton, 21 November [1880] and n. 4). On CD's beetle specimen, see 'Recollections', p. 379, and *Correspondence* vol. 1, letter to W. D. Fox, [15 July 1829] and n. 1; the publication in which CD's name appeared was J. F. Stephens 1828–46 (*Illustrations of British entomology*).

⁴ Haliburton had read the review of *Movement in plants* in *The Times*, 20 November 1880, p. 9. A highly critical review of *Descent* had appeared in *The Times*, 7 April 1871, p. 3, and 8 April 1871, p. 5; see *Correspondence* vol. 19, letter to John Murray, 13 April [1871].

⁵ CD stayed at Erasmus Alvey Darwin's house in London from 7 to 11 December 1880 (CD's 'Journal' (Appendix II)).

From J. D. Hooker 22 November 1880

Royal Gardens Kew

Nov. 22/80

Dear old Darwin

I must just thank you for the “Movements”, which seems a most capital production, & I am so pleased to see Franks name associated with your's in it—¹ I have read only two chapters, vii & viii. & they are splendid, but I hate the zigzags!—² *Bauhinia* leaf closing is a curious case; does it not show that said leaf consists of two leaflets?—³

The fact that for good action the leaves want a good illumination during the preceding day is very suggestive of experiments with the electric light. They are like the new paint that only shines by night after sun-light by day.⁴ There are heaps of points I should like to know more about.

Dyer & Baker are taken aback by the keel of the *Cucurbita* seed;—which keel was a wonderful discovery in *Welwitschia*!!!⁵

I have had no time to read more than the 2 chapters as yet, for I have a stock of half read books on hand & no time for any of them. I am only $\frac{2}{3}$ through Wallace;⁶ it is splendid— what a number of cobwebs he has swept away.— that such a man should be a Spiritualist is more wonderful than all the movements of all the plants.⁷

He has done great things towards the explanation of the N. Zeal Flora & Australian, but marred it by assuming a preexistent S.W. Australian Flora—⁸ I am sure that the Australian Flora is very modern in the main; & that the S.W. peculiarities are exaggerations due to long isolation during the severance of the West from the East by the inland sea or straits that occupied the continent from Carpentaria to the Gt. Bight. I live in hopes of showing by an analysis (botanical) of the Australian types, that they are all derived from the Asiatic continent.—⁹

Meanwhile I have no chance of tackling problems— I must grind away at the Garden, the Bot. Mag & Indian Flora, which I cannot afford to give up, & Gen. Plant. which alone I delight in.¹⁰ I am at Palms, a most difficult task: but sometimes weeks elapse & not a stroke of work done! I am getting very weary of “working for a living”, & am beginning to covet rest & leisure in a way I never did before; but I must first look out for the education of three sons,—all hopeful I am glad to say, but one still an infant!¹¹

The Grays will be back in a fortnight, they have changed their plans & will spend 2 or 3 winter months here & then go abroad (with us) for the spring.¹² They will go into lodgings in Kew. We contemplate getting out a paper or book on the distribution of U.S. plants together (as one of Hayden’s Reports.)¹³

Have you read Pagets Lecture on plant diseases?¹⁴ it is very suggestive & a wonderful specimen of style aiding in giving great importance to possibly very superficial resemblances between animal & vegetable malformations: still there must be a great deal in the subject to be investigated.

I suppose we should get “Nobbe’s Handbuch der Samenkunde”.—¹⁵ is it an expensive work— our funds for purchase are rather short— but if inexpensive book I will order it at once

Ever affy Yrs | J D Hooker

Paget has started the idea of a Vegetable Pathologist for Kew & I have asked him to corkscrew Gladstone¹⁶ about it.—

We were very sorry to see Miss Wedgwoods death in the paper— I fear that Mrs Darwin will feel it a great deal.¹⁷

DAR 104: 142-5

CD ANNOTATIONS

9.1 I suppose ... at once 9.3] *scored red crayon and pencil*; ‘Answer’ *red crayon*

End of letter: 'what w^d you use for zig-zag | The account of cutting off tip | Last chapter' pencil del ink

- ¹ Hooker's name appears on CD's presentation list for *Movement in plants* (see Appendix IV). The words 'assisted by Francis Darwin' appear below CD's name on the title page of the book.
- ² *Movement in plants* contained numerous diagrams showing circumnutation over time; CD described many of the patterns as 'zigzag' (see, for example, *ibid.*, p. 71).
- ³ In *Movement in plants*, pp. 373–4, CD described the two halves of each leaf of *Bauhinia* rising up and closing completely at night, 'like the opposite leaflets of many Leguminosae'. While in Germany, Francis had observed *Bauhinia richardiana*, reporting: '2 large leaflets drop' (see *Correspondence* vol. 26, letter from Francis Darwin, [12 July 1878]).
- ⁴ CD remarked that in some genera it was indispensable that leaves be well illuminated during the day in order that they should assume a vertical position at night (see *Movement in plants*, pp. 318–19). On the new luminous paint, see the *Chemical Gazette*, 17 December 1880, p. 302.
- ⁵ William Turner Thiselton-Dyer and John Gilbert Baker. In *Movement in plants*, pp. 102–6, CD described the development of a heel or peg on the summit of the radicle that aided in opening the seed-coats in species of Cucurbitaceae, the cucumber family. On a similar structure in *Welwitschia*, see Bower 1881, pp. 27–8 (see also letter from W. T. Thiselton-Dyer, [after 23 November 1880]).
- ⁶ Alfred Russel Wallace's new book, *Island life*, was dedicated to Hooker (Wallace 1880a).
- ⁷ Hooker had been highly critical of Wallace's spiritualism; see *Correspondence* vol. 24, letter from J. D. Hooker, [24 September 1876], and *Correspondence* vol. 27, letter from J. D. Hooker, 18 December 1879.
- ⁸ Wallace remarked that parts of south-western Australia were especially rich in 'purely Australian types' of flora, and concluded that it was a 'remnant of the more extensive and more isolated portion of the continent in which the peculiar Australian flora was principally developed' (see Wallace 1880a, pp. 463–4).
- ⁹ Hooker had written an essay on the flora of Australia and Tasmania (J. D. Hooker 1859); however, he never published another major work on the subject.
- ¹⁰ Hooker was the director of the Royal Botanic Gardens, Kew, editor of *Curtis's Botanical Magazine*, and had been engaged for many years in the multi-volume works *The flora of British India* (J. D. Hooker 1872–97) and *Genera plantarum* (Bentham and Hooker 1862–83).
- ¹¹ Hooker's three youngest sons were Brian Harvey Hodgson Hooker, Reginald Hawthorn Hooker, and Joseph Symonds Hooker.
- ¹² Hooker and his wife, Hyacinth Hooker, had planned to join Asa Gray and his wife, Jane Loring Gray, in Italy in December (see letter from J. D. Hooker, 24 September 1880 and n. 2).
- ¹³ J. D. Hooker and Gray 1880 was published in the *Bulletin of the United States Geological and Geographical Survey*, edited by Ferdinand Vandeveer Hayden.
- ¹⁴ CD had received a copy of James Paget's lecture (Paget 1880; see letter to James Paget, 14 November 1880).
- ¹⁵ In *Movement in plants*, p. 105 n., CD had referred to Friedrich Nobbe's *Handbuch der Samenkunde* (Handbook of seed science; Nobbe 1876).
- ¹⁶ William Ewart Gladstone was the prime minister.
- ¹⁷ Elizabeth Wedgwood, Emma Darwin's sister, had died on 8 November 1880 (CD's 'Journal' (Appendix II)). Her death was reported in *The Times*, 9 November 1880, p. 1.

From O. A. Ainslie 23 November 1880

48—Lincolns Inn Fields—
Nov^r. 23^d 1880—

Private

My <de>ar D^r Darwin

I observe with s(ome) regret, that a relative of Mrs Darwin, has passed <a>way—¹ I suppose, that she was the recent proprietor of Tromer Lodge—formerly called Pond House—My lamented Father—the Rev^d Robert Ainslie—visited Down, & had <an

in)terview with Miss Wed(g)wood—when he was re(ctor) at Roundhill Crescen(t,) Brighton—& some few years or so—² I b(elieve)—befo(re) his illness—which ultimately obliged him to give up his p() work— Now, I u(ndersto)od then, & have a(lwa)ys understood, that (the) transfer of Tro(m)er Lodge to the Purchasers, was in opposition to a higher offer, which was made at the Auction Mart— This of co(u)rse rests as it is— My Father had not funds to appeal. () there were some (p)ieces of Basso Relievo (le)ft in the Drawing (Ro)om & I suppose still the(re), which he laid a (cla)im to on the occasion of his visit (to) Miss Wedgewood.

If one of his creditors, who was the principal, who led him into his (dif)ficulties, which he so nob(ly con)tended with, had () a few years ea(rlier) () tle and I would () have () our fa(ther's) () me" (h)ouse."— He die(d,) I c(onsider, a *victim* to (his) generosity, & the *negl(ect)* of th(e) Public to a () Man—who if he had n(ot) had a considerable (pri)ivate property woul(d) not have died so () as he did— (2 or 3 words) in writing (2 or 3 words) whom I we(ll) remember, as a Boy myself,—& who was once an acquaintance & (on f)riendly terms with m(y fa)ther— is to request t(hat, i)n the event of Tr(omer) Lodge bei(ng) again in (the) market, I may ha(ve) the e)arliest intim(atio)n of the fact, thro(ugh so)me member of you(r or) your wife's famil(y or) (in the) possible event of (y)our () to be lamented decease.

() also to ask you to (k)indly inform me—((pri)vately) whether the (1 or 2 words) old deeds of the () ()ve been transferred (2 or 3 words) (as I believe) Possessor³

With the greatest (2 or 3 words) the honour () (Da)rwin Yrs mo(st) () Ainslie

D^r Charles Darwin— F(RS)

DAR 159: 11a (fragile)

¹ Elizabeth Wedgwood had died on 8 November 1880 (CD's 'Journal' (Appendix II)).

² Robert Ainslie was a Methodist minister who had lived in Down at Pond House (later Tromer Lodge) from 1845 to 1858 (see *Correspondence* vol. 7, letter to W. E. Darwin, 14 [May 1858] and n. 5). The house was sold by Ainslie in 1862 and purchased by Elizabeth Wedgwood in 1868 (see *Correspondence* vol. 10, letter to J. B. Innes, 22 December [1862], and *Emma Darwin* (1904) 2: 218–19).

³ Tromer Lodge was renamed 'Tower House' and advertised for sale in *The Times*, 4 June 1881, p. 15. When the house was sold on 13 August 1881, a new title was created with the following stipulation: 'the purchaser shall not require the production of or investigate or make any requisition or objection in respect of the prior title whether the same shall appear to be in the vendor's possession or power or not' (London Borough of Bromley Archives, sale particulars collection, 1200/258).

From Alphonse de Candolle¹ 23 November 1880

Genève

23 nov. 1880.

Mon cher Monsieur

Je m'empresse de vous remercier de votre nouveau volume sur les Mouvements des plantes. Il me parait d'un grand intérêt. Je me réjouis de le lire, et mon fils qui s'est occupé du sujet en profitera encore mieux. Asa Gray, dans son petit ouvrage

destiné aux théologiens, avait insisté d'une manière qui m'avait étonné sur l'absence de limite claire entre les deux règnes.² Vous donnez des faits bien curieux à l'appui. Je ne lis jamais un ouvrage de vous sans penser au plaisir que mon père³ en aurait éprouvé s'il avait vécu plus longtemps, car il était bien de ces savants dont nous parlions qui aimaient les choses nouvelles et originales, même contraires à ce qu'ils avaient admis dans leur jeunesse.

Comme les ruisseaux doivent couler dans les fleuves, je veux vous raconter un fait qui confirme ce que vous avez dit sur les hybrides (*Animals and plants under domestication* 2 p. 44). Il m'a été donné par M^r Ed. Bertrand, zélé apiculteur, qui publie *l'Apiculteur de la Suisse romande*.⁴ Je l'avais questionné sur les effets des piqûres d'abeilles et il me dit, entre autres choses:

“Ce sont surtout les abeilles croisées (appelées hybrides ou métisses) qui nous piquent. Le croisement le plus commun est celui de l'abeille jaune ou italienne avec l'abeille noire ou commune. Je reçois 50 piqûres d'une ruche croisée pour 10 d'une noire ou 1 d'une italienne pure. C'est à peu près la proportion. Les races carniolienne, italienne et assure-t-on caucasienne (celle-ci surtout) sont plus douces avec les hommes que la race commune. Mais un mélange de sang les rend toutes beaucoup plus méchantes. Il en est de même chez les mulets, et les bardots, avec cette différence que les croisés abeilles sont féconds. Cette méchanceté des hybrides est universellement admise, comme un fait général comportant des exceptions; elles sont excellentes comme butineuses”

Je n'ai pas eu le temps d'expliquer à Monsieur votre fils un procédé graphique dont je me suis servi pour représenter les faits d'hérédité. Il faudra que je vous soumette une fois un dessin. Pour le moment je suis occupé d'une seconde rédaction du chapitre de ma Géographie botanique sur l'origine des plantes cultivées, qui fera un petit volume in-8°. Les faits découverts depuis 1855 ne font absolument que confirmer, soit les assertions soit les doutes, de mon premier travail. C'est satisfaisant dans un sens, mai peu amusant à exposer.⁵

Mes compliments, je vous prie, à Madame Darwin, et à M^r Francis⁶ et croyez moi toujours, mon cher Monsieur, | votre très dévoué | Alph. de Candolle

DAR 161: 26

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

² Candolle's name is on the presentation list for *Movement in plants* (see Appendix IV). Candolle's son, Casimir de Candolle, had published on movements of *Dionaea muscipula* (Venus fly trap) and winding tendrils (C. de Candolle 1876 and 1877). For Asa Gray's comments on the vegetable and animal kingdoms, including chlorophyll in animals and insectivorous plants, see *Natural science and religion* (A. Gray 1880, pp. 10–19).

³ Candolle's father was Augustin Pyramus de Candolle.

⁴ In *Variation* 2: 44, CD discussed reversion from crossing, citing hens produced from a cross of non-sitting breeds that recovered the instinct to sit on eggs. Edouard Bertrand edited *Bulletin d'apiculture pour la Suisse romande* from its first publication in 1879 (Crane 1999, p. 456).

⁵ See A. de Candolle 1855, 2: 809–993; the revised version was *Origine des plantes cultivées* (The origin of cultivated plants; A. de Candolle 1883).

⁶ Francis Darwin.

To G. H. Darwin 23 November [1880]¹

[Down.]

My dear George—

I am extremely glad about the ripples; but you do not explain how they form long, continuous lines. Do they start on any little projection, & do your eddies, acting laterally carry them on transversely to the stream.²

I cannot remember having seen drift snow rippled. I think that rippled sand has been seen at 20 fathoms depth, where the water was very clear near the Channel Isl^{ds}— I daresay your visualising is correct—anyhow my memory now-a-days is worth nothing & I ought not to have trusted to it— If I have $\frac{1}{2}$ hr. to spare I will look to photographs.—³ I heard from old Anthony Rich the other day & he says that “my friend, George, if he will permit me to call him so”, ought to spend the winter in Rome, as he found it did his health such wonderful good.—⁴

I am delighted to hear that light is dawning on the Planetary system in your eyes.—

How goes on the Pendulum.—⁵

Good night my dear George | C. D.—

Nov 23^d

DAR 210.1: 99

¹ The year is established by the relationship between this letter and the letter from G. H. Darwin, 19 November 1880.

² CD had asked for details about George’s ripple theory in his letter of 20 November [1880]; George’s reply has not been found.

³ See letter from G. H. Darwin, 19 November 1880 and nn. 2 and 3. George had queried the source of some of the woodcuts in *Movement in plants*.

⁴ Rich wrote ‘my “friend George” (if he will consent to accept that title)’ (see letter from Anthony Rich, 20 November [1880]).

⁵ George and Horace Darwin were constructing a pendulum to measure the lunar disturbance of gravity (G. H. Darwin 1907–16, 5: 1; see letter from G. H. Darwin, 27 July 1880 and n. 3).

To W. E. Darwin 23 [November 1880]¹

Down, Beckenham, Kent. | Railway Station | Orpington. S.E.R.
23rd (Nov. 1880.).

My dear old W.

Your note has pleased me much.² I write in hurry to catch post, as I have just remembered that you have an Acacia (*Robinia pseudo-acacia*) in your garden, and probably there are others in Rogers’ garden.³ I hear that worms draw the petiole of the leaves into the mouth of their borrows, thus



I want much to know whether they draw them in by blunt base or by apex—and whichever end is drawn in, whether this is uniformly done. It is rather late for this work, but I daresay you could find old tufts, and if pulled carefully up you could soon see which end has been drawn in.

In a very few days I shall know better whether it would be important to me to have castings from Beaulieu.⁴

Have had very buttery letters from Hooker and Dyer about Book. The publication will not cost me quite so much as I expected. Murray has sold 800 copies. The Times ought to help.⁵

Good bye my dear old fellow | C. Darwin.

Read only last Chapt. of my book.⁶

Copy

DAR 153: 137

¹ The year and month are established by the references to the publication of *Movement in plants* (see n. 5, below).

² See letter from W. E. Darwin, 22 November [1880].

³ William Henry Rogers ran Red Lodge nursery in Southampton. For the observations made in February 1881 of petioles of *Robinia pseudoacacia* (black locust or false acacia) extracted from worm burrows, see *Earthworms*, pp. 81–2. In 1877, CD and William had made observations of the movements of the leaves of this tree in William's garden; see *Correspondence* vol. 25, letter from W. E. Darwin, [24 August 1877].

⁴ CD had written that he might need William to acquire more worm-castings from Beaulieu Abbey, Hampshire; see letter to W. E. Darwin, 10 September [1880].

⁵ For Joseph Dalton Hooker's receiving of *Movement in plants*, see the letter from J. D. Hooker, 22 November 1880; William Turner Thiselton-Dyer's letter has not been found, but see the letter to W. T. Thiselton-Dyer, 23 November [1880]. CD had agreed to cover any losses from the sale of *Movement in plants* and was expecting to lose £50 if all 1000 copies sold; see letters from R. F. Cooke, 4 November 1880 and 10 November 1880. John Murray was CD's publisher. A review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9.

⁶ The last chapter of *Movement in plants*, pp. 546–73, was titled 'Summary and concluding remarks'.

To J. D. Hooker 23 November 1880

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

Nov. 23^d/80

My dear Hooker

Your note has pleased me much; for I did not expect that you would have had time to read *any* of it.—¹ Read the last chapt. & you will know whole result, but without the evidence. The case, however, of radicles bending after exposure for an hour to geotropism, with their tips (or brains) cut off is, I think worth your reading (bottom of p. 525); it astounded me. The next most remarkable fact, as it appears to me (p. 148) is the discrimination by the tip of the radicle between a slightly harder & softer objects affixed on opposite sides of tip. But I will bother you no more about my book.— The sensitiveness of seedlings to light is marvellous.—²

I have read Wallace with the greatest interest & admire it *extremely*; but I cannot swallow or digest all his conclusion;³ perhaps my brain-digestion is weak from old age. I can hardly credit that the opening of one or two N. & S. sea-channels across

Europe & Asia w^d warm Arctic regions enough for them to support such organisms as they formerly supported. It stumps me to believe that northern plants travelled down Andes to Fuegia,—thence to the supposed Antarctic continent, &c thence by insular halting places to S. Australia & (I think to) C. of Good Hope. That they sh^d have been subjected to such a series of changes & retained the same specific character is to me almost incredible.⁴ But what I most object to is that seeds have been blown from one mountain-summit to another distant one. Wallace argues from dispersal to oceanic islands; but in this case we have sea-currents, & birds with dirt on feet & base of beak & seeds in stomach.⁵ No bird w^d fly directly from the tops of the Alps to the tops of the Pyrenees, & fancy seeds being often thus blown & not merely to the tops, but to where a debacle had recently occurred. I still remain convinced that when same species inhabits distant mountain-summits, they must formerly have inhabited intermediate low-lands,—⁶ Though Wallace argues very well against several former glacial periods & almost convinced me, I feel inclined to admit them, as the sole means of explaining the temperate forms at the C. of Good Hope. How I wish you had time to take up this great subject again. It pleased me to see how well Wallace appreciates your work.—⁷

I feel inclined to abide by view where oceans now extend, they have always (ie since Silurian times) nearly extended, & so with continents; but it is an awfully perplexing subject.—

I was delighted with Pagets Essay;⁸ I hear that he has occasionally attended to this subject from his youth. I thought he made too much of the symmetry of decay in leaves.— I am very glad he has called attention to galls— this has always seemed to me a profoundly interesting subject; & if I had been younger would take it up.—⁹ By Jove his essay will have borne good fruit if it gives you a pathologist.—¹⁰

Frank shall answer about Nobbe, when he comes home.—¹¹

What word could I use for zig-zag which you hate so much?¹²

With hearty thanks | farewell my dear Hooker. | Yours ever | Ch. Darwin

Reading this over again I do not think I have expressed strong enough admiration of Wallace's book.— His weakness is want of sound judgment, as it appears to me,— videlicet Spiruatilism.—¹³

DAR 95: 496–9

¹ Hooker had commented on *Movement in plants* in his letter of 22 November 1880.

² See letter from J. D. Hooker, 22 November 1880 and n. 4.

³ CD had sent Alfred Russel Wallace notes on his new book, *Island life* (Wallace 1880a; see letter to A. R. Wallace, 3 November 1880 and enclosure.

⁴ On the existence of large inland seas and their warming effects, see Wallace 1880a, pp. 82, 92–4, 504; on the importance of mountain chains as a means of migration for plants from the northern to the southern hemisphere, see *ibid.*, pp. 480–91. See also letter from A. R. Wallace, 8 November 1880.

⁵ On wind as a means of seed dispersal, see Wallace 1880a, pp. 248–9. See also letter from A. R. Wallace, 8 November 1880 and n. 7.

⁶ For CD's theory of migration across lowlands, see *Origin* 6th ed., pp. 338–40.

⁷ Wallace's book was dedicated to Hooker and cited Hooker's work on geographical distribution (especially J. D. Hooker 1859) throughout.

⁸ Hooker had praised James Paget's lecture (Paget 1880; see letter from J. D. Hooker, 22 November 1880. For CD's comments on the lecture, see the letter to James Paget, 14 November 1880.

- ⁹ On symmetry of decay in leaves, see Paget 1880, pp. 611–12; on galls, *ibid.*, pp. 649–51; for CD's interest in galls, see the letter to James Paget, 14 November 1880 and n. 6.
- ¹⁰ Hooker had mentioned the possibility of appointing a vegetable pathologist at the Royal Botanic Gardens, Kew (see letter from J. D. Hooker, 22 November 1880).
- ¹¹ Francis Darwin was in Cambridge (letter from Francis Darwin, [11 or 12 November 1880]). Hooker was interested in purchasing a copy of Nobbe 1876 (see letter from J. D. Hooker, 22 November 1880 and n. 15).
- ¹² See letter from J. D. Hooker, 22 November 1880 and n. 2.
- ¹³ *Videlicet*: to wit, namely (Latin). See letter from J. D. Hooker, 22 November 1880 and n. 7.

From Frederick McDermott 23 November 1880

Common Room, | Middle Temple. E.C.
23rd Nov 80.

Sir,

The reason of my intrusion—which I trust you will pardon—is this. I have a great desire to read your books—the more so after finding (in his Life written by his wife) that Charles Kingsley strongly recommended them¹—but I am a busy man & not at all a clever man, and if I am to have pleasure in reading your books I must feel that at the end I shall not have lost my faith in the New Testament. My reason in writing to you therefore is to ask you to give me a Yes or No to the question Do you believe in the New Testament. If you could answer me Yes I should most gladly enter upon the study of your wonderful books but without that assurance I fear my brain is not fine enough to argue out doubts which might be suggested by your works, but if I can say that the author of these doctrines believes as I do that Christ was the Son of God, I can say it is only in matters of detail that M^r Darwin differs from Charles Kingsley and I may read with full pleasure of all the wonders of nature which he has collected.

If you will write on the back of this page Yes or No you will be doing a real kindness which I will certainly not abuse by sending a paragraph to the theological papers headed “M^r Darwin on the New Testament”

yours truly | Fred^{rk} M^cDermott.

DAR 201: 22

¹ Frances Eliza Kingsley's memoir of Charles Kingsley included letters that expressed enthusiasm for CD's work (see Kingsley ed. 1877, 2: 135–6, 171–3, 247–50). See also *Correspondence* vol. 7, letter from Charles Kingsley, 18 November 1859.

From Samuel Newington 23 November 1880

Ticehurst.
23^d Nov^r 1880.

My dear Sir

I have read the critique in the Times of your book on the “Movement of Plants”.¹ I think it might have been more scientific. I perceive it refers to the sensibility of

the radicles. I was just writing an article for one of the Hort. papers.² I think I have proved that heat is generated by the exudation of oxygen from the bark of the peripheral roots, (& probably from that of all roots) by the combination of oxygen with carbonaceous matter. I think I have also proved that there is an electrical action always going on from the leaves to the rootlets. the air being positive & the earth negative. I have not yet worked out a sympathetic system on which these currents act, but since the sap is never in a quiescent state I have presumed it is kept moving by the electric forces passing from the air through the leaves to the roots.

I have such a short time in the morning to work out my favorite studies but I have not yet been to get up the article for publication.³

It is very evident that Zoophytes have the property of secreting gastric juice to render soluble albuminous matters.⁴

There is still a large field of discovery left, I suppose you are aware that the step in walking or running is synchronous with the pulse, or rather the pulse with the step. & however fast a person runs the action of the heart commences at the same moment. This is no doubt due to the whole mass of blood being lifted up by the movement of the body, the valves of the heart & vessels preventing the return of the blood, the intention being to supply oxygen in the same ratio for the disintegration of nervous & muscular tissues, it is a discovery I made some years since when I passed it on to my old friend, Sir J Hirshcell, from whom I received a letter corroborating my views, since then I have made accurate experiments with the Sphygmograph & proved my views correct.⁵

You may recollect the grapes I sent you, last season I cut off the vine which I believed caused the berries to assume their normal shape & allowed the Madresfield Court to grow about four times the size of the Black Hamburgh. & by this means the former overcame the established type, & the berries were those of the Madresfield Court. & splendid berries they were, I grow them in large tubs & do not water them for six weeks before they are ripe, & thus prevent splitting.⁶

believe me Yours very truly | Samuel Newington

DAR 172: 37

¹ A review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9.

² The article has not been found. In his letter of 2 September 1875 (*Correspondence* vol. 23), Newington mentioned having written articles on the exudation of carbonic acid in solution from the rootlets of plants and the exudation of oxygen in solution, neither of which have been found.

³ Newington was superintendent of the mental hospital at Ticehurst House, Sussex.

⁴ The term zoophyte generally referred to any animal that superficially resembled a plant, including those now classified within the phyla Cnidaria (corals, sea anemones, true jellyfish, etc.) and Ctenophora (comb jellies); see, for example, Grant 1834.

⁵ Newington had sent CD the letter from John Frederick William Herschel corroborating his views on pulse and step in 1875; see *Correspondence* vol. 23, letter to Samuel Newington, 17 September 1875. Herschel's letter has not been found.

⁶ In 1875, Newington had described and said he would send grapes from grafted vines of Black Hamburgh and Madresfield, varieties of *Vitis vinifera* (wine grape); see *Correspondence* vol. 23, letter from Samuel Newington, 2 September 1875.

To W. T. Thiselton-Dyer 23 November [1880]¹

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

Nov. 23^d

My dear Dyer

Very many thanks for your most kind note, but you think too highly of our work, not but what this is very pleasant.—²

I am deeply interested about *Welwitschia*. When at work on the pegs or projections, I could not imagine how they were first developed before they could have been of mere mechanical use.³ Now it seems possible that a circle between radicle & hypocotyl may be permeable to fluids & thus have given rise to projections so as to expose larger surface. Could you test *Welwitschia* with permanganate of potassium, if like my “pegs” the lower surface w^d be coloured brown like radicle & upper surface left white like hypocotyl.—⁴ If such an idea, as yours of absorbing organ had ever crossed my mind, I w^d have tried my hypocotyls in weak C. of Ammonia to see if it penetrated on line of junction more easily than elsewhere.— I daresay projection in *Abronia* & *Mirabilis* may be an absorbant organ.⁵

It was very good fun bothering the seeds of *Cucurbita* by planting them edgewise, as would never naturally occur & then the peg could not act properly.⁶ Many of the Germans are very contemptuous about making out use of organs; but they may sneer the souls out of their bodies, & I for one shall think it the most interesting part of Natural History.

Indeed, you are greatly mistaken if you doubt for one moment on the very great value of your constant & most kind assistance to us.

I have not seen the pamphlet & shall be very glad to keep it.⁷ Frank,⁸ when he comes home will be much interested & pleased with your letter—.

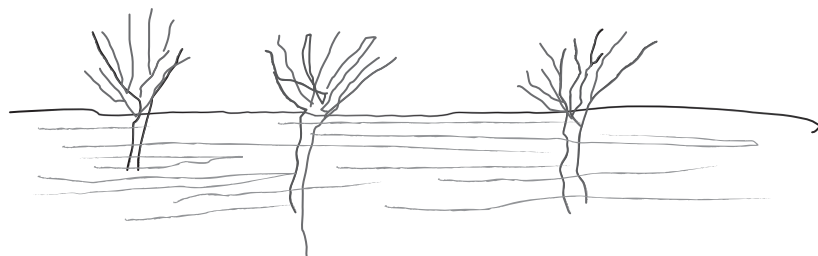
Pray give my kindest remembrances to M^{rs} Dyer.⁹

Ever yours very sincerely | Ch. Darwin

This is very untidy note, but I am very tired with dissecting worms all day.—

Read last Chapter of our Book & then you will know whole contents.—

P.S. | When next you walk in garden look under any tree on bare ground or on poor turf of *Robinia pseudo-acacia* for me,—& see if worms have drawn the petioles of leaves into mouths of burrows.¹⁰



I want to know whether they uniformly or almost uniformly draw them in by basal, blunt end or by apex. I do not suppose that they are now at work, but by pulling up any old tuft you w^d easily see which end of petiole was within mouth of burrow.—

My whole soul is absorbed with worms just at present!

Royal Botanic Gardens, Kew (Darwin: Letters to Thiselton-Dyer, 1873–81: ff. 209–11)

¹ The year is established by the reference to *Movement in plants*.

² The letter from Thiselton-Dyer has not been found; it contained remarks on *Movement in plants* (see letter to W. E. Darwin, 23 [November 1880]).

³ In *Movement in plants*, pp. 102–6, CD had discussed the role of the heel or peg in opening the seed-coats in species of Cucurbitaceae and several other plants; Joseph Dalton Hooker had mentioned Thiselton-Dyer's interest in this point (see letter from J. D. Hooker, 22 November 1880 and n. 5).

⁴ CD had applied permanganate of potassium to the petioles in *Megarhiza californica*, and to the peg in *Cucurbita ovifera*, noting that these organs were stained brown like the radicle, suggesting that they acted functionally like a root (see *Movement in plants*, pp. 81, 102–4).

⁵ CD had described the heel at the base of the hypocotyl in *Abronia umbellata* (sand verbena) in *Movement in plants*, pp. 105–6; on a similar structure in *Welwitschia mirabilis*, see the letter from J. D. Hooker, 22 November 1880 and n. 5.

⁶ See *Movement in plants*, pp. 103–4.

⁷ Bower 1881 (see letter from W. T. Thiselton-Dyer, [after 23 November 1880], n. 2).

⁸ Francis Darwin.

⁹ Harriet Anne Thiselton-Dyer.

¹⁰ CD reported on the manner in which leaves of *Robinia pseudoacacia* (black locust or false acacia) were drawn into worm burrows in *Earthworms*, pp. 81–2.

From W. T. Thiselton-Dyer [after 23 November 1880]¹

monocotyledons. When you say that the peg in *Abronia* is absorbent I suspect you mean in a different sense to what I do. Bower's paper will come out in the January Q. J. M. S.²

I have been looking at worm-burrows here and it seems to me that in the case of *Robinia* either end of petiole is drawn in indifferently. But I have been only able to look at their behaviour on grass. It may be that the mechanical difficulties prevent their exercising the choice they might do on bare ground³

Believe me | yours sincerely | W. T. Thiselton Dyer

Incomplete

DAR 178: 105

CD ANNOTATION

1.1 monocotyledons ... M. S. 1.3] *crossed ink*

¹ The date is established by the relationship between this letter and the letter to W. T. Thiselton-Dyer, 23 November [1880].

² In his letter to Thiselton-Dyer of 23 November [1880], CD had expressed an interest in Thiselton-Dyer's comments about *Welwitschia* from a now missing letter; CD had thought that the pegs or

projections in the seedlings of *Abronia* (sand verbena) and *W. mirabilis* might be absorbent organs. Thiselton-Dyer used the term ‘absorbing organ’ in the typical botanical sense, i.e. the sense in which roots absorb water or leaves absorb sunlight (active absorption). CD clearly means it in the sense of staining (passive absorption), since he refers to trying potassium permanganate (KMnO₄) on the peg; see *ibid.* and n. 4. Frederick Orpen Bower’s paper ‘On the germination and histology of the seedling of *Welwitschia mirabilis*’ (Bower 1881) was published in the *Quarterly Journal of Microscopical Science* in January 1881.

- ³ CD had asked Thiselton-Dyer to observe whether worms drew the petioles of leaves of *Robinia pseudo-acacia* (black locust or false acacia) into the mouths of their burrows, and, if they did so, by which end, the apex or base; see letter to W. T. Thiselton-Dyer, 23 November [1880].

To Frederick McDermott 24 November 1880

Down, | Beckenham, Kent. (Railway Station | Orpington. S.E.R.)
Nov. 24th 1880

Private

Dear Sir

I am sorry to have to inform you that I do not believe in the Bible as a divine revelation, & therefore not in Jesus Christ as the son of God.¹

Yours faithfully | Ch. Darwin

Bonhams, New York (dealers) (21 September 2015)

- ¹ In his letter of 23 November 1880, McDermott had asked whether CD believed in the New Testament.

From Raphael Meldola 24 November 1880

Offices | 50, Old Broad Street. | E.C. | Atlas Works, | Hackney Wick, | London, E.
Nov. 24th 1880

My dear Mr. Darwin,

I must beg you to excuse the trouble I am about to put you to in asking your kind assistance in a small matter connected with Part II of Weismann’s “Studies”.¹ I really feel bound in the first place, however, to offer you some explanation of the great delay which has occurred in the appearance of this work. Owing to the very small amount of my leisure time I took up the work in the first place with the idea of acting chiefly as editor—i.e. getting the translation done roughly at first & then revising it myself. I tried one translator who is a good German scholar but knows nothing of Biology, so that the revision of his work gave me far more trouble than if I had translated the whole of it myself. I then got one or two other translators to give me specimens of their work, but the result was the same—absolute nonsense! I determined therefore to do the whole work myself. Part II is now partly printed & I will I hope be out soon after Xmas.

The Author & the reviewers have all expressed themselves satisfied with Part I—so I suppose I must be.

Now to the object of this letter. Some time ago you were good enough to lend me ‘Kosmos’ for Dec. 1877 containing a paper by Fritz Müller which furnishes so

many beautiful illustrations of Weismann's conclusions that I propose to translate it (with the permission of the Editors— are you not one?) & give it as an Appendix to Part II. For this purpose I have just procured a copy of this No. of *Kosmos*, but on looking at the title of Fritz Müller's paper ("Beobachtungen an brasialanischen Schmetterlingen") I find that it is headed "III", as though it were the third of a series of communications on this subject. If, without giving any great trouble, you could kindly let me know the Nos. of this publication which contain the previous papers I would order them (through Friedländer).² It is possible that they may also contain observations bearing on the subject in hand & as 'Kosmos' appears to be in very few private libraries to which I have access & as I have no time to go reference hunting at the libraries of the learned Societies I hope you will pardon my troubling you.

Please address to "21 John St. Bedford Row, London, W.C."³

I am extremely glad to hear such good accounts of your health & hope that we shall long hear reports equally favourable.

I always saw that Sir C. W. Thomson had failed to grasp the idea of Natural Selection & am glad that an opportunity has occurred of putting him to rights on this subject.⁴

Yours very truly, | R. Meldola.

DAR 171: 140

CD ANNOTATIONS

3.1 Some time ago ... Fritz Müller 3.2] *scored red crayon*

3.6 "Beobachtungen ... Schmetterlingen 3.7] *underl red crayon*

*End of letter: I August. | II October 1877*⁵ *blue ink*

¹ Meldola was translating August Weismann's *Studien zur Descendenz-Theorie* (Weismann 1875–6). CD had subscribed to the translation (Weismann 1880–2), which was published in three parts; see *Correspondence* vol. 27, letter to Raphael Meldola, 12 December [1879].

² CD had lent Meldola the October 1877 issue of *Kosmos* containing part two of 'Beobachtungen an brasilianischen Schmetterlingen' (Observations on Brazilian butterflies; F. Müller 1877); see *Correspondence* vol. 26, letter from Raphael Meldola, 2 January [1878]. R. Friedländer und Sohn was a bookseller and scientific publisher based in Berlin.

³ Twenty-one John Street, London, was Meldola's home address until 1886. This letter was sent from his work address; he worked from 1877 to 1885 at the Atlas Works, where he developed aniline dyes and photographic developers (Travis 2010, pp. 145 and 152–3).

⁴ For CD's critique of Charles Wyville Thomson's understanding of natural selection, see the letter to *Nature*, 5 November [1880].

⁵ The annotations are notes for CD's reply of 25 November 1880.

From G. E. Mengozzi¹ 24 November 1880

Rome | 3 Piazza del Popolo
24 Novembre 1880

Illustre Monsieur,

Avec grand plaisir je reçu a temp Votre Portrait, firmé par Vous, à Londres. Je Vous en remercie infiniment.²

Ma première pensée à Rome, c'était de convoquer la Magistrature de la Royale et Imperial Accademie "La Scuola Italica" pour Vous proposer à Premier Ancien d'Honneur de la même. À unanimité et avec enthousiasme ils Vous ont accueilli en se tenant très honorés d'avoir a Membre d'honneur le Prince vivant de la philosophie de la Nature.

Moi en me faisant interprète de Votre grand Esprit, j'ai assurée mes Collegues, que Vous aurai accepté avec bon gré, cet signe obsequieux que l'Italie il Vous offre, quoique soit peu de chose devant Vous.

Dans cette occasion, moi comme Membre Fondateur depuis l'année 1860 (et le Roi d'Italie, Humbert 1^{er}. Président General d'Honneur) et obligée que je suis de Votre noble accueil faite à mes pauvres lettres, je me prend la liberté de Vous dédier un des mes livres; "La générations des animaux et nouvelle Classification des êtres naturels" ci Vous me le permettez il sera honorée et il acquêtera ce prix que il n'a pas en lui.

Pour à présent je Vous prié d'agrèer le livre que je Vous envoie comme signe de ma reconnaissance et devotion; avec ces sentiments je me repète maintenant et toujours³ | De Vous Illustre Monsieur | Devouè | Comm. Prof^c G. E. Mengozzi M.D.

Ill. Monsieur | Charles Darwin | Down— Londres

DAR 202: 112

¹ For a translation of this letter, see Appendix I. Mengozzi's French (not his native tongue) was extremely poor.

² In his letter of 28 October 1880, Mengozzi had sent a portrait of CD and asked CD to sign it.

³ Mengozzi had founded the Accademia Nazionale, la Scuola Italica, in 1860; Umberto I was king of Italy from 1878 to 1900. No copy of *Nuova classificazione degli esseri naturali e saggio sulla generazione degli animali* (New classification of natural beings and essay on the generation of animals; Mengozzi 1881) has been found in the Darwin Library–CUL or the Darwin Library–Down; it contained a dedication to CD on pp. vii–xxv.

To G. E. Mengozzi [after 24 November 1880]¹

[Down.]

Dear Sir.

I beg leave to acknowledge & thank you for your extremely k. l. of Nov. 24th & for the gift of your magnificent volume.

I further beg that you will give to your Society my cordial thanks for the great honour which they have in so distinguished manner conferred on me.—²

I remain, Dear Sir | With much respect | Yours faithfully & much obliged | C. D.

ADraftS

DAR 202: 112v

¹ The date is established by the relationship between this letter and the letter from G. E. Mengozzi, 24 November 1880.

² With his letter of 24 November 1880, Mengozzi sent Mengozzi 1881, which contained a dedication to CD on pp. vii–xxv, and told CD he had been made the first honorary member of the Accademia Nazionale, la Scuola Italiana.

From O. A. Ainslie 2[5] November 188[0]¹

48—Lincolns Inn Fields— | W. C
Thursday Nov^r. 2(5^t)^h | 188(0)

D(ear) D^r Darwin

I am really v(ery) much obliged to you for your letter, & only sorry, that I should give you the trouble (o)f writing—² You are probably (a)ware of the decease of my lamented Sister, M^{rs} Reynolds, in January—³ She had been some time prepared for the grea(t) change, but her end at l(a)st was somewhat sudden— I think in my (n)ote to you—I said “principal creditor”—(by a slip of the (pen) instead of “principal D(ebtor)” to my Father—⁴ (2 or 3 words) shall communicate with M(r) Wedgwood about the old tit(le) deeds—leases and mor(tgages) which I fancy w(ere) ()ed to Miss Wedgwoo(d)⁵ (1 or 2 words) M^{rs} Reynolds, but () not necessary I unders()d to complete the () I have had nothing to do with these matters as my sister—at My Father’s desire, I believe, acted through D^r Reynolds,⁶ when necessary I do not care at present to make enquiries of D^r Reynolds, & therefore I intimated my desire, that this corre(s)pondence should be “privatim”— () I am tolerably sure that he (wou)ld at once most frankly (answ)er any question on the () (t)hat I might think (1 or 2 words) to ask him—

(3 or 4 words) ()ld associations (3 or 4 words) to obtain (poss)ession of what remains () the Pond House or Tromer Property—& if it i(s) again (s)old, it is impossible (to) say (if it does not co() ()e) how long it may () it comes again in (the) Market— You are c() () respect to proximity (to) the name of the first alienee, through my Father’s agents, (as) I may call them—but as I (d)o not wish to enter on that part of the case—at least at present—I may simply say, (t)hat my Father thought M^r H⁷—was favoured by the Auctioneer—& the rese(rve) bid was not adhered to— () I suppose be useless to () this made a mat(ter) of ()ion now—& of course (2 or 3 words) idea of the k() (2 or 3 words) may be (4 or 5 words) Miss Wedgwood gave M^r () for the property, you wi(ll) kindly let me know. M^r H() () t a good deal on () ()od— In this way I () (b)e able to form (a judgment)—of what the (e)xecutor may expect for it— I have room for the Basso relieve here, if it or they are really worth the carriage, but on this subje(ct) I would probably spea(k to) D^r Reynolds, & communicate with M^r Wedgwood.— My idea was I might miss an A(d)vertisement of the sale (of Tr)omer—

Again thanking (you) for your letter | I have the honour | to be | Dear Sir | (Yours ve)ry faithfully | ()ful— Ainslie

Charles D(arwin) | D^r—K(ent)

DAR 159: 11b

CD ANNOTATION

Top of letter: '£4,500' pencil

- ¹ The date is established by the relationship between this letter and the letter from O. A. Ainslie, 23 November 1880; in 1880, the Thursday following 23 November was 25 November.
- ² CD's letter has not been found; Ainslie had inquired about the sale of Tromer Lodge (see letter from O. A. Ainslie, 23 November 1880 and n. 2).
- ³ Margaretta Susannah Reynolds.
- ⁴ Robert Ainslie.
- ⁵ Tromer Lodge was the home of Elizabeth Wedgwood, who had died on 8 November 1880 (CD's 'Journal' (Appendix II)). The sale of the house was evidently handled by Henry Allen Wedgwood and Hensleigh Wedgwood; their names appeared on the conditions of sale (London Borough of Bromley Archives, sale particulars collection, 1200/258).
- ⁶ John Russell Reynolds.
- ⁷ Robert Haswell.

To V. O. Kovalevsky 25 November [1880]¹

From Mr. C. Darwin, Down, Beckenham.
25 November

As you have not arrived today, I write one line to say that I shall be greatly pleased to see you here any day,

C.D

Thursday | Down— (Orpington St on the S. E. Ry)²

ApcS

Institut Mittag-Leffler

- ¹ The year is established by Kovalevsky's visit to CD in London (see n. 2, below).
- ² Kovalevsky had proposed visiting CD at Down (letter from V. O. Kovalevsky, [after 21 November 1880]); he and his wife, Sofia Vasilyevna Kovalevskaya, later met CD for lunch at Queen Anne Street, London (see letter to G. H. Darwin, 9 December [1880]) and possibly at Down on 29 November (letter from Emma Darwin to H. E. Litchfield, [28 November 1880] (DAR 219.9: 253)). Orpington Station was the closest to Down on the South Eastern Railway.

From M. T. Masters 25 November 1880

The Gardeners' Chronicle Office, | 41 Wellington Street, Strand, W.C. | London
Nov. 25 1880

My dear Sir/

I have just been glancing over your book on Plant Movements as a preliminary to a more careful & leisurely survey when opportunity offers— I cannot refrain from expressing my admiration of your labors and my great interest in what you say about Root movements— I see you quote Chatin as to the movement of the leaves of Conifers— I have paid a good deal of attention lately to this in several species including some under my own windows & I cannot help thinking Chatin has made a mistake— I always find the white surface exposed in the day time¹

I send you an extract from the Linnean Journal wherein the subject is alluded to but since it was published I have seen the movements in numerous other species but always in the day time.²

I have alluded to the matter in this week's G. C.³
faithfully y^{rs}. | Maxwell T. Masters

C Darwin Esq

DAR 171: 87

¹ Masters's name is not on the presentation list for *Movement in plants* (see Appendix IV) but CD had asked for *Gardeners' Chronicle* to be sent a review copy; see letter to R. F. Cooke, 20 October 1880. In *Movement in plants*, p. 389, CD quoted a note by Joannes Chatin in *Comptes rendus hebdomadaires des séances de l'Académie des sciences* 82 (1876): 171–2 on the leaves of *Pinus nordmanniana* (a synonym of *Abies nordmanniana*) rising up at night to reveal the white under surface of the leaves.

² Masters sent his paper 'Relations between morphology and physiology in the leaves of certain conifers' from the *Journal of the Linnean Society, Botany* (Masters 1879). On p. 550, Masters observed that the white hue of *Abies nordmanniana* was more conspicuous when the branches were exposed to the full rays of the sun.

³ See *Gardeners' Chronicle*, 27 November 1880, pp. 692–4.

To Raphael Meldola 25 November 1880
Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Nov. 25/80

My dear Sir

I can well believe that your labour must have been great. & everyone is bound to aid you in any way.

No I of F. Müller's paper is in the August no for 1877

No II— is in the October number 1877.

Both these articles, I remember thinking excellent.¹

I am not one of the Editors of *Kosmos*, only a kind of patron(!) & therefore cannot give permission; but when you write to Editors you can say that I have expressed a hope that permission would be granted; you acknowledging source of papers.²

Heartily wishing you success & in haste to catch first Post, I remain | Yours very faithfully | Ch. Darwin

Oxford University Museum of Natural History (Hope Entomological Collections 1350: Hope/Westwood Archive, Darwin folder)

¹ In his letter of 24 November 1880, Meldola had asked CD in which issues of *Kosmos* parts one and two of Fritz Müller's paper on observations on Brazilian butterflies (F. Müller 1877) appeared.

² Meldola thought that CD was one of the editors of *Kosmos* and asked for his permission to translate F. Müller 1877, but CD had backed the journal only by allowing his name to appear in the full title, *Kosmos: Zeitschrift für einheitliche Weltanschauung auf Grund der Entwicklungslehre in Verbindung mit Charles Darwin und Ernst Haeckel* (*Kosmos: journal for uniform worldview based on the theory of development in connection with Charles Darwin and Ernst Haeckel*); see *Correspondence* vol. 25, letter from Ernst Krause, 11 March 1877.

From Wilhelm Pfeffer¹ 25 November 1880

Tübingen
d. 25 Nov. 1880

Sehr geehrter Herr!

Habe ich auch bis dahin in Ihrem Buche "The power of movements etc" nur herumblättern können, so genügte doch dieses, um einigen Einblick in die Fülle von Beobachtungen und geistreichen Combinationen zu erhalten, welche Sie auch hier wieder niederlegten. Jedenfalls drängt es mich Ihnen sogleich für die freundliche Zusendung Ihres Werkes meinen besten Dank auszusprechen.² Hoffentlich kann ich mich in etwa $\frac{1}{2}$ Jahre revanchiren, da morgen das Manuscript für den ersten Band eines Handbuches der Physiologie des Stoffwechsels und Kraftwechsels in die Druckerei wandert. Auch für den 2 Band ist alle Vorarbeit so weit fertig, dass ich hoffen darf in einem $\frac{1}{2}$ Jahre auch diesen druckfertig abzuschliessen.³

Diese Arbeit hat mir leider keine Zeit gelassen irgend eine ausgedehnte Untersuchung anderer Art anzustellen, doch wird in nicht ferner Zeit wohl ein Heft physiol. Arbeiten von Schülern erscheinen.⁴ Uebrigens habe ich das hiesige botanische Institut, dessen Räumlichkeiten vortrefflich sind, allmählich mit einem sehr vollständigem physiologischem Apparat ausgestattet.

Mit vorzüglicher Hochachtung | Ihr | ergebener | D: W. Pfeffer.

DAR 174: 37

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

² Pfeffer's name is on the presentation list for *Movement in plants* (see Appendix IV).

³ Pfeffer's *Pflanzenphysiologie: ein Handbuch des Stoffwechsels und Kraftwechsels in der Pflanze* (Plant physiology: a handbook of metabolism and energy exchange in plants; Pfeffer 1881) was published in 1881.

⁴ In 1881, Pfeffer founded the series of occasional papers *Untersuchungen aus dem Botanischen Institut zu Tübingen* (Investigations from the Botanical Institute of Tübingen).

From William Preyer 25 November 1880

Jena
November 25, 1880

Dear Sir—

The beautiful leading article in the Times of Nov. 20, 1880 on your new book awakens in me so vivid a desire to read it that I take the liberty of addressing myself directly to the author. Would you kindly instruct the publisher (is it M: Murray?) to send me a copy by book-post?¹ It would take several weeks to get one through a bookseller here and I am rather impatient wishing particularly to know what methods you employed in studying the movements of plants. They may be applicable to animal embryos which I am investigating and the earliest movements of which have never been experimented on.

At the same time I beg to be informed about your papers in "Nature" VIII. p. 417. 1873 (Origin of certain instincts) and in "Zoologist" VIII. 3488. 1873 (Perception in

the lower animals).² I would return the copies within one week, if you will kindly send them. But I do not wish to trouble you, if you have none left.

Please accept my newest book not as merely “popular lectures”. It contains many special researches some of which may perhaps have some little interest for you.³

With my best compliments to M^{rs}. & Miss Darwin⁴

I remain Your's | faithfully | W^m. Preyer | in Jena

To M^r. Ch. Darwin | in Down

DAR 174: 71

¹ A review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9; John Murray was CD's publisher. Preyer's name is on CD's presentation list for *Movement in plants* (see Appendix IV).

² See *Correspondence* vol. 21, letters to *Nature*, [before 13 March 1873] and [before 3 April 1873]. The letters were printed in *Nature*, 3 April 1873, pp. 417–18, and 13 March 1873, p. 360 (reprinted in the *Zoologist* 2d ser. 8 (1873): 3488–9).

³ A copy of Preyer's *Naturwissenschaftliche Thatsachen und Probleme, populäre Vorträge* (Scientific facts and problems, popular lectures; Preyer 1880) is in the Darwin Library–Down.

⁴ Emma and Elizabeth Darwin.

To Carlos Ribeiro 25 November 1880

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

November 25. 1880

Dear Sir

I am much obliged to you for your great kindness in having sent me your great work on Prehistoric Remains in Portugal & for your paper on Tertiary Formations.¹

With much respect I remain | Dear Sir | Yours faithfully & obliged | Charles Darwin

Laboratório Nacional de Energia e Geologia (Records of the Portuguese Geological Commission 1857–1918)

¹ Ribeiro had sent the second part of his *Notícia de algumas estações e monumentos prehistoricos* (Report on some prehistoric sites and monuments; Ribeiro 1878–80) and his paper ‘Des formations tertiaires de Portugal’ (Tertiary formations of Portugal; Ribeiro 1880).

To M. T. Masters [after 25 November 1880]¹

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

My dear Sir

I must write one line to thank you for your kind note.— I had great misgivings about Chatin, & I much wish that I had recollected your article; but it is now too late. I assumed that Chatin was quite trust worthy.²

Yours very sincerely | Ch. Darwin

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

¹ The date is established by the relationship between this letter and the letter from M. T. Masters, 25 November 1880.

² In his letter of 25 November 1880, Masters drew CD's attention to a possible mistake in the work of Joannes Chatin, which CD had quoted in *Movement in plants*, p. 389.

From R. F. Cooke 26 November 1880

50A, Albemarle Street, London. W.
Nov 26 1880

My dear Sir

We must print off immediately 500 more copies of the *Movement of Plants*, as we are in *want of copies*.¹

I have written to Clowes & beg you will send any *corrections at once* I have desired them not to wait²

Yours faithfully | Rob^t Cooke

Cha^s Darwin Esq.

DAR 171: 513

¹ In his letter of 10 November 1880, Cooke had reported that 800 copies of *Movement in plants* had already sold out of the 1000 printed.

² In his letter of 8 November [1880], CD had asked Cooke how much it would cost for the type of *Movement in plants* to be kept up for two months at the printers, William Clowes & Sons.

From Francis Darwin to Édouard Heckel 26 November 1880¹

Down

[Signed autograph letter dictated by his father, with indications on the movement of flowers conveyed to Heckel;² it is the Council of the Royal Society that elects members and it is not customary to recommend someone.³]

Aguttes (dealers) An Aristophil sale (17 November 2019, lot 43)

¹ The date is given in the sale catalogue.

² Heckel was supervising the translation *Movement in plants* into French (Heckel trans. 1882; he had asked CD about the movement of corollas, stamens, and pistils in his letter of 23 September 1880.

³ See letter from Édouard Heckel, 23 September 1880 and n. 5.

From W. E. Darwin 26 November [1880]¹

Ridgemount, | Basset, | Southampton.
Nov 26

My dear Father,

I have today looked both here and at Rogers's at the worm heap near the Robinias.²

I have examined 7 to 9 in all. Owing to the wet they are battered and decayed, and the leaves broken and doubled in in some cases, and some of the leaves seemed to have been pulled in sideways. On the whole I think there were decidedly more drawn in by the tips of the leaves than by the stalk end; in 3 cases if not in 4 almost all the leaves were drawn down point first, but in these cases there were also some broken bits in a sideways position or crumpled, so that I could not say what would be the proportion in any one heap; but I certainly saw none where any large proportion of the leaves had been drawn in stalk downwards, in several cases they seemed to be almost equally divided. A month or so ago one could have decided the proportion fairly well.

I looked at all I could find but no doubt I could find more if you would like me to look again.

I return George's note which is interesting, and I should like to hear if he makes out the law of the distances from crest to crest of the ripples.³ Please thank mother for her letter. We shall be a very small Christmas party and I fear we shall not have Horace & Ida.⁴

There was a long notice in the Standard yesterday, it is not worth looking at. It opened in a civil but condescending tone. I shall like to see D^r Hooker's when at Down—.⁵

Your affect son | W E. Darwin

I find Sara has returned George⁶

DAR 162: 110

CD ANNOTATION

4.1 I return ... Down— 5.3] *crossed blue ink*

¹ The year is established by the relationship between this letter and the letter to W. E. Darwin, 23 [November 1880].

² In his letter of 23 [November 1880], CD had asked William to observe how worms drew the petioles of the leaves of *Robinia pseudoacacia* (black locust or false acacia) into the mouths of their burrows in his own garden and William Henry Rogers's garden.

³ See letter from G. H. Darwin, 19 November 1880; George Howard Darwin's paper 'On the formation of ripple-mark in sand' discussed the distance between crests and the factors that affected wavelength (G. H. Darwin 1883, pp. 40–1).

⁴ The letter from Emma Darwin to William has not been found. Both Horace and Ida Darwin and William and Sara Darwin were at Down for Christmas 1880 (Emma Darwin's diary (DAR 242)).

⁵ A review of *Movement in plants* appeared in the *Standard*, 25 November 1880, p. 2. In CD's letter to William of 23 [November 1880], CD described Hooker's letter about receiving a copy of *Movement in plants* as 'very buttery' (letter from J. D. Hooker, 22 November 1880).

⁶ Sara Darwin; see n. 4, above.

From J. D. Hooker 26 November 1880

Nov. 26/80

Dear Darwin

Huxley spoke to me yesterday about the pension for Wallace, which you proposed to me not very long ago, & which we both thought at the time would be a hopeless

attempt under the circumstance of his Spiritualism, & the fact that he had obtained £500 by the bet on the Sphericity of the Globe.—¹

H. tells me that the bet tells all the other way, for that he believes he gave the money to a charity: & if this be so, which I have no reason to doubt, though he certainly has the credit if the contrary—there remains only the Spiritualism: which should not I think be an objection to urging his claim,—though I am doubtful as to whether it should not be mentioned privately to the Minister. I am writing to Huxley to this effect. ie. asking his opinion.²

I need not add that if you & he decide I will follow & do my best.

Ever aff Yrs | J. D. Hooker.

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 349)

¹ CD had sent Thomas Henry Huxley a draft memorial and a list of potential signatories in support of a civil-list pension for Alfred Russel Wallace; see letter from T. H. Huxley, 14 November 1880. For Hooker's and CD's previous opinions on Wallace's chances of a pension, see *Correspondence* vol. 27, letter from J. D. Hooker, 18 December 1879, and letter to A. B. Buckley, 19 December 1879. Wallace had denied that natural selection could account for the development of human intelligence and looked to spiritualist explanations; for his interest in spiritualism from the mid-1860s, see Kottler 1974 and Fichman 2004, pp. 139–210. In 1870, Wallace, a qualified surveyor, had accepted John Hampden's challenge to scientific men to prove the convexity of a stretch of inland water, offering £500 if the proof was accepted by an intelligent referee. Wallace's proof was accepted by the referee, who gave him the £500, but Hampden refused to accept the result and subjected Wallace to a twenty-year campaign of abuse. Although Wallace mostly won the many court battles that resulted, the legal costs were so great that he lost out financially. (See Raby 2001, pp. 206–7.)

² Hooker also informed Huxley of his objections (letter from J. D. Hooker to T. H. Huxley, 26 November 1880; Imperial College of Science, Technology and Medicine Archives, Huxley 3: 259).

From Ernst Krause¹ 26 November 1880

Berlin N.O. Friedenstrasse 11. 3 Tr.
den 26.11.80.

Hochverehrter Herr!

Mit der Zusendung Ihres neuen Werkes: "The Power of Movement in Plants" haben Sie mir eine grosse Freude bereitet und ich danke Ihnen von ganzem Herzen dafür.² Ich erstaune über die grosse Fülle von Arbeit und Experimenten, die Sie in demselben niedergelegt haben und ich glaube alle Welt wird dieses freudige Erstaunen theilen, immer von Neuem von Ihnen Arbeiten zu erhalten, welche ein ganzes Gebiet der Forschung reformiren, und oft wie die Arbeit eines Menschenlebens aussehen. Herr Dr. Hermann Müller in Lippstadt will die Güte haben, über den Inhalt des Werkes im Kosmos zu berichten, ich selber habe vor, dies in einer deutschen (nicht naturwissenschaftlichen) Revue zu thun.³

Der Kosmos dürfte in nächster Zeit vielleicht ganz eingehen. Der Verleger, welcher, wie es scheint, weder die Mittel noch die gehörige Energie besitzt, um das Unternehmen durch einige schwierige Jahre zu führen, ist entschlossen, dasselbe zu verkaufen, und wie ich höre, geht man damit um, es in eine Wochenschrift zu verwandeln. Da hiermit wahrscheinlich ein vollständiges Aufgeben des

wissenschaftlichen Characters verbunden sein würde, so beabsichtige ich nicht, diese Wandlung mit durchzumachen, würde es vielmehr möglicherweise versuchen, einen andern Buchhändler für Wiederaufnahme des Journals in seiner bisherigen Form (mit verändertem Titel) zu interessiren.⁴

Von Herrn Fritz Müller aus Itajahy sind kürzlich betrübende Nachrichten eingelaufen. Sein Wohnort hat durch eine Ueberschwemmung zu leiden gehabt, bei der er sich (Mitte September) schwimmend, und bis über die Brust im Wasser watend, retten musste. Indessen hat er glücklicherweise keine Verluste in seiner Familie zu beklagen, und er hat mir vor einigen Tagen bereits wieder einige seiner stets so werthvollen Mittheilungen über neue Beobachtungen gesandt, als Zeichen, dass der Zwischenfall vollständig überwunden ist. Es befindet sich darunter ein kleiner polemischer Artikel gegen Wagner's Absonderungstheorie und eine sehr interessante Beobachtung über Reproduction von Krebsfüssen, die in der Gestalt von Gliedern der muthmasslichen Vorfahren der Art auftreten.

In der Abtheilung der Garneelen, der auch obiger Fall angehört, hat er ausserdem wieder einige höchst lehrreiche Metamorphosen beobachtet, über welche er demnächst einen ausführlichen Bericht senden will.⁵

Mit dem Wunsche, dass diese Zeilen Sie in erwünschtem Wohlsein antreffen mögen, zeichne ich, hochverehrter Herr | Ihr | dankbar ergebener | Ernst Krause

DAR 169: 110

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

² Krause's name is on CD's presentation list for *Movement in plants* (see Appendix IV).

³ Hermann Müller's review of *Movement in plants* appeared in *Kösmos* (H. Müller 1880e). Krause reviewed it in *Die Gartenlaube* under the pseudonym Carus Sterne (Sterne 1881).

⁴ From vol. 9 in 1881, *Kösmos* was published by E. Schweizerbart'sche Verlagsbuchhandlung instead of Ernst Günther of Leipzig (run by Karl Alberts); the typeface changed from Fraktur to roman, and the journal continued to be published monthly until 1886. Krause continued as editor until 1882.

⁵ For more on the flood that had occurred on 22 September 1880 and had risen to 14.6 metres, see West 2016, pp. 154–5. Fritz Müller published 'Zur Kritik der Absonderungstheorie' (Critique of segregation theory; F. Müller 1881c), 'Haeckel's biogenetisches Grundgesetz bei der Neubildung verlorener Glieder' (Haeckel's biogenetic law and the rebuilding of lost limbs; F. Müller 1881b), and 'Farbenwechsel bei Krabben und Garneelen' (Colour change in crabs and shrimps; F. Müller 1881a) in *Kösmos*. His more detailed report on the shrimps was '*Atyoida Potimirim*, eine schlammfressende Süswassergarneele' (*Atyoida Potimirim*, a mud-eating freshwater shrimp; F. Müller 1881d), also in *Kösmos*. *Atyoida potimirim* is a synonym of *Potimirim potimirim*, the tiny or neon shrimp. Wagner: Moritz Wagner.

To T. H. Huxley [after 26 November 1880]¹

[Down.]

My dear Huxley

I am so extremely glad that Hooker will sign.— You have managed the affair wonderfully. His former letter made me give up the ghost completely. I cannot see that there is the least necessity to call any minister's attention to Spiritualism, or to repeat (what you said) to Gladstone—that Spiritualism is not worse than the prevailing superstitions of this country!²

Hurrah— I am sanguine
Yours affectionately | Ch. Darwin

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 349)

- ¹ The date is established by the relationship between this letter and the letter from J. D. Hooker, 26 November 1880.
- ² In his letter of 26 November 1880, Joseph Dalton Hooker had provisionally agreed to sign the memorial that CD had prepared in support of a civil list pension for Alfred Russel Wallace but had wondered whether Wallace's belief in spiritualism should be mentioned privately to the minister. For Hooker's previous letter stating that Wallace's chances of a pension were hopeless, see *Correspondence* vol. 27, letter from J. D. Hooker, 18 December 1879. William Ewart Gladstone was a correspondent of CD's and visited Down on 11 March 1877 (see, for example, *Correspondence* vol. 27, letter from W. E. Gladstone, 24 July 1879, and Emma Darwin's diary (DAR 242)). For Gladstone's sympathy with spiritualist beliefs, see Windscheffel 2006.

To G. H. Darwin [27 November 1880]¹

[Down.]

My dear George

Hurrah for the old bloody Times,² Murray says 500 copies urgently required, so I have looked to Photographs—³ There is one of B fig 147 & of both of 162, but none of fig 149— But my memory was not so badly to blame, as I have **other** splendid photographs of plants awake & after being shaken, but not so much depressed, as when asleep, & this misled me.

yours affect. | C. Darwin

DAR 210.1: 100

- ¹ The date is established by the relationship between this letter and the letter from R. F. Cooke, 26 November 1880; see n. 3, below.
- ² The journalist and reform campaigner William Cobbett named the paper 'The Bloody Old Times' because of 'its having uniformly advocated punishment, cruelty, proscription and blood against all those, in whatever country, who were striving for freedom' (*Cobbett's Political Register*, 16 October 1830, p. 507; Spater 1982, 2: 542–3).
- ³ A review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9. In his letter of 26 November 1880, Robert Francis Cooke, partner in the firm of CD's publisher, John Murray, said that another 500 copies of *Movement in plants* were to be printed and asked for any corrections. George had expressed his disappointment at the quality of the woodcuts, and said that he had drawn figs 147, 149, and 162 from nature rather than from photographs as stated in *Movement in plants*, pp. 356, 358, and 385, respectively; see letter from G. H. Darwin, 19 November 1880. In his reply to George of 20 November [1880], CD said that he would check by looking at the original photographs.

To James Geikie 27 November 1880

Down | Beckenham, Kent. (&c).
Nov^r 27th. 1880

My dear Sir.

I received this morning your magnificent Book, & I thank you cordially. Before long I will read it, & have no doubt that it will give me as much or more pleasure

than your Great Ice Age—¹ It delights me that you should have thought my notion about frozen snow & drift worth insertion.²

Believe me | My dear Sir | Yours very faithfully & obliged | Charles Darwin.

Copy

DAR 144: 333

¹ Geikie had sent his *Prehistoric Europe: a geological sketch* (Geikie 1881); the second edition of his *The great ice age and its relation to the antiquity of man* was published in 1877 (Geikie 1877).

² In his letter of 22 July 1880, Geikie thanked CD for his permission to quote from two letters (see *Correspondence* vol. 24, letter to James Geikie, 16 November 1876, and this volume, letter to James Geikie, 19 July 1880) on differential movement in drifts (see J. Geikie 1881, pp. 141–2).

To Hermann Müller 27 November 1880

Down, Beckenham, Kent

Nov: 27. 1880

My dear Sir

— — — — — I also had a letter from Dr Ernst Krause by this post, telling me of the dreadful risk from a flood which your admirable brother, Fritz, has barely escaped from with his life.¹ I rejoice that none of his family were lost. Has he lost many of his books, microscope, apparatus or other property? If he has suffered in this way, nothing would give me so much pleasure as to be allowed to send him £50 or £100. Do you think he would permit me to do so? The money would be sent solely for the sake of science, so that science should not suffer from his loss of property. Pray have the great kindness to advise me. Nothing would grieve me so much as to offend your brother, and nothing would please me so much as to be able *slightly* to assist him in any way.

My dear Sir, Yours very sincerely | Charles Darwin

Pray let me hear soon.

Copy

DAR 146: 442

¹ Fritz Müller; see letter from Ernst Krause, 26 November 1880 and n. 5.

From Hermann Müller 27 November 1880

Lippstadt

27/11 1880.

My dear Sir!

My heartiest thanks for your work “on the power of movement in plants” which you have kindly sent to me.¹ I have only read hitherto the introduction and Chap. XII and I. But this is sufficient to show the generality of the circumnutating movement in the development of plants and its paramount bearing on the origin of nearly all sorts of movements in plants, which hitherto separately and without connection have been studied and described.

It is with the greatest admiration that I have learned the astonishingly simple fundamental idea of your researches, your sagacious methods of experimenting and of pursuing this idea in all its consequences, the overpowering army of your careful and accurate special observations, by which any doubt about the universality of the circumnutating movement in the vegetable Kingdom is dispersed. It is, therefore, with high enjoyment, that I will read the rest of your new admirable work, which again has opened a new and most fruitful dominion of botanical research.

Please to accept with indulgence my work on alpine flowers which in this days has been edited and which I have sent to you yesterday.²

My son resides in London since several weeks, but he is not yet acclimated there. In the next days he intends to make use of your kind offer to make your personal acquaintance³

With sincere admiration | yours | very faithfully | H. Müller.

DAR 171: 317

¹ Müller's name is on CD's presentation list for *Movement in plants* (see Appendix IV).

² Müller had sent his *Alpenblumen, ihre Befruchtung durch Insekten: und ihre Anpassungen an dieselben* (Alpine flowers, their fertilisation through insect agency and adaptations for this; H. Müller 1881).

³ No record of CD's meeting Müller's son, Wilhelm Hermann Müller, has been found.

To William Preyer 27 November 1880

[Down.]

Nov: 27th. 1880.

My dear Sir

It will give me much pleasure to send you a copy of my book, and I have directed Mr. Murray to send you one; but there may be a little delay, as I heard this morning that every copy was sold; but the type is yet up and more copies will be printed off directly.— I fear my methods will not be applicable to embryos.¹

I enclose copies of the articles to which, I suppose, you refer, and which I discovered after a long search.— As I have no other copy I should be obliged if you would return them. I never wrote in the *Zoologist*, so I suppose something was copied out of *Nature*.²

Your book has not yet arrived, but probably will to-morrow, as they are often delayed a day or two by our Post.³ I am very much obliged to you for your kindness in having sent it to me.

My dear Sir | Yours sincerely | Ch. Darwin

Copy

DAR 147: 270

¹ Preyer had asked CD to send him a copy of *Movement in plants* and hoped that some of the methods in it might be relevant to his studies of animal embryos; see letter from William Preyer, 25 November 1880. For the news that CD's publisher, John Murray, was printing off more copies of *Movement in plants*, see the letter from R. F. Cooke, 26 November 1880.

² See letter from William Preyer, 25 November 1880 and n. 2.

³ See letter from William Preyer, 25 November 1880 and n. 3; Preyer had sent Preyer 1880; a copy is the Darwin Library–Down.

From O. A. Ainslie to Francis Darwin 28 November [1880]¹

48—*Lincolns Inn* | *Fields. W.C.*
Advent Sun(day) | No(v)^r—28 (1880)

My dear Sir—

As I shall proba(bl)y be very busy tomorrow, & Tuesday, & was very busy yesterday, I infringe on my general rule, in replying to your note on this day— I beg to thank you & your Father for it—² The price of (the) property in question was, what I underst() was given for it, so I am much obliged for a confirmation of ()e report—my Father & I— received— I am not sure, w() the “owner”, you re() is still ali()— w() & have an i(m)p(ression, (that) he is not—without referring to some paper(s I) have, I think, if I remem(ber) rightly, the name was—Haswell³ & not Haslam—but as one cannot be too particular, of (wha)t one writes or says () anyone—especially in (the) event of future litiga(tio)n—which as I have ($\frac{1}{4}$ line) expressed, I have ($\frac{1}{4}$ line) ()t or possible ()ll () of— I did not trouble your honoured Father with this correction— For (the) same reason I called Mr H—the first ()ee—and you—*rightly* cal() () owner—if he w(as un)der all the circumstan(ce)s of the case real (or rightful) owner— You imply very properly he had a legal ti(t)le to convey, & *that possession*, which is said to be nine tenths, parts or points of the Law— But I have wished to avoid, as far as I can, any reference to disputed title— Miss W(edgwo)od’s title was no dou(bt) (abso)lutely bonâ f(ide as far) as she was concerned—⁴ it is merely a very complicate(d) question of prescription, w(he)ther I, as Heir at L(a)w of (my F)ather, could do any th(ing to) obtain a D(egr)ee— () the Conveyance to H as wel(l) perhaps without any fault on his part,) was invalid. My Father always considered his estate suffered £500—by the sale besides, or including, the ()s of the property itself—& supposing Mr H—spent £1500 on the property, which I think far above the real estimate & gave £1500 for it, which (I) think, is also above the mark. () [earned] £1500 by the (transact)ion— If these figure(s) (1 or 2 words) ()t, to my mind, i(s) (2 or 3 words) ()nes—

With many | (ki)nd regards— () ()in Dear Sir () ()ly Oliver A. Ainslie
 Francis D(arwin)

(PS)— I omitted to tell D^r Darwin th(a)t my Father, when he settled with those responsible for the sale, protested ag(ains)t it—

DAR 159: 11c

¹ The year is established by the relationship between this letter and the letter from O. A. Ainslie, 23 November 1880.

- ² The note from Francis to Ainslie has not been found. Ainslie was inquiring about the sale of Tromer Lodge (see letter from O. A. Ainslie, 23 November 1880 and n. 2, and letter from O. A. Ainslie, 2[5] November 188[o]).
- ³ Robert Ainslie had sold Tromer Lodge to Robert Haswell in 1862.
- ⁴ Elizabeth Wedgwood had acquired Tromer Lodge in 1868; on the matter of the title, see letter from O. A. Ainslie, 23 November 1880, n. 3.

To J. D. Hooker 28 November [1880]¹

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

Nov. 28

My dear Hooker

I sh^d. very much like to see Frank an F.R.S. before I die.² It appears to me very doubtful whether a father ought to propose a son: I did not do so (or put my name at all down) in the case of George, but then there was the additional reason of my not knowing mathematicks.—³ Will you be so kind as to tell me what you think on this head? Should you object to proposing him? I am sure that this w^d. please him much more than my doing so. But I forgot you are (I suppose) on the Council & cannot (I believe) back or propose a man.

Will you let me hear what you think & about the Council. Like an ass I did not keep list of Council.⁴

Forgive your old friend for troubling you.— | Ever yours | C. Darwin

P.S. I was delighted to get your note about Wallace—whom I pity much.— I have hardly ever wished for anything so much as to get him on the pension list.—⁵

DAR 95: 500-1

- ¹ The year is established by the relationship between this letter and the letter from J. D. Hooker, 26 November 1880.
- ² Francis Darwin was elected a fellow of the Royal Society of London after CD's death on 8 June 1882 (*Record of the Royal Society of London*).
- ³ George Howard Darwin had been elected a fellow of the Royal Society on 12 June 1879 (*Record of the Royal Society of London*).
- ⁴ Hooker was on the council of the Royal Society until 30 November 1880 (*Proceedings of the Royal Society of London* 29 (1879): 431; 31 (1880-1): 101).
- ⁵ For Hooker's letter about signing a memorial in support of a civil list pension for Alfred Russel Wallace, see letter from J. D. Hooker, 26 November 1880.

To Ernst Krause 28 November 1880

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

Nov. 28th 80

My dear Sir

I must thank you for your *most* interesting letter.¹ It is very bad news about Kosmos: I shall regret much its cessation, for I found in every number something which interested me greatly. Anyhow you must have the satisfaction of feeling that you have been an excellent editor, & have aided Science in every way.—²

It is, also, grievous news about Fritz Müller,—that is if he has lost his instruments & books. I have long looked on him as the best observer in the world. I have written to his brother, Hermann, to learn whether he has suffered much loss, & whether for the sake of Science, he would allow me to aid him in purchasing new microscopes &c &c.—³

I do not know when I have been so much astonished, as by your account of the Crustacean which repairs its legs by those of an ancestral form. If I understand the case, it must be a kind of localised reversion! This seems to me to support, the hypothesis of Pangenesis, which has hardly any friends in this world. I can conceive a small collection of molecules (ie one of my imaginary gemmules) remaining dormant in an organism for almost any length of time; but I think it will be difficult for Hæckel to make others believe that certain molecules, of which the body is built, have gone on vibrating for countless generations in a peculiar manner, so as to form when occasion offers an ancestral limb. If I remember rightly the reproduced tail of *Lacerta* differs from the normal tail. I have given one case in some slight degree analogous, namely that of a hen which when barren assumed the male plumage of an ancestral breed, & not of her own breed.—⁴ But I did not intend to trouble you with my notions or about pangenesis.—

My dear Sir | Yours sincerely | Chas. Darwin

The Huntington Library (HM 36209)

¹ Letter from Ernst Krause, 26 November 1880.

² See letter from Ernst Krause, 26 November 1880 and n. 4; Krause continued as editor of *Kosmos* until 1882.

³ See letter to Hermann Müller, 27 November 1880.

⁴ In his letter of 26 November 1880, Krause told CD about a short article by Fritz Müller, 'Haeckel's biogenetisches Grundgesetz bei der Neubildung verlorener Glieder' (Haeckel's biogenetic law and the rebuilding of lost limbs; F. Müller 1881b). For CD's hypothesis of pangenesis, see *Variation* 2: 357–404; it was revised in *Variation* 2d ed. 2: 349–99. For Ernst Haeckel's theory of heredity, perigenesis, see Haeckel 1876. *Lacerta* is a genus of lizards; for CD's observations, see *Journal of researches* (1860), p. 390. For CD's examples of hens that acquired male characteristics, see *Variation* 2: 51, 54, and 399.

To Walter White 28 November [1880]

From Mr. C. Darwin, Down, Beckenham.

Nov. 28th

Could you do me the favour to give me a list of the Council of R. Soc. I believe members of the Council never back up certificates, & therefore it is necessary for me to know their names. I stupidly quite forgot to keep the last list.¹

Yours sincerely | C. Darwin

ApcS

Postmark: N 29 80

Liverpool Central Library

¹ See letter to J. D. Hooker, 28 November [1880] and n. 4.

From Florence Dixie 29 November [1880]¹

Bosworth Park. | Hinckley. | Leicestershire.

Nov^{ber}. 29th.

Dear Mr. Darwin.

I have great pleasure in forwarding to you the Acc^t. of my travels in Patagonia and trust the book will meet with your approval.—²

I fear you will find it devoid of much interest. While begging you to look on this my first literary production with leniency written as it was during the never ending interruptions of a London Season I hope that the hurried acc^t. of a few of our adventures & occupations in that far off land will be of sufficient interest to carry you thro' its pages.— I venture at the same time to send you a little tragedy which I wrote some years ago as a child of 14.— It was printed for private circulation at the request & after the death of the late L^d. Lytton but having given some offence I have since suppressd its circulation.³

With many apologies for what may appear presumptions on my part allow me to remain | yr^s. very sincerely | Florence Dixie.

DAR 162: 184

¹ The year is established by the reference to Dixie 1880 (see n. 2, below).

² In her letter of 4 November [1880], Dixie offered to send CD a copy of *Across Patagonia* (Dixie 1880); CD's copy is in the Darwin Library-CUL.

³ Edward George Earle Lytton Bulwer-Lytton. A copy of *Abel avenged: a dramatic tragedy* (Douglas 1877), published under Dixie's maiden name, Florence Douglas, is in the Darwin Library-CUL.

From J. D. Hooker 29 November 1880

Royal Gardens Kew

Nov^r 29/80

Dear Darwin

I see no good reason why a Father should not propose a son, though I think it would not be a wise course if the claim of the son was not a rather commanding one.¹

The chief draw-back would be that, in case of the election being long delayed, the friends would feel for the two parties much more than if they were not visibly joined as it were in one candidature.

I go off the Council tomorrow, & will if you like make a little enquiry as to Frank's prospects of tolerably *speedy* selection,—having done which I would inform you, & act *with the greatest pleasure* exactly as you should wish.²

I have only one prior claim upon me, & that is D^r Dickie late Prof^t of Botany in Aberdeen, who has labored on Algae for upwards of 40 years, & published some 54 papers on them.³

Frank is certain to get in sometime, & in your life-time too! but he would be more certain of speedy selection if he had made more communications—⁴ His ability &

the standard i.e. importance of his work are all that could be desired, but without looking it up I am not prepared to say that there is enough of it to make a sure claim for speedy selection; except there should be a dearth of older workers on the list, which is not likely.

Be all this as it may, it will be a real pleasure to me to take care of Frank's interests & foster them, whether or no I can make them available at once. Oliver succeeds me on the Council & he is a sure friend of Franks.⁵

I know you will not be angry with my cautions—
Ever affy Y^{rs} | Jos D Hooker.

DAR 104: 146–7

- ¹ See letter to J. D. Hooker, 28 November [1880]; CD wanted to propose Francis Darwin for fellowship of the Royal Society Society of London.
- ² Hooker was on the council of the Royal Society until 30 November 1880; see letter to J. D. Hooker, 28 November [1880] and n. 4.
- ³ Hooker proposed George Dickie for fellowship of the Royal Society; he was elected on 2 June 1881 (*Record of the Royal Society of London*).
- ⁴ Francis Darwin had communicated one paper, 'On the protrusion of protoplasmic filaments from the glandular hairs of the common teasel' (F. Darwin 1877), to the Royal Society.
- ⁵ Daniel Oliver was elected as a member of the council of the Royal Society on 30 November 1880 (*Proceedings of the Royal Society of London* 31 (1880–1): 101).

From J. B. Innes 29 November 1880

Milton Brodie | Forres—
29 Nov^r. 1880—

Dear M^r Darwin,

I heard from Hoole on Saturday, and as Downe gossip he says he understands Tromer Lodge is to be sold. If this is true, and *if* the land on the West side of the road is to be sold apart from the house I should be glad to have the offer of it, as at some future time it may be an advantage to the Parsonage.¹ Probably neither of these *ifs* may prove realities.

You will be glad to hear that M^{rs} Hoole continues to improve.² She gets out a good deal every day in spite of cold weather, takes, for her, quite long walks, & seems to be making a new start.

In our, usually mild, country we have had an unusually early frost. I got my ice house filled on the 23^d. The earliest date in any former year was the 6th Dec^r. Since the 23rd there has been no frost, a furious Gale of wind on Friday.

With my wife's³ kindest regards, love to M^{rs} Darwin, Believe me | Faithfully Yours
| J Brodie Innes

DAR 167: 38

- ¹ Stanley Hoole lived at Downe Lodge. Tromer Lodge, Down, was vacant after the death of Elizabeth Wedgwood on 8 November 1880 (CD's 'Journal' (Appendix II)). Innes had considered buying the

house in 1860, when he was perpetual curate of Down; see *Correspondence* vol. 8, letter to John Innes, 18 July [1860]. It was renamed ‘Tower House’ and advertised for sale in *The Times*, 4 June 1881, p. 15, and sold on 13 August 1881; the amount of land being sold with the house was much reduced from when the estate was sold to Elizabeth Wedgwood in 1862. See also letter from O. A. Ainslie, 23 November 1880.

² Innes’s niece, Alice Mary Hoole.

³ Eliza Mary Brodie Innes.

To A. W. Howitt [before 30 November 1880]¹

[I also received a letter from Charles Darwin to whom I sent a copy from myself as he some seven years ago suggested to me to place on record all I could gather about the blacks.² He writes me that after glancing through the book it seemed to him so important in our “new views and facts” that he at once sent it to his neighbour Mr McLennan who although in terribly bad health is striving to complete a book before he dies.³ When Mr Darwin gets the book back he says he shall read it again very carefully.]

St Mark’s National Theological Centre Library (Tippett Collection TIP 70/10/30/1)

¹ The date is established by the relationship between this letter and the letter from J. F. McLennan, 30 November 1880.

² The text is from a letter from Howitt to Lorimer Fison, 10 January 1881. In his letter to Howitt of [September 1874] (*Correspondence* vol. 22), CD had suggested that Howitt should write a book on the mental powers and beliefs of Australian aborigines. Howitt had sent *Kamilaroi and Kurnai* (Fison and Howitt 1880).

³ John Ferguson McLennan accepted CD’s offer to lend him Fison and Howitt 1880; see letter from J. F. McLennan, 30 November 1880. McLennan died in 1881. He lived at Hayes Common, Kent, three and a half miles from Down (*ODNB*); his work on the origin of patriarchal family structures was published posthumously (McLennan 1885).

To W. E. Darwin 30 November [1880]

[Down.]
(Nov. 30th)

My dear W.

If you can get a fine day & can spare a day (but it is a horrid shame to bother so busy a man) I sh^d like to examine some castings from Beaulieu Abbey. Or Netley w^d possibly do.—¹

I sh^d like to have some from within the precincts of the Abbey, from the turf & **not** from the square places with trap-doors where the tiles are exhibited—also some castings from those square places with trap-doors.— I am awfully perplexed about the trituration of the swallowed fragments & cannot make up my mind.

Very many thanks about Acacia petioles: if weather keeps mild the worms will perhaps work again & if so look under your Robinia.—²

Give my best of loves to Sara & my heart-felt sympathy for her misfortune in having married into such a dreadful family: our dear old mother feels the same for herself & for Sara.³

your affect. Father | C. Darwin

Murray writes that 500 more copies of the Book must instantly be printed off.— instead of losing 1 or 2 hundred pound, Frank & I shall make a few pounds.⁴

Postmark: No 30 80

DAR 210.6: 163

¹ In his letter to William of 23 [November 1880], CD wrote that he would know whether he needed William to acquire more worm-castings from Beaulieu Abbey, Hampshire, in a few days. Beaulieu is thirteen miles from Bassett, Southampton, where William lived; Netley Abbey is seven. For CD's comments on the importance of trituration of small particles of stone in the gizzards of worms, see *Earthworms*, pp. 249–58. CD was probably unsure whether the rounding of the particles of stone was due to the action of the worm's gizzard; see letter to W. E. Darwin, 17 December [1880].

² William had made observations on how worms drew the petioles of the leaves of *Robinia pseudoacacia* (black locust or false acacia) into the mouths of their burrows; see letter from W. E. Darwin, 26 November [1880].

³ Sara Darwin.

⁴ CD's publisher John Murray had sold the initial print run of *Movement in plants*; see letter from R. F. Cooke, 26 November 1880. CD had estimated that he would make a loss on the initial 1000 copies printed, especially after considering the large number of review and presentation copies; see letter from R. F. Cooke, 4 November 1880, and letter to R. F. Cooke, 8 November [1880].

From J. F. McLennan 30 November 1880

Hayes Common

Nov^r. 30 1880.

My dear M^r: Darwin,

I have not seen the work you mention & will be much obliged by your lending it to me.¹

I know both writers however; Fison from Morgan's paper; & Hewitt from a paper in Smyth's "Aborigines of Victoria" I had hoped I had written my last word on Australian Kinship on which I prepared about three years ago a chapter of some length for my book. But I had better see all they have to say. It is a deplorable instance of the helplessness of the human mind when speculating on false facts; & nearly all the so called facts about the Kamilaroi, Baralong &^c, are obvious mistakes of observation. Besides this these excellent men are under the guidance of M^r: Morgan whose system, I say it deliberately, turns on a falsehood without uttering which he could not have moved. "When addressing a person who is not a relative they say *my friend*".² Lafiaū, from whom he mainly took his account of the Iroquois (without acknowledgement) says expressly the contrary & so without exception do all the Jesuit fathers.³ Where ever you find "the classificatory" system you find all strangers addressed by terms of relationship indicative of the respect sought to be paid to them. "My father" "My uncle" "My brother" &^c.

I have been very ill since Thursday last.⁴ This afternoon I feel better. My diet is now milk & lime water!

Thank D^r: Frank for the copies of Nature & with kindest regards from my wife⁵ & self to you & M^{rs}: Darwin I am always | Yours sincerely J. F M^cLennan

DAR 171: 26

¹ See letter to A. W. Howitt, [before 30 November 1880]. CD had offered to lend McLennan *Kamilaroi and Kiurnai: group-marriage and relationship* (Fison and Howitt 1880).

² Lorimer Fison contributed the appendices to Lewis Henry Morgan's paper 'Australian kinship' (Morgan 1872). Alfred William Howitt contributed 'Aborigines of Cooper's Creek' and the 'System of consanguinity and kinship of the Brabrolong tribe, North Gippsland' as appendices to Robert Brough Smyth's *Aborigines of Victoria* (Smyth 1878, 2: 300–9 and 323–32). Morgan's account of the Iroquois was *League of the Ho-dé-no-sau-nee, or Iroquois* (Morgan 1851).

³ Joseph-François Lafitau, a Jesuit priest, had discussed the social organisation of the Iroquois in his *Moeurs des sauvages américains, comparées aux mœurs des premiers temps* (Customs of the American Indians compared with the customs of primitive times; Lafitau 1724).

⁴ McLennan was gravely ill with consumption (*ODNB*).

⁵ Francis Darwin; McLennan's wife was Eleonora Anne McLennan.

From Hermann Müller 30 November 1880

Lippstadt
Nov. 30., 1880.

My dear Sir,

I answer immediately your kind letter in order to moderate your apprehension about my brother Fritz. He has, indeed, suffered some damage by the unprecedented flood (the river Itajahy surpassed its normal level $1\frac{2}{3}$ Meter!) but a moderate one, which will not seriously trouble him in his scientific researches.¹ His books have been saved almost completely, likewise his microscope and apparatus, and he has already recommenced his scientific working. Two small but important articles have been lately sent by him to Dr. E. Krause for the *Kosmos*.² I do not believe, therefore, that your generous offer would be accepted by my brother, but I will send him your letter in order to let him know how highly you value his work.³

I have continued the lecture of your admirable work until to Chapt. VI. with ever increasing interest.⁴ It would, indeed, be impossible to find out any matter of more universal bearing in the whole vegetable Kingdom!

As you wish to hear soon about my brother I close this letter, as I am called off by my office until this evening

With the greatest thankfulness | yours very sincerely | H. Müller.

DAR 171: 316

¹ See letter to Hermann Müller, 27 November 1880. Fritz Müller's homestead was beside the Itajahy river (now called Itajaí Açu), about twenty-five miles inland from the town of Itajahy, in the north-east of Santa Catarina state in Brazil. It was later incorporated within the town of Blumenau.

² See letter from Ernst Krause, 26 November 1880 and n. 5.

³ In his letter to Müller of 27 November 1880, CD offered £50 or £100 to Fritz Müller to cover any losses from the flood.

⁴ Müller was reading *Movement in plants*; see letter from Hermann Müller, 27 November 1880. He later reviewed it in *Kosmos* (H. Müller 1880e).

From W. E. Darwin 1 December [1880]¹

Bank—

Dec 1

My dear Father,

I shall soon manage a day for Beaulieu—. We have a Yankee cousin of Sara's coming—and I can do him & the worms at the same time.²

I am extremely glad the book is going off so well.—³

We think of going to see the Roman Villa at Brading I of Wight on Friday Is there any point you wish observed, I am told there are worms as big as eels there!⁴

Sara sends her best love, she is quite aware what a half bred Darwin she is, but she has the decency to be delighted when any review speaks properly of you

My love to Mother. Please tell Bessy⁵ I will enquire about what she wants. | Your affect son | W. E Darwin

We were much interested in Mothers letter about L.^d Derby—⁶

Please say I shall be very glad of the Book if L.H.P do not want them.⁷

We had a smart dinner party last night—including M^{rs} Fleming⁸

Cornford Family Papers (DAR 275: 84)

¹ The year is established by the relationship between this letter and the letter to W. E. Darwin, 30 November [1880].

² CD had asked William to collect some worm-castings from Beaulieu Abbey, Hampshire, in his letter of 30 November [1880]. The Cistercian abbey, founded in 1204, was a popular tourist attraction. Sara Darwin's cousin has not been identified.

³ After a positive review of *Movement in plants* appeared in *The Times*, 20 November 1880, p. 9, CD's publisher had printed a further 500 copies (see letter from R. F. Cooke, 26 November 1880).

⁴ Ruins of a Roman villa at Brading on the Isle of Wight had been discovered in 1879; a description of the uncovered remains appeared in the *Antiquary* (Nicholson 1881).

⁵ Elizabeth Darwin.

⁶ A letter from Emma Darwin to H. E. Litchfield, [28 November 1880] (DAR 219.9: 253) described a visit from Lord and Lady Derby (Edward Henry and Mary Catherine Stanley) and the various topics discussed.

⁷ The Darwins visited Leith Hill Place, Surrey, the home of CD's sister Caroline Sarah Wedgwood, from 11 to 15 December 1880. It is unclear what book William refers to; he would already have received a copy of *Movement in plants*, since his name is on CD's presentation list (see Appendix IV).

⁸ Ida Mary Sheldon Fleming.

To J. D. Hooker 1 December 1880

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

December 1st 1880

My dear Hooker

I thank you cordially for your advice. I never for a moment expected that Frank would be elected until 2 or 3 years had elapsed; for I know that even such men as Bates were not soon elected.—¹

The wish to be proposed now was **wholly** my doing; as Frank himself thought it would be better to wait. To save you trouble I enclose a list of Franks papers, which he has just drawn up at my request, & which perhaps you will be so good as to return hereafter.—

I will follow your advice whatever it may be.

Perhaps I had better get some younger man to propose him, if he is to be proposed.² I daresay, however, it would be better to wait.

I again thank you very truly | Ever yours | C. Darwin

I need hardly add that if he is proposed & whoever proposes-him, I would send round the certificate for other signatures.—

DAR 95: 502–3

¹ See letter from J. D. Hooker, 29 November 1880. Hooker had advised CD about proposing Francis Darwin for fellowship of the Royal Society of London. Henry Walter Bates was first proposed for election to the Royal Society in 1879 by CD (see *Correspondence* vol. 27, letter to H. W. Bates, 26 January 1879). He was elected in June 1881 (Royal Society archives, GB 117 EC/1881/09).

² In the event, Francis was proposed by Michael Foster in January 1881 and elected in June 1882 (Royal Society archives, GB 117 EC/1882/09).

To W. E. Darwin 2 December [1880]

[Down.]

Dec. 2^d.—

My dear William.

When you go to Brading take a measure with you & if the land is nearly level, so that fine earth c^d not have been washed down over the remains—measure how thick the vegetable mould is over them.¹ The floor & walls will probably be covered by layer of rubbish which ought to be measured.—

If floors have been uncovered, see whether they have sagged in middle.² It is not likely that foundations of any wall will have been displayed but if this sh^d have been done, look for burrows beneath wall—& for any recent casting on tops of broken-down walls or on floors.—

By all means collect any castings from over the ruins, especially where soil happens not to be very thick.—

James Geikie has sent me a grand & *excellent* book (containing my notions about gravel near Southampton) which when I have read I can lend you.—³ I wish you or I c^d make out the relation of the brick-earth to the angular gravel. I suspect the former is the washings of the latter. I have much to write.

your affect. | C. D.—

Postmark: DE 2 80

DAR 210.6: 164

¹ William planned to visit the ruins of a Roman villa at Brading on the Isle of Wight on 3 December 1880 (see letter from W. E. Darwin, 1 December [1880] and n. 4).

² For more on the parts of the villa uncovered by this time, see Nicholson 1881; particularly noted were the mosaic pavements.

³ James Geikie had quoted from several of CD's letters about gravel drift near Southampton in *Prehistoric Europe: a geological sketch* (see Geikie 1881, pp. 141–2, and letter from James Geikie, 22 July 1880 and nn. 1 and 2).

To J. B. Innes 2 December 1880

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

Dec. 2^d 1880

My dear Innes

I have delayed answering your note of the 29th, until hearing from the acting executor, M^r Hensleigh Wedgwood.¹ He writes to day that the land will be sold by auction with the house: this will be soon, but date not yet fixed. I have an abominable number of letters to answer so pray excuse brevity; not that I can remember any news of this place worth telling you.—

Yours very sincerely | Ch. Darwin

Elizabeth Margaret Elliott Lucas (private collection)

¹ See letter from J. B. Innes, 29 November 1880. Innes wanted to acquire some of the land adjacent to Tromer Lodge, the home of Elizabeth Wedgwood, who had died on 8 November 1880 (CD's 'Journal' (Appendix II)). No letter from Hensleigh Wedgwood on the subject has been found.

From William Preyer 2 December 1880

Jena

Dec. 2, 1880

Dear Sir—

I feel very much obliged to you for having sent me your new book, the results of which seem to inaugurate quite a new theory of irritability.¹ Besides I think your graphic method may be applied to the chick in the egg. I shall try in spring.²

What you say about the origin of certain instincts which probably must be ascribed to “modifications” or “variations” of the brain, not to experience and habit, was quite new to me and is to a certain degree proved to be true by Brown-Séquards experiments and Westphals observations, which shew that epileptiform-movements may not only be artificially produced in guinea-pigs but that the young of such individuals show the same abnormal phenomena without having been pricked. I remember not exactly when and where this important fact has been published.³ But it confirms not only your theory, it proves also that such instincts which are of no “service to the species” may be inherited. I wish you could find time to write on “the marvellous facts of instinct” as indicated in your ‘Variation of Animals and Plants under domestication’ 1868 vol. 1, p. 8.⁴ With many thanks I enclose the short notes from ‘Nature’⁵

I am dear Sir your's most faithfully | W^m. Preyer

DAR 174: 72

- ¹ See letter to William Preyer, 27 November 1880. Preyer's name is on CD's presentation list for *Movement in plants* (see Appendix IV). Preyer refers to CD's theory that sensitivity to irritation was localised in specific areas of plants and transmitted to other parts, which then reacted in variable ways (*Movement in plants*, pp. 569–73).
- ² In *Movement in plants*, CD had employed line graphs to represent the circummutation of different plant organs. Preyer was studying embryonic movement in vertebrates. His conclusions on different causes of embryonic movement were detailed in *Specielle Physiologie des Embryo: Untersuchungen ueber Lebenserscheinungen vor der Geburt* (Special physiology of the embryo: investigations into phenomena of life before birth; Preyer 1885, pp. 441–60).
- ³ Preyer had asked CD for copies of letters on instincts that CD had written to *Nature* (see letter from William Preyer, 25 November 1880 and n. 2); in one of the letters, CD discussed the possibility that some instincts were the result of modifications of the brain (see *Correspondence* vol. 21, letter to *Nature*, [before 3 April 1873]). CD also discussed instincts as modifications of the brain in *Descent* 2d ed., p. 68. Charles Édouard Brown-Séquard had experimented on the artificial production of epileptic-like convulsions in guinea pigs and the transmission of these to offspring (Brown-Séquard 1860). Carl Westphal had noted further methods of inducing convulsions, but stressed the difference between these induced seizures and those of classical epilepsy in 'Ueber künstliche Erzeugung von Epilepsie bei Meerschweinchen' (On artificial production of epilepsy in guinea pigs; Westphal 1871).
- ⁴ CD had proposed to investigate instinct as part of a projected work discussing variation of organisms in a state of nature (*Variation* 1: 8). He later gave the draft of his work on the subject to George John Romanes, who published it as an appendix to *Mental evolution in animals* (G. J. Romanes 1883, pp. 355–84).
- ⁵ See letter from William Preyer, 25 November 1880 and n. 2. CD had sent copies of the articles with his letter to Preyer of 27 November 1880.

From W. E. Darwin 3 December [1880]¹

Basset.
Dec 3rd

Dear Father

I went today to the Roman Villa at Brading, and though I made out nothing very definite I will give you an account.² The villa is on a ploughed field which has a fairly regular slope of nearly 3° towards the S or S.E.

I should not wonder if the field had been in cultivation ever since the Romans; I tried the depth of mould in two places about 4 yards from each other, and it was rather over 3 feet, and 4 feet; but from what I was told by one man, and by the look of a hole in another part, & by the depth to bottom of foundations, I think it is over 5 feet deep in parts. The mould rests on a hard deep red compact sand, so hard that you could not dig it with a spade but required a pick the proprietor & finder of the villa told me.³

The remains consist of tessalated and I think sort of concrete pavements of little squares about 1 inch in size, surrounded by walls of about 1 foot in height. The tops of these walls were covered to various depths, in some cases only being 4 to 5 inches below the surface so that the owner told me that for the last 30 years he had occasionally struck them with his plough, and in other cases as much as 15 to 18 inches. I examined with the owner one spot of 15 inches in dept, and he said

there was pure mould to within about 4 or 5 inches to top of walls, & then there was mixed mould and stones mortar and loose rubbish from the destroyed walls. *Where I saw the top of wall only 4 or 5 inches below the surface, there did not seem to be loose rubbish on top of them, but the mould on top did not appear to be so pure. Not far from where the top of walls appear so near the surface I saw by holes that had been dug that the mould was of a considerable depth therefore though the spot where the wall came so near the surface was perhaps 20 to 25 yards higher up in the field than the part I measured where there was a depth of 15 inches, I doubt whether the slope of rather less than 3° (or 1 in 19.) could have had much to do with it. The proprietor convinced me that the foundations, which were about 3 ft 6 (roughly) below the tessalated pavements and therefore about 5 ft 9 below the surface of the field where I examined it, rested on the hard red sand; therefore I cannot think that the foundations have sunk, but I had no chance of examining whether there were any worms which were working in the red sand, as only a very narrow ditch had been dug close to the wall which had been partly filled in, but we sounded down on to the red sand with a heavy pointed iron bar which came up all red; I saw no trace of red worm casting and I should think the sand would be too hard to tempt the worms down.

The floors of most of the rooms were on the same level, but a few which were supposed to be offices as they had not ornamental pavement varied in level one with the other. On the whole the floors were level, but in 2 rooms which I measured by laying a board along them the floors sloped 1° degree in the same direction as the field itself.

In one of the rooms with a beautiful tessalated pavement there was an uneven sinking at the lower end below the general level of the floor of 4 or 5 inches (I unfortunately forgot to measure this and I only judge by memory) the pavement was fairly perfect where this sinking was, but he told me that they had taken out 20 tons (probably meaning loads) of stones and rubbish from this spot so that one cannot say how much the weight had to do with it

The largest room which is 40 by 18 at one end and 40 by 15 at the other, was very fairly level, though perhaps sloping a little to S.E. (the cantankerous agent of this part of building would not let me measure and was most indignant at my taking a worm casting) in one part there was a depression, (but not so great as in other room) where the tessalated pavement was gone, which had just the look as if it had been dripped upon thro' a hole thro' the roof; this floor had also had a mass of rubbish removed from it, and the agent said he supposed the depression to be caused by falling in of roof, which was made of heavy slabs of limestone or marble split into pieces of perhaps $\frac{3}{4}$ of inch thick.

The field itself was full of worm castings, and I was told by the under agent of the big room, that he had to continually sweep them off, especially on Monday morning. I saw several signs of castings on the edges of the room that had not been swept away, and I send you 3 samples which came up between the small square bits of pavement; these I took up myself from 3 different parts of the building— all

the castings I saw looked like pure mould. The proprietor of the part I was able to examine said there were plenty of worms in the rubbish they had removed from the pavements.

Where the hypocaust⁴ had been the floor of which was lower by 1 ft to 1 ft 6 than the tessalated pavement. I was able to examine to the depth of a foot or so the wall which separated the hypocaust from a room covered with pavement.

This wall consisted of large flints lumps of chalk a large pieces of apparently not hard limestone full of shells and lumps of mortar; the interstices were full of mould and worm holes, and here & there was a casting, and it looked as if the mortar had to some extent been replaced by mould. Unfortunately I had no chance of seeing what was under the tessalated p. as they had not touched it in any place, but as the castings appeared all to be of pure mould, and as the floors has so fairly kept their level, it seems probable that the foundation of the pavements was made like the wall I have described as being next to and a foot or so lower than the pavement

Would it not be likely that the worms might be continually bringing up mould through the pavement, and that any interstices in the rubble below would be filled up by worms working from outside, so that the mould would rise on the pavement without much if any sinkage.

If there are any points to be examined I should enjoy going again.

You will see by slip that villa was inhabited till 4th century. I should like the slip again⁵

I hope to go to Beaulieu soon.⁶

I shall like very much to see Geikie at Christmas⁷

Your affect son | W. E Darwin

*all the walls of the house itself appeared to be about the same depth from the surface namely 15 to 18 inches where I measured the depth was 15 inches, the agent of part of building where I was not allowed to measure said the depth was about 18—the part where there was only a depth of 4 to 5 inches seemed to be a narrow outhouse or passage with plain concrete bottom, in which I only saw one trace of a worm hole and no castings.

DAR 162: 111

CD ANNOTATIONS

1.3 3° ... S.E.] *underl blue crayon*

2.1 cultivation] *underl blue crayon*

2.3 over 3 feet.] *underl blue crayon*

2.3 4 feet;] *underl blue crayon*

2.5 5 feet deep] *double underl blue crayon*

2.5 mould ... a hard] *underl blue crayon*

3.3 tops ... depths.] *underl red crayon*

3.3 being ... 5 3.4] *underl red crayon*

3.5 with his plough,] *underl red crayon*

3.5 15 to 18 inches. 3.6] *underl red crayon*

3.6 15] *underl blue crayon*

3.7 4 or 5] *underl blue crayon*

- 3.15 The proprietor ... sunk, 3.19] *scored red crayon*
 3.19 the foundations have sunk,] *underl red crayon*
 4.1 floors ... rooms] *underl red crayon*
 4.3 On ... itself. 4.5] *scored red crayon*
 5.1 In ... it 5.6] *scored red crayon*
 6.1 The largest ... thick. 6.9] 'Depression supposed to be caused by rubbish.' *blue crayon*
 7.1 The ... building— 7.5] *double scored red crayon*
 7.6 The ... pavements. 7.8] *scored red crayon*
 8.1 Where ... pavement. 8.3] 'Bits of stone often taken deep down into burrows & then may be resaw-
 lowed.' *pencil*
 9.2 the ... mould. 9.4] *scored red crayon*
 9.2 mould and worm holes, 9.3] *underl pencil and red crayon*
 9.4 Unfortunately ... sinkage. 10.4] *crossed pencil*
 10.1 Would] 'W' over 'W' *pencil*
 10.1 Would ... sinkage. 10.4] 'W' *scored pencil*
 12.1 You ... again 12.2] *double scored red crayon*
End of letter: '18 Chambers discovered at end of October | Latest coin 337 A.D.' pencil

- ¹ The year is established by the relationship between this letter and the letter to W. E. Darwin, 2 December [1880].
² William was investigating worm activity at Brading on the Isle of Wight, where a Roman villa had been partly excavated earlier in 1880 (Nicholson 1881).
³ William Munns was the farmer who had discovered the Roman remains on his farm (Nicholson 1881).
⁴ In Roman villas, the hypocaust was a space under the floor in which heat from the furnace accumulated (*OED*).
⁵ The slip has not been found; it was probably a printed description of the remains.
⁶ Beaulieu Abbey (see letter from W. E. Darwin, 1 December [1880] and n. 2).
⁷ CD had received a copy of James Geikie's *Prehistoric Europe: a geological sketch* (Geikie 1881) and offered to lend it to William in his letter of 2 December [1880]. William and Sara Darwin were at Down from 23 to 29 December 1880 (Emma Darwin's diary (DAR 242)).

From James Paget 3 December 1880

Harewood Place | Hanover Square | London W.
 Dec^r 3. 1880

My dear Darwin

Let me thank you for your note and for the great pleasure I have had in even a partial reading of your new book—though it makes me feel that we must go beyond plants for a really elemental pathology—¹ I wish I knew enough of crystals to work at them—I gladly take note of your promise to come to luncheon.² Any day will do and the sooner after 1 the better for the longer time—

Sincerely your's | James Paget.

DAR 174: 12

- ¹ Paget's name is on CD's presentation list for *Movement in plants* (Appendix IV). CD had commented on Paget's lecture on elemental pathology (Paget 1880; see letter to James Paget, 14 November 1880).
² See letter to James Paget, 14 November 1880. The Darwins were in London from 7 to 11 December 1880 (CD's 'Journal' (Appendix II)).

From Friedrich Hildebrand 4 December 1880

Freiburg ¹/_{B.}
Decbr 4th 1880.

Dear and honoured Sir,

I intended to read first your new book and then to write to you, but now it seems to me, that this would last too long, so I do not hesitate any longer to give you my best thanks for your new kindness.¹

I do not like to make much words and I beg your pardon, when I only say, that, after having done some glances into your book, my admiration of you is as great as it was ever before. In my lectures to the students there will be much more to say this winter about the movements of plants and your name will be mentioned again and again.²

The translation of the botanical garden—from one part of the town to another has almost taken all my time these two years and hindered me to work as I should have liked, but now I shall have more opportunity to make new observations and experiments.³

I beg you to give my remembrances to Mr Francis Darwin and remain | Dear Sir
| yours | respectfully | Hildebrand

DAR 166: 216

¹ Hildebrand's name is on CD's presentation list for *Movement in plants* (Appendix IV).

² Hildebrand was professor of botany at Freiburg im Breisgau.

³ Hildebrand was director of the Freiburg im Breisgau botanic garden, which moved to new premises in 1878; in 1879 he opened the Botanical Institute (Correns 1916, pp. 31–2).

From J. D. Hooker 4 December 1880

Royal Gardens Kew
Dec 4/80

Dear Darwin

I quite hope that Frank will let me propose him for the Royal at once:—assuming that I should not run him against Dickie,¹ who will I should hope get in at once, as Huxley² highly approves my bringing him forward. We might then hope for Frank's election in 1882.

What do you think of the Haughton & Starkie Gardener correspondence in "Nature"? It appears to me that neither of them have the smallest notion of the biological factors of the problem they are blundering about.— They both assume that the only element that has to do with the restriction of *Araucaria Cunninghamii* to its limited area in Australia is Climate!³

Ever affy yrs | Jos D Hooker

The Grays return to Kew on Monday for 2 months, & we now talk of all going South in February⁴

DAR 104: 148–9

- ¹ Francis Darwin; see letter to J. D. Hooker, 1 December 1880 and n. 2. George Dickie was proposed for fellowship in the Royal Society of London by Hooker on 7 February 1881 and elected on 2 June 1881 (Royal Society archives, GB 117 EC/1881/09).
- ² Thomas Henry Huxley.
- ³ In his article 'A chapter in the history of the coniferæ', *Nature*, 1 July 1880, pp. 199–202, John Starkie Gardner hypothesised that trees of the genus *Araucaria*, which was restricted to the southern hemisphere, originated and became differentiated in the northern hemisphere. A letter from Samuel Haughton published in *Nature*, 7 October 1880, pp. 532–3, referred to Gardner's article and challenged geologists of the uniformitarian school to explain how the climate of Bournemouth could have been similar to that of Queensland, Australia. Letters to the journal continued the discussion on climate (see *Nature*, 4 November 1880, pp. 8–9; 18 November 1880, pp. 53–4; 2 December 1880, pp. 98–9). Further letters on the topic by Gardner, Haughton, and others appeared in *Nature* up to 24 February 1881. *Araucaria cunninghamii* (Moreton Bay pine) is a tree species native to parts of New Guinea and Australia (Queensland).
- ⁴ Asa and Jane Loring Gray had been in Paris from around October 1880; they travelled through Italy with Hooker and his wife, Hyacinth Hooker, in the spring of 1881 (J. L. Gray ed. 1893, p. 701).

From Ernst Krause¹ 4 December 1880

Berlin, | Friedenstrasse 11. 3. Tr.
den 4.12.80.

Hochverehrter Herr!

Ihr gütiges Schreiben vom 28.¹ Nov., welches ich einer kleinen Reise wegen, erst heute empfang, verursachte mir eine grosse Freude.² Ich glaube Ihnen versichern zu können, dass die Pangenesis-Theorie auf dem Continente mehr Anhänger hat, als die Perigenesis-Theorie, die den Meisten nur als ein Bild erscheint, was momentan bestechen kann. Viele stossen sich nur an dem Worte "Keimchen", weil sie dabei an Knospen u. dergl. denken, während doch jedes Partikelchen organisirter Materie diesem Begriffe entsprechen kann.³

Da Ihnen die Mittheilung über Fr. Müllers Beobachtung von Interesse ist, und es möglicherweise, weil das Januarheft des Kosmos bereits gefüllt ist, noch vier Wochen dauern kann, bis sie im Kosmos erscheint, so füge ich Ihnen eine Abschrift derselben bei, weil ich fürchte, dass Ihnen meine kurze Mittheilung darüber eine nicht ganz genaue Idee gegeben haben möchte.⁴ Die betreffende Mittheilung lautet, wie folgt:

"Wenn bei Krebsen verlorene Glieder sich neu bilden, haben sie mehrere Häutungen zu bestehen, ehe sie ihre volle Grösse und ihre regelrechte Gestalt wiedererlangen. Wie bei der Entwicklung des ganzen Thieres, geschieht es auch bei dieser Neubildung einzelner Gliedmassen nicht selten, dass die früheren Zustände den Gliedmassenbau der Vorfahren wiederholen. Zwei hübsche Beispiele bot mir eine kleine Garneele des Itajahy (*Atyoida Potimirim*)⁵ Die Scheeren der beiden ersten Fusspaare des Mittelleibes sind bei dieser Art in ganzer Länge gespalten, so dass sie fast nur aus den beiden Fingern, ohne eigentliche Hand bestehen; das Enddrittel jedes Fingers trägt einen dichten Pinsel sehr langer Borsten. Bei einer in Neubildung begriffenen Scheere war eine deutliche Hand vorhanden, fast so lang wie die Finger, und von diesen war der bewegliche ein wenig länger, als der unbewegliche Daumen. So erinnerte die junge Scheere an die in der verwandten

Gattung *Caridina* gewöhnliche Bildung (wie sie Milne Edwards von *C. typus*, Heller von *C. Desmarestii* gezeichnet hat),⁶ zeigte sich jedoch noch ursprünglicher darin, dass die Finger nicht löffelartig ausgehöhlt und am Ende nur mit sehr wenigen ganz kurzen Dornen besetzt waren.

Noch schlagender ist der zweite Fall. Beim dritten und vierten Fusspaare des *Mittelleibes* trägt der Schenkel an seinem Unterrande drei, seltener vier starke bewegliche Dornen und ein solcher steht an der Aussenseite nahe dem Ende des Schenkels; das letzte Glied dieser Füße hat ausser dem starken Enddorn, seinen Unterrand mit 5–8 krummen Dornen bewehrt. Das fünfte Fusspaar weicht dadurch ab, dass der Unterrand des Schenkels nur einen oder zwei bewegliche Dornen besitzt und dass der lange grade Unterrand des letzten Gliedes einen Kamm trägt, der aus zahlreichen (bis gegen 40) dichtstehenden, schlanken, graden Dornen gebildet ist. Ein in Neubildung begriffenen, der Häutung naher Fuss des fünften Paares zeigte nun den etwas gebogenen Unterrand des letzten Gliedes in seinen beiden letzten Dritteln mit etwa fünfzehn ziemlich weitläufig stehenden, meist ebenso begogenen Dornen besetzt, während unter der Haut schon ein prächtiger regelrechter Kamm für die nächste Häutung fertig lag. Der Schenkel trug, wie der des dritten und des vierten Fusspaares, drei grosse bewegliche Dornen am Unterrande; unter der Haut aber lagen nur zwei neue Dornen, so dass also der Schenkel nach der Häutung nicht mehr denen der vorangehenden Fusspaare, sondern dem anderen desselben Paares geglichen haben würde. Man darf diesen Befund wohl dahin deuten, dass bei den Vorfahren der *Atyoida* die drei letzten Fusspaare gleichgebildet waren und dass erst später das fünfte Paar einen oder zwei der Schenkeldornen verlor und an seinem Ende einen Kamm zum Reinigen namentlich der Hinterleibsfüße erhalten habe.⁷

Was die Angelegenheit des Kosmos betrifft, so höre ich, dass der Verleger, hinter meinem Rücken (!) die Zeitschrift an einen andern Buchhändler verkauft hat (Herrn E. Koch⁷ in Stuttgart, den Verleger Ihrer Werke) Derselbe geht damit um, den Kosmos in eine Wochenschrift zu verwandeln, als deren Redacteur der bekannte Reisende u. Geograph Friedrich von Hellwald⁸ ausersehen sein soll. Da dieser auf geographischem und ethnologischen Gebiete ungemein fruchtbare Autor von Naturwissenschaften (Physik, Chemie, Botanik, Zoologie, Geologie, Astronomie) auch nicht die leiseste Ahnung besitzt, so werden die deutschen Vertreter des Darwinismus diese buchhändlerische Speculation allgemein verabscheuen, da es sich hierbei nur um eine offenbare Schädigung der Sache handeln kann. Man rath mir nun, ich möchte die Monatschrift in der bisherigen Form in einem andern Verlage fortsetzen, aber ich weiss nicht, ob ich einen Verleger finden werde, der hierzu den Muth besitzt. Die Mitarbeiter würden mit wenigen Ausnahmen sämmtlich meiner Zeitschrift getreu bleiben, da man allgemein annimmt, dass eine darwinistische Zeitschrift unter Hellwald's Leitung der Sache nicht nützen kann. Ich für meine Person würde viel lieber die Zeitschrift völlig und ohne Nachfolge eingehen, als sie in völlig unberufene Hände ausgeliefert sehen. Prof. Haeckel, Dr. Fritz u. Hermann Müller⁹ und viele andere sind genau derselben Ansicht. Aber gegen Buchhändler-Complotte ist schwer anzukämpfen.

Dass Professor Jaeger in Stuttgart das Unglück gehabt hat, unter einen Eisenbahnzug zugerathen habe ich soeben mit der grössten Theilnahme in den Zeitungen gelesen.¹⁰ Die Räder sollen ihm über die Beine gegangen sein, und die Amputation des einen Beines nöthig machen. Hoffentlich ist das Unglück nicht so gross, wie es aus der Nachricht erscheinen könnte. Man klammert sich bei solchen Schreckensbotschaften gern an die Hoffnung, dass es eine falsche Nachricht sein könnte.

Ich wünsche sehr, Ihnen ein ander Mal erfreulichere Mittheilungen machen zu können und zeichne, hochverehrter Herr | In innigster Dankbarkeit | Ihr | ergebenster | Ernst Krause

DAR 169: 111

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

² See letter to Ernst Krause, 28 November 1880.

³ In the German translation of *Variation*, Julius Victor Carus had translated the word 'gemmule' as 'Keimchen', a diminutive form of 'Keim', which can be translated as 'bud', 'sprout', or 'germ' (see Carus trans. 1873, p. 492 and passim). CD had expressed the opinion that the case of limb regeneration Krause had mentioned could be better explained by CD's theory of heredity, pangenesis, than by Ernst Haeckel's theory, perigenesis (see letter to Ernst Krause, 28 November 1880 and n. 4). On pangenesis, see *Correspondence* vol. 13, letter to T. H. Huxley, 27 May [1865]; on perigenesis, see *Correspondence* vol. 24, letter from Ernst Haeckel, 9 May 1876.

⁴ Fritz Müller's short communication, 'Haeckel's biogenetisches Grundgesetz bei der Neubildung verlorener Glieder' (Haeckel's biogenetic law in the regeneration of lost limbs; F. Müller 1881b), the text of which is copied in full in this letter, appeared in the February 1881 issue of *Kosmos*.

⁵ *Atyoida potimirim* is a synonym of *Potimirim potimirim*, a species of freshwater shrimp of the family Atyidae, known in Brazil as the tiny or neon shrimp.

⁶ The genus *Caridina* is also in the family Atyidae. Henri Milne-Edwards figured *C. typus* (Australian amano shrimp) in Milne-Edwards 1834–40, Atlas, Pl. 25 bis, figs. 4 and 5. Camil Heller figured *C. desmarestii* (a synonym of *Atyaephyra desmarestii*) in Heller 1863, Pl. VIII, fig. 3.

⁷ Eduard Koch was the head of E. Schweizerbart'sche Verlagsbuchhandlung.

⁸ Friedrich von Hellwald was at this time the editor of the popular weekly *Das Ausland*.

⁹ Fritz and Hermann Müller were regular contributors to *Kosmos*.

¹⁰ Gustav Jäger had been a co-editor of *Kosmos* from 1877 to 1879. The newspaper report has not been identified; Krause later discovered that the report referred to another person (see *Correspondence* vol. 29, letter from Ernst Krause, 2 January 1881).

To J. D. Hooker 5 December 1880

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

Dec. 5 1880

My dear Hooker

I thank you much for being willing to propose Frank; I had begun to doubt whether I had not been too impudent in asking such a veteran to propose him, & was a little vexed with myself.— I do not see how your proposing him can interfere in the least with the higher claims of anyone else.— If you have not thrown away the list of Frank's papers please return them; & then I will fill up a certificate & send it you for signature & afterwards get other signatures.¹ But probably I shall not be

able to do this for about 10 days, as on Tuesday morning we go to “6 Queen Anne St” until Saturday, whence for a few days to Leith Hill Place.² I fear that there is no chance of our seeing you in London.

Again thanking you warmly | My dear Hooker | Ever Yours | Ch. Darwin

I did not attend to or care about the Houghton controversy; it seemed chiefly about the range of a single or at most two species.—³

I should have hugely enjoyed talking over with you Wallace’s book.—⁴

Can you give me seeds of *Commelina caelestis*? I have applied to Thompson of Ipswich, & he has not any.—⁵

DAR 95: 504–5

¹ See letter from J. D. Hooker, 4 December 1880 and n. 1. Francis Darwin was to be proposed for fellowship of the Royal Society of London. Proposal certificates listed the candidate’s publications. In the event, Francis was proposed by Michael Foster in January 1881 and elected in June 1882 (Royal Society archives, GB 117 EC/1882/09).

² The Darwins were in London at the home of CD’s brother, Erasmus Alvey Darwin, from 7 December 1880, then travelled to Leith Hill Place, Surrey, on 11 December, and returned home on 15 December (CD’s ‘Journal’ (Appendix II)).

³ See letter from J. D. Hooker, 4 December 1880 and n. 3. Samuel Houghton had argued against a uniformitarian view of geological change in a series of letters published in *Nature*.

⁴ See letter from J. D. Hooker, 22 November 1880, and letter to J. D. Hooker, 23 November 1880. Alfred Russel Wallace’s recently published work, *Island life* (Wallace 1880a), had been dedicated to Hooker.

⁵ *Commelina caelestis* is dayflower; CD’s notes, dated 24 December 1880 to 11 January 1881, on geotropism in young cotyledons of this species, are in DAR 67: 60–4. William Thompson (1823–1903) owned a nursery in Ipswich.

From Edouard Strasburger¹ 5 December 1880

Jena
le 5/12 | 1880.

Très honoré Monsieur.

Permettez moi de vous remercier bien sincèrement du beau cadeau que vous m’avez fait.² Je ne sais vraiment ce que je dois admirer davantage en vous: la finesse de vos observations, ou cette vigueur, cette force infatigable de travail, que vous avez du Vous conserver jusque dans un âge si avancé. Je me réjouis de voir Monsieur Votre fils Vous seconde si bien dans vos recherches et je Vous en félicite cordialement.³

Agrez, je Vous prie, l’assurance de ma plus haute considération et de tout mon dévouement.

E. Strasburger

DAR 177: 266

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

² Strasburger’s name is on CD’s presentation list for *Movement in plants* (Appendix IV).

³ The title page of *Movement in plants* acknowledged the assistance of Francis Darwin.

From Federico Delpino¹ 6 December 1880

Genoa
addì 6 Xbre 1880

Venerando mio maestro!

La ringrazio per il dono del suo grande lavoro sui moti delle piante; è uno nuovo titolo di straordinaria benemeranza verso la scienza.² La fitofisiologia odierna delirava, e la S.V. la rimette sul retto sentiero.

Poche settimane prima di ricevere il suo libro, nel discorso inaugurale che io feci nella Università di Genova, io lamentava che la moderna fisiologia vegetale commetteva il deplorabile errore di ritenere per azioni *dirette e non riflesse* le influenze della luce e della gravità e i moti che ne derivano.³ Ora la S.V. ha nel campo sperimentale smascherato questo gravissimo errore.⁴

Mille felicitazioni ed augurii! | Suo ossequentissimo | ammiratore | Federico Delpino

P.S. Mi prendo la libertà di | spedirle la mia fotografia.⁵

DAR 162: 157

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

² Delpino's name is on CD's presentation list for *Movement in plants* (Appendix IV).

³ In his inaugural lecture, *Il materialismo nella scienza* (Materialism in science; Delpino [1880]), Delpino attacked the materialist view that plant physiology could be explained purely in physico-chemical terms and argued that plants had what he called a perception of their own needs so that their responses to external stimuli were mediated by internal reactions (Delpino [1880], pp. 22–5).

⁴ Among CD's experimental findings was the discovery that sensitivity was localised in particular parts of plants, which signalled to other parts, which responded with various movements. For example, the sensitive part of the radicle or embryonic root was the tip, but movement occurred throughout the length, indicating that the tip directed the movement. CD compared the process to the action of the brain in lower animals (*Movement in plants*, p. 573).

⁵ The photograph has not been found.

To T. H. Huxley [7 December 1880]¹

6. Queen Anne St | Cavendish Sq^r
Tuesday

My dear Huxley

I sh^d very much like to see you for 5 minutes to hear about the Wallace affair.—²
Will you send me a card telling me at what hour in the morning of Thursday or Friday, you will be at Kensington? We leave London early on Saturday morning—³
It is of no real consequence if you could not see me, & I may not be up to coming.—

Ever yours | Ch. Darwin

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 355)

¹ The date is established by the relationship between this letter and the letter from T. H. Huxley, 7 December 1880; in 1880, 7 December was a Tuesday.

- ² CD was trying to obtain a government pension for Alfred Russel Wallace and had enlisted Huxley's help in getting signatures for the memorial (see letter to T. H. Huxley, 13 November 1880).
³ The Darwins were in London from 7 to 11 December 1880 (CD's 'Journal' (Appendix II)).

From T. H. Huxley 7 December 1880

4 Marlborough Place, | Abbey Road. N. W.
 Dec 7 1880

My dear Darwin

You will find me at South Kensington either Thursday or Friday morning up to 1. P.M. & very glad to see you—¹

I have been horridly busy for the last week or ten days & so the Wallace business did not press home /but/ it stand over²

Ever | Yours very faithfully | T H Huxley

DAR 166: 354

- ¹ Huxley was professor of natural history at the Royal School of Mines, South Kensington, London.
² CD and Huxley were trying to obtain a government pension for Alfred Russel Wallace (see letter to T. H. Huxley, [7 December 1880] and n. 2).

From T. M. Reade 7 December 1880

Dec^r 7th 80

My dear Sir

Will you kindly read the letter of Topley's in the Geo. Mag I send you. p—573— You are quoted by him as an authority in favor of the views of the permanence of the positions of Oceans & Continents through all geological time—¹

It appears to me after reading what you say in the 4th Edition of your "Origin of Species" that the words Mr Topley quotes, mostly the qualifying "if". scarcely bear out the inference he wishes drawn—² You put the views forward distinctly as an *hypothesis* to meet certain difficulties in the evidence of geology with respect to evolution— The question is a very important one and can scarcely be settled by quoting authority— it is a matter of *evidence*.

Faithfully Yours | T. Mellard Reade

D: C^{has} Darwin FRS. &c

Will you kindly return the Magazine.—

Contemporary Copy³
 University of Liverpool Library (TMR2.D.1.3)

- ¹ William Topley's letter appeared in the *Geological Magazine*, December 1880, pp. 573–4. Topley quoted a passage from *Origin* 3d ed., p. 335, and commented on an article by Reade in the September issue of the journal, 'Oceans and continents' (Reade 1880).

² See *Origin* 4th ed., p. 373; the passage quoted from the third edition is essentially unchanged in the fourth.

³ The copy was made by Reade.

From Arnold and Carolina Dodel-Port¹ 8 December 1880

Zürich,
8. December 1880.

Herrn Charles Darwin in Down. | (England.)

Hochgeehrtester Herr!

Vor 2 Tagen erhielt ich unter Kreuzband Ihr durch J. Murray an mich adressirtes Werk: “*The power of movement in plants*”, welches Exemplar Sie mir zu dediciren die Güte hatten.² Es ist wohl nicht nothwendig, Sie zu versichern, dass Sie mir durch diese herrliche Gabe eine *sehr grosse Freude* bereitet haben. Das hier behandelte Thema ist von so grossem Interesse, dass es gewiss nicht nur die Botaniker von Fach, sondern auch Zoologen & Biologen im weiterer Sinne fesseln wird. Mit grossem Vergnügen lesen wir von Zeit zu Zeit in den Zeitungen & Zeitschriften, dass Sie sich trotz der reichen Fülle von Jahren noch munter befinden und es ist unser herzlichster Wunsch, dass es noch recht lange so bleiben möge.

Es sind nun fast 10 Jahre her, seit ich gewagt habe, an der Züricher Universität den Darwinismus zum ersten Mal öffentlich zur Sprache zu bringen. *Vorher* verhielten sich die berufenen Vertreter der biologischen Disciplinen unserer Hochschule entweder ganz abweisend oder aber indifferent. Ich bin bekanntlich mit meinen Vorlesungen über die neue Lehre auf heftigen und auch auf verkappten heimtückischen Widerstand gestossen.³ Die Gegner *aller* Farben haben sich damals vereinigt, um mir das Leben und Lehren zum Theil recht sauer zu machen. Fast 10 Jahre lang haben sie mich unterdrückt, weil ich eine **lebendige** Idee zu vertreten wagte. Ja, die Behörden des eidgenössischen Polytechnicums in Zürich haben mir sogar das Abhalten eines Collegs über Darwinismus verboten, während man an der Hochschule etwas freisinniger handelte.

Im Juni letzthin wurde ich denn auch zum ausserordentlichen Professor der Botanik an der Universität ernannt.⁴ Bei diesem Anlass hat sich auch herausgestellt, dass der Darwinismus während der letzten 10 Jahre an der Hochschule Boden gewonnen hat. Mit Ausnahme des alten Oswald Heer erklären nun *alle* Lehrer der Botanik & Zoologie an der Universität und am Polytechnicum dahier, dass sie *Darwinianer* seien. Sie sehen, dass die Macht der Ideen stärker ist, als das althergebrachte Vorurtheil.

Wir dürfen also *jetzt* mit dem Stand der Dinge zufrieden sein.

Von unserm “Anatomisch-physiolog: Atlas der Botanik” habe ich Ihnen vor 10 Tagen die IV. Lieferung gesandt, die Sie nun wohl erhalten haben.⁵ Die Vollendung des Werkes ist—Gesundheit vorausgesetzt—*gesichert*. Bis Sommer 1882 dürfte der Atlas vollendet sein.⁶ (Im August 1880 erschien die *englische Ausgabe* bei Johnston in Edinburgh & London.)⁷

Unsere besten Wünsche für Ihr Wohlbefinden & unsere Empfehlungen an Sie & Ihren Herrn Sohn, Francis Darwin, Ihren braven Mitarbeiter: | von Ihren dankbarst ergebenen | D^r. A. & C. Dodel-Port

DAR 162: 199

- ¹ For a translation of this letter, see Appendix I.
- ² Dodel-Port's name is on CD's presentation list for *Movement in plants* (Appendix IV). The book was sent by CD's publisher, John Murray.
- ³ On the reception of Dodel-Port's early lectures, see *Correspondence* vol. 22, letter from Arnold Dodel, 13 December 1874.
- ⁴ Arnold Dodel-Port had held the position of privat-dozent (lecturer) in botany at Zurich since 1870 (*Historisches Lexikon der Schweiz*).
- ⁵ The plates of the *Anatomisch-physiologischer Atlas der Botanik* (Anatomical-physiological atlas of botany; Dodel-Port and Dodel-Port 1878[–83]) were published in seven instalments. In the accompanying volume of text, each section is separately paginated, since the plates and text did not appear in the order in which they were numbered. CD's copy of the work has not been found. Dodel-Port had sent CD the first part of the atlas in June 1878 and mentioned that a publisher might approach him about an English translation (see *Correspondence* vol. 26, letter from Arnold Dodel-Port, 12 June 1878). CD had promised to recommend a translation of the work 'in the strongest possible terms' (*ibid.*, letter to Arnold Dodel-Port, 15 June 1878). Further parts were sent in 1879 (see *Correspondence* vol. 27, letters to Arnold Dodel-Port, 12 February 1879 and 6 October 1879).
- ⁶ The work was completed in 1883; at that time a revised title page for the volume containing the description of the plates was issued with the title, *Erläuternder Text zum anatomisch-physiologischen Atlas der Botanik* (Illustrated text for the anatomical-physiological atlas of botany).
- ⁷ For the English translation, see Dodel-Port and Dodel-Port 1880–3. The cartographical publishing firm W. & A. K. Johnston was based in Edinburgh (*Post Office Edinburgh directory*).

From Hermann Vöchting 8 December 1880

Basel,
8. Dec. 1880.

My dear Dr. Darwin.

Since the announcement of your new work I had daily expected it, and was greatly and most agreeably surprised in getting a specimen of your own hand.¹ Allow me firstly and above all to tell you my best thanks for this most valuable present.

From the day when it was delivered to me, your book has been almost my only reading, and the latter has given me the greatest delight. I am sure, the multitude of new facts detected by you and the conclusions you have drawn from them, will have a reforming influence on a great part of botanic physiology. In congratulating you to this new publication I can merely tell, that it is only rivalled by the former works of the same great author.

Your book was the more important for me, because I was led on ground of own researches not yet published to results quite according to several of yours. For instance I was since long time convinced the bending of a horizontal radicle not being the direct effect of gravitation, but caused by the tip. To much occupied with

other objects I had as yet not made any attempts to verify my conjecture, and am now much surprised by the most ingenious method you have shown the foundation of my supposition.²

With this letter I take permission to send you some little publications made by myself in the last years, begging not to deal too hard with the trials of a beginner.³ I should have dispatched them sooner, but I thought them scarcely worthy enough for this purpose, and from this cause alone you will excuse the delay. In a future time I hope to appear before you with better works—

More than nine years have elapsed since you honored me with the allowance of paying you a visit in the Down.⁴ I shall never forget this day, the finest remembrance I have taken with me from my abode in England.

Paying my best respects to Mr. Francis Darwin and repeating my thanks for your work, I remain, my dear Dr. Darwin, in high veneration | always and sincerely yours | H. Vöchting.

DAR 180: 9

CD ANNOTATION

Top of letter. 'My dear F. I have thought that you w^d like to see this letter. I have utterly forgotten the man.
C. D.' *pencil*

¹ Vöchting's name is on CD's presentation list for *Movement in plants* (Appendix IV).

² CD and Francis Darwin performed numerous experiments demonstrating that the sensitivity of the radicle or embryonic root was located at the tip of the organ (see, for example, *Correspondence* vol. 27, letter to Francis Darwin, [before 5 June 1879]; see also *Movement in plants*, pp. 572–3).

³ Vöchting sent an offprint of his PhD dissertation, which appeared in the *Jahrbücher für wissenschaftliche Botanik*, 'Beiträge zur Morphologie und Anatomie der Rhipsalideen' (Contributions on the morphology and anatomy of the Rhipsalideae; Vöchting 1873), as well as his work 'Der Bau und die Entwicklung des Stammes der Melastomeen' (The structure and development of the stem of the Melastomaceae; Vöchting 1875). CD's copies are in the Darwin Library–Down. Vöchting also sent a copy of his book *Über Organbildung im Pflanzenreich* (On organ formation in the plant world; Vöchting 1878); CD's copy, annotated by both him and Francis, is in the Darwin Library–CUL.

⁴ No record of Vöchting's visit has been found, but he spent three months in 1871 at the Royal Botanic Gardens, Kew, and visited CD during this time (see Fitting 1919, p. 43).

From Hugo de Vries 8 December 1880

Amsterdam

8 Dec 1880

Dear Sir!

I am very much obliged to you for your great kindness of sending me your Work on the Power of Movement in plants, which I have read with the greatest interest.¹ I was much pleased to learn by your experiments that circumnutation is a general phenomenon in all growing plants, and that it is the basis of most of the other movements of vegetable organs. The little oscillations you describe in these circumnutating movements seem to remind of the "stossweise Aenderungen des Wachsthum's" of Sachs.² For if it is allowed to assume that these little "shocks" do not

occur at the same time on all sides of the growing organ, the result must be a similar movement as those described by you. I was especially interested by your experiments on the movements and the curious sensitiveness of the roots and plumules of young seedling-plants, which I hope to repeat as soon as I shall have an occasion, for I desire very much to observe myself these interesting and unexpected phenomena.³ I always remember the great pleasure, I had in repeating the experiments, described in your work on Insectivorous plants, with all those species, which I could procure either in botanical gardens or on excursions.⁴ By so doing I not only obtained a better knowledge of the subject, but often had the opportunity of showing these phenomena to others. And now I always cultivate some *Utricularia*, *Drosera* and *Pinguicula*, so as to be able to show their insectivorous habits to my students every year.

Your considerations on the embryology of leaves remembered me the curious case afforded by the young plants of *Sium latifolium*, which have their leaves divided in a much higher degree than the pinnated leaves of the older plants, and so show their descent from an Umbelliferous type with highly divided leaves.⁵ So your experiments and remarks on the danger, occasioned by the radiation during cold nights, suggested to me, that perhaps the hairs of plants might in many cases have been acquired for the same purpose as the sleeping movements, and that this supposition would account for the curious instance that so many leaves are covered with hairs when young, and lose them when growing older.⁶

In your Work you often speak of my papers on the same subject, and I am much indebted to you for your very kind judgment on them, which will be a stimulus to me in endeavouring to contribute my part to the advancement of science.⁷

With many thanks | Yours sincerely. | Hugo de Vries.

DAR 180: 25

¹ De Vries's name appears on CD's presentation list for *Movement in plants* (Appendix IV).

² 'Stossweise Aenderungen des Wachstums': sporadic changes in growth (German). Julius Sachs had used this expression in his article 'Ueber den Einfluss der Lufttemperatur und des Tageslichts auf die stündlichen und täglichen Aenderungen des Längenwachstums (Streckung) der Internodien' (On the impact of air temperature and daylight on the hourly and daily changes in linear growth (elongation) of internodes; Sachs 1872, p. 103) to describe small changes in the direction of growth not due to external causes. For CD's description of the irregular movements he described as circummutation, see *Movement in plants*, pp. 1–2.

³ CD had described the movement of radicles or embryonic roots, noting that only the tips were sensitive (see *Movement in plants*, pp. 129–200).

⁴ De Vries had mentioned repeating experiments described in *Insectivorous plants* while in Sachs's laboratory in the summer of 1875 (see *Correspondence* vol. 23, letter from Hugo de Vries, 7 November 1875).

⁵ *Sium latifolium* (wideleaf water-parsnip) is in the carrot and parsley family, Apiaceae; Umbelliferae is a synonym of Apiaceae.

⁶ In *Movement in plants*, pp. 284–97, CD had described a number of experiments preventing leaves from assuming a vertical position at night (nyctitropism) and concluded that these movements protected the upper surfaces of leaves from radiation and cold.

⁷ See *Movement in plants*, pp. 2, 6, 108, 217, 267–8, 283, 440–3, 485, 502, and 557. The work most cited by CD was Vries 1872.

To G. H. Darwin 9 December [1880]¹

6. Q. A. St
Dec. 9th

My dear George

The Kowalevskys have been to lunch & a very interesting visit it was—² Madame has been **greatly** interested by your papers & if you can spare a copy of your last one do send her one to “13 Montagu Place Russell Sq.”—³ I never saw such a funny little woman she bubbled over with enthusiasm about Sir W. Thomson’s papers & work— She was indignant with Cayley & declares that he makes his work far more difficult than it really is.—⁴

Yours affect | C. Darwin

I am half-dead with K. grating Voice.—

DAR 210.1: 101

¹ The year is established by the relationship between this letter and the letter to V. O. Kovalevsky, 25 November [1880].

² Sofia Vasilyevna Kovalevskaya and Vladimir Onufrievich Kovalevsky. Kovalevsky had planned a visit to Down on 25 November but was unable to go (see letter to V. O. Kovalevsky, 25 November [1880]). The Darwins were in London from 7 to 11 December 1880 (Emma Darwin’s diary (DAR 242)).

³ George had been working on problems associated with the rotation of a viscous or elastic body; his most recent paper was ‘On the analytical expressions which give the history of a fluid planet of small viscosity, attended by a single satellite’ (G. H. Darwin 1880). Kovalevskaya was interested in the rotation of a rigid body around a fixed point; she later published a prize-winning essay on the topic (Kovalevskaya 1889). For more on her contributions to mathematics, see Koblitz 1983.

⁴ Arthur Cayley was a professor of mathematics at Cambridge. William Thomson was well known for his work in thermodynamics and the age of the earth; he had encouraged George in his work on secular cooling

To T. H. Huxley 9 December 1880

6. Queen Anne St
Dec. 9th 1880

My dear Huxley

Whilst returning home, I thought of your second proposal, & it seems to me that if any such document was presented to a man like L^d Aberdare, he would say “what the devil do I care what he wishes”.¹ And as for Owen he would send the whole concern to the Devil rather than sign²—not that his signature signifies very much.— Whatever you decide on all points that will I do.—

Ever Yours | Ch. Darwin

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 351)

¹ CD had planned to meet with Huxley to discuss the memorial to obtain a government pension for Alfred Russel Wallace (see letter to T. H. Huxley, [7 December 1880]). Henry Austin Bruce, first Baron Aberdare, was president of the Royal Geographical Society; CD had offered to approach Bruce to sign the memorial (see letter to T. H. Huxley, 13 November 1880).

² Richard Owen and CD had not been on speaking terms since shortly after the publication of *Origin*, when Owen had written what CD described as a ‘spiteful’ review (see *Correspondence* vol. 8, letter to J. S. Henslow, 8 May [1860]).

From Ernst Krause¹ 9 December 1880

Berlin N.O. Friedenstr. 11. 3 Tr.
den 9.12.80.

Hochverehrter Herr!

Meine Absicht ist heute nur Ihnen mitzutheilen, dass ich das abscheuliche Buch von S. Butler: *Unconscious Memory* gestern empfangen und durchgelesen habe.² Es würde, falls Sie nichts dagegen einzuwenden hätten, meine Absicht sein, demselben im "Kosmos" zu antworten, darin zu sagen: 1, dass Sie mir lange vor Ersche(i)nen seines Buches, Ihre Absicht meinen Artikel übersetzen zu lassen zu erkennen gegeben haben, 2, dass Sie nicht nur die betreffenden Interpolationen contra Butler nicht veranlasst, sondern mir im Gegentheil gerathen hatten, von dem Buche gar keine Notiz zu nehmen. 3, dass *ich* dem Butler'schen Buche in Bezug auf Erasmus Darwin für die revidirte Ausgabe absolut nichts verdanke.³ Sagen Sie mir, bitte, mit zwei Worten, wenn Sie es lieber sehen, dass ich das Buch ignorire.

Ich glaube in der That, dass es diesem Querulanten, dem offenbar nur darum zu thun ist, mit *Ihnen* einen Streit zu haben—denn nur darum preist er auf den ersten Seiten Ihren Namen⁴—mit seinem Buche so gehen wird, wie mit dem Athenäum-Artikel: Niemand wird von dieser zu einem Elephanten aufgebläheten Mücke sprechen. Ich kann Ihnen nicht sagen, wie schmerzlich es mir ist, dass ich dazu beigetragen habe, Ihnen diesen vor keinen Mittel zurückschreckenden Gegner auf den Hals zu hetzen, denn eine derartige bodenlose Gemeinheit war nur denkbar und ist nur erklärlich, weil ihm der versetzte Hieb, bis ins Mark gegangen ist. Allerdings habe ich nicht das Gefühl, ihm zuviel gethan, oder ihn ungerecht beurtheilt zu haben, er verdiente im Gegentheil noch viel schlimmer mitgenommen zu werden.⁵

Verzeihen Sie, hochverehrter Herr, diese unerfreulichen Früchte | Ihrem | von Herzen ergebenen | Ernst Krause

DAR 92: B60

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

² Samuel Butler's *Unconscious memory* (Butler 1880) was published in November 1880 (*Publishers' Circular*, 15 November 1880, p. 1001). In a chapter titled "The manner in which Mr. Darwin met "Evolution, old and new" (Butler 1880, pp. 58–79), Butler accused CD and Krause of making unacknowledged use of his earlier work, *Evolution, old and new* (Butler 1879).

³ Krause was the editor of the journal *Kosmos*. For Butler's earlier accusations against CD and Krause in regard to *Erasmus Darwin*, made in a letter to the *Athenaeum*, see letter to H. E. Litchfield, 1 February [1880], enclosure 1. When Butler 1879 first appeared, CD had advised Krause to ignore the work (see *Correspondence* vol. 27, letter to Ernst Krause, 9 June [1879]), and this volume, letter to Ernst Krause, 9 February 1880). By 'revised edition' (*revidirte Ausgabe*), Krause means the essay published in *Erasmus Darwin*, revised from the article in *Kosmos* (Krause 1879a). Krause further revised and expanded the essay for the German edition (Krause 1880).

⁴ In the introduction to Butler 1880, pp. 2–4, Butler discussed the widespread approbation of CD's theory of evolution, before launching into his critique of it.

⁵ For Krause's critique of Butler 1879, see Krause 1879b.

To T. M. Reade 9 December 1880

[6 Queen Anne Street,] London
Dec. 9th 1880

My dear Sir

I am sorry to say that I do not return home till middle of next week, & as I order no pamphlets to be forwarded to me by post, I cannot return the *Geolog. Mag.* until my return home.—¹ Nor could my servants pick it out of the multitude which come by the Post. As I remarked in a letter to a friend, with whom I was discussing Wallace's last book, the subject to which you refer seems to me a most perplexing one.² The fact which I pointed out many years ago that all Oceanic isl^{ds} are volcanic (except St. Paul's, & *now* this is viewed by some as the nucleus of an ancient volcano) seems to me a strong argument that no continent ever occupied the great oceans.— Then there comes the statement from the Challenger that all sediment is deposited within 1 or 200 miles from the shores; though I sh^d have thought this rather doubtful with respect to great rivers like the Amazons.—³

The Chalk formerly seemed to me best case of an Ocean having extended where a continent now stands, but it seems that some good judges deny that the chalk is an oceanic deposit.⁴ On the whole I lean to the side that continents have since Cambrian times occupied, approximately, their present positions.⁵ But as I have said the question seems a difficult one, & the more it is discussed the better.

Believe me | My dear Sir | Yours faithfully | Ch. Darwin

P.S. I have been compelled to write this note in haste—

University of Liverpool Library (TMR1.D.7.7)

¹ See letter from T. M. Reade, 7 December 1880 and n. 1; Reade had sent a copy of the *Geological Magazine* by the same post as his letter. The Darwins were in London from 7 to 11 December 1880; from 11 to 15 December, they stayed at Leith Hill Place, Surrey, the home of CD's sister Caroline Sarah Wedgwood (CD's 'Journal' (Appendix II)).

² Alfred Russel Wallace's book *Island life* (Wallace 1880a) included a section on geological age and climate (*ibid.*, pp. 203–29), and a discussion of the permanence of continents and oceans (*ibid.*, pp. 81–102; see p. 82 for Wallace's critique of Reade on the question of whether oceans had once been dry land). CD discussed Wallace 1880a with Joseph Dalton Hooker (letter to J. D. Hooker, 23 November 1880).

³ CD had discussed the volcanic origin of oceanic islands in *Volcanic islands*, pp. 124–9. CD concluded that St Paul's Rocks (Saint Peter and Saint Paul Archipelago) were the top of a submarine mountain (see *Beagle' diary*, p. 36). On the extent of shore deposits as surveyed by HMS *Challenger*, see Murray 1876, p. 519. Wallace cited John Murray (1841–1914) on this point in Wallace 1880a, pp. 83–4.

⁴ See Wallace 1880a, pp. 87–9.

⁵ See *Origin* 6th ed., pp. 288–9.

From T. M. Reade 10 December 1880

Canning Chambers, | 4, South John Street, | Liverpool,
Dec^r 10th. 1880

My dear Sir—

I am much obliged to you for expressing your views so fully on the subject of Oceans & Continents My enquiry was made to know if M^r Topley had correctly

represented them¹—No doubt the fact that these oceanic islands are volcanic is an argument in favour of the views you hold but on the other hand if continents had resisted & become submerged to the depth of the water surrounding these islands it is highly improbable that any palæozoic or secondary rocks would appear above the surface—All the high ranges of mountains are tertiary & the highest points *mostly* volcanic—Mr Judd has pointed out very forcibly that the reason the older mountain ranges are lower than the newer ones is because of the waste they have suffered which goes on at a much more rapid rate in the higher regions of the Atmosphere.² Probably also many of these oceanic islands have been formed since the submergence of land—if there existed any—

There is also an argument to be drawn from analogy which I think of some force—If we look at the surface of the moon we find it covered most thickly over with volcanic rings—intersecting one another in every conceivable manner. If such a length of time as from the present to the Cambrian had elapsed without the oceanic areas distinctly altering should we find such an extent of level bottom in the oceans as the soundings disclose? There being no atmosphere in the moon the volcanic rings remain, the reason the earth presents such a difference in its surface configuration to that of the moon I maintain is solely on account of the ejected matter of volcanoes on the earth being worn away by atmospheric influences & made into sedimentary rocks (see my *Moon & the Earth* published by Bogue)—³

During the whole of this time also carbonate of lime would have been continuously deposited as ooze in these great ocean areas and as corals round some of the oceanic Islands—It would have been abstracted to that extent which is considerable from the sedimentary rocks which have been constructed & reconstructed since the Cambrian times & these rocks having the carbonate of lime only returned to them in part would have become progressively less calcareous—Instead of this the reverse is the case & analogies of water in various parts of the world show that the newer rocks are on the whole *more* calcareous—I fully agree with you that it is a “perplexing” subject—I would prefer those who differ from the views expressed in my “Oceans & Continents” meeting the arguments instead of referring to complaints—⁴ I have given the subject considerable attention lately & can only say that there are immeasurable circumstances to be taken into consideration in attempting to settle it which do not seem to have entered Mr Wallaces head—⁵ Of course I may be all wrong in my ideas but I could quote some eminent geologists who have written agreeing with me but I dont believe in that sort of argument.

Again thanking you for indicating your present views on the subject & knowing you are quite prepared to change them if you see sufficient cause | I remain | Dear Sir | Yours faithfully | T. Mellard Reade

Chas Darwin—

I must apologise for troubling you with so much writing but being deeply interested must be my excuse

DAR 176: 31

- ¹ See letter to T. M. Reade, 9 December 1880; CD confirmed his belief that the position of oceans and continents had not changed since the Cambrian period. William Topley had quoted a passage from *Origin* 3d ed., p. 335, in support of his views in a letter that appeared in the *Geological Magazine*, December 1880, pp. 573–4 (see letter from T. M. Reade, 7 December 1880).
- ² John Wesley Judd made this argument in Judd 1876, pp. 530–7; Alfred Russel Wallace had criticised Judd on this point in *Island life* (Wallace 1880a, p. 173 n.).
- ³ Reade's presidential address to the Liverpool Geological Society on 12 October 1875 was published in the society's *Proceedings* (T. M. Reade 1875) and appeared as an offprint entitled 'On the moon and the earth'. At this time, it was generally accepted that craters on the moon were the result of volcanic action (see, for example, Nasmyth and Carpenter 1874, pp. 89–116).
- ⁴ See n. 1, above. Reade's paper 'Oceans and continents' (Reade 1880) appeared in the *Geological Magazine*, December 1880, pp. 385–91.
- ⁵ Wallace had written on the permanence of continents and oceans in Wallace 1880a, disagreeing with Reade on several points (*ibid.*, pp. 81–102; see letter to T. M. Reade, 9 December 1880 and n. 2).

From G. J. Romanes 10 December 1880

18 Cornwall Terrace, Regent's Park, N.W.
December 10, 1880.

I return by this post the book on Hybridism, with many thanks.¹ It has been of great use to me in giving an abstract of the history.

I have read your own book with an amount of pleasure that I cannot express.²

One idea occurred to me with reference to luminous stimulation, which, if it has not already occurred to you, would be well worth trying. The suggestion suggests itself. How about the period of latent stimulation in these non-nervous and yet irritable tissues? And especially with reference to luminous stimulation it would be most interesting to ascertain whether the tissues are affected by brief *flashes* of light. If you had an apparatus to give bright electrical sparks in a dark room, and were to expose one of your plants to flashes of timed intervals between each other, you might ascertain, first, whether *any* number of sparks in *any* length of time would affect the plants at all; and second, if so, what number in a given time. I should not wonder (from some of my experiments on Medusæ, see 'Phil. Trans.' vol. clxvii. pt. ii. pp. 683–4)³ if it would turn out that a continuous uninterrupted series of sparks, however bright, would produce no effect at all, owing to the plant tissues being too sluggish to admit of being affected by a succession of stimuli each of such brief duration. But if any effect were produced, it would still be interesting to make out whether this interrupted source of flashing light were considerably less effective than a continuous source of the same intensity.

Very sincerely and most respectfully yours, | GEO. J. ROMANES.

E. D. Romanes 1896, pp. 103–4

¹ CD had sent Romanes *Die Pflanzen-Mischlinge* (Plant hybrids; Focke 1881) by Wilhelm Olbers Focke (see letter to G. J. Romanes, 14 November [1880] and n. 4).

² Romanes's name appears on CD's presentation list for *Movement in plants* (Appendix IV).

³ In his paper on the locomotor system of medusae (G. J. Romanes 1877, pp. 683–4), Romanes had measured the response of the hydromedusa *Tiaropsis polydiademata* (a synonym of *Mitrocomella polydiademata*) to bursts of light.

From T. H. Huxley 11 December 1880

Science and Art Department | South Kensington
Dec 11. 1880

My dear Darwin

Inclosed I send the draft of the memorial but a hard piece & much alteration in the last as struck me on re-reading it.¹

Also the list of people to send it to which I wrote out as we were talking²

I don't think your suggestion about Aberdare cutting up rough worth one single straw—so I tell you—but perhaps it is better to have the thing in the ordinary fashion³

Ever | Yours very faithfully | T H Huxley

I send this on spec to Queen Anne St^r.⁴

DAR 166: 355

¹ The draft memorial for Alfred Russel Wallace has not been found (see the letter to T. H. Huxley, 9 December 1880 and n. 1). An earlier version, in CD's hand, is in DAR 91: 95–8 (see Appendix VI).

² Huxley's list has not been found, but a list of potential signatories, written partially in CD's hand, is in DAR 91: 99–100.

³ CD was doubtful about presenting the memorial to Henry Austin Bruce, first Baron Aberdare (see letter to T. H. Huxley, 9 December 1880).

⁴ CD had been staying at 6 Queen Anne Street, London, the home of Erasmus Alvey Darwin, since 7 December 1880, but travelled to Leith Hill Place, Surrey, on 11 December (see 'Journal' (Appendix II)).

From James Torbitt 11 December 1880

J. Torbitt, | Wine Merchant. | 58, North Street, | Belfast.
11 Decr 1880.

Charles Darwin Esqr. | Down.

My dear Sir,

In the two private notes I failed to convey my ideas.¹

What I wanted was, that you should look at the product, and express an opinion as to its value, if you found it proper to do so.²

Best thanks for all your kindness | most respectfully yours | James Torbitt

DAR 178: 169

¹ The private notes have not been found (see letter to James Torbitt, 29 October 1880).

² Torbitt had tried to interest CD in a secret process that related to waste food use (see letter to James Torbitt, 29 October 1880, and A. Evans *et al.* 1996, p. 8).

From S. H. Haliburton 12 December [1880]¹

Bridge House | Richmond | S.W.
Dec^r. 12th.—

Dear Charles Darwin

It is no use! I cannot resist writing to tell you, what a real & great pleasure it was to me, to see you, & such a goodly Assemblage of Darwins besides, a gratification I had hardly hoped for—² You are one of my oldest remaining friends, & you

are so happily associated with the palmy days of yore, that it is indeed a heartfelt satisfaction to me to see you, & to feel assured, that old times are still fresh in your memory, & your friendly regard unabated—³

I can only hope this satisfaction may be renewed at no very distant period for Life is short, & uncertain; But while it lasts, believe that I am always most affectionately your's, | S. H. Haliburton.

PS. | Our meeting had but one drawback, you called me "*M^{rs}. Haliburton*" twice— This offence must not be repeated—⁴

DAR 99: 209–10

CD ANNOTATION

Top of letter: 'done'

¹ The year is established by the relationship between this letter and the letter to S. H. Haliburton, 13 December 1880.

² Haliburton had visited CD and Emma Darwin at the home of Erasmus Alvey Darwin some time during their stay in London from 7 to 11 December 1880 (see letter to S. H. Haliburton, 13 December 1880).

³ Haliburton had been a close friend and neighbour of CD before the *Beagle* voyage.

⁴ See letter to S. H. Haliburton, 22 November 1880.

To Ernst Krause [12 December 1880]¹

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

[Leith Hill Place, Surrey.]

Sunday morning

(Away from Home return on Wednesday)²

My dear Sir

I am writing in a great hurry on account of Sunday Post.— I am *much* pleased that you will answer M^r Butler— I have been advised not to do so.—³ What you say in your letter is perfectly true.— Your conduct towards me has been in every respect & at all times most generous.— I think that your answer had better be short.—

Butler has written an abusive letter about me to the newspaper & I have written to a friend to send it you as soon as possible.⁴ I have not read & do not intend to read a word which he writes— I really think that he is half insane & a lady who knows him well, says it is extreme vanity—

Pray send me a **corrected** proof of your answer.

Yours ever gratefully | Ch. Darwin

In great Haste

The Huntington Library (HM 36203)

¹ The date is established by the relationship between this letter and the letter from Ernst Krause, 9 December 1880. The Sunday following 9 December 1880 was 12 December.

² The Darwins had visited CD's brother, Erasmus Alvey Darwin, in London from 7 December 1880; from 11 December, they stayed at Leith Hill Place, Surrey, the home of CD's sister Caroline Sarah Wedgwood. They returned home on Wednesday 15 December (CD's 'Journal' (Appendix II)).

³ See letter from Ernst Krause, 9 December 1880 and n. 2. Krause was planning to respond to Samuel Butler's *Unconscious memory* (Butler 1880).

⁴ Butler's letter appeared in the *St James's Gazette*, 8 December 1880, p. 5.

To James Geikie 13 December 1880

Down. | Beckenham, Kent &c. [Leith Hill Place, Surrey]

Dec 13th 1880.

My dear Sir:

You must allow me the pleasure of thanking you for the great interest with which I have read your *Prehistoric Europe*—¹ Nothing has struck me more than the accumulated evidence of interglacial periods & assuredly the establishment of such periods is of *paramount* importance for understanding all the later changes on the earth's surface.² Reading your book has brought vividly before my mind the state of knowledge or rather ignorance, half a century ago, when all superficial matter was classed as Diluvium & not considered worthy of the attention of a Geologist.— If you can spare the time (though I ask out of mere idle curiosity) I should like to hear what you think of Mr. Mackintosh's paper illustrated by a little map with lines showing the courses or sources of the erratic boulders over the midland counties of England.—³ It is a little suspicious their ending rather abruptly near Wolverhampton, yet I must think that they were transported by floating ice— Fifty years ago I knew Shropshire well, & cannot remember anything like till,—but abundance of gravel & sand beds with recent marine shells. A great boulder which I had undermined on the summit of Ashly Heath 7203 (?) ft above the sea rested on clean blocks of the underlying red sandstone.⁴ I was, also, greatly interested by your long discussion on the löss;⁵ but I do not feel satisfied that all has been made out about it— I saw much brick earth near Southampton in some manner connected with the angular gravel, but had not strength enough to make out relations— It might be worth your while to bear in mind the possibility of fine sediment washed over & interstratified with thick beds of frozen snow, & therefore ultimately dropped irrespective of the present contour of the country.

I remember as a boy—that it was said that the floods of the Severn were more muddy when the floods were caused by melting snow, than from the heaviest rains; but why this should be so I cannot see.

Note. (I have run short of paper & am away from home).⁶

Another subject has interested me much. viz. the sliding & travelling of angular debris Ever since seeing the “Streams of Stones at the Falkland Islands—I have felt uneasy in my mind on this subject— I wish Mr Kerr's notion could be fully elucidated about frozen snow.⁷ Some one ought to observe the movements of the fields of snow which supply the glaciers in Switzerland.

Yours is a grand book, & I thank you heartily for the instruction & pleasure which it has given me.—

Pray believe me. | My dear Sir. | Yours sincerely— | Ch. Darwin

For Heavens sake forgive the untidiness of this whole note.—

Copy

DAR 144: 334

¹ Geikie had sent CD a copy of *Prehistoric Europe: a geological sketch* (Geikie 1881; see letter to James Geikie, 27 November 1880).

- ² Geikie devoted two chapters of his work to a discussion of the evidence of interglacial epochs, or periods of warmer climate interspersed with colder periods during the Ice Age (see Geikie 1881, pp. 252–330).
- ³ Daniel Mackintosh had published the results of a survey on drift-deposits of the erratic blocks of England and Wales (Mackintosh 1879).
- ⁴ CD had written about the boulder in 1842 in his ‘Ancient glaciers of Caernarvonshire’, p. 186 n. Ashley Heath is in Staffordshire. CD’s note on the boulder, made in June 1846 when he was visiting relatives at Maer, is in DAR 5: B31–2. See also *Correspondence* vol. 27, letter to Daniel Mackintosh, 9 October 1879; Mackintosh had mentioned the boulder in Mackintosh 1879, p. 442.
- ⁵ See Geikie 1881, pp. 143–68; löss or loess is a sedimentary deposit of silty or loamy material.
- ⁶ This phrase is written in the margin of the copy, at a point where the copyist notes the paper changed. CD was writing from Leith Hill Place, Surrey, the home of his sister Caroline Sarah Wedgwood. The Darwins returned home on 15 December (CD’s ‘Journal’ (Appendix II)).
- ⁷ CD discussed the gravel found on the Falkland Islands in *South America*, p. 21; he suggested that its occurrence might be the result of ice action. In Geikie 1881, p. 229, Geikie had referred to Washington Caruthers Kerr’s *Report to the Geological Survey of North Carolina* (Kerr 1875, p. 156); Kerr argued that successive freezing and thawing contributed to the downward movement of gravels, resulting in deposits similar to those caused by glacial drift.

To S. H. Haliburton 13 December 1880

Leith Hill Place | Dorking
Dec. 13. 1880

(Home tomorrow)

My dear Sarah

It was very good of you to write, & your note has given me much pleasure.¹ It is not too common to find anyone in this world as true as steel. Your postscript is your own dear old self.—²

Immediately you left (Queen Anne St. Emma & I said to one another we must try when the weather gets a little better, whether she will face the dullness of Down & pay us a little visit.³ So that in the early spring you will have to make up your mind.

I had hoped to call & see whether M^{rs}. Biddulph would admit me, & had got her address, but a Russian naturalist came to luncheon & dined me half to death & then an American naturalist, & I was half dead.⁴ But next time that I am in London I will try. I think that there must be some M^{rs} Biddulph living in Leamington, for I was told so positively that our M^{rs} Biddulph lived there, that I have thought of enquiring. In former years I was, also, rarely fit to see anybody.⁵

Let me call you | my dear old friend | Yours affectionately | Charles Darwin

Caroline is a little better & came down to dinner the first time for three months. She sends you her very kind love.⁶

DAR 185: 25

¹ See letter from S. H. Haliburton, 12 December [1880].

² Haliburton had chided CD for addressing her as Mrs Haliburton (letter from S. H. Haliburton, 12 December [1880]).

³ Haliburton had visited CD and Emma at CD’s brother’s house, 6 Queen Anne Street, London.

⁴ Fanny Myddelton Biddulph lived at 26 Grosvenor Place, London (Census returns of England and Wales 1881 (The National Archives: Public Record Office RG11/98/92/5)). The Russian naturalist was Vladimir Onufrievich Kovalevsky, who visited with his wife, Sofia Vasilyevna Kovalevskaya. The

American naturalist has not been identified, but may have been Asa Gray, who was working at Kew between December 1880 and March 1881 (J. L. Gray ed. 1893, 2: 713–14).

⁵ For more on CD's health and periods of illness, see *Correspondence* vol. 13, Appendix IV.

⁶ The Darwins were staying at Leith Hill Place, Surrey, the home of CD's sister, Caroline Sarah Wedgwood. Caroline had been ill since October (see letter to T. H. Farrer, 1 October 1880).

To James Paget 13 December 1880

[Leith Hill Place, Surrey]
Dec. 13th, 1880.

Perhaps you would like to see a very small “tumour” on a lateral branch of the Silver Fir, caused by an Œstrum, as stated (with references) in my *Power of Movement in Plants*.¹ These tumours are sometimes almost as big as a child's head. At what age they emit the *upright* shoot, I do not know.

Paget 1901, p. 409 n.

¹ See *Movement in plants*, pp. 188–9. The oestrum or parasite was identified as *Aecidium elatinum* (a synonym of *Melampsorella caryophyllacearum*, fir broom rust). For more on CD's and Francis Darwin's research on the topic, see *Correspondence* vol. 27, letter from Francis Darwin, [after 2 June 1879]. Paget had recently given a lecture that included a discussion of plant pathologies (see letter from James Paget, 12 November 1880, and letter to James Paget, 14 November 1880).

To G. J. Romanes 13 December 1880

Leith Hill Place
Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Dec. 13th 80

(return home tomorrow)

My dear Romanes

Your suggestion seems to me an **excellent** one, but we have no apparatus.¹ I will, however, show your note to Frank.² Certainly alternations of light & darkness at *long* intervals stimulates plants. W^d it not be worth your while to try seedlings of Canary grass or cabbage?^{2–3} The former w^d be best.— A whole pot of seedlings c^d be tried together. They are exqui(sitely) sensitive to light. They migh(t) be tried now, but possibly it might be a little better in early spring.—

We staid in London on ou(r) road here for 3 days & I had hoped to see you, but I had to see other people, & by the afternoon was dead beaten each day.⁴

I am *delighted* that my book has interested you.—⁵

I suppose that you have cases of dogs calling on each other & tempting one another to go out hunting by themselves.— There is a case here of pet dogs in 2 houses about $\frac{1}{2}$ mile apart, & their owners have agreed to shut up their dogs on alternate weeks, so that the 2 are never free at the same time for if they are, they will (go) hunting.—

My dear Romanes | Yours sincerely | Ch. Darwin

I hear that Mr S. Butler abuses me as ⟨a⟩ liar & scoundrel in his new book, but I ⟨do⟩ not intend to look at it—⁶

Bodleian Library (MS.Eng.d.3823 ff. 6–7)

- ¹ See letter from G. J. Romanes, 10 December 1880 and n. 3.
- ² Francis Darwin had assisted CD with experimental work for *Movement in plants*.
- ³ Canary grass is *Phalaris canariensis*; cabbage is *Brassica oleracea*. CD and Francis had studied the light response of cotyledons of *P. canariensis*, and noted the localised sensitivity as well as differences in response-time and angle of inclination of seedlings grown in the dark compared with those grown in light (see *Movement in plants*, pp. 455–77).
- ⁴ The Darwins stayed in London from 7 to 11 December 1880 (CD's 'Journal' (Appendix II)). On CD's visitors while in London, see the letter to S. H. Haliburton, 13 December 1880 and n. 3.
- ⁵ See letter from G. J. Romanes, 10 December 1880 and n. 2.
- ⁶ Samuel Butler's *Unconscious memory* contained a chapter titled "The manner in which Mr. Darwin met "Evolution, old and new"" (Butler 1880, pp. 58–79); Butler accused CD and Ernst Krause of making unacknowledged use of Butler's earlier work, *Evolution, old and new* (Butler 1879).

To James Torbitt 13 December 1880

Down, [Leith Hill Place, Surrey.]

Dec. 13, 1880.

My dear Sir

Absence from home has prevented me from at once thanking you for your report, which appears to me satisfactory and I hope satisfies you.¹ I will get it copied and send it to Mr. Caird and to Mr. Farrer; but the latter has been seriously unwell and all his official duties, I fear, are in arrear.² I will then call their attention to your offer to send them some of the varieties. You will remember that I hold money subscribed in aid of your experiments. As I am still away from home, I forget the exact sum, £75, or £80, or £85, I think. Will you kindly let me hear about this. I should like to return some to the subscribers. I do not want a penny back of what I subscribed, for it has been a pleasure to me to aid you in your noble endeavours to confer on your country a public benefit. I do not think that Mr. Farrer would care about any part of his subscription being returned. Please tell me how you are situated, and then when I send copies of your Report, I can report to the subscribers about the money. I told Mr. Caird that I held the above amount to be advanced to you or partly or wholly returned to him for the co-subscribers.³

Under the present dreadful state of Ireland, I fear it will be impossible to attract Mr. Forster's attention.⁴

With renewed good wishes for your success in all ways, I remain, | My dear Sir | Yours sincerely | Ch. Darwin.

Copy

DAR 148: 124

- ¹ Torbitt's report has not been found. CD was writing from Leith Hill Place, Surrey, the home of his sister, Caroline Sarah Wedgwood. The Darwins returned home on 15 December (CD's 'Journal' (Appendix II)).
- ² James Caird and Thomas Henry Farrer had helped raise funds for Torbitt, who was carrying out large-scale experiments aimed at producing blight-resistant potatoes. CD evidently sent the report to Farrer and asked him to pass it on to Caird (see letter to T. H. Farrer, 30 December 1880).
- ³ See letter to James Caird, 24 March 1880.
- ⁴ William Edward Forster was chief secretary for Ireland. Following the return of potato blight in 1879, Forster had introduced legislation in June 1880 to compensate evicted tenants under certain conditions. When the bill failed, Forster recommended that the Irish Land League leadership should be prosecuted for incitement. By December 1880, he faced ferocious opposition in the House of Commons. (*ODNB*; for more on the Land League and government policy on property rights at this time, see Comerford 2016.)

To James Murie 14 December [1880]¹

Down, | Beckenham, Kent. | (*Railway Station Orpington. S.E.R.*)
[Leith Hill Place, Surrey.]
Dec. 14th Tuesday

My dear Dr. Murie

Would you have the kindness to send me a P. Card with Prof. Allman's address on next Friday & Saturday, as I wish to communicate with him *promptly*, & believe that he is sometimes in London & sometimes in the country. I daresay as he is our President you will know his address.—²

Believe me | Yours very faithfully | Ch. Darwin

Linnean Society of London (LL/4)

- ¹ The year is established by the relationship between this letter and the letter to G. J. Allman, 17 December 1880.
- ² Murie was assistant secretary of the Linnean Society; George James Allman was president.

From G. J. Romanes 14 December 1880

Linnean Society, Burlington House, Piccadilly, London, W.:
December 14, 1880.

My dear Mr. Darwin,—

I am glad that you think the experiment worth trying. As you say you have not got the requisite apparatus for trying it, I have written to Professor Tyndall to see if he would allow it to be carried through at the Royal Institution.¹

If I had known you were in town I should have called to tell you about the Echinoderms.² My paper on them is now written (70 pages), so I have begun to come here (Burlington House) to read up systematically all the literature I can find on animal intelligence. Hence it is that, having left your letter at home, and not remembering the address upon it, I have to send this answer to Down.³

[Butler] is a lunatic beneath all contempt—an object of pity were it not for his vein of malice.⁴

Very sincerely and most respectfully yours, | GEO. J. ROMANES.

E. D. Romanes 1896, p. 104

¹ See letter from G. J. Romanes, 10 December 1880, and letter to G. J. Romanes, 13 December 1880. Romanes had suggested that experiments similar to those he had performed on medusae could be tried on plants. John Tyndall was superintendent of the Royal Institution of Great Britain.

² The Darwins were in London from 7 to 11 December 1880 (CD's 'Journal' (Appendix II)). Echinodermata is the phylum of sea urchins, starfishes (sea stars), brittle stars, sea lilies, and sea cucumbers.

³ Romanes's paper, co-authored with James Cossar Ewart, was on the locomotor system of echinoderms (G. J. Romanes and Ewart 1881 (see letter from G. J. Romanes, 5 November 1880)). It was delivered as the Croonian lecture of the Royal Society of London on 24 March 1881. Romanes was working on his book, *Animal intelligence* (G. J. Romanes 1882; see letter from G. J. Romanes, 22 April 1880).

⁴ A dash was printed instead of the name in the printed source of this letter. Samuel Butler had devoted a chapter of his latest book, *Unconscious memory* (Butler 1880), to an attack on CD and Ernst Krause for unacknowledged reaction to Butler's work (see letter to G. J. Romanes, 13 December 1880 and n. 6).

From Francis Darwin [before 15 December 1880]¹

4 Bryanston S^t

My dear Father,

If you dont want anything I think I will stay here till after the Linn Soc, but if you do I can come down quite well on Wednesday pm till Thursday pm if you telegraph.²

Will you please send me the abstracts of my papers registered to Bry Street³ Each abstract consists of one sheet of paper, and they are clipped with 2 or 3 (not more) other sheets, and lie on my *little straw* table; but please & for goodness sake as Ubbadub⁴ says dont hunt but write & say they cant be found & I will come on Thursday & get them. I have my potash M.S. here to go on with.⁵

Yrs affec | F.D

I have made short abstracts of my papers for Nature. but I'm afraid they are too short to be interesting.⁶

DAR 274.1: 62

¹ The date is established by the date on which Francis's papers were read at the Linnean Society (see n. 2, below).

² Francis presented two papers at the Linnean Society on Thursday, 16 December 1880 (F. Darwin 1880a and F. Darwin 1880b).

³ Four Bryanston Street, London, was the home of Henrietta Emma and Richard Buckley Litchfield.

⁴ Bernard Darwin.

⁵ Francis had been experimenting with potash (see letter to G. H. Darwin, 30 May [1880]). He began these experiments based on Julius Sachs's suggestion that potash was exuded from leaves (see *Correspondence* vol. 26, letter from Francis Darwin, [4–7 August 1878], and *Correspondence* vol. 27, letter to Edward Frankland, 8 February 1879).

⁶ Francis summarised his two papers in *Nature*, 23 December 1880, pp. 178–81. See letter to Francis Darwin, 27 December [1880].

From G. E. Mengozzi¹ 15 December 1880

Rome | 3 Piazza del Popolo
15. 10^{mbrc}. 1880

Illustre Monsieur

C'était pour moi et mes Collegues, composants la Magistrature de la Royale et Imperiale Accademie "La Scuola Italica" de Rome, une grande consolation l'avoir appris par Votre lettre dernière, que Vous avez accepté la nomination à Membre d'Honneur de la même Accademie.²

Dans quelques jours aura lieux une Séance pour en effectuer à unanimité Votre éléction et après Vous envoyer le relatif Diplôme, comme on à déjà participé au Président Genl. Honoraire le Roi d'Italie Humbert 1^{er}. et le Senateur M. Giacchi, Vice President Effectif,³ Votre généreuse acceptation qui apporte majeur lustre a la Royale Assemblée et utilité pour l'avancement des doctrines naturelles que Vous cultivez.

Dans ma dernières lettre je Vous avait priez d'agréez la dédicace de un des mes ouvrage, sur la: *Génération des animaux, et Nouvelle Classification des Êtres Naturels*;⁴ de cet ouvrage je vous envoie à présent trois *Tableaux Synoptiques*, pour qui Vous puissiez-le voir synthétiquement et abrégè.

Cette dédicace par Vous accepté trouvera accession parmi les savants, et sera honorée de la leur critique de quoi je désire pour me tenir sûre en propos.

Je sait que je n'ai pas aucun titre ni mérite pour obtenir de Vous si grand honneur et encouragement; mai, je connait la bienfaisance de Votre esprit et j'en doute pas de Votre adhésion.

J'ai relus avidement les dernières pages de votre ouvrage par Vous indiquées; "*Variations des animeaux et des Plantes sous l'état de domestication*".⁵

Le resultat dans ma pensée était que pendant qui est démontré par la organografie comparée et par Vos recherches sur la cause génétique des existences, que chaque être, y compris l'homme, à origine par une préexistente forme, pour ça il ne vient pas annullée la Divinité etant l'existence de Dieu assez clairement et sufficentement démontrée par l'intelligence humaine, parce que la même est plus élevée comparativement à celle des toutes autres existences, ainsi comme sagement vous dites dans le grand volume sur l'Origine de l'homme; c'est à dire, que seulement pour l'élévation des facultés intellectuelles l'homme pervient reconaître l'existence de un créateur et Regulateur de l'Univers; l'amoureux createur, comme Vous m'écrivait.

Dans la sûreté que Vous Vous daignerez d'accepter la dedicace de mon pauvre ouvrage du quel je Vous envoie unis à la presente lettre, les tableaux synoptiques; je Vous remercie en anticipation pendant que à present et toujours je me repéte | Très dévoué et obbligé | Comm. Prof. G. E. Mengozzi M.D.

P.S. On m'á dit que Vous avez publiée un nouveaux ouvrage: *The power of Movement in the Plants*, est il vraie?⁶

Quadro Sinottico sulla Generazione

La generazione può essere	Aessuale	Gemmipara	Polipi				
		Fissipara					
	Sessuale	Unisessuale (con ovario semplice)	Radiari				
			Monoica	Con ermafroditismo imperfetto	In cui si richiede l'accoppiamento di due sessi	Elici, Limaci, Aplisie	
		Con ermafroditismo perfetto		In cui ogni individuo si feconda da se medesimo	Quasi tutti i Molluschi		
		Dioica		Senza antagonismo dei sessi	Coll'azione riunita di ambi i sessi	Nelle Rane escluse le salamandre	
					Coll'azione separata -----	In molti Pesci ossei. Salamandre ecct.	
		Bisessuale	Con antagonismo	Vero accoppiamento	Tutti i vertebrati a sangue caldo; tutti i Rettili, escluso l'ordine delle Rane; i Pesci cartilaginosi e molti articolati.		

[Enclosure 1]

Quadro Sinottico sullo svolgimento delle uova

Lo svolgimento dell'uovo può avvenire	Fuori del corpo materno	Senza influenza materna	Coll'azione del calore	Ovipari perfetti
		Con influenza materna	Per mezzo di un incubazione	
	Nel corpo materno	Esternamente	Cogli organi nutricatori	Ovipari imperfetti e di transizione
			Cogli organi incubatori	
		Internamente	Nella cloaca	Ovo-vipari
			Nell'utero	Vivipari



COMMENDATORE

DOTTOR GIOVANNI ETTORE MENGOZZI RIMINESE

nato in Loreto

*Professore dell'Università Romana
della Sapienza*

G. E. Mengozzi.

Mengozzi 1881, frontispiece.

From the collection of The Royal College of Surgeons of England.

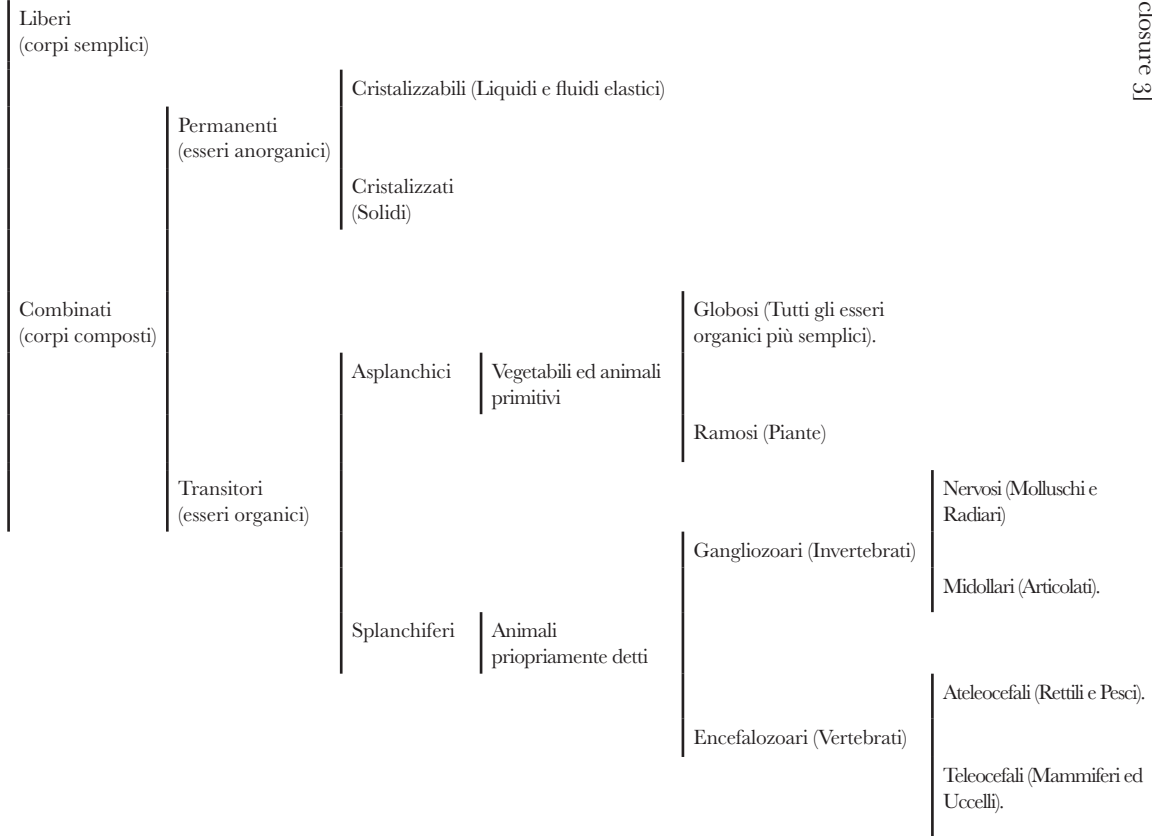
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DAR 171: 155

Tutti gli esseri Naturali,
possono dividersi in ...

Quadro Sinottico sulla Classificazione degli esseri Naturali



[Enclosure 3]

December 1880

481

CD ANNOTATIONS

4.1 Cette ... *domestication*". 6.2]: 'X' pencil

Third enclosure: 'Very much obliged for your courteous & very interesting letter & He has published Power of M will direct his publisher to send you a copy'⁷ pencil

- ¹ For a translation of this letter, see Appendix I. Mengozzi's French (not his native tongue) was extremely poor.
- ² See letter to G. E. Mengozzi, [after 24 November 1880].
- ³ Umberto I; Michele Giacchi.
- ⁴ See letter from G. E. Mengozzi, 24 November 1880. The book, *Nuova classificazione degli esseri naturali e saggio sulla generazione degli animali*, published in 1881, contains a long introductory dedication to CD (Mengozzi 1881, pp. vii–xxv).
- ⁵ See letter to G. E. Mengozzi, [before 28] October 1880; CD advised Mengozzi to read the last pages of *Variation*. The French translation was Moulinié trans. 1868.
- ⁶ *Movement in plants* was published on 6 November 1880 (Freeman 1977).
- ⁷ CD's annotations are notes for his reply to Mengozzi, which has not been found. Mengozzi's name is not on CD's presentation list for *Movement in plants*.

From James Torbitt 15 December 1880

J. Torbitt, | *Wine Merchant*. | 58, North Street, | Belfast.
15 Decr 1880.

Charles Darwin Esqr. | Down.

My dear Sir,

In continuance of my last respects—¹

Under date of Saturday last Mr Forster wrote me that "owing to his numerous and pressing official engagements he regrets that he is at present unable to attend to the matter of growing disease-proof potatos"²

I forward today a dozen copies of enclosed to Downing Street, and shall send you reply on receipt thereof.

Most respectfully | my dear Sir | James Torbitt

[Enclosure]

of grea⟨t⟩ importance in the opinion of Sir Joseph Hooker, Mr Caird CB and others³

RIGHT HON. W. E. GLADSTONE,⁴

FIRST LORD OF THE TREASURY, &C.

RIGHT HON. SIR,

*A Committee of the House of Commons has recommended that the Government should now undertake the work of producing new varieties of the potato, for National purposes.*⁵

Now, I most respectfully beg leave to state that, acting under the strong encouragement, and even pecuniary assistance, of some of the highest authorities on this subject in the Kingdom, I have been engaged on this work during the last six years, and upon a scale far larger than was ever before attempted. During each year I have grown 5,000 varieties of the plant, selected the best of those varieties which were disease-proof, and what is of vast importance, I have intercrossed—cross-bred—these best disease-proof varieties, and I have now in my possession seed cross-bred during three generations, each generation having been disease-proof. That disease-proof varieties

have been obtained I can adduce ample evidence, of which I most respectfully beg leave to submit a specimen:—Captain C. R. Barton, J.P., D.L., *The Waterfoot, Pettigo, County Fermanagh*, grew for me last season 82 disease-proof varieties of 1879, and he reports to me that in not one of these 82 varieties is there any disease whatever.⁶

Under these circumstances, I would most respectfully propose that, next season, I should launch into life one hundred thousand new varieties of the plant, each single variety being endowed with vitality sufficient to gather from the air, and from the earth, millions of tons of food.

And these new varieties, during the first years of life yield enormous crops. Knight found 34 tons;⁷ I have found 24 tons, while the average yield of old varieties is not more perhaps than 6 tons per acre—one fourth very often rotten.

And I would estimate the advantages to be gained by growing new varieties as follows:—five to ten millions per annum saving, by suppression of the disease; twenty to thirty millions from doubled crops; and twenty to thirty millions from doubled area of cultivation.

It would mean a supply of home grown food sufficient to defy foreign competition, and at the same time maintain intact, the rental of England—and to Ireland it would mean peace.⁸

Praying for inquiry into the matter, | I am, most respectfully, | Right Honourable Sir, | Your most obedient servant, | JAMES TORBITT.

BELFAST, 15th Dec., 1880.

DAR 178: 170, 171/3

¹ See letter from James Torbitt, 11 December 1880.

² See letter to James Torbitt, 13 December 1880 and n. 4. William Edward Forster was chief secretary for Ireland.

³ This sentence was added in an unknown hand at the top of the printed letter. Joseph Dalton Hooker and James Caird. CB: Companion of the Bath (a British order of chivalry).

⁴ William Ewart Gladstone was prime minister at this time (*ODNB*).

⁵ The *Report on potato crop* was published on 9 July 1880. The select committee had been formed in May 1880 to inquire into the best means of diminishing the frequency and extent of potato-crop failures; Torbitt had not been asked to give evidence by the committee.

⁶ Charles Robert Barton had been growing potatoes supplied by Torbitt since 1876 (see *Correspondence* vol. 26, letter from James Torbitt, 15 March 1878, enclosure).

⁷ Torbitt had previously referred to the potato experiments of Thomas Andrew Knight in the enclosure to his letter of 24 February 1878 (*Correspondence* vol. 26).

⁸ The failure of the potato crop in Ireland in 1879 had contributed to the eviction of tenant farmers, the formation of the Irish Land League, and violence against English landlords (see Comerford 2016).

From W. R. Browne 16 December 1880

38, Belgrave Road, | London, S.W.
16 December, 1880.

Sir,

I am authorized by His Grace The Archbishop of Canterbury¹ to inform you that he has consented to the request contained in the Memorial, of which I enclose a copy, and that it will give him much pleasure if you are able to attend the proposed Private Conference, which is to be held at Lambeth Palace, on Friday, January 7th, 1881, at 3 p.m.

A reply to the above address will be a favour.

Yours truly, | Walter R Browne

The Arch^p particularly hopes you may find yourself able to to be present.

The names attached to the Memorial are as follows:

G. G. STOKES.	G. D. LIVEING.
BALFOUR STEWART.	W. K. PARKER.
H. C. SORBY.	G. M. HUMPHRY.
P. G. TAIT.	J. H. GLADSTONE.
G. E. PAGET.	W. H. DALLINGER.
GEORGE ROLLESTON.	J. W. REYNOLDS.
T. G. BONNEY.	JAMES STUART.
C. WYVILLE THOMSON.	J. M. WILSON.

[Enclosure]

We, the undersigned, respectfully solicit the attention of Your Grace to the following Statement.

1. *Almost the whole of intelligent modern Infidelity rests on the assumption that the proved conclusions of modern Science are hopelessly at variance with the fundamental doctrines both of natural and of revealed religion.*

2. *We believe this assumption to be unwarranted by the facts of the case; and we have reason to think that our belief is shared by a large number of men of high eminence in all departments of knowledge.*

3. *Nevertheless, while those who hold the assumption to be true are continually impressing this fact upon the world, those who, like ourselves, hold it to be false, are mostly found from various motives, to keep the belief to themselves.*

4. *The result of this is that the case in favour of Religion is very much left to go by default; and the reading public are naturally led to conclude that the breach between Religion and Science is admitted by all Scientific men.*

5. *In these circumstances it seems to us desirable that men of Science who do not hold the above assumption should confer together as to the means of accomplishing two objects: (1) to bring the weight of their authority to bear on the judgement of the reading public, as a counterpoise to the authority of those who maintain and promulgate the above assumption; (2) to investigate fully the real relations between Religion and modern Science, and this under two heads, (a) an examination into the conclusions of Science, especially those of the inexact Sciences, to determine which are really proved and which are not; (b) an examination into the doctrines of Religion to determine where they come in contact with the proved conclusions of Science, whether they are at variance with them, and if so, whether they can be modified into accordance with them: and if not, how far the two can be held together.*

6. *We respectfully submit that these objects are of the very first importance to the interests of Religion, and therefore may claim the attention of all those to whom those interests are confided.*

7. *Recognizing the sympathy which Your Grace has already evinced towards these objects, and the steps you have taken towards realising them, we venture to solicit you to summon and preside at a Private Conference, as suggested above, where the best means of attaining them may be discussed.*

TO HIS GRACE THE ARCHBISHOP OF CANTERBURY.

Cambridge University Library (MS Microfilm10682); DAR 202: 17

¹ Archibald Campbell Tait was the archbishop of Canterbury.

From Otto Hahn 16 December 1880

Reutlingen | Württemberg | Germany
16 December 1880

Dear Sir!

I thank you for your kind letter of 4. Novbr. and send you now my book “Die Meteoriten und ihre organismen”.¹

Professor Dr. Quenstedt (Tübingen) declared, that he had never seen such structure in any mineral or rock: the same declared that the corals and minerals of my book are veritable corals and crinoids.²

It is now the beginning of life on earth—the beginning of the earth itself nearly sure and it is found a confirmation for your immortal discoveries.

I am dear Sir Your | faithfull | Dr Otto Hahn

DAR 166: 83

¹ CD's letter has not been found. CD's copy of *Die Meteorite (Chondrite) und ihre Organismen* (Meteorites (chondrites) and their organisms; Hahn 1880) is in the Darwin Library–Down. Hahn believed he had found various organisms in a stony (chondritic) meteorite.

² No publication by Friedrich August Quenstedt with these comments has been identified.

To Hermann Vöchting 16 December 1880

Down | Beckenham Kent (&^c)
Dec^r. 16. 1880

My dear Sir.

Absence from home has prevented me from sooner thanking you for your kind present of your several publications—¹ I procured some time ago your “*Organbildung &c.*”—but it was too late for me to profit by it for my book, as I was correcting the press,— I read only parts, but my son Francis read the whole with care and told me much about it, which greatly interested me.² I also read your article in *Bot. Zeitung*³ My son began at once experimenting to test your views & this very night will read a paper before the Linnean Soc, on the roots of *Rubus*, & I think that you will be pleased to find how well his conclusions agree with yours.—⁴ He will of course send you a copy of his paper when it is printed I have sent him your letter which will please him if he agrees with me, for your letter has given me *real* pleasure and I did

not at all know, what the many great physiologists of Germany Switzerland & Holland would think of it—I was quite sorry to read Sachs' views about root forming matter &c for I have an unbounded admiration for Sachs.—⁵ In this country we are dreadfully behind in Physiological Botany—

Once again thanking you for your very kind letter I remain | My dear Sir. | Yours sincerely. | Ch. Darwin.

The copy of your work which I procured shall be sent to the Royal or Linnean Society as on reflexion we think it would be most useful.—⁶

Copy

DAR 148: 197

¹ See letter from Hermann Vöchting, 8 December 1880 and n. 3. CD was away from home from 7 to 15 December 1880 (CD's 'Journal' (Appendix II)).

² CD's copy of Vöchting 1878, annotated by both him and Francis Darwin, is in the Darwin Library–CUL.

³ Vöchting's article was a reply to Julius Sachs's critique of the theory of fixed polarity expressed in Vöchting 1878 (see Sachs 1880 and Vöchting 1880).

⁴ Francis's paper was 'The theory of the growth of cuttings; illustrated by observations on the bramble, *Rubus fruticosus*' (F. Darwin 1880b). Francis had tested the competing theories of Vöchting and Sachs regarding the growth of roots and branches from cuttings; he concluded that Vöchting was correct in claiming that the development of specific organs at the apex and base of the cutting was principally determined by the morphology of the cutting independent of gravitation.

⁵ Vöchting had told CD that *Movement in plants* would have 'a reforming influence on a great part of botanic physiology' (letter from Hermann Vöchting, 8 December 1880). CD was aware that Sachs had different views on several aspects of plant movement, notably the function of the root tip (see, for example, *Correspondence* vol. 27, letter to Francis Darwin, 28 June [1879]).

⁶ There is a copy of Vöchting 1878 in the library of the Linnean Society; no copy has been found at the Royal Society of London.

To G. J. Allman 17 December 1880

Down
D 17/80

My dear Prof Allman

Will you be so kind as to read over the enclosed memorial, about which I feel the deepest interest.¹ If you agree, will you sign it and append "Pres Linnean Soc". Huxley & I think it best to get only about a dozen signatures and all of men who have done good scientific work.² Hooker & Lubbock will sign—³ It will save a day's post if you will place the memorial in the envelope addressed to Prof Flower Pres Zoolg Soc—⁴

Believe me my dear Prof Allman | Yours very sincerely | Ch Darwin

Copy

DAR 143: 18

¹ The enclosure was the memorial to obtain a government pension for Alfred Russel Wallace. It has not been found, but there is a draft of it in DAR 196: 3; for a transcription, see Appendix VI.

² Allman was president of the Linnean Society. CD and Thomas Henry Huxley had discussed the wording of the memorial when CD was in London earlier in the month (see letter from T. H. Huxley, 11 December 1880).

³ Joseph Dalton Hooker and John Lubbock.

⁴ William Henry Flower was president of the Zoological Society of London.

To W. E. Darwin 17 December [1880]¹

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

Dec. 17th

My dear old W.

I have carefully washed & examined the 4 castings from Brading & they have yielded a large number of bits of tiles or bricks, which are more or less rounded, & a multitude of bits of mortar & white calc. cement.—² But it is extremely difficult to judge whether the rounding is due to the action of the gizzards of worms.— I am very much obliged for the great trouble which you took about the Brading ruins & I am very glad to have your account.³ Everything agrees pretty closely with what I have seen or heard elsewhere.— It is odd some of the floors having subsided in the line of slope of the field.—

There are 2 questions, which you may *perhaps* be able to answer: are the ruins in the upper or lower part of the field which slopes at about 3°; for if in the lower part much earth will have been washed down.

Secondly can you remember whether the floor of the great room (40 ft x 18 ft) whence you got the castings, lay at some considerable depth beneath the surface of the field.—

I return slip to avoid chance of loss.

Judging from letters from Germany the “Power of Movement” will prove a good hit in Phys. Botany. There is no one in this country who knows anything of the subject.—⁴

It will be very nice seeing you & Sara here so soon.—⁵

When you have any leisure (if such time ever comes) try & make out relation of the brick earth near you to the angular gravel-bed.

Ever yours affect | C. Darwin

American Philosophical Society (Mss B.D25)

¹ The year is established by the relationship between this letter and the letter from W. E. Darwin, 3 December [1880].

² William had visited the recently uncovered Roman villa at Brading on the Isle of Wight (letter from W. E. Darwin, 3 December [1880]). Calc.: calcareous (i.e. composed of calcium carbonate).

³ For William’s description of the ruins and surrounding area, see the letter from W. E. Darwin, 3 December [1880].

⁴ CD had received several letters from German correspondents praising *Movement in plants* (see, for example, letter from Friedrich Hildebrand, 4 December 1880, and letter from Hermann Vöchting, 8 December 1880).

⁵ William and Sara Darwin arrived at Down on 23 December 1880 and stayed until 29 December (Emma Darwin’s diary (DAR 242)).

To W. H. Flower 17 December 1880

Down, Beckenham, Kent.

Dec^r 17th. 1880

My dear Flower

Please read over enclosed, about which I feel a very deep interest, & if you think fit sign it, appending "Pres. Zoologo. Soc."—¹ Please return it as soon as you can in enclosed envelope—I have asked Allman to forward it to you.² There will be, only about a dozen signatures & all of good men.—Hooker, Huxley & Lubbock have agreed to sign—³ I sincerely hope that you may approve—

Believe me | Yours very sincerely | Ch. Darwin

Copy

DAR 144: 130

¹ The enclosure was the memorial to obtain a government pension for Alfred Russel Wallace. It has not been found, but there is a draft of it in DAR 196: 3; for a transcription, see Appendix VI.

² George James Allman received this letter enclosed with the letter to him of 17 December 1880.

³ Joseph Dalton Hooker, Thomas Henry Huxley, and John Lubbock.

From G. J. Romanes 17 December 1880

18 Cornwall Terrace, Regent's Park, N.W.:

December 17, 1880.

My dear Mr. Darwin,—

Just a line to let you know that Professor Tyndall has kindly placed at my disposal the apparatus required to conduct the experiment with flashing light.¹

Frank's papers at the Linnean were, as you will probably have heard from other sources, a most brilliant success, as not only was the attendance enormously large and the interest great, but his exposition was a masterpiece of scientific reasoning, rendered with a choice and fluency of language that were really charming.² I knew, of course, that he is a very clever fellow, but I did not know that he could do that sort of thing so well.

I have now got a monkey. Sclater let me choose one from the Zoo, and it is a very intelligent, affectionate little animal.³ I wanted to keep it in the nursery for purposes of comparison, but the proposal met with so much opposition that I had to give way. I am afraid to suggest the idiot, lest I should be told to occupy the nursery myself.⁴

Very sincerely and most respectfully yours, | GEO. J. ROMANES.

E. D. Romanes 1896, pp. 104–5

¹ See letter from G. J. Romanes, 14 December 1880 and n. 1. John Tyndall was superintendent of the Royal Institution of Great Britain.

² Francis Darwin read two papers at the meeting of the Linnean Society on 16 December 1880 (see F. Darwin 1880a and 1880b).

- ³ CD had first suggested that Romanes try keeping a monkey to observe its mental capacity in a letter of 20 August 1878 (*Correspondence* vol. 26). Philip Lutley Sclater was secretary of the Zoological Society of London.
- ⁴ Romanes alludes to a comment in the letter to G. J. Romanes, 2 September [1878] (*Correspondence* vol. 26). Francis Darwin had jokingly remarked that Romanes should keep an idiot, a deaf-mute, a monkey, and a baby in his house. At this time the term 'idiot' was used in both law and medicine to refer to a person congenitally deficient in reasoning powers (*OED*). Romanes's daughter, Ethel Georgina, had been born on 19 February 1880 (E. D. Romanes 1918, p. 42). In the event, the animal, identified by Romanes as a brown capuchin monkey, *Cebus fatuellus* (a synonym of *Sapajus apella*, the tufted capuchin), was kept by Romanes's sister Charlotte Elizabeth Romanes (for her diary of observations on the monkey, see G. J. Romanes 1882, pp. 484–95).

To W. R. Browne 18 December 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
December 18th 1880

Sir

The state of my health will not allow me to attend the meeting at Lambeth Palace, though I should feel it an honour to meet there so many distinguished men.¹ It would, however, not be sincere on my part to assign want of strength as the sole reason for not attending, in as much as I can see no prospect of any benefit arising from the proposed conference.²

I beg leave to remain, Sir, | Your obedient servant | Charles Darwin

LS (photocopy)

British Library (Surrogate RP 7385)

¹ See letter from W. R. Browne, 16 December 1880. The aim of the meeting, which was to be held in January 1881, was to challenge the assumption that science and religion were irreconcilable.

² In a draft of this letter in DAR 202: 17, in place of the clause that begins 'in as much', CD had written 'for many [*interl*] parts of the Bible [*above del* 'Old Testament'] seem to me quite irreconcilable, with what *(according to my judgement)[*interl*] is presently [*interl*] known of the history of the organic & inorganic world'.

To James Torbitt 19 December 1880

Down,
Dec. 19, 1880.

My dear Sir

I am *extremely* sorry to trouble you again but I have not made my meaning clear to you.¹ I hold in my hands £90 (as by annexed slip of paper) subscribed in aid of your experiments. Now I do not understand whether you now require part or the whole of this sum. If you do not, I would propose to the subscribers that I should retain part or the whole, as they may decide, in aid of your experiments next year. As soon as I hear again from you, I will draw up a statement and send it to Mr. Farrer and Caird with respect to money advanced by me and my brother to you and of what

I still hold in hand.² I will then state your wishes for next year and just suggest to them to allow me to retain a part or all the money for next year. I will then also send copies (already made) of your letter and Report of Dec. 11th, calling their attention to your offer of sending them varieties and telling them no result from your letter to Mr. Forster.³ You speak in your letter of repaying me my advance of £150 and that by my brother of £25.⁴ Your conduct has always been most handsome and straight-forward, but unless you should make good profit from sale of the varieties I want no repayment, nor, as I believe, would my brother. With respect to the other subscribers I certainly should like to return a part of the £90 to the subscribers, or get their permission to retain it for next year.

A single line in answer will suffice, and I will then write at once to Mr. Farrer and Caird.

Yours sincerely | Ch. Darwin

Copy

DAR 148: 125

¹ CD's most recent extant letter to Torbitt was that of 13 December 1880, in which he asked about Torbitt's plan for the funds that had been subscribed for his experiments aimed at producing blight-resistant potatoes. Torbitt's reply of 15 December 1880 made no mention of the subscribed money. There is a missing letter from Torbitt, dated 17 December 1880 (see letter to T. H. Farrer, 30 December 1880).

² Thomas Henry Farrer and James Caird had helped raise funds for Torbitt. The slip of paper mentioned has not been found; for the subscribers, see the letter from T. H. Farrer, 8 March 1880.

³ The letter and report from Torbitt have not been found. William Edward Forster was chief secretary for Ireland. Torbitt had reported that Forster told him he could not help with the matter (see letter from James Torbitt, 15 December 1880).

⁴ See letter to James Torbitt, 4 March 1880, and letter from James Torbitt, 12 March 1880; Erasmus Alvey Darwin had sent a cheque for £25 to Torbitt. CD pledged £50 in addition to the £100 he sent in 1878 (see *Correspondence* vol. 26, letter from James Torbitt, 6 March 1878).

To Patrick Geddes 20 December 1880

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

Dec 20th 1880

Dear Sir

You are perfectly welcome to copy for the Encyclop. Brit. any of the figures in my *Insectivorous Plants*.—¹

Pray forgive the manner of directing the envelope, but no one in my house could read your signature.²

Dear Sir | Yours faithfully | Ch. Darwin

National Library of Scotland (MS.10521)

¹ Geddes's article 'Insectivorous plants' was published in *Encyclopaedia Britannica* 9th ed., 13: 134–40. Of the sixteen illustrations, eight are from *Insectivorous plants*.

² The letter from Geddes has not been found

From James Geikie 20 December 1880

Perth
Dec. 20. 1880

My dear Sir

I am much pleased to hear that “Prehistoric Europe” has interested you, and that you are of opinion that the establishment of “interglacial epochs” is of some importance.¹ Evidence on this head is continually increasing: only a few days ago I heard from D^r Penck of the Geol. Survey of Saxony that the well-known lignites of Imberg in Bavaria are interglacial.² The section he gives as follows:

		mètres
<i>Ground-moraine</i>	— —	20.
Conglomerate	— —	3.
Lignite with clay	— —	0.5
Conglomerate	— —	2.
Lignite	— —	1
Conglomerate	— —	10. to 15.
<i>Ground-moraine</i>	— —	10.

The section is exposed for some 400 mètres—so that there is no doubt about the matter.

M^r Mackintosh’s paper I read with much interest, but without being convinced that any of his erratics have been floated by sea-ice.³ The “intercrossings” of boulders upon which he rests his belief, are not unknown in Scotland in our till. Thus over a belt of country extending from Ayrshire in the west to the coast of Berwickshire in the east we find similar & even more remarkable intercrossings of boulders than any of those cited by M^r Mackintosh. Boulders from the Northern Highlands are here mixed with erratics which have come from the Southern Uplands. The belt of land is simply the debateable grounds which mark the meeting of the ice-flow from North & South respectively, and over which now the one and now the other ice-stream prevailed. Then we have evidence in Forfarshire & Aberdeenshire of a similar crossing of boulders upon the low maritime tracts—and these we explain by the pressure exerted by the Scandinavian ice-sheet which now & again [pushed/ back the Scotch ice.

Similar intercrossings of boulders are common in Scandinavia and in North Germany, and to some of these I refer in a Note in Appendix of Prehistoric Europe.⁴

The origin or cause of such intercrossings is clearly revealed in the glacial phenomena of the Rhone valley between Bourg & Lyons etc. In the beautiful map of M. M. Falsan & Chantre, we see that boulders have crossed each other at all angles.⁵ Nay, in some cases it is proved that the boulders have travelled *in exactly opposite directions!* Thus in the Valromey glacial striae & boulders indicate the passage of a glacier down the valley—while many erratics occur in the valley even up to its

head which indicate a flow of ice *up* the valley! Falsan & Chantre show that before the Great Rhone glacier had reached its greatest development numerous glaciers came down from the hills of Savoy & Dauphiny, but by & by the great ice-streams of the Rhone overwhelmed these local glaciers—grinding over their moraines & commingling them with its own. In many cases the flow of the Rhone glacier was at acute & right angles to the course of the local glaciers, & even in some cases flowed up the smaller valleys & passed across dividing cols. Of course when the Rhone glacier decayed in importance, the local glaciers again became independent, and a new series of “intercrossings” of moraine-material & erratics took place.

In some such way I believe the “intercrossings” of the boulders referred to by M^r. Mackintosh must be explained. The objections that might be urged to the “ice-berg” origin of the English erratics are many and strong, but I hesitate to bore you with them; and they are probably already present in your mind. During the growth or increase and subsequent gradual disappearance of the great mer de glace that filled up the basin of the Irish Sea, there must have been many modifications in the ice-flow of the contiguous tracts in north-west of England—quite enough I can conceive to give rise to the crossing of boulders again & again.

I may mention that I spent some time this past Autumn in in studying the drifts of South Wales & the borders of the Bristol Channel. I could find no trace of marine action anywhere, altho’ I was constantly on the outlook for it among the drifts. I traced boulder-clay from the hills right down to the margin of the recent marine flats. In the hill-valleys the boulder-clay is covered with much angular débris, some of which appeared to be truly morainic, while much seemed to me to point rather to the action of névé, frost & thaw etc.—in fact to be of the same nature as the “Head” of Cornwall. In the low maritime tracts the till is overlaid with morainic or “diluvial” gravel & sand, of the same character as the similar deposits in Scotland & Sweden etc.

Enclosed is a paper by M^r. Kerr which may interest you, if you have not already seen it.⁶ Pray do not trouble to return it, as I have another copy.

I hope you will excuse this long & I fear somewhat incoherent letter—and with highest regards, believe me | Sincerely yours | James Geikie

DAR 165: 32

¹ See letter to James Geikie, 13 December 1880 and nn. 1 and 2. CD had just read *Prehistoric Europe: a geological sketch* (Geikie 1881) and was particularly interested in the evidence Geikie provided for the existence of intervening warmer periods during the Ice Age, known as interglacial epochs.

² Albrecht Penck was an assistant geologist for the Land Survey of Saxony (*Landesaufnahme in Sachsen*). Amberg is in the Upper Palatinate, Bavaria.

³ Daniel Mackintosh suggested that the intercrossing of routes taken by boulders was a result of currents changing course with the rising and falling of the seabed (Mackintosh 1879, p. 427).

⁴ See Geikie 1881, pp. 565–6.

⁵ The map of the Bourg region was one of six maps made by Albert Falsan to accompany the monograph by him and Ernest Chantre, *Monographie géologique des anciens glaciers et du terrain erratique de la partie moyenne du bassin du Rhône* (Geological monograph on the ancient glaciers and erratic terrain of the middle part of the Rhone basin; Falsan and Chantre 1875–80, Atlas: map 1).

⁶ CD’s copy of Washington Caruthers Kerr’s ‘Gold gravels of North Carolina’ (Kerr 1880) is in the Darwin Pamphlet Collection–CUL.

To Otto Hahn 20 December 1880

Down | Beckenham Kent
December 20. 1880

Dear Sir

I am much obliged for the gift of your magnificently illustrated work and your very courteous letter.¹ If you succeed in convincing several judges as trustworthy as Professor Quenstedt,² you will certainly have made one of the most remarkable discoveries ever recorded.

I remain Dear Sir | your faithfull and obliged | Charles Darwin

Copy

DAR 251: 3334

¹ See letter from Otto Hahn, 16 December 1880 and n. 1. Hahn sent a copy of Hahn 1880.

² Friedrich August Quenstedt.

To J. D. Hooker 20 December 1880

Down, | Beckenham, Kent. | Railway Station | Orpington. S.E.R.
Dec. 20th

My dear Hooker

I send the certificate for your signature, & I hope & believe that your adopted child will not disgrace you.—¹ Please ask Dyer, whether he will be so kind as to back up Frank.—² The memorial for Wallace has just started on its rounds for signature, & has met with the most cordial reception in the two first cases.³ I have thought of keeping your name rather low down, so as to end the short list with a flourish.

Ever my dear Hooker | Yours affect | Ch. Darwin

Your old envelope will serve to return the Certificate

DAR 95: 507–8

¹ See letter to J. D. Hooker, 5 December 1880. Francis Darwin was being proposed for fellowship of the Royal Society of London; Hooker had agreed to propose him, but, in the event, Francis was proposed by Michael Foster in January 1881 and elected in June 1882 (Royal Society archives, GB 117 EC/1882/09).

² William Turner Thiselton-Dyer also signed the certificate for Francis.

³ CD had prepared a memorial to obtain a government pension for Alfred Russel Wallace (see letter to W. H. Flower, 17 December 1880 and n. 1).

To G. J. Romanes 20 December 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Decr 20 80

My dear Romanes

Your note has pleased me extremely & I thank you heartily, for Frank seemed to think that his papers were complete failures, as far as interesting or making anyone understand what they were about.—¹

But I write now to make a few trifling suggestions about your experiments.— I w^d advise Canary grass & cabbage seedlings when only $\frac{1}{4}$ or $\frac{1}{2}$ inch high. Your pots must be filled with earth up to very top, otherwise the rim will shade the seedlings— They ought to be grown in **complete** darkness, & so kept until experimented on. This makes them more sensitive to light, & allows them to grow quite upright.²

I am so ignorant that I do not know what effect a rapidly intermittent light has on the nervous system of animals, but I can clearly see that any manner of comparing the sensitiveness in the two kingdoms cannot fail to be interesting.

Your last sentence amused me. M^{rs} Romanes is quite right not to allow the monkey to enter the nursery, for how dreadful it would be if the monkey received more attention than the baby!³ It seems to me very wise your observing a monkey closely.—

Yours very sincerely | Ch. Darwin

American Philosophical Society (Mss.B.D25.576)

¹ See letter from G. J. Romanes, 17 December 1880. Francis Darwin had read two papers at the meeting of the Linnean Society on 16 December 1880 (F. Darwin 1880a and 1880b).

² Canary grass is *Phalaris canariensis*; cabbage is *Brassica oleracea*. Romanes was planning to experiment on the sensitivity of plants to different types of exposure to light (see letter from G. J. Romanes, 14 December 1880 and n. 1). CD had performed some experiments of this nature for *Movement in plants* (see letter to G. J. Romanes, 13 December 1880 and n. 3).

³ See letter from G. J. Romanes, 17 December 1880 and n. 4. CD refers to Romanes's wife and child, Ethel and Ethel Georgina Romanes.

From W. R. Browne 21 December 1880

38 Belgrave Road | SW
Dec 21 80

Dear Sir,

I regret to learn that your health will in any case prevent your attending the proposed Conference at Lambeth: but to prevent your having any misconception as to its objects, I venture to enclose a proof of the Proceeding as at present sketched out.¹ I know the Abp² is anxious to obtain expressions of opinion, even from those who do not see the desirability of holding such a meeting; & that he w^d particularly value such an expression from you. I am sure therefore he w^d be obliged if, in returning the paper, you could kindly make any remarks either on particular points, or on the subject in general.

Yours truly | Walter R Browne

Chas Darwin Esq FRS

DAR 202: 18

¹ See letter to W. R. Browne, 18 December 1880 and n. 1. The enclosure has not been found.

² Abp': archbishop; the archbishop of Canterbury, Archibald Campbell Tait.

To Daniel Mackintosh 21 December 1880¹

Down, Beckenham, Kent,

[A letter on botanical matters ... writes to thank the recipient for his '... interesting letter.'² I write now to send you an article, I just received from J. [Geikie], which may interest you & may be returned at your leisure.³ Perhaps referring to a botanical experiment, he continues: 'You will see that it is important to know whether the laminae of slate has ever been bent up-hill.'⁴]

Incomplete⁵

Christie's, New York (dealers) (9 June 1999)

¹ The date is given in the sales catalogue. The correspondent has been identified by the relationship between this letter and the letter from Daniel Mackintosh, 24 December 1880.

² The letter from Mackintosh has not been found, but see the letter from Daniel Mackintosh, 21 November 1880. The subject matter was geological rather than botanical. In geology, laminae are layers of rock; in botany, they are leaf blades (see n. 4, below).

³ CD had mentioned Mackintosh's paper on drift-deposits (Mackintosh 1879) to James Geikie in a letter of 13 December 1880. Geikie was not convinced by Mackintosh's view that the intercrossing of erratic boulders was a result of changes in sea level; he sent CD a paper by Washington Carruthers Kerr in which Kerr proposed that the uplift of slaty rocks was caused by a phenomenon he called 'frost drift' (Kerr 1880; see letter from James Geikie, 20 December 1880). CD sent the paper to Mackintosh (letter from Daniel Mackintosh, 24 December 1880).

⁴ CD evidently thought that the bending of laminae, or thin layers of rock, resulted from the process of alternate freezing and thawing that Kerr described as 'frost drift' (see n. 3 above).

⁵ The original letter is complete and is described in the sale catalogue as being one page long.

To W. R. Browne 22 December 1880

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

Dec 22 1880

Dear Sir

I am much obliged for your very courteous note.¹

I regret that it w^d be impossible for me to explain the causes of my disbelief in any good being derived from the conference, without treating the subject at inordinate length. I will only add that in my opinion, a man who wishes to form a judgment on this subject, must weigh the evidence for himself; & he ought not to be influenced by being told that a considerable number of scientific men can reconcile the results of science with revealed or or natural religion, whilst others cannot do so.

I beg leave to remain | Dear Sir | Yours faithfully | Charles Darwin

LS (photocopy)

British Library (Surrogate RP 7385)

¹ See letter from W. R. Browne, 21 December 1880. Browne had invited CD to a conference at Lambeth Palace, London, about the compatibility of modern science with religious belief.

To Albert Günther 22 December [1880]¹

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

Dec. 22^d

My dear D^r Günther

I am extremely anxious that you should consider a Memorial which will be sent you in 2, 3, or 4 days.² It has just occurred to me that may not be at the Museum towards the close of this week, & as delay would be very injurious will you kindly send me on a Post Card your address for Friday & the few succeeding days.

I trust that you will forgive me for troubling you & remain, Yours sincerely |
Ch. Darwin

Shrewsbury School, Taylor Library

¹ The year is established by the relationship between this letter and the letter to Albert Günther, 27 December 1880.

² CD had prepared a memorial to obtain a government pension for Alfred Russel Wallace (see letter to W. H. Flower, 17 December 1880 and n. 1).

To J. L. Hawkins 23 December 1880

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

December 23^d 80

Dear Sir

I am much obliged for your very courteous note & kindness in sending me a correction.¹ I was not at all aware that cats avoided certain species of mice, though I knew that they would not eat shrews. Perhaps Col. Newman was mistaken.² Should I ever again correct the Origin, (which is not very likely) I would modify the passage.—³

I remain | Dear Sir | Yours faithfully & obliged | Charles Darwin

Private collection

¹ The letter from Hawkins has not been found. According to the letter from H. M. Wallis, 27 March 1881 (*Correspondence* vol. 29), Hawkins wrote about 'mice in their relation to cats & hawks'.

² CD had cited Henry Wenman Newman's information on cats catching mice (Newman 1851) in *Origin*, p. 74. The passage was not altered in subsequent editions.

³ The final printing of *Origin* made during CD's lifetime was the 1876 reprint of *Origin* 6th ed.

To D. F. Nevill 23 December 1880

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

December 23. 1880

Dear Lady Dorothy Nevill

I thank you for your kindness in writing to me.¹ I have heard of a good many cases of one breed being so prepotent over another, that a cross seems to produce hardly any effect.—²

I do not think that I shall be in London for some time, but whenever I am, I will have the pleasure of calling on your Ladyship & beg leave to remain | Yours very faithfully | Charles Darwin

Cleveland Health Sciences Library (Robert M. Stecher collection)

¹ Nevill's letter has not been found.

² CD discussed prepotency in *Variation* 2: 65–71.

From James Torbitt 23 December 1880

J. Torbitt, | *Wine Merchant*. | 58, North Street, | Belfast.
23 Decr 1880.

Charles Darwin Esqr. | Down.

My dear Sir

No amount of writing to you would be any trouble to me— In reply to your letter of 19th last if you plea(s)e I would prefer tha() you should hold the £90 till say 10th prox. when I will know how I stand—in any case the whole of the money subscribed is a first charge on the new varieties, which is only just.¹

Might I beg of you to read enclosed, and, if I have no reply, say should I ask Mr Gladstone's liberty to send it to the press, or should I address the papers direct.²

I think it should attract attention some where

I am my dear Sir | most respectfully | James Torbitt.

[Enclosure]

RIGHT HON. W. E. GLADSTONE,

FIRST LORD OF THE TREASURY, &C.

RIGHT HON. SIR,

In addition to my proposal to produce next spring 100,000 new varieties of the potato, I would most respectfully suggest that they should be distributed gratis to the people, through the Post Office; they would weigh only a few ounces each, and the distribution would cost nothing. Probably 20,000 or 30,000 of them would be disease-proof, and there would be no lack of applicants.

And further, I would most respectfully propose, that this process of production and distribution should be continued year by year until the whole kingdom should be flooded with disease-proof new varieties.

And the necessity for the production of multitudes of varieties, if the maximum power of the plant is to be made available, is two-fold—first, because it is many years before one variety attains to any considerable bulk, or becomes of any value; second, because the yield of tubers, of new-disease-proof varieties, is enormous during the first few years of life; but after a few years (more or less according to the constitution of the variety) it begins to fall off, and the varieties begin to become subject to the disease; and when this double defect reaches a certain point, those varieties ought to be discarded, and new lives substituted; consequently, new varieties should be continually coming forward in multitudes, to replace those wearing out.

I most respectfully beg leave to submit herewith "Knight's" opinion on this subject, and the results of his experiments in growing new varieties; and given the means, I am now prepared to supply to every man in the kingdom who may want it, with a new variety of the potato, which shall be so prolific, and so free from disease, that it shall give, after separating any small proportion of diseased tubers, which may be found, a larger yield of sound tubers than the old varieties give of sound and diseased tubers taken together.

I am, most respectfully, | Right Honourable Sir, | Your most obedient servant, | JAMES TORBITT.

58, NORTH STREET, BELFAST, | 23RD DEC., 1880.

DAR 52: E5; DAR 178: 171 ff. 1–2

- ¹ See letter to James Torbitt, 19 December 1880. The money had been subscribed to further Torbitt's work on developing a blight-resistant variety of potato.
- ² The letter to William Ewart Gladstone that Torbitt enclosed was mentioned in reports in the *Globe*, 17 January 1881, p. 2, the *South Wales Daily News*, 20 January 1881, p. 2, the *Herts & Cambs Reporter & Royston Crow*, 21 January 1881, p. 2, the *Southend Standard and Essex Weekly Advertiser*, 21 January 1881, p. 2, the *East Kent Gazette*, 22 January 1881, pp. 2–3, and several other newspapers.

From Daniel Mackintosh 24 December 1880

36 Whitford Road, | Tranmere, | Birkenhead
Dec. 24th 1880

Dear Sir,—

I have to thank you for lending me the American pamphlet which has caused me to add to what I have already written, the enclosed remarks of which I have retained a copy, & which you need not return.¹

Mr. Strahan of the Geol. Survey, has described in some memoir (as he has informed me) somewhat similar phenomena to those on Moel Tryfan which he observed in Cheshire in thin layers of Keuper marl, and which he feels sure must have been caused by floating ice.²

Yours truly, | D. Mackintosh.

[Enclosure]

Bent and shattered edges of Slaty Laminæ

The section in which these appearances occur bears no real resemblance to some which have been found in districts where traces of ice-action are absent, in England, the United States, &c. The finely laminated sand on Moel Tryfan shews no sign of having been disturbed by the percolation of rain-water or frost, while the numerous shell-fragments arranged, along with very small pebbles, in continuous layers, have probably been preserved (as Darbshire long ago suggested)³ by the clay above the sand (which last spring was about ten feet thick) preventing the downward passage of acidulated rain-water. The erratic stones on Moel Tryfan could only have been brought by ice which must have stranded on the mountain before it could have left

its erratic freight; and why have recourse to any other cause for the bending and shattering of the slates, seeing that ice not only capable of accomplishing the task, but likewise of producing the wonderful associated phenomena, must at one time have been present.

(The upper limit of the bent part of the slates inclines a very little, but not the line marking the commencement of the bending, somewhat like this—)



DAR 171: 12

- ¹ See letter to Daniel Mackintosh, 21 December 1880 and nn. 3 and 4. CD sent a paper by Washington Carruthers Kerr (Kerr 1880).
- ² Aubrey Strahan, an assistant geologist with the Geological Survey of England and Wales, described the deposits in 'On the lower Keuper sandstone of Cheshire', but did not mention floating ice; he argued that the regularity of the bedding suggested 'deposition in tranquil water' (Strahan 1881, p. 397).
- ³ Robert Dukinfield Darbishire, in his paper 'On the marine shells in stratified drift at high levels on Moel Tryfaen, Carnarvonshire' (Darbishire 1863, p. 178), had suggested that the sandy clay tended to preserve the fossil shells found below.

To J. M. Herbert 25 December [1880]¹

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

Dec. 25th

My dear Old Friend

I was glad to receive your card, but I wish it had been accompanied by a note, telling me a little about yourself—how your health & strength is, & how you support your solitary life.— I sh^d, also much like to hear anything about Whitley.²

Though we have no communication my memory often goes back to Cambridge days, & not long ago the scene of receiving the microscope with the anonymous note came most vividly before my mind.—³ My youngest son Horace now lives with his charming little wife in Cambridge; & when I walked this summer through the courts of St. Johns, I thought of Van John & old days.⁴ Oh dear, life was worth then living, not that I have anything to complain of. My seven children have never given us a moments uneasiness, except on the score of health,—three of them ailing though not seriously, having inherited my poor constitution.⁵ They are good dear affectionate children, & some of them will do good work. My health is better than it used to be, but I live in a perpetually half knocked-up condition. I go on working at Science & in fact I am turned into a sort of machine for observing facts & grinding out conclusions, & am never happy except when at work.—

But I have written too much about myself.— Do sometime let me hear something about yourself.

Farewell | my old friend | Yours ever sincerely | Ch. Darwin

My wife desires to be very kindly remembered to you.—

American Philosophical Society (Mss.B.D25.577)

- ¹ The year is established by the relationship between this letter and the letter from J. M. Herbert, 28 December 1880.
- ² The card has not been found; it was sent by Herbert's wife, Mary Charlotte Herbert (see letter from J. M. Herbert, 28 December 1880). CD and Charles Thomas Whitley were contemporaries at Shrewsbury School and Cambridge University.
- ³ Herbert had sent the microscope to CD with an anonymous note (see *Correspondence* vol. 1, letter from [J. M. Herbert], [early May 1831]). In a letter to Herbert of 21 November 1872, CD had written, 'Do you remember giving me anonymously a microscope? I can hardly call to mind any event in my life which surprised & gratified me more.'
- ⁴ Horace Darwin and his wife Ida lived in Cambridge, where Horace designed and made scientific instruments. St John's was Herbert's college when he was a student at Cambridge. 'Van John': university slang for vingt-et-un, a card game (Freeman 1978).
- ⁵ CD may refer to his sons William Erasmus Darwin, George Howard Darwin, and Horace Darwin (see *Correspondence* vol. 20, letter to W. D. Fox, 29 October [1872] and n. 6).

To James Torbitt 25 December 1880

Down,
Dec. 25, 1880.

My dear Sir

I will wait till I hear again from you before writing to the subscribers; but I hope that it will not be much after the 10th, as I think that I ought soon to report.¹

With respect to your letter to Mr. Gladstone I should think under the present state of affairs, it would be quite hopeless to attract his attention; but I can see no objection to your publishing the letter, that is publicly writing to him. It would aid in calling attention to your work.² I should fear that the P. Office would object to your plan of distribution, and say that it would give openings to fraud.³ But on all these points my opinion is worth no more than that of any man of common sense.

My dear Sir | Yours faithfully | Ch. Darwin

Copy
DAR 148: 126

- ¹ See letter from James Torbitt, 23 December 1880 and n. 1.
- ² Torbitt had enclosed a copy of a printed letter to William Ewart Gladstone and asked CD for his opinion on whether to make it public (see letter from James Torbitt, 23 December 1880 and n. 2).
- ³ In his letter to Gladstone, Torbitt proposed distributing new potato varieties through the Post Office (see letter from James Torbitt, 23 December 1880, enclosure).

From F. J. Cohn 26 December 1880

Breslau
26 Dec. 1880

My dearest Sir

I can not let finish the year before I did send you my kindest thanks for the admirable present you forwarded to the whole scientific world as well as to myself.¹ It is a fresh leaf to the wreath you have gained in the battle of science, evergreen as

all the other's which adorn your brow. Immediately after having received your book, I went about studying and repeating the principal experiments upon which you have founded your theory of circumnutation; of course I succeeded in ascertaining the curious curves the apex of a growing plant describes; as for your experiments about the sensitive qualities of the top of the radicle, most of your results were not foreign to me as I was engaged many years ago with studying the germination of seeds (my inaugural dissertation treats a "symbola ad seminis physiologiam, and the researches of Ciesielksy were made in my laboratory,);² several important facts you did discover, I am about of ascertaining by repetition of your experiments. I wish, I could personally discuss with you the many questions your book arouses, for to write about requires rather a book than a letter. In the whole I agree totally with you; I am quite sure that in plants exist tissues—which are only sensitive and transmit a stimulus to other tissues which are not sensitive but contract or swell by irritation. But I am not convinced that the theory of *alternative turgescence* which you adopt for the circumnutation and other movements, does touch the truth, as you and most German physiologists accept.³ If I am not mistaken the force of movements does not dwell upon the quantity of water but upon the quality of protoplasm; the latter, in plants not less than in animals, is the truly contractile substance; and all theories which propose an essential difference between the movements of plants and animal's cells, walk out of the right way.⁴ As far as my own observations go, which however, I confess, are not yet concluding, the movements of vegetable tissues (circumnutation, heliotropism etc) depend upon changes in the shape of the protoplasmic bodies of the cells (they become longer but thinner etc.) without change of volume (conservative of turgescence) or the changes of turgescence are only secondary ones. The changes of shape are conformable to some purpose or useful to the plant and therefore acquired by heredity:— But this is a theme for a long dissertation and so I refrain from; I may only add that my views repose mostly upon the study of unicellular plants where the biological facts are more palpable than in the higher and more complicated classes (cf. *Oscillaria*, a very fine specimen of circumnutation).⁵

I don't know if I should dare to express how much I admire in your last book as much as in your former, all the qualities of a great biologist and philosopher

May to you be reserved a long series of happy years for the benefit of human knowledge. So I send you my kindest congratulations for the coming new year | and many happy returns of this day, | Truly yours Ferdinand Cohn

DAR 161: 206

¹ Cohn's name is on CD's presentation list for *Movement in plants* (Appendix IV).

² Cohn's inaugural dissertation *Symbola ad seminis physiologiam* (Contribution on the physiology of seeds; Cohn 1847) was a study of germination in ripe and unripe seeds. Theophil Ciesielski, a student of Cohn's, had published on tip sensitivity in roots and radicles in Cohn's journal, *Beiträge zur Biologie der Pflanzen* (Ciesielski 1872).

³ In *Movement in plants*, p. 99, CD had stated, 'Circumnutation depends on one side of an organ growing quickest (probably preceded by increased turgescence), and then another side, generally almost the opposite one, growing quickest'.

- ⁴ CD had argued that curvature of cotyledons when exposed to light showed that light acted more as a stimulus, similar to the effect on the nervous system of animals, and not in a direct manner on the cell or cell-walls that contracted or expanded (*Movement in plants*, p. 461). He also discussed hygroscopic movements (*ibid.*, p. 489), but did not mention contractile properties of protoplasm.
- ⁵ CD had described the circummutation of an *Oscillaria* (at that time, classified as a thallogen or plant with no differentiation such as algae, but now considered to be a form of cyanobacteria; see *Movement in plants*, p. 259).

To Ernst Krause 26 December 1880

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Dec. 26th 1880

My dear Sir

Since I wrote to you my sons have been looking into M^r. Butler's 'Unconscious Memory'; & they inform me that he refers to your quotation from Buffon & another passage in 'Erasmus Darwin' (pp 147, 134 of the English Translation) as having apparently been taken from his 'Evol. Old & New' (pp 119 & 21).¹ My sons say, after examining the passages carefully, that your quotations & the remark as to Coleridge have the appearance of having been taken from M^r. B.'s book.² I have not looked into the question myself, but I think that if you publish anything as to the matter in *Kosmos*, it would be well for you to give a clear explanation on this point.—

You will understand that I am not taking any public notice of M^r. Butler's attack on myself, & I would ask you to be kind enough to confine anything you may write on the matter as much as possible to the part you have yourself taken in the *Life of Erasmus D.*—³

Pray excuse me for troubling you, but I thought that it might be useful to you to hear the impression of my sons in relation to the above points.—

Believe me, my dear Sir | yours sincerely | Ch. Darwin

The Huntington Library (HM 36210)

- ¹ See letter to Ernst Krause, [12 December 1880]. Samuel Butler's *Unconscious memory* contained a chapter in which Butler accused CD and Krause of making unacknowledged reference to Butler's earlier work, *Evolution, old and new* (Butler 1880, pp. 58–79; Butler 1879).
- ² Both Krause and Butler refer to an article in the *Athenæum*, 27 March 1875, p. 423, as the source for remarks by Samuel Taylor Coleridge (see *Erasmus Darwin*, p. 134, and Butler 1879, p. 21). The quotation from Georges Louis Leclerc, comte de Buffon, on pigs is not given a source in *Erasmus Darwin*, p. 147, but is cited in Butler 1879, p. 120, as 'Tom. v. p. 104, 1755' (a reference to Buffon *et al.* 1749–1804, 5: 104).
- ³ Krause and CD co-authored *Erasmus Darwin*; Krause's section was a revised version of an earlier article (Krause 1879a).

To G. D. Campbell [before 27 December 1880]¹

[Down.]

My Lord Duke

I hope that your Grace will excuse the liberty which I take in asking you to read the enclosed copy of a memorial, in which I take the deepest interest.² I am aware

that you disagree with many of M^r Wallace's scientific views;³ but I believe that this disagreement, if it were to have any influence on your judgment, would lead you to look as favourably as possible on other points of his scientific career.— Supposing for a moment, that your Grace, would have signed the memorial as a simple scientific worker, would it be asking too great a favour to request you to inform M^r Gladstone to this effect; or as the memorial will soon be printed will you allow me to mention this fact in a private letter to M^r Gladstone, if he cannot spare time to receive a Deputation.)⁴

The memorial will be signed exclusively by men who for their special scientific work, or for their positions as Presidents of Scientific Societies, have some claim to be heard.—

If my request is unworthy one, I hope that your Grace will believe that this is due to this is entirely due to my ignorance of what is customary, as to Govt

I beg leave to remain my Lord Duke [with sincere regards] | yours faithfully |
Ch. D.

ADraftS

DAR 202: 23

¹ The date is established by the relationship between this letter and the letter to Albert Günther, 27 December 1880.

² The enclosure was the memorial to obtain a government pension for Alfred Russel Wallace. It has not been found, but there is a draft of it in DAR 196: 3; for a transcription, see Appendix VI.

³ Campbell was a proponent of design in nature; his book *The reign of law* (Campbell 1867) was critical of CD's theory. Wallace had written a critical review of Campbell's book (Wallace 1867).

⁴ As a member of the government (lord privy seal), Campbell could not sign the memorial, but he did write a letter to the prime minister, William Ewart Gladstone, expressing his approval (see letter to T. H. Huxley, 27 December 1880).

To George Bentham 27 December 1880

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

Dec 27 1880

My dear M^r Bentham

If you would not object to sign the enclosed Certificate for my son Francis, it would greatly please him & me.¹ Though he has done no systematic work, some of his Botanical papers appear to me to possess some value.—² If, however, you doubt whether my son ought to be proposed, I trust that you will merely return the certificate without your signature in the enclosed envelope.—

Believe me, my dear M^r Bentham | Yours sincerely | Charles Darwin

Royal Botanic Gardens, Kew (Bentham Correspondence, Vol. 3, Daintree–Dyer, 1830–1884, GEB/1/3: f. 723)

¹ Bentham's signature appears on Francis Darwin's certificate of proposal for fellowship of the Royal Society of London; Francis was elected on 8 June 1882 (Royal Society archives, GB 117 EC/1882/09).

² In addition to his work in physiological botany, Francis had also worked on animal morphology and physiology.

To Francis Darwin 27 December [1880]¹

[Down.]

My dear F.

I think that you have told everything important in your abstract in an excellent manner.² Every part is as clear as daylight.—

Pray give my kindest remembrances to Marshall, Marshall & Co.— No, this is too impudent, give my kindest remembrances to them as simple Christians³

Ever yours affect | C. D

Dec. 27th

DAR 211: 68

¹ The year is established by the reference to the Marshalls (see n. 3, below).

² Francis had recently presented two papers at the Linnean Society (F. Darwin 1880a and F. Darwin 1880b; he summarised these in *Nature*, 23 December 1880, pp. 178–81 (see letter from Francis Darwin, [before 15 December 1880]).

³ Francis and his son, Bernard Darwin, had gone to Coniston (Emma Darwin's diary (DAR 242)). CD had stayed at the Waterhead Hotel, Coniston, on Victor Marshall's Monk Coniston estate, in 1879. Marshall's wife was Victoria Alberta Alexandrina Marshall; his cousin William Cecil Marshall was a friend of CD's son Horace Darwin at Cambridge and had designed extensions to Down House (Freeman 1978).

To Albert Günther 27 December 1880

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

Dec. 27th 1880

My dear D^r Günther

Enclosed is the Memorial about which I take a very deep interest.¹

Will you kindly read & consider it, & if you approve, I hope that you will sign, appending your official title at the Museum.² We intend to get only a few signatures, viz of ⟨men⟩ who from their special studies, or as Pres. of Scientific Socs. have some claim to be heard.

Hooker, Lubbock & Huxley will sign.³ The D. of Argyll has written to Mr Gladstone that he highly approves of the Memorial.⁴ Now I want to ask a favour of you, unless for any reason you dislike granting it, namely, to send the memorial to Owen; you might say that you had been asked to lay it before him, as his signature w^d carry great weight. If he asks who originated the memorial you will of course have to ⟨tell⟩ him that it was I; & this w⟨ill⟩ I fear prejudice him against it. I could not myself send it as I have not spoken to him for 20 years.⁵ I enclose an envelope for the return of the memorial, as time is of consequence on account of the meeting of Pa(r)^h.

Pray forgive me for troubling you & believe me | My dear D^r Günther | Yours very faithfully | Charles Darwin

I thank you much for giving me your address so fully.⁶ There has been great delay owing to the disturbance of the Post.—

Lord Aberdare⁷ signed it on the 23^d & I have received it only this morning!

LS(A)

Shrewsbury School, Taylor Library

- ¹ The enclosure was the memorial to obtain a government pension for Alfred Russel Wallace. It has not been found, but there is a draft of it in DAR 196: 3; for a transcription, see Appendix VI.
- ² Günther was keeper of the zoological department at the British Museum.
- ³ Joseph Dalton Hooker, John Lubbock, and Thomas Henry Huxley.
- ⁴ The letter of George Douglas Campbell, eighth duke of Argyll, to William Ewart Gladstone has not been found, but see the letter to G. D. Campbell, [before 27 December 1880].
- ⁵ Richard Owen and CD had not been on speaking terms since shortly after the publication of *Origin* (see letter to T. H. Huxley, 9 December 1880 and n. 2).
- ⁶ See letter to Albert Günther, 22 December [1880]; Günther's reply has not been found.
- ⁷ Henry Austin Bruce, first Baron Aberdare, was president of the Royal Geographical Society.

To T. H. Huxley 27 December 1880

*Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)*Dec. 27th 1880

My dear Huxley.

Spottiswoode, Allman, Flower, L^d Aberdare & Ramsay have signed & all have expressed themselves strongly in favour of the Memorial.¹ There has been much delay in getting the signatures, but I hope there will be less delay in future, for I will send addressed envelopes with notes, one within another.— The D. of Argyll highly approves, & has written to M^r Gladstone.—²

Now I want your decision on a knotty point. I suppose a small Deputation w^d be the best plan, but under present circumstances I sh^d think Gladstone could not spare time, & we sh^d be shunted off to his secretary, or altogether refused.—³ It has therefore occurred to me that perhaps the best plan w^d be for me to write a short note to M^r G. with the Memorial, saying why we did not ask him to receive a Deputation. I could somehow show how anxious I was on subject, & call his attention to the fact that all the men who signed were from their studies or their positions qualified to judge.— I could offer to answer any questions with respect to Wall^d But then w^d it suffice for me to say that I had been asked to send the Memorial to him, if the asking is only by you & myself: ought you or I to consult at least some one else? Sh^d I write to Spottiswoode, or w^d this be troubling him for nothing? If on the other hand you think we had better ask M^r G. to receive a Deputation, of whom sh^d it consist? Would you attend? I w^d gladly come up, & I c^d ask Lubbock,⁵ but I do not in the least know whether he w^d agree.—

Please let me hear what you decide.—

Ever yours truly | Ch. Darwin

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 354)

- ¹ William Spottiswoode, George James Allman, William Henry Flower, Henry Austin Bruce, first Baron Aberdare, and Andrew Crombie Ramsay had signed the memorial for a government pension for Alfred Russel Wallace.
- ² The letter of George Douglas Campbell, eighth duke of Argyll, to William Ewart Gladstone has not been found, but see the letter to G. D. Campbell, [before 27 December 1880].

³ CD probably alludes to the political ramifications of the recent potato famine in Ireland (1879–80) and the Land League agitation (*ODNB* s.v. Gladstone, William Ewart).

⁴ ‘Wall’: Wallace.

⁵ John Lubbock was the Liberal MP for London University.

From Henry Johnson 27 December [1880]¹

Burnham | Somerset
Dec 27.

My dear Darwin

The Shrewsbury Newspaper of 2 or 3 days ago contained a Paragraph that distressed me very much. It said that *you* were very ill & I cannot help writing a line to ask you to be so kind as to let me know, some thing about yourself.²

I am here in such an out of the way place that I cannot learn anything about you except from your self or some of your family.

I trust in Goodness you will be able to send me more comforting news about yourself. I fear & have long feared that you were not careful enough of yourself. If one of your Family would write me a few lines I should be for ever obliged & thankful.

Believe me | dear Darwin your affectionate old Friend | Henry Johnson.

Mary³ is with me & quite participates in my anxiety for news.

DAR 168: 71

¹ The year is established by the reference to a newspaper report (see n. 2, below).

² The report in the Shrewsbury paper has not been identified, but short reports appeared in several newspapers stating that CD was confined to his bed but able to read and converse (see, for example, the *Sheffield and Rotherham Independent*, 23 December 1880, p. 5).

³ Mary Elisabeth Johnson was Johnson's daughter.

To Francis Darwin [after 27 December 1880]¹

[Down.]

My dear F.

Your water-proof coat was left behind, & then your leggings were found, so we have sent off 2 parcels by Rail to you.—²

Enclosed 2 letters.—³ The D. of Argyll, hurrah, has written most civilly to say that he has written to M^r Gladstone to say that he highly approves of pension for Wallace.—⁴ The Duke wants to come to Down—the Lord have mercy on me—but I shall write & offer to call on him when next in London.—⁵ There is Bot Zeitung with article by Stahl on relation of forms of cells to intensity of Light—⁶ I will not send it unless so new—

Yours affect.— Kiss Dubbah⁷ for me | C. D.—

DAR 211: 69

- ¹ The date is established by the relationship between this letter and the letter to Francis Darwin, 27 December [1880].
- ² Francis was staying in Coniston (see letter to Francis Darwin, 27 December [1880] and n. 3).
- ³ The enclosures have not been found.
- ⁴ The letter from George Douglas Campbell, the duke of Argyll, has not been found. CD was circulating a memorial to obtain a government pension for Alfred Russel Wallace. As a member of the government (lord privy seal), Campbell could not sign the memorial, so he wrote a letter to the prime minister, William Ewart Gladstone, expressing his approval.
- ⁵ No letter has been found concerning the visit, but CD visited Campbell in London on 28 February 1881 (see *Correspondence* vol. 29, letter to G. H. Darwin, 27[–8] February [1881]).
- ⁶ The article by Ernst Stahl in *Botanische Zeitung*, 17 December 1880, was ‘Ueber den Einfluss der Lichtintensität auf Structur und Anordnung des Assimilationsparenchym’s (On the influence of light intensity on the structure and sequence of assimilation of the parenchyma; Stahl 1880b). George John Romanes was planning to experiment on the effect of brief flashes of light on plant tissues (see letter from G. J. Romanes, 10 December 1880).
- ⁷ Bernard Darwin.

From George Bentham 28 December 1880

25, Wilton Place. | S.W.
Dec^r 28/80

My dear M^r Darwin

I have the greatest pleasure in signing the enclosed certificate though I have strongly resisted the depreciation of systematic Botany now prevalent in Germany I have never underrated the paramount importance of physiological studies especially when carried out as M^r Francis Darwin has been doing¹

Ever yours sincerely | George Bentham

DAR 160: 172

- ¹ See letter to George Bentham, 27 December 1880 and n. 1. For a German perspective on the development of systematic botany in Germany around this time, see Strasburger 1895, pp. 249–57.

From Albert Günther [28 December 1880]¹

British Museum
Tuesday Morning

Dear Mr. Darwin

I have only a minute to spare after returning the memorial which I sign with the greatest pleasure.² Owen has not been seen or heard of for some days; & I do not know whether he is at home. Unfortunately, my relations to him are not of so intimate a nature, as to allow me to make enquiries as to his present whereabouts; if he had been here, I should have asked him without hesitation; but I do not care to intrude into his house.³ A letter would not do, as he may be from home.

In great haste | Yours very truly | A Günther

DAR 165: 257

¹ The date is established by the relationship between this letter and the letter to Albert Günther, 27 December 1880. The Tuesday following 27 December 1880 was 28 December.

² See letter to Albert Günther, 27 December 1880 and n. 1; the memorial was to obtain a government pension for Alfred Russel Wallace.

³ See letter to Albert Günther, 27 December 1880 and n. 5. Richard Owen was superintendent of the natural history departments at the British Museum.

From J. M. Herbert 28 December 1880

Rocklands | Ross. | Kerne Bridge Station | Telegrams to Ross.
28 Dec^r. 1880

My dear Darwin,

Your very kind & affectionate letter has much cheered me—tho' it was extracted from you by my wife sending you the Xmas Card, without my authority.¹ A young friend of hers has produced a great many sketches of Pigs in various human attitudes, & playing childrens' games, of which I send M^{rs} Darwin 3 Specimens—and having exhausted her porcine vein, she sent some imaginary specimens of Antediluvians, which my wife thought might win a smile from you, even if you did not recognize any conformity to the prehistoric types—and I believe it was done in the hope of receiving your autograph to be inserted in her Book of Autographs of British & European Celebrities.² It appears from your letter that you have not heard of my 2^d Marriage, tho' it took place 3 $\frac{1}{4}$ years ago. It has been to me one continuous source of comfort & happiness— She was Mary Phillpotts, a great niece of the Bishop of Exeter—³

I have not heard from Whitley but once since his Wife's death, which happened several months ago to the great destruction of his happiness; for never were man & Wife better suited for each other, or more devoted to each other, than they were—⁴ He has generally very fair health—but a delicate throat often obliges him to winter at Bournemouth, or some sheltered place on the South Coast.

I don't think I have ever met but one of your Sons, & him I casually met on the Railway travelling with my friend Archdeacon Crawley; & I challenged him to be your Son from his likeness to you in voice & features.⁵ When will you be content with the fame, which the amazing amount of your good work has secured for you? It seems to me that in the estimation of Europe, you already stand on a higher pedestal than Cuvier⁶—And I feel it to be one of the real joys of my life to be acknowledged by you as a friend—

With kindest remembrances to M^{rs} Darwin, & every kind wish for yourself & all your family; I am always— | Very truly yrs | J. M. Herbert

C. Darwin Esq

DAR 166: 187

¹ See letter to J. M. Herbert, 25 December [1880]. Herbert's wife was Mary Charlotte Herbert.

² The friend has not been identified and the sketches have not been found.

- ³ Herbert and his second wife were married on 4 September 1877 (*England, select marriages, 1538–1973* (Ancestry.com, accessed 27 June 2019)). The bishop of Exeter was Henry Phillpotts.
- ⁴ Charles Thomas Whitley, Herbert, and CD were contemporaries at Cambridge. Whitley's wife, Frances Whitley, had died on 15 December 1879.
- ⁵ William Crawley was archdeacon of Monmouth; his son, Charles Crawley, was a friend of Francis Darwin, so Francis was probably the son the Herbert had met.
- ⁶ Georges Cuvier.

From T. H. Huxley 28 December 1880

4 Marlborough Place, | Abbey Road, N. W.
Dec 28th | 1880

My dear Darwin

I do not see the least good in a deputation and as you suggest, I doubt if M^r Gladstone would receive one at present¹

It appears to me that far the best course would be for you to send the Memorial yourself to M^r Gladstone accompanied by a private note—² You have drawn up the Memorial & the people who have signed it have done so knowing it was to be sent to the Prime Minister & then returned it to you—

You are therefore justified in seeing that it reaches its destination in any way you think best and I have no doubt whatever that sending it yourself is the best way— M^r Gladstone can do a thing very gracefully when he is so minded and unless I greatly mistake he will be so minded if you write to him—

We have had all the chicks (& the husbands of such as are therewith provided) round the Christmas table once more and a pleasant sight they were though I say it that should'n't— Only the granddaughter left out the young woman not yet having reached the age when change & society are valuable³

I don't know what you think about anniversaries— I like them being always minded to drink my cup of life to the bottom & take my chance of the sweets & bitters

With the warmest good wishes for the New Year from all of us to all of you | Ever Yours faithfully | T H Huxley

P.S. My wife⁴ is a little better. It is therefore with the utmost difficulty that I can restrain her from waiting upon everybody—

DAR 166: 356

¹ See letter to T. H. Huxley, 27 December 1880. The memorial for a government pension for Alfred Russel Wallace had been organised by CD; he had asked Huxley about the best way to present it to the prime minister, William Ewart Gladstone.

² See *Correspondence* vol. 29, letter to W. E. Gladstone, [4 January 1881].

³ Huxley had two married daughters: Jessie Oriana, whose husband was Frederick William Waller, and Marian, whose husband was John Collier. His other children were Leonard Huxley, Rachel Huxley, Nettie Huxley, Henry Huxley, and Ethel Gladys Huxley. Oriana Huxley Waller had been born on 11 February 1880 (*BMD (Death index)* s.v. Haynes, Oriana Huxley).

⁴ Henrietta Anne Huxley.

To Henry Johnson 28 December [1880]¹

Down, | Beckenham, Kent. | (Railway Station | Orpington. S.E.R.)
Decr 28th

My dear Johnson

I am as well as ever I am & working away on worms.²

Heaven knows how the report arose; but this morning my son had a letter from Lady Thompson in Scotland, asking about me & saying she had read in a Scotch newspaper that I was dangerously ill!³ The false report has done me one good turn in bringing me so *extremely* kind a letter as that of yours.—

My dear Johnson | Yours very sincerely | Ch. Darwin

Private collection

¹ The year is established by the relationship between this letter and the letter from Henry Johnson, 27 December [1880].

² CD had begun writing on the action of worms in the autumn of 1880 (CD's 'Journal' (Appendix II)).

³ Frances Anna Thomson was a friend of George Howard Darwin. A short report appeared in the *Aberdeen Weekly Journal*, 25 December 1880, p. 5, stating that CD was confined to his bed but able to read and converse. The same report had appeared in other newspapers, including the *Sheffield and Rotherham Independent*, 23 December 1880, p. 5.

To H. W. Bates [before 29 December 1880]¹

[Down.]

Private

Huxley thinks that the Presidents had better sign in a body, so that the Memorial shall be sent to you **afterwards** for your signature with an envelope addressed to Sir J. Hooker, if you will forward it without delay.—²

C. D

As I do not know L^d Aberdare's address I have not enclosed an envelope: could you write outside the envelope "To be forwarded if not at home"; for it is very important that the Memorial sh^d reach M^r Gladstone before Parliament meets—³ I have got the Duke of Argyll to write a private letter to M^r G. in favour of the pension.—⁴

Cleveland Health Sciences Library (Robert M. Stecher collection)

¹ The date is established by the relationship between this letter and the letter to H. W. Bates, [29 December 1880].

² Thomas Henry Huxley was advising CD on the memorial for a government pension for Alfred Russel Wallace. CD had explained that he wanted Joseph Dalton Hooker to sign near the end of the memorial 'so as to end the short list with a flourish' (letter to J. D. Hooker, 20 December 1880).

³ Henry Austin Bruce, first Baron Aberdare, was president of the Royal Geographical Society; Bates was assistant secretary. William Ewart Gladstone was the prime minister.

⁴ See letter to G. D. Campbell, [before 27 December 1880]. George Douglas Campbell, eighth duke of Argyll was a member of the government (lord privy seal), so could not sign the memorial.

To H. W. Bates [29 December 1880]¹

Down Beckenham Kent

My dear Bates.

After signing the Memorial, please put it in enclosed envelope & post it without loss of time to Sir J. Hooker.—²

Yours very sincerely | Ch. Darwin

P.S | I am inclined to think that you had better annex your official Title to your signature, but please do as you think best.—³

Cleveland Health Sciences Library (Robert M. Stecher collection)

¹ The date is established by the relationship between this letter and the letter to P. L. Slater, 29 December 1880. This letter would have been enclosed with the letter to Slater.

² See letter to H. W. Bates, [before 29 December 1880] and n. 2. The memorial for a government pension for Alfred Russel Wallace was being circulated by having signatories forward the memorial to the next person for whom an envelope had been provided, thus reducing the time needed to get all the signatures. CD had wanted Joseph Dalton Hooker to sign near the end of the memorial.

³ Bates was assistant secretary of the Royal Geographical Society.

To J. S. Burdon Sanderson 29 December 1880

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

Dec 29th 1880

My dear D^r Burdon Sanderson.

If you would not object to sign the enclosed certificate for my son Francis, it would greatly please him & me.— But you must not think of doing so if you doubt about the propriety of his candidateship.¹

I enclose an addressed envelope. & remain | Yours very sincerely | Ch. Darwin

Please enclose a slip of paper with the date of your Lecture at R. Institution.—²

University of British Columbia Library, Rare Books and Special Collections (Darwin - Burdon Sanderson letters RBSC-ARC-1731-1-24)

¹ Burdon Sanderson's signature appears on Francis Darwin's certificate of proposal for fellowship of the Royal Society of London; Francis was elected on 8 June 1882 (Royal Society archives, GB 117 EC/1882/09).

² Burdon Sanderson read his lecture on 'Excitability in plants and animals' at the Royal Institution of Great Britain on 25 February 1881 (*Nature*, 9 December 1880, p. 130).

To J. D. Hooker [29 December 1880]¹

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

My dear Hooker

Will you please read the enclosed Memorial & as I hope sign it, & then put it in enclosed envelope for Huxley.—² No time sh^d be lost on account of the meeting of Parliament.—³

Ever yours | Ch. Darwin

Jeffrey Winograd (private collection)

- ¹ The date is established by the relationship between this letter and the letter to P. L. Sclater, 29 December 1880. This letter would have been enclosed in the letter to H. W. Bates, [29 December 1880], itself enclosed in the letter to Sclater.
- ² The memorial was to obtain a government pension for Alfred Russel Wallace. It has not been found, but there is a draft of it in DAR 196: 3; for a transcription, see Appendix VI. Thomas Henry Huxley would be the last to sign before CD submitted it to the prime minister, William Ewart Gladstone.
- ³ Parliament would convene on 6 January 1881 (*Journals of the House of Commons. Session 1881*).

To T. H. Huxley [29 December 1880]¹

Down Beckenham Kent

My dear Huxley

I have asked Hooker to forward the memorial, & after signing it, please place it in the enclosed envelope. for me.²

N.B. Letters placed in pillar post even so late at night reach Down at noon next day.— I read a few days ago in the Times a splendid Lecture by you before the Zoolog. Soc on Evolution.—³

How I do hope that all our trouble about the memorial may be successful.—

Ever Yours | Ch. Darwin

Imperial College of Science, Technology and Medicine Archives (Huxley 5: 363)

- ¹ The date is established by the relationship between this letter and the letter to P. L. Sclater, 29 December 1880.
- ² Joseph Dalton Hooker was the most recent person to receive the memorial for a government pension for Alfred Russel Wallace (see letter to P. L. Sclater, 29 December 1880).
- ³ The article, headed 'Professor Huxley on evolution', was in *The Times*, 25 December 1880, p. 4. Huxley's lecture was delivered on 14 December 1880 at the Zoological Society of London (*Nature*, 23 December 1880, p. 187).

To P. L. Sclater 29 December 1880

Down, | Beckenham, Kent. | (*Railway Station | Orpington. S.E.R.*)
December 29th 1880

My dear M^r Sclater

I earnestly hope that the enclosed Memorial will meet with your approval, & if it does, that you will sign it. There will be only a few other signatures, M^r Bates (as his co-traveller in Amazonia) Hooker, Huxley & self.¹ The Duke of Argyll has written to M^r Gladstone about this—² As you will see most of the men have appended their offices, & this looks more *imposing*(!) & therefore, I think, you had better add yours, but of this you will be a better judge than I can be.—³ No time sh^d be lost, so please put the Memorial in enclosed envelope & post it at once; or if you are in Hanover Sq^c, could you send it by messenger?⁴

I feel a very deep interest in the success of the Memorial.

Believe me, my dear M^r Sclater | Yours very sincerely | Charles Darwin

Wellcome Library (MS.7781/18)

- ¹ The memorial was for a government pension for Alfred Russel Wallace. It has not been found, but there is a draft of it in DAR 196: 3; for a transcription, see Appendix VI. Henry Walter Bates had travelled to the Amazon with Wallace in 1848; Wallace returned to England in 1852 while Bates remained until 1859. Joseph Dalton Hooker and Thomas Henry Huxley were to be the last to sign.
- ² See letter to G. D. Campbell, [before 27 December 1880]. George Douglas Campbell, eighth duke of Argyll was a member of the government (lord privy seal), so could not sign the memorial, which was to be presented to the prime minister, William Ewart Gladstone.
- ³ CD had explained that the plan was to have only a few signatures of eminent men such as presidents of scientific societies (see letter to Albert Günther, 27 December 1880).
- ⁴ The Zoological Society of London, of which Sclater was president, had its headquarters in Hanover Square.

To *Nature* [before 30 December 1880]¹

[Down.]

Black Sheep

The following extract of a letter from Mr. Sanderson of Chislehurst, who permits me to publish it, seems worth placing on record.² It relates to the former frequent appearance of spotted or black sheep in the Australian flocks, as long as animals thus coloured were of use to man, although they were never, as far as Mr. Sanderson knows, separately bred from, and certainly not in his own case. On the other hand, as soon as coloured sheep ceased to be of use they were no longer allowed to grow up, and their numbers rapidly decreased. I have elsewhere assigned reasons for the belief that the occasional appearance of dark-coloured or piebald sheep is due to reversion to the primeval colouring of the species.³ This tendency to reversion appears to be most difficult quite to eradicate, and quickly to gain in strength if there is no selection. Mr. Sanderson writes:—"In the early days before fences were erected and when shepherds had charge of very large flocks (occasionally 4000 or 5000) it was important to have a few sheep easily noticed amongst the rest; and hence the value of a certain number of black or partly black sheep, so that coloured lambs were then carefully preserved. It was easy to count ten or a dozen such sheep in a flock, and when one was missing it was pretty safe to conclude that that a good many had strayed with it, so that the shepherd really kept count of his flock by counting his speckled sheep. As fences were erected the flocks were made smaller, and the necessity for having these spotted sheep passed away. Their wool also being of small value the practice soon grew of killing them off as lambs, or so young that they had small chance of breeding, and it surprised me how at the end of my sheep-farming experience of about eight years the percentage of coloured lambs produced was so much smaller than at the beginning. As the quantity of coloured wool from Australia seems to have much diminished, the above experience would appear to be general."

CHARLES DARWIN

Nature, 30 December 1880, p. 193

¹ The date is established by the date of publication of this letter in *Nature*.

- ² John Sanderson was a wool merchant who had lived in Australia. The original letter (presumably to CD) has not been found.
- ³ CD had discussed reversion to dark or dark-spotted fleece in sheep in *Variation 2*: 30–1.

To T. H. Farrer 30 December 1880

Down. | Beckenham, Kent (&c)
Dec 30. 1880.

My dear Farrer.

I should be very much obliged if you would look over the enclosed papers.— These consist of a statement of the money transactions with M^r Torbitt, & a Report of this years success, which seems fairly favourable.¹

On receiving the report I wrote to M^r Torbitt asking whether he intended to draw all or part of the £90 which I held for him, as his answer (Dec 17) of which I send all the important parts copied, was not precise, I wrote again, & you will see by the copy of his answer. (Dec 28) that he desires me to return the £90 with sincere thanks to the subscribers—² This seems to me to be a pity, for you will observe in his letters that he speaks doubtfully about being able to continue without aid his experiments on a large scale this coming Spring; & on the principle of selection every successive generation increases greatly in value.— I do not intend myself to give more than the £150 already given; but if I may make a suggestion, I would ask to be permitted to keep half the £90 until the Spring, & allow M^r Torbitt to draw on it or not as he may find necessary; & if this amount is not drawn, then to return it to the subscribers—³

I think that M^r Torbitt has shewn that he may be trusted.— Now will you be so kind (to save me much copying) as to forward all these papers including this note, to M^r Caird & would you afterwards consult with him—⁴ Whatever you & he may decide shall be immediately followed.

Believe me, my dear Farrer. | Yours very sincerely. | Charles Darwin.

Copy
DAR 144: 102

- ¹ The enclosures have not been found. James Torbitt was working on developing blight-resistant potato varieties.
- ² See letter to James Torbitt, 13 December 1880; Torbitt's reply of 17 December 1880 has not been found, but in his letter of 23 December 1880, he asked CD to hold on to the £90 until 10 January 1881.
- ³ For CD's earlier contributions to Torbitt, see the letter to James Torbitt, 19 December 1880 and n. 4.
- ⁴ James Caird had also helped to raise funds for Torbitt. CD had sent him a statement of Torbitt's experiments earlier in the year (see letter to James Caird, 24 March 1880).

From J. S. Burdon Sanderson 31 December 1880

7 White Rock Place, Hastings.
Dec 31st. 1880.

Dear M^r Darwin,

Your letter, with the Certificate, has been forwarded to me here. I have great pleasure in signing it.¹

The date of my Friday Evening Lecture, about which you are so kind as to enquire, is February 25. I send a Ticket as a Memorandum of the Date.² I came here chiefly for the purpose of writing or at all events, preparing it. My leading position is that in the excitable parts of Plants, the mode by which the excited part influences other parts at a distance from it is (notwithstanding the absence of nerves) essentially the same as in the simpler excitable structures of animals. Prof Munk of Berlin in his long paper on *Dionæa*, as well as Sachs, denies the possibility of transmission or propagation of an excitatory effect, *except by migration of liquid*. I am going to make this point plain by a strict comparison of plant with animal phenomena.³

I have been reading more carefully the “Movements of Plants” I am specially interested in the 6th & 7th chapters.⁴

With best wishes for the New Year | very truly yours | JS Burdon Sanderson

PS. Mr. Busk has just been here. He w^d have signed the Certificate had he not been on the Council.⁵

University of British Columbia Library, Rare Books and Special Collections (Darwin - Burdon Sanderson letters RBSC-ARC-1731-1-42)

¹ See letter to J. S. Burdon Sanderson, 29 December 1880 and n. 1. CD had sent Francis Darwin's certificate of proposal for fellowship of the Royal Society of London.

² See letter to J. S. Burdon Sanderson, 29 December 1880 and n. 2. Burdon Sanderson's lecture was part of a series organised by the Royal Institution of Great Britain.

³ Hermann Munk, in his paper 'Die elektrischen und Bewegungs-Erscheinungen am Blatte der *Dionæa muscipula*' (The electrical and movement phenomena in leaves of *Dionæa muscipula*), had concluded that the electromotive effect on the leaf cell was a direct result of the movement of water out of the cell (Munk 1876, p. 203). Julius Sachs had briefly discussed the effect of an electric current on the movement of water in the cell-sap (Sachs 1875, pp. 688–9).

⁴ The chapters dealt with nyctitropic (sleep) movements in cotyledons and leaves (see *Movement in plants*, pp. 280–448).

⁵ George Busk was on the council of the Royal Society (*Proceedings of the Royal Society of London* 31 (1880–1): 101).

From W. E. Darwin 31 December [1880]¹

Basset, | Southampton.

Dec 31

My dear Father,

I send you the diagrams as to Teg Down, and there is another on the back of Rifle Valley.²

I cannot read your pencil notes on the back.³ It seems to me that all one can say in each case is that there is always more in the valleys, and that in case of slopes the thickness of mould diminishes with the angle of slope.

I suppose if the worms cast up earth in inverse ratio to the steepness of slope, and hardly worked at all where the mould was only just enough to support grass, you might have a constant thickness of mould in any place continuing the same for centuries.

I think the diagrams explain themselves— if you wanted any points cleared up I could easily go there. I am looking again into the St Catherine Hill notes & will write soon.⁴

I hope Mother is better and downstairs, please give her my love and say what a pleasant Christmas we had.⁵

Your affect son | W. E. D

DAR 162: 88

CD ANNOTATION

2.3 thickness] 'thickness' *added pencil*

¹ The year is established by the reference to the Christmas visit (see n. 5, below).

² Teg Down and the Rifle valley are two valleys near Winchester, Hampshire.

³ William had made diagrams and notes on the thickness of mould in Teg Down and the Rifle valley in 1872; CD added his own notes on the back and wrote several pages of additional notes. The notes, dated from 27 March to 6 April 1872, are in DAR 63: 112–15.

⁴ William's notes on St Catherine Hill (a chalk hill near Winchester with the remains of an Iron Age fort) have not been found, but see *Earthworms*, pp. 302–4.

⁵ William and Sara Darwin visited Down from 23 to 29 December 1880 (Emma Darwin's diary (DAR 242)).

From A. L. Kielland¹ 31 December 1880

Stavanger
den 31^{te} Dec. 1880.

Høistærede Mr. Darwin!

—naar jeg hengav mig til min Yndlingsforføielse i Fontainbleau: at fodre Karperne, betragtede jeg ofte de to kinesiske Løver, som staa der tæt ved Dammen.²

Og en Dag fik jeg den pludselige Inskydelse, som jeg nu vil fortælle Dem—halvt i Spøg, halvt i den Tanke, at jeg maaske er slumpet op i en interessant Ide.

Disse “Løver” ere nemlig i Virkeligheden meget ulige Løver og have igrunden ligesaa stor Lighed med Padder. Betragter man nu Løvepadden tilhøire, saa bliver man strax opmærksom paa to tykke Traade af Slim, som flyder ud af dens Munnviger, slynger sig om dens Legeme og forener sig i en rund Kugle—fuld af smaa Stjerner, som den holder i Forlabben.

Løvepadden tilvenstre har de samme udflydende Traade af Slim; men den holder ingen Kugle. Derimod har den en liten Unge ved Foden, og denne Unge er næsten en fuldstændig Løveunge, det paddeagtige er hos den næsten ganske udslettet.

Min Ide er nu, at i disse to Figurer er Evolutionstheorien—Kontinuiteten i Naturen—symboliseret; Løven—Dyrenes Konge—er sat i det noreste Slægtskab med den slimede, foragtede Padde.

Løvepadden til høire laver med sine Forben en Kugle af det Slim som flyder ud af dens Mund. Dette oppfatter jeg som et udtryk for aldgamle Forestillinger om dette Dyrs ejendommelige Forplantningsmaade. Og selve Kuglen med de smaa stjerne lignende Punkter er paafaldende lig de slimede Klumper med Padderogn = ^{asg}, som vi finder i Grøfter og Vandpytter om Vaaren.

Løvepadden tilvenstre viser Udviklingen [et] skridt længere fremme. Her er Slimkuglen forsvunden; men den lille Unge, som er fremkommen, staaer Løvetypen *mangfoldige* Trin nærmere end Moderdyret.



Chinese guardian lions, male (left) and female (right).
Cour de la Fontaine, Château de Fontainebleau, Fontainebleau, France.
(Inga Leksina / Alamy Stock Photo)

Saaledes—mener jeg—har den store Sammenhængstanke fundet sit halvt mystiske Udtryk i disse to Værker af ældgammel (?) kinesisk Kunst.³

Jeg er Romanforfatter og ikke Spor av Videnskabsmann. Derfor beder jeg Dem opfatte dette mere som et fantastisk Indfald end som en Iagttagelse, der gjør Fordring paa alvorlig Opmærksomher. Imidlertid - skulde der være noget af Interesse heri, saa vilde jeg selvfølgelig være meget glad ver at høre noget derom.

Tillad mig tilslut i den dybeste Ærbødighed at sende Dem min lille Tak for det store, nye Livssyn, som aabnede sig for mig, da jeg blev bekjendt med Deres Værker.

Deres meget ærbødige | Alexander L. Kielland

[Contemporary translation]

Very highly honoured Mr Darwin,

When for my amusement at Fontainebleau I took to feeding the Carps (Karper) I often looked at the two Chinese lions (Löver) which stand close by the tank. And one day the ?grotesque idea occurred to me which I will now tell you half—in joke (Spög) and half with the idea that I may perhaps have stumbled on an interesting Idea. These “lions” are in fact very unlike lions and have great likeness to toads (?Padder). If one examines the lion-toads closer one at once observes two thick threads of slime (?Slim) which flow out of the corners of their mouths (Mundviger) wind round their bodies and end in a round ball full of small stones which is held in the forepaws (Forlabben)

The lion-toad on the left has the same thread of slime flowing out, but it has no ball. On the other hand it has a small young one at its feet, & this young one is almost completely a young lion; in it the toad nature is almost completely excluded. My idea is then that these two figures are the continuity of the evolution theory in natural symbols. The king of the race of lions is ... (sat); the . . . race with the slimy despised toad The lion-toad on the right holds (?laver) with its foreleg a ball of that slime which flows out of its mouth This I take as an expression by primitive representation concerning the peculiar mode of propagation of this animal. And the ball itself with the small round stone-like lumps is strikingly like the slimy clots with toads eggs which we find in ditches & ponds in the Spring.

The lion-toad on the left shows the development at a much more advanced stage. Here the ball of slime has disappeared but the small young one which has been produced stands very far nearer the lion type than does the mother animal.

Thus as I think has the great idea of correlation (or unity) found its half mystic expression in these two works of antique (? sic in orig.) chinese art

I am a novelist & not in the least a man of science Therefore I beg you will consider this more as a fantastic idea than as an observation which demands careful (alvorlig) attention If however there sh^d be anything of interest herein, I should naturally be very glad to hear something on the subject.

In conclusion permit me in deepest veneration to send you my insignificant thanks for the great & new light which was opened to me when I made acquaintance with your works | A.L.Kielland

Löve = lion; compare with German Löwe } throughout, I do not know
 Padde = toad } these words | G.H.D

Macbeth | “Paddock calls”⁴ Paddock is a toad & “ock” is merely a diminutive as in bullock

DAR 201: 18, 18a

- ¹ For a translation of this letter, see Appendix I. The contemporary translation was made by George Howard Darwin (see n. 4, below).
- ² According to a contemporary guidebook, the large pond at Fontainebleau castle was said to contain carp that were 200 years old (*Handbook for visitors to Paris*, p. 118). The two Chinese guardian lions are situated at the entrance to the Chinese museum in the Cour de la Fontaine (Fontaine courtyard) adjacent to the pond. See plate on p. 517.
- ³ In Chinese tradition, the male lion was depicted with an embroidered sphere (representing supremacy over the world) and the female lion with a cub (representing nature).
- ⁴ ‘G.H.D.’: George Howard Darwin. *Padde*: an old German word for toad (the modern German is *Kröte*). ‘Paddock calls’: spoken by the second witch in *Macbeth* 1:1. The toad is the familiar of the witch.

