

ANNALS OF SCIENCE

A QUARTERLY REVIEW OF THE HISTORY OF
SCIENCE AND TECHNOLOGY SINCE THE RENAISSANCE

VOL. 14

JUNE, 1958

No. 2

(Published August, 1960)

FURTHER UNPUBLISHED LETTERS OF CHARLES DARWIN

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THE publication of the correspondence of a great man presents numerous attractions and many difficulties. There is a fascination in tracing out the significance of minute and often ephemeral details that are hidden in a great life, well known only in its major features. The question whether any particular manuscript is worthy of publication does not permit of easy solution, since it is impossible without hindsight to determine what details may ultimately be of significance in some as yet uncommenced research. A matter to which little attention is paid is the establishment of the exact date on which men met one another for the first time, and it is often of importance to know this. Any researcher trying to scour the literature to discover when this first happened to Darwin and Lyell will have difficulty because the biographers did not trouble to find out or print any document in which this fact can easily be established. The following selection therefore has no pretensions to importance as a major contribution to the life of Darwin, but it may fill in some details concerning his circle of correspondents and the work on which he was engaged at various times.

Most of the letters are autograph manuscripts preserved in the British Museum and British Museum (Natural History), printed by permission of the Trustees. Four of the letters, addressed to Alfred Newton, are preserved in the Balfour Library, Cambridge, where they were found by Miss June Scrivener, and are reproduced by kind permission of Professor Sir James Gray, C.B.E., F.R.S. It was intended that they should appear (with other letters to Newton) in the Darwin Centenary Number of *Notes and Records of the Royal Society*, but shortage of space has led to their inclusion in this selection. One letter to Hooker is printed by courtesy of Academician Eugen Nikoranovitch Pavlovsky and the Pushkin House, Leningrad.

Sir Gavin de Beer *on*CHARLES BABBAGE¹.[*Charles Darwin to Charles Babbage*]²36, Grt. Marlbro' St^t

Wednesday Morning [1837-38]

My dear Mr. Babbage

I am very much obliged to you for sending some cards for your parties, but I am afraid of accepting them, for I should meet some people there, to whom I have sworn by all the saints in Heaven, I never go out, & should, therefore, be ashamed to meet them.—

I assure you I feel very grateful for your kindness to me.

Believe me

Most sincerely Y^r

Chas. Darwin

[*Charles Darwin to Charles Babbage*]³36 Gt. Marlbro' St^t

Sunday Morning [1837-8]

My dear Mr. Babbage

Will you have the kindness to take charge of the accompanying small parcel for Professor Henslow?—You will have cold weather for your journey.

Yours truly obliged

Chas. Darwin.

[*Charles Darwin to Charles Babbage*]⁴36 Grt. Marlborough St^t

[1837-8]

My dear Sir

I received a letter last week from Mr. Lyell, in which he asks me to tell you that by some misunderstanding he did not know you wanted Mr. Whewell's History of the Induct : Sc : & that he consequently sent it up to Scotland. He begs me to send my copy on to you, in place of his.

Yours most truly

Cha. Darwin

[*Charles Darwin to Charles Babbage*]⁵

36 Gt. Marlbro'

[1837-8]

My dear Sir

I am very much obliged for your kind invitation for tomorrow evening, and whether for beauty or for shells, I should have had great pleasure in accepting if I had not happened to be engaged.

Yours truly obliged

Chas. Darwin

¹ Charles Babbage (1792-1871), F.R.S. (1816), mathematician.

² British Museum Add. MSS. 37191, f. 81. This letter is dated roughly by the address at which Darwin lived from 13 March 1837 till January 1839.

³ British Museum Add. MSS. 37190, f. 320.

⁴ British Museum Add. MSS. 37190, f. 322.

⁵ British Museum Add. MSS. 37190, f. 326.

[*Charles Darwin to Charles Babbage*]¹

12 Upper Gower St
Friday [1839]

My dear Mr. Babbage

I am very much obliged to you for your several cards which you have so kindly sent us, & which I should have had greater pleasure in accepting than any other parties, had not I found by dear-bought experience that my health will not at present stand going out in the evening.

I am going to take a liberty, which I trust you will excuse, it is to ask permission for my sister, who is staying with us to accompany my brother, whom I understand you have also kindly asked, to your party tomorrow evening: Mrs Darwin not being well enough to go out at present.

Believe me

My dear Sir

Yours very truly obliged

C. Darwin

[*Charles Darwin to Charles Babbage*]²

12 Upper Gower St
Saturday Evening [1839]

Dear Babbage

I am exceedingly sorry that I trouble you unnecessarily for an extension of your kind invitation, as I happen to be so unwell to day, that I am quite unable to come.—

Pray excuse me having troubled you & believe
me yours very truly

Chas. Darwin

[*Charles Darwin to Charles Babbage*]³

12 Upper Gower St
Thursday [1839]

My dear Mr. Babbage

You will have the kindness to excuse me once again asking your permission to bring with Mrs. Darwin and myself, to your party on Saturday Evening, a relation of ours, Miss Allen⁴ of Cresselly who is staying with us.—

Believe me

very truly yours

Chas. Darwin

¹ British Museum Add. MSS. 37191, f. 296 & 296 v. This and the following three letters are roughly dated 1839 because Darwin rarely attended parties after October of that year.

² British Museum Add. MSS. 37191, f. 297.

³ British Museum, Add. MSS. 37191, f. 298.

⁴ Presumably Frances Allen.

Sir Gavin de Beer *on*

[*Charles Darwin to Charles Babbage*]¹

12 Upper Gower St
19th— [1839]

My dear Sir

My sister is at present staying with us, will you be so kind as to allow me to bring her to your party on Saturday, that she may see the *World*.—

I am yours
most truly

Chas. Darwin

[*Charles Darwin to Charles Babbage*]²

12 Upper Gower St
May 26th [1840]

My dear Sir

I have called with this note to beg a favour of you. M. Sismondi³, the historian & Madam Sismondi⁴ (my wife's aunt) have just arrived in England and are coming together with Miss Allen⁵ to stay with us, would you therefore, permit us to bring them to the parties you have been kind enough to ask us to.

My sisters have left town, & therefore will be deprived of the pleasure of accepting your kind invitation to them.

With many thanks

Believe me
My dear Sir
Yours very truly
Charles Darwin

HEINRICH ANTON DE BARY⁶

[*Charles Darwin to Heinrich Anton de Bary*]⁷

Down, Beckenham, Kent.
Feb. 11 1879.

Dear Sir,

I have often received from various writers copies of the *Bot : Zeitung* ; but lately they have come so regularly that I think I must be indebted to you for them. I assure you that I fully appreciate your kindness & the honour which you have thus done me. But I write to beg you not to send them in future as I regularly take in this your invaluable publication ; & have procured the volumes from the commencement.

With the highest respect
I remain dear Sir
yours faithfully
[Signed] Charles Darwin

¹ British Museum Add. MSS. 37191, f. 229 & 299 v.

² British Museum Add. MSS. 37191, ff. 294–295.

³ Jean-Charles-Simon de Sismondi (1773–1842).

⁴ Jessie de Sismondi née Allen (1777–1842).

⁵ Presumably Frances Allen (1781–1875).

⁶ Heinrich Anton de Bary (1831–1888), Professor of Botany at the University of Strasbourg.

⁷ B.M. (N.H.) General Library, Autograph Collection.

[*Charles Darwin to Heinrich Anton de Bary*]¹

Down, Beckenham, Kent
August 5 1879

Dear Sir,

My son Francis has shown me the two wonderful specimens of *Utricularia*, which you were so kind as to give him ; & I hope that you will allow me to urge you to publish an account of your observations. This seems highly desirable because the case of *Utricularia* is widely different from that of *Drosera*. I could not detect any evidence of true digestion in *Utricularia*, though the state of the cells in contact with *decaying* animal matter plainly showed that there had been absorption.— The fact of young seedlings developing bladders is also very interesting. I hope that you will excuse the liberty which I have taken in writing to you ; & this note requires no sort of answer. With great respect & with my thanks for your kindness to my son, I remain

Dear sir

Yours faithfully

Charles Darwin.

HENRY EDWARDS².

[*Charles Darwin to Henry Edwards*]³

Down, Beckenham, Kent.
July 15 [1873].

Dear Sir,

I am much obliged to you for having sent me the account of the Mexican ant, about which I had read nothing before.

Your facts are most wonderful & interesting.

Dear Sir

Yours faithfully,

Ch. Darwin.

[addressed]
H. Edwards, Esq.
California Theatre
San Francisco
California,
U. States.

¹ B.M. (N.H.) General Library, Autograph Collection.

² Henry Edwards (1830–1891), actor and amateur entomologist, who had written to Darwin about the honey-making ant of Mexico (*Myrmecocystus mexicanus*).

³ B.M. (N.H.) General Library, Autograph Collection.

Sir Gavin de Beer *on*

JOHN FORDYCE¹.

[*Charles Darwin to John Fordyce*]²

Down, Beckenham, Kent.

[May 1879]

Dear Sir,

It seems to me absurd to doubt that a man can be an ardent Theist and an Evolutionist. You are right about Kingsley³. Asa Gray⁴, the eminent botanist, is another case in point. What my own views may be is a question of no consequence to any one but myself. But, as you ask, I may state that my judgment often fluctuates. Moreover, whether a man deserves to be called a Theist depends on the definition of the term, which is much too large a subject for a note. In my most extreme fluctuations I have never been an atheist in the sense of denying the existence of a God, I think that generally (and more and more as I grow older), but not always, that an Agnostic would be the more correct description of my state of mind.

Dear Sir,

yours faithfully,

Ch. Darwin.

WILLIAM EWART GLADSTONE⁵.

[*Charles Darwin to William Ewart Gladstone*]⁶

Down, Beckenham, Kent.

Oct. 2. 1877

My dear Sir,

As this note requires no answer, I hope that you will excuse my writing to you. I have just seen your article⁷ on Dr Magnus' view⁸; & as you are interested in the subject, you may like to hear that this view has been well criticised under a natural history point of view in a German journal; *Kosmos*⁹; & that Dr Magnus has answered the criticism

¹ John Fordyce, author of *Aspects of Scepticism*, London, 1883.

² *Aspects of Scepticism*, pp. 189-90. The year in which this letter was written is given in *Life and Letters of Charles Darwin*, vol. i, p. 304; the month is estimated from Fordyce's reply to this letter dated 8 May, 1879, MS. in B.M. (N.H.).

³ Charles Kingsley (1819-1875).

⁴ Asa Gray (1810-1888).

⁵ William Ewart Gladstone (1809-1898), statesman.

⁶ British Museum Add. MSS. 44455, ff. 120-121.

⁷ W. E. Gladstone had written *Studies on Homer and the Homeric Age* (Oxford 1858), in which he attempted to show that Homer referred only to red and gold colours.

⁸ Hugo Magnus, *Die geschichtliche Entwicklung des Farbensinns*, Leipzig 1877.

⁹ Ernst Krause: "Die Geschichtliche Entwicklung des Farbensinns," *Kosmos*, Leipzig, vol. I, 1877, p. 264.

in a succeeding number¹. In one of these numbers I² have given some facts tending to show that very young children have great difficulty in distinguishing colours; or as I suspect, of attaching the right names to them, but why this should be so I know not. If you would like to see these numbers, & would inform me by a post-card, I should have great pleasure in sending them; but if, as is probable, you have no spare time, I shall understand that this is the case by not hearing from you.

I beg leave to remain

with the greatest respect

Yours faithfully

[signed] Charles Darwin

[Charles Darwin to William Ewart Gladstone]³

Down, Beckenham, Kent.

Oct. 25 [1877]

My dear Sir

I send by this post the two numbers. At p. 264, you will find the criticism & at 423 Dr Magnus' answer, & at p. 367⁴ my few words on infants.

The subjects seems to me an extremely curious one, whatever the explanation may be. A missionary could say whether low savages have names for shades of colour. I sh^d expect that they have not, & this w^d be remarkable for the Indians of Chiloe & Tierra del Fuego have names for every slight promontory & hill,—even to a marvellous degree.— I beg leave to remain with

great respect

Yours faithfully

Ch. Darwin

Will you please direct your Secretary to return the numbers, when you have quite finished with them.

[Charles Darwin to William Ewart Gladstone]⁵

Coniston

Aug. 4th [1879]

Dear Mr. Gladstone

Although you are so kind as to tell me not to acknowledge the receipt of your Essay⁶, in which you show how wonderfully Homer distinguished different kinds of movement, yet I must beg permission to thank you for this honour, & I remain

Faithfully yours

Charles Darwin

¹ Hugo Magnus : " Zur Entwicklung des Farbensinns ", *Kosmos*, Leipzig, vol. 1, 1877, p. 423.

² Charles Darwin : " Biographische Skizze eines kleinen Kindes ", *Kosmos*, Leipzig, vol. 1, 1877, p. 367. This is a translation of Darwin's " Biographical sketch of an infant ", published in *Mind* for July 1877.

³ British Museum Add. MSS. 44455, f. 210.

⁴ 376 in MS.

⁵ British Museum Add. MSS. 44471, f. 5.

⁶ W. E. Gladstone : *Homeric Synchronism*, London 1876.

Sir Gavin de Beer *on*

[*Charles Darwin to William Ewart Gladstone*]¹

Down, Beckenham, Kent.

Jan. 7th 1881

My dear Sir

Your note just received has given me an extraordinary amount of pleasure, for I was much grieved that Mr. Wallace² should be so hard worked in his declining years & with failing health. Your giving me this good news in your own hand-writing has been one of the kindest actions which I have experienced in my life.

I remain with great respect

My dear Sir

Yours gratefully

Charles Darwin.

[*Charles Darwin to William Ewart Gladstone*]³

Down, Beckenham, Kent

May 2^d 1881

My dear Sir

I am much obliged for the honour⁴ which you have proposed to me, & this I should have gladly accepted, had my strength been sufficient for anything like regular attendance at the meetings of the Trustees. But as this is not the case, I think that it is right on my part to decline the honour.—

I beg leave to remain with the greatest respect

My dear Sir

Yours faithfully

Charles Darwin

GEORGE ROBERT GRAY⁵.

[*Charles Darwin to George Robert Gray*]⁶

12 Upper Gower St

Tuesday [1840]

My dear Sir,

Will you be so good as to give my servant the skins of the Merulidae, Anthus, as I want to look at some of them.—and likewise the stuffed specimen of Upucerthia as I forgot yesterday to measure its beak.—I will not forget to try to negotiate the specimen for you from the Soc. Zoy.

From what I see of the localities of the Anthuses I almost suspect they will all turn out one species.— I will look over the specimens with

¹ British Museum Add. MSS. 44468, f. 11.

² Alfred Russel Wallace had been granted a Civil List Pension as a result of Darwin's solicitation of the Prime Minister (J. Marchant, *Alfred Russel Wallace. Letters and Reminiscences*, London 1916, vol. 1, p. 314). In spite of this remark Wallace lived until 7 November 1913.

³ British Museum Add. MSS. 44469, f. 218.

⁴ The honour referred to is membership of the Board of Trustees of the British Museum.

⁵ George Robert Gray (1808–1872), F.R.S. (1866), Assistant in the Natural History Department of the British Museum.

⁶ British Museum, Egerton MS. 2348, ff. 235 and 236. The letter is dated from the subject of its contents which refer to the volume on Birds in the *Zoology of the Voyage of H.M.S. Beagle*, and which was published in 1841.

my notes & see whether I can get any aid from habits.—

With respect to the Falkland *Emberiza* I can give little assistance.

1919
1920
1922 female } “ shot in same large *scattered* flock in the *hills* ”

But 1920 & 1923 & 1879 “ I think more commonly occur on the *plains* ”
I put mark of ? to 1923 whether different species or not.—In another part of my catalogue I say I saw 1046 & 1047 together. My specimens were shot in March corresponding to September & this is all the very little inform: I can tell you —

Believe me

Most truly yours

Chas. Darwin

[Charles Darwin to George Robert Gray]¹

12 Upper Gower St

Friday [1840]

My dear Sir

I have been obliged to defer from the state of my health, until to day the pleasure of thanking you for your very obliging present of some volumes of the *Genera of Birds*². It is a work, which cannot fail, I should think of being very useful to ornithologists & indeed to describers in other branches of Natural History.

I trust now you have completed this work, you will oblige me by kindly finishing the remaining MS. for the *Birds of Beagles Voyage*.— I had hoped before this to have finished the part, but I have of late been so frequently unwell that all my plans have disarranged.

I shall esteem it a great favour should you be able to finish at once Gould's³ MS. & Believe me

My dear Sir

with many thanks

Yours very truly

Chas. Darwin.

[Charles Darwin to George Robert Gray]⁴

Sunday [1840]

My dear Sir

In the two last proofs you have called one ground-woodpecker *Chrysoptilus campestris* & the second you left “ *Colaptes Chilensis Vigors* ”.—I have altered this to “ *Chrysoptilus Chilensis G. R. Gray* ”— Is this right? You need not look at any other part of this Revise.— Will you [the]n leave these sheets, as you [retu]rn home.

Yours most truly,

C. Darwin.

¹ British Museum, Egerton MS. 2348, ff. 237 and 238.

² Gray's work *A List of the Genera of Birds* was published in 1840.

³ John Gould was the author of the descriptions in Part III (*Birds*) of the *Zoology of the Voyage of H.M.S. Beagle*.

⁴ British Museum, Egerton MS. 2348, f. 239.

Sir Gavin de Beer *on*[*Charles Darwin to George Robert Gray*]¹12 Upper Gower St
Saturday [1840]

My dear Sir

Will you oblige me by looking over the proof of the index, & just marking any error you may observe.— It is such an interminable job to compare every name with text & your memory possibly is far more accurate than mine.—

I will send proof of your Errata soon.— If any of the specimens of Birds are at hand the Bearer may as well bring some back.

Yours most sincerely,
Cha. Darwin

[*Charles Darwin to George Robert Gray*]²Down, Bromley, Kent
Feb. 17 [1860]

My dear Gray

Many thanks for Nathusius³ rec^d safely. When in London I will call on you & shall be much obliged if you will tell me your views on the specific differences of pigs. I do not know who has cut me up so severely in the Ath.:⁴ nor do I know whom you mean by the "great friend of John Murray".

From 2 or 3 little points I suspected the author to be your great man⁵ in the Museum—

My dear Gray
Yours sincerely,
Ch. Darwin

[*Charles Darwin to George Robert Gray*]⁶Caerdeen, Barmouth
July 12 1869

Dear Mr Gray

As you have always given me information & aided me in every way in the kindest manner at the British Museum, it would give me great pleasure to send you a Testimonial⁷ of your abilities as a naturalist.

¹ British Museum, Egerton MS. 2348, f. 240.

² British Museum, Egerton MS. 2348, f. 234.

³ Hermann Engelhard von Nathusius (1809–1879), author of *Die Racen des Schweines*, Berlin, 1860.

⁴ The review of the first edition of *The Origin of Species* published in the *Athenaeum* for 19 November, 1859.

⁵ Richard Owen (1804–1892), F.R.S. (1834), Superintendant of the Natural History Departments of the British Museum.

⁶ British Museum, Egerton MS. 2348, ff. 232 and 233.

⁷ This letter may be compared with that which Darwin wrote to Alfred Newton on 29 October 1865 declining to give him a Testimonial for his candidature to the Chair of Zoology at Cambridge. ("Some unpublished letters of Charles Darwin", edited by Sir Gavin de Beer, *Notes and Records of the Royal Society of London*, 1959, 14, letter 28, p. 45.)

But you are aware that I am not an ornithologist or entomologist ; & it is impossible for me to express any opinion regarding the merits of your works on these subjects ; but I may add that I have heard those who are capable of judging speak in high terms of your knowledge of the species of Birds, & especially of your knowledge of the literature of ornithology.—

Pray believe me

Dear Mr Gray

Yours very faithfully

Ch. Darwin.

JOHN EDWARD GRAY¹.

[Charles Darwin to John Edward Gray]²

[January 1851]

My dear Gray

I will be here [*recte* : there] tomorrow morning at 10 o'clock, & as I have very much to do will you kindly oblige me by having Spengler³ on Cirripedes (which you once showed me) ready for me. Also I want to look at the rest of the sessile Cirripedes in the Collection, for praise be to Heaven, I have finished Balanus.

Could Garth⁴ & I see Mr. Cocks' collection⁵

Forgive this trouble

Yours

[Ch. Darwin]

[Charles Darwin to John Edward Gray]⁶

Down, Farnborough, Kent

March 28th [1854]

Dear Gray

As you were so kind as to say that you would complete for me, as far as possible, the Zoology⁷ of the Erebus & Terror⁷, I send a list of what I have. I sh^d be very glad to have the missing Parts.

As the numbering of the Parts is not consecutive I thought I sh^d make my deficiencies plainest by giving the pages of the text of that which

¹ John Edward Gray (1800–1875), Assistant Keeper of Zoology at the British Museum, Keeper 1840–1847, F.R.S. (1832).

² B.M. (N.H.) Zoological Department, Letters, vol. ii, f. 57.

³ Lorenz Spengler (1720–1807) : *Auserlesne Schnecken, Muscheln und andre Schaalthiere*, Copenhagen, 1758.

⁴ Unidentified.

⁵ W. P. Cocks sent a small collection from Ireland to the British Museum on 1 February 1849. Among the items in the collection, No. 4 was *Balanus tintinnabulum*, "Dec. 1848 procured more than thirty dead valves and three living specimens on the beach Bar-point after a severe storm wind N.E. The latter I kept in my experimental bottles until the 27th of the same month. I then destroyed two for my cabinet, and the third I placed in a hole in a rock, Gwyllyn Base, half tide :—hope to have a young fry this year." This was without doubt what Darwin was curious to see. (Minute report, Zoological Department B.M. (N.H.) 1848–53, ff. 13–14.)

⁶ B.M. (N.H.) Zoological Department, Letters, vol. ii, f. 56.

⁷ Sir John Richardson & J. E. Gray, *The Zoology of the Voyage of H.M.S. Erebus and Terror, under the command of Captain Sir J. C. Ross . . . during . . . 1839–43*; edited by J. Richardson & J. E. Gray, London, 1844–75.

I have. Perhaps you will be so good as to have any Parts (together with Busk¹, whenever you can screw a copy out of the Trustees) put together for me, & I will call for them in about 3 weeks. Pray forgive this trouble & believe me, Dear Gray

Yours very sincerely

C. Darwin.

Has the arrangement of the Cirripedes made any progress? I hope that you will make my work² be of such value as it may possess, for keeping my names, though it may be only temporarily. I sh^d so much like to see all in a Cabinet. I am going soon to arrange Mr Cuming's³ for him, or rather to superintend their arrangement.

SYMINGTON GRIEVE⁴.

[*Charles Darwin to Symington Grieve*]⁵

Down,
March 22, 1882.

Dear Sir,

The subject of your essay would, I think, be well worth pursuing. I have long known that stones were transported by floating Fuci, but I cannot remember my authority. Perhaps cases are given by Lyell. It is now, however, quite new to me that stones are thus dragged along the bottom, leaving a trail behind them.

Ch. Darwin.

R. GRIFFIN & Co.⁶

[*Charles Darwin to R. Griffin & Co.*]⁷

Down, Bromley, Kent

Jan. 29th [1860]

Sir

Absence from home has prevented my returning your MS. earlier. Part was so inaccurate & there was so important an omission, that I have

¹ George Busk (1807–1886), man of science, Hunterian Professor and Trustee of the Hunterian Museum, F.R.S. (1850).

² Charles Darwin, *The Balamidae (or Sessile Cirripedes)*, Ray Society, London, 1854.

³ Hugh Cuming (1791–1865). His historic collection, known as the "Cuming Collection" was purchased by the Trustees of the British Museum in 1866, and is one of the most important collections made by a private individual.

⁴ Symington Grieve (1848–1932), author of works on botany of Scotland and on ornithology.

⁵ *Sunday Times*, 20 January, 1929, p. 19.

⁶ R. Griffin & Co., publishers of *Comprehensive Dictionary of Biography*, London 1860.

⁷ British Museum Add. MSS. 28509, f. 408–410.

had a new copy made¹. If too long you can easily strike your pen through any parts.

Sir

Your obed. servt.

Charles Darwin

I return your own M.S. that you may see alterations.

Darwin Charles. *M.A. Cantab. F.R.S.* an eminent naturalist, is distinguished both as an author and a man of Science. *He accompanied Capt. Fitz Roy in his voyage round the world in H.M.S. Beagle during the years 1831 to 1836.—His journal first appeared in 1839 as part of the general narrative of the voyage, and was subsequently republished in a modified form under the title of Journal of Researches into the Natural History and Geology of the countries visited by the Beagle. In 1842 his work on the Structure and Distribution of Coral Reefs appeared which was followed by two Volumes, namely, his Geological Observations on Volcanic Islands and in 1846 on South America. Since this period he has contributed several papers to the Geological Transactions and to other scientific periodicals. His chief contribution to Zoology is the "Monograph on the Family Cirripedia" in two large volumes in which he points out many curious and interesting particulars in relation to the history and economy of the barnacles and sea-acorns, and furnishes a minute description of every known species of the Family. He has recently² (November 1859) published a work entitled "The Origin of Species by means of Natural Selection or the preservation of favoured races in the struggle for life"; this volume, as stated in the introduction, gives only in a condensed form the result of more than twenty years study; and will hereafter be followed by more detailed treatise on the same subject. Mr Darwin's writings exhibit close observation and untiring industry in collecting and arranging facts. As a geologist he stands in one of the highest ranks and was for some time Secretary to the Geological Society. As a Zoologist he is, though learned, very cautious in arriving at conclusions without possessing sufficient data; while his style is so clear that it at once affords pleasure to the student and information to the professor.*

ALBERT GÜNTHER³.

[Charles Darwin to Albert Günther]⁴

Down, Beckenham, Kent

28 Jan. 1876

My dear Dr Günther

I have signed the enclosed certificates with the greatest pleasure. If you think it worth while to send me the numbers of any labels which

¹ In the transcription of this copy the words inserted by Darwin himself are printed in italics.

² This remark, and the date of publication of the Dictionary fix the date of this letter.

³ Albert Karl Ludwig Gotthilf Günther (1830–1914), joined the staff of the British Museum in 1859, Keeper of the Department of Zoology (1875–1895), F.R.S. (1867).

⁴ B.M. (N.H.) Zoological Department: Letters 1877 (A–M), f. 113.

Sir Gavin de Beer *on*

are still attached, I can give you the habitats with certainty. In some few cases I recorded the colours, & more especially the habits of the species.

Believe me
Yours very sincerely
Ch. Darwin.

[Signature only in Darwin's hand]

[*Charles Darwin to Albert Günther*]¹

Down, Beckenham, Kent
Feb. 25 [1877]

My dear Dr Günther,

Your note has led me to discover to my dismay that my catalogue of specimens, which I lent several years ago to the Museum at Cambridge, has never been returned to me. I have written about it, & if it has not been lost will hereafter answer your query. I shall be very sorry if it is lost, but it will not signify much with respect to the spiders, as the labels have been detached. That poor mad creature Adam White² no doubt was the sinner. It was too bad of him, for I told him that I had notes about the habits of some of the species.

Believe me
yours very sincerely,
Ch. Darwin.

[*Charles Darwin to Albert Günther*]³

Down, Beckenham, Kent
March 3, 1877

My dear Dr Günther

I think No 2 cannot be my number; if printed on white paper, it then may be mine & the entrance in my catalogue is "caught on board the Beagle".

No 235 on tin label is a spider caught at Rio de Janeiro which I say is closely allied to *Epira*. The abdomen coloured brilliant red. The animal makes a very regular nearly horizontal web with concentric circles & rests in the centre on the inferior surface, where there is an irregular & thin tissue of net work. But this tissue is sometimes placed in the centre above the concentric web.

No 1442 Spiders caught by sweeping at King Georges Sound Australia. I am sorry to say this is all the information I can give.

Yours very sincerely
Ch. Darwin

[Signature only in Darwin's hand]

¹ B.M. (N.H.) Zoological Department : Letters 1877 (A-M), f. 114.

² Adam White (1817-1879), on the staff of the Zoological Department of the British Museum, writer of numerous publications on insects, crustacea, and mammalia.

³ B.M. (N.H.) Zoological Department : Letter 1877 (A-K), f. 114.

[*Charles Darwin to Albert Günther*]¹

Down, Beckenham, Kent
May 7, 1877

My dear Dr Günther,

I have just received the enclosed MS. about the Yarkand pigeons which ought to be deposited with the specimens as it [adds] greatly to their value.

Yours sincerely,
Ch. Darwin.

[Signature only in Darwin's hand]

[*Charles Darwin to Albert Günther*]²

Down, Beckenham, Kent
22 Dec. [1881]

My dear Dr Günther,

I thank you sincerely for all the trouble, which you have so kindly taken. My case is hopeless since my man is 31 years old. Nor w^d I influence the Archbishop & the other two great men.

With many thanks

Yours very sincerely,
Chas. Darwin.

JOSEPH DALTON HOOKER³.

[*Charles Darwin to Joseph Dalton Hooker*]⁴

2 [recte 4] Bryanston St.
Portman Sq
Dec. 10 [1879]

My dear Hooker

We have come up for 10 days holiday ; the first half here & second half at 6 Queen Anne St.—

Now if by any lucky chance you sh^d be forced to come to London *do try* & come to lunch any day at 1 o'clock

Yours ever
C. Darwin.

¹ B.M. (N.H.) Zoological Department : Letters 1879 (A–K), f. 112.

² B.M. (N.H.) Zoological Department : Letters 1882, Jan.–June, f. 105.

³ Joseph Dalton Hooker (1817–1911), botanist, director of the Royal Botanic Gardens at Kew, F.R.S. (1847), President of the Royal Society (1873–8).

⁴ Pushkin House, Leningrad 137, 1, No. 36, communicated by courtesy of Academician E. N. Pavlovsky.

This letter is dated from the notes in Darwin's Journal which show that in December 1879 he went to stay with his daughter Mrs. Litchfield at 4 Bryanston Street, and then went on to his brother Erasmus at 6 Queen Anne Street.

Sir Gavin de Beer *on*

THOMAS MCKENNY HUGHES¹.

[*Charles Darwin to Thomas McKenny Hughes*]²

Down, Beckenham, Kent.

August 23rd 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.^v 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. 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.

[*Charles Darwin to Thomas McKenny Hughes*]³

Down, Beckenham, Kent

Aug 26 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 the local workers, so that you will see how fully I approve of the rule. As you truly say, the knowledge of the wish of the Council of the Chester Nat. Hist. Soc.^v to honour me, & not the mere reception of the medal, would have been the real gratification to me ; & this I fully profess, & shall never forget.

Believe me, my dear Sir,

Yours sincerely,

Charles Darwin.

ALFRED NEWTON⁴.

[*Charles Darwin to Alfred Newton*]⁵

Bull Hotel [Cambridge]

Sunday, May 22 1870

My dear Newton,

Many thanks for your kind note. I intend trying to see Sedgwick⁶ to morrow morning as I did not find him today ; so if 4 1/4 will suit

¹ Thomas McKenny Hughes (1832–1917), F.R.S. (1889), Woodwardian Professor of Geology, Cambridge from 1873 until his death.

² B.M. (N.H.) General Library, Autograph Collection.

³ B.M. (N.H.) General Library, Autograph Collection.

⁴ Alfred Newton (1829–1907), F.R.S. (1870). First Professor of Zoology and Comparative Anatomy, Cambridge, from 1 March 1866 until his death.

⁵ Balfour Library, Cambridge.

⁶ Adam Sedgwick (1785–1873), F.R.S. (1830), Woodwardian Professor of Geology, Cambridge, 1818–1873.

you to call here & pick me up we will go on together to the Museum.

Yours very sincerely,
Charles Darwin.

[Signature only in Darwin's hand]

[*Charles Darwin to Alfred Newton*]¹

Down, Beckenham, Kent
May 30th 1871

My dear Professor Newton,

Very many thanks for your facts & corrections. The case of the gull² must come out. Oh Lord how difficult accuracy is! Yesterday we had a letter from Frank³ groaning in spirit at the thoughts that it was his last term in Cambridge, which is also dear to me.

Yours very sincerely
Ch. Darwin.

[*Charles Darwin to Alfred Newton*]⁴

Down, Beckenham, Kent
March 9th 1874

My dear Professor Newton,

If you sh^d be in London, will you be so kind as to give your vote at the Athenaeum (& speak to any friends if you properly can) in favour of my nephew, Henry Parker⁵. He is a fellow of Oriel, & I can assure you that he is a fit man for election for his abilities & in every other respect. His ballot comes on at the first meeting, on the 16th of this month.

Pray believe me

Yours very sincerely,
Ch. Darwin.

[*Charles Darwin to Alfred Newton*]⁶

Down, Beckenham, Kent
Jan 2 1877

Dear Newton,

Many thanks for telling me about the complex cross among the wag-tails. I am surprised at so much close interbreeding not checking their propagation. I do not suppose that I shall ever have strength to work up my data on Hybridism, so I think I will not write to Mr. Monk⁷.

Pray believe me yours

very sincerely,
Charles Darwin

[Signature only in Darwin's hand]

¹ Balfour Library, Cambridge.

² The reference is to *The Descent of Man*, London, 1871, vol. ii, p. 228.

³ Francis Darwin (1848–1925).

⁴ Balfour Library, Cambridge.

⁵ Henry Parker (1827–1892), second son of Darwin's eldest sister Marianne.

⁶ Balfour Library, Cambridge.

⁷ Unidentified.

RICHARD OWEN¹[*Charles Darwin to Richard Owen*]²36 Grt Marlbro' St.
Thursday 28th [Dec. 1837]

Dear Owen,

As my servant is passing your way, I send a perfect revise of *Toxodon*³. I have read it over, & marked the very few errata which caught my eye, which may help you in your final revision. I hope with all my heart you will return quite comfortable, and you must not work so hard, as usual for some time.

Yours most truly
Chas. Darwin

N.B. I altered the 12,000 Toises into 1200 Toises which I suspect is right, though it did not occur to me, when I first read it over. Perhaps you had better just refer to Cuvier⁴.

[*Charles Darwin to Richard Owen*]⁵12 Upper Gower Street,
Aug. 25th [?1841]

My dear Owen,

I remember showing you some time ago the accompanying old tooth of Elephant from African coast & I have some faint recollection, that you said you would like to have it from its state of decay—if not *please throw it away*. I saw some weeks since at Mr. Shillinglaw's⁶ at the Geographical Soc. a piece of *tusk* of Mastodon or Elephant from Upper Peru. Does not Cuvier enter into long discussion on a species of tusk brought by Humboldt⁷ from S. America, doubting whether it belonged to an elephant. Would this be worth your looking at for that end.

Yours very truly,
C. Darwin.

¹ Sir Richard Owen (1804–1892), F.R.S. (1834), anatomist; superintendent of the natural history collections of the British Museum (1856–1883).

² B.M. (N.H.) Owen Correspondence, vol. ix, ff. 209–10.

³ Owen's article on *Toxodon* appeared in the *Zoology of the Voyage of the Beagle*, edited by Charles Darwin. From the *Life of Richard Owen* (1894, i, 121), this article was published before 28 February 1838. In 1837, Thursday was the 28th in September and December; but Darwin was at Maer from 25 September to 21 October when he came to London. This enables this letter to be dated 28 December 1837.

⁴ Baron Georges Cuvier (1769–1832), palaeontologist, F.R.S. (1806).

⁵ B.M. (N.H.) Owen Correspondence, vol. ix, f. 207.

The date was arrived at because in 1839 Darwin was away from London on 25 August, and in 1840 he was unwell.

⁶ John Joseph Shillinglaw (1830–1905), geographer.

⁷ Friedrich Heinrich Alexander von Humboldt (1769–1859), naturalist, F.R.S. (1815).

[*Charles Darwin to Richard Owen*]¹

Down, Farnborough, Kent
Saturday, Evening. [1845]

My dear Owen,

Our last letters crossed on the road, & I deferred writing again to say how vexed I was to have caused you so much trouble in vain (though innocently, for it was not possible to suppose from Muniz's² letter to me, that he wished to sell his specimens until I could send you a translation (made by a Mr. J. Morris) of the paper on the *Machairodus* (?).) If you can spare time, I hope you will look it through, for two reasons : first it will, especially the table of measurements, give you some idea how perfect the skeleton is (I see he has a young head as well as the old one) & you might, perhaps, feel warranted in saying that the College would give some sum for it ; I should think, there would be a far better chance of the specimen being sent to this country if some sum, though not a very large one, was offered certainly for it. I wd if you chose communicate, through Mr. Morris with him. It would be easy to contract that the skeleton sent, s^d contain at least all the bones enumerated in the Paper. I have, however, no doubt from all that I have heard, that Muniz is a honest & zealous man.

My second object in wishing you to read it, is to have your opinion, whether any portion could appear in the *Geolog. Journal*, as a Translation, or in *Tagloss's Journal* ; I am very doubtful about it, from the loose manner in which it appears to me drawn up, & from the translator not being an anatomist. Would the table of measurements do to publish ? If so, would you mark any portions, & add any note saying whether it is the *Machairodus* & how far the remains are more perfect than any hitherto found. My sole reason for wishing this, is that I think it would greatly encourage Muniz in his search ; & a S[outh] American osteologist is a prodigy in nature.

I fear you will have some little difficulty in making out the paper : I send the original Spanish, which please not to throw away. I trust to your interest in the subject for forgiveness for all this trouble. What a grand object a mounted *Machairodus* would be by the side of a Tiger's skeleton.

Believe me most sincerely Yours
Charles Darwin.

P.S.

The pamphlet on *Scarletina* I send to your Library ; it may perhaps be worth depositing ; some one working on ferns might some day be glad to see an account of the ravages of the Scarlet Fern in the Pampas.

¹ B.M. (N.H.) Owen Correspondence, vol. ix, ff. 192-3.

² Don Francisco Javier Muniz, of Luxan, author of a paper in *La Gaceta Mercantil*, Buenos Aires, 9 October 1845. R. Owen (*Memoir on the Megatherium*, London, 1860, pp. 11 and 83) states that specimens from Luxan arrived in England in 1845 and were purchased by the Trustees of the British Museum. This gives the probable date of this letter. Cf. *Journal of Researches*, 2nd edn. 1845, reprinted, John Murray, London, 1912, p. 145.

[*Charles Darwin to Richard Owen*]¹

Down, Farnborough, Kent
Sunday 21 [? 1848]

My dear Owen,

I have just heard from Capt. Sullivan² R.N. that he is arrived in London with six casks of fossil bones from the *southern* part of Patagonia, as I before mentioned to you. He is anxious to have them inspected by you & I sh^d be extremely glad to be present. I send this note open through him to make any alterations. I have suggested to him to send the casks direct to the College of Surgeons, & perhaps you could direct some one to get them unpacked & ready for inspection. He proposes to call with me on you on Monday next the 29th at two oclock if, as I hope, that may suit you.

He has not made up his mind to which Public Body he will present the fossils, but I apprehend you will not object to receiving them at the College temporarily, & no doubt there will be some duplicates : in case he sh^d think the British Museum better than the College.

Yours very truly
C. Darwin.

[*Charles Darwin to Richard Owen*]³

Down, Farnborough, Kent
Sept. 10th [1849]

My dear Owen,

I always take shame to myself to trouble you, but I hope soon to go to press with my wearyful fossil Cirripedia, & am anxious to see the account in Mr Dixon's⁴ work on the species therein described. I believe you have superintended the whole : can you oblige me by a *loan* of the proof sheets on Cirripedia?

Mr. Sowerby⁵ has shown me the Plates.

Yours very sincerely
C. Darwin

(over

P.S.

It this minute occurs to me that I have been intending to beg another favour of you to get me permission to examine (if possible) one of the

¹ B.M. (N.H.) Owen Correspondence, vol. ix, ff. 204–5.

² Captain Bartholomew James Sullivan (1810–1890) was a shipmate in the *Beagle*. This letter is on black-edged notepaper and was therefore written shortly after the death either of Darwin's father on 13 November 1848 or of his daughter on 24 April 1851. In 1850 Captain Sullivan was in the Falkland Islands where T. H. Huxley saw him (*Life and Letters of T. H. Huxley*, vol. i, p. 53). Cf. *More Letters of Charles Darwin*, ii, 234.

³ B.M. (N.H.) Owen Correspondence, vol. ix, ff. 198–9.

⁴ Frederick Dixon (1799–1849). *The Geology and fossils of the Tertiary and Cretaceous formations of Sussex*. Edited with a Preface by Sir Richard Owen, London 1850 [i.e. 1852]. Darwin went to press with *Fossil Cirripedia* in 1849 (*Life and Letters*, ii, 37), which ates this letter.

⁵ George Brettingham Sowerby (1812–1884), conchologist and artist.

smaller specimens of *Balanus glacialis* in the Museum attached to some shrimp like crustacean. If such be possible, will you send parcel to

C. Darwin
Care of G. Snow
Nag's Head
Borough

I have in *vain* elsewhere endeavoured to see this *Balanus*. I write formal note to show if required.

[Charles Darwin to Richard Owen]¹

Down, Farnborough, Kent
Sept. 10th [1849]

My dear Sir,

You are aware that I have been employed for a considerable time on a Monograph on the Cirripedia. There is a specimen in the Museum of the College which I am particularly anxious to describe namely the *Balanus glacialis*: to do this it is indispensable to disarticulate one small specimen; & could you oblige me by laying this request before the Governing Body? I would of course return the value of the one specimen disarticulated, if I gain permission.

Pray believe me

Yours sincerely
C. Darwin.

Professor Owen &c., &c., &c.

[Charles Darwin to Richard Owen]²

Down, Farnborough, Kent
Nov. 25th [1850]

My dear Owen,

Having worked out pretty carefully my new articulated *Balanus* (to be call[ed] *Arthrobalanus*)³ I have become so much interested in the structure of the sessile Cirripedes, that I am dissecting 5 or 6 of the other genera. I work out mouths & cirri carefully, muscular structure & tunics of the sack, & a some [*sic*] of the structure of the viscera; whether this is worth doing I am not sure, but I have not forgotten your great kindness in acceding to my request of reading over this time my descriptions. I have, however, strayed from my object, which was to ask, whether you could by any chance supply me with a few specimens in Spirits of any of the *Sessile* genera, for my specimens are all small & some immature. If the College had any large set of specimens, of any of the genera, perhaps it would be possible to let me have a few; I could return the parts, after having them drawn, preserved, & dissected in Spirits.

Pray excuse the trouble

Ever yours truly
C. Darwin.

¹ Enclosure of the previous letter.

² B.M. (N.H.) Owen Correspondence, vol. ix, ff. 201-2. This letter is dated by comparison with the correspondence between Darwin and Albany Hancock (*Trans. Nat. Hist. Northumberland, Durham and New Castle upon Tyne*, 1886, 8, 263-5).

³ *Arthrobalanus* is now called *Cryptophialus minutus*.

P.S.

Most, perhaps all of the shells, about which I more than once tormented you, I have found at Sowerby's.

Fresh pudor on me.

[*Charles Darwin to Richard Owen*]¹

Down, Bromley, Kent
Dec. 13th [1859]

Dear Owen,

My notes for latter chapters are a chaos, but I bethought me to look in large 8vo Edit. (1833) of Lyell's² *Principles* & I find in vol. 3 p. 144 the desired reference to Mr Clifts³ paper viz Ed. New. Phil. Journal No. XX p. 394 & apparently (?) Proc. Geolog. Soc. 1831 p. 321. I find with surprise that Lyell remarks "These facts are full of interest, for they prove that the peculiar type of organisation which now characterizes the Marsupial tribes, has persisted from a remote period in Australia" &c., &c.

You made a remark in our conversation something to the effect that my book could not probably be true as it attempted to explain so much. I can only answer that this might be objected to any view embracing two or three classes of facts. Yet I assure you that its truth has often & often weighed heavily on me; & I have thought that perhaps my book might be a case like Macleay's⁴ Quinarian system. So strongly did I feel this, that I resolved to give it all up, as far as I could, if I did not convince at least 2 or 3 competent judges. You smiled at me for sticking myself up as a martyr: but I assure you, if you had heard the unmerciful & I think unjust things said of my book and to me in a letter by an old & very distinguished friend⁵, you would not wonder at me being sensitive, perhaps ridiculously sensitive. Forgive these remarks. I sh^d be a dolt not to value your scientific opinion very highly. If my views are *in the main* correct, what ever value they may possess in pushing on science will now depend very little on me, but on the verdicts pronounced by men eminent in science.

Believe me yours very truly

C. Darwin.

DANIEL SHARPE⁶.

[*Charles Darwin to Daniel Sharpe*]⁷

Down, Farnborough, Kent
Sunday [Postmark 2 November 1846]

My dear Sir

I have been much interested with your letter & have been delighted that you have thought my few remarks worth attention. My observations on foliation are more deserving confidence than those on cleavage;

¹ B.M. (N.H.) Owen Correspondence, vol. ix, ff. 195-6. Incompletely printed in the *Life of Sir Richard Owen*, by his grandson, London 1894, vol. ii, pp. 90-1.

² Charles Lyell's *Principles of Geology* 1st edition, London, 1833, vol. iii.

³ William Clift (1774-1849) F.R.S. (1823).

⁴ William Sharp MacLeay (1792-1865) zoologist, author of *Horae Entomologicae*, propounding the circular or quinary system of classification.

⁵ Adam Sedgwick.

⁶ Daniel Sharpe (1806-1856), F.R.S. (1850), geologist.

⁷ British Museum Add. MSS. 37725, ff. 4 and 5.

for during my first year in clay-slate countries I was quite unaware of there being any marked differences between cleavage & stratification ; I well remember my astonishment at coming to the conclusion that they were totally different actions, & my delight at subsequently reading Sedgwick's views ; hence at that time I was just getting out of a mist. With respect to cleavage-laminae dipping inwards on mountain-flanks, I have certainly often observed it, so often that I thought myself justified in propounding it as usual ; I might perhaps have been some degree prejudiced by Von Buch's¹ remarks, for which in those days I had a somewhat greater deference than I now have.— The mount at M. Video (p. 146 of my Book²) is certainly an instance of the cleavage laminae of an hornblendic schist dipping inwards on both sides ; for I examined this hill *carefully* with compass in hand & note book. I *entirely* admit, however, that a conclusion drawn from striking a rough balance in one's mind is worth *nothing*, compared with the evidence drawn from one *continuous* line of section. I read Studer's³ Papers carefully & drew the conclusion stated from it ; but I may very likely be in an error. I only state that I have "*frequently*" seen cleavage-laminae dipping inwards on mountain-sides ; that I cannot give up, but I daresay a general extension of the rule (as might justly be inferred from the manner of my statement) would be quite erroneous. Von Buch's statement is in his Travels in Norway⁴; I have unfortunately lost the reference & it is a high crime, I confess, even to refer to an opinion, without a precise reference. If you never read these Travels, they might be worth skimming, chiefly as an *amusement* ; & if you like to send me a line by the Gen. Post on Monday or Tuesday, I will either send it up with Hopkins⁵ on Wednesday, or bring it myself to Geolog. Soc. I am very glad you are going to read Hopkins⁶ ; his views appear to me *eminently* worth well comprehending : false views & language appear to me to be almost universally held by geologists on the formation of fissures, dikes & mountain-chains. If you would have the patience, I sh^d be glad if you w^d read in my "*Volcanic Islands*" from page 65, or *even* 54 to 72, viz. on the lamination of volcanic rocks : I may add that I sent the series of specimens there described to Prof. Forbes⁷ of Edinburgh, & he thought they bore out my views. There is a short extract from Prof. Rogers⁸ in the last Eding. New Phil. Journ⁹. well worth your

¹ Christian Leopold von Buch (1774–1853).

² Charles Darwin, *Geological Observations on South America*.

³ Bernhard Studer (1794–1887).

⁴ C. L. von Buch, *Travels in Norway and Lapland in 1806, 1807, and 1808*, translated by John Black, London, 1813.

⁵ William Hopkins (1793–1866), geologist. The reference is to his book.

⁶ W. Hopkins, *On the internal pressure to which Rock-masses may be subjected and its possible influence in the production of laminated structure*, Cambridge, 1847 (presumably a proof of it).

⁷ James David Forbes (1809–1868), F.R.S. (1832), Professor of Natural Philosophy in Edinburgh.

⁸ Henry Darwin Rogers (1809–1866), geologist.

⁹ Report of a paper "On Cleavage of slate-strata", delivered by H. D. Rogers at the meeting of the Association of American Geologists in April 1845; *Edinb. New Phil. J.*, 1846, 41, 422 (not 414 as in index).

attention on the cleavage of the Appalachian Chain, & which seems far more uniform in direction of dip, than in any case, which I have met with : the Roger's doctrine of the ridges being thrown up by great waves I believe is monstrous ; but the manner in which the ridges have been thrown over (as if by a lateral force acting on one side on a higher level than on the other) very curious, & he now states that the cleavage is [parallel] to the axis-plane of these thrown over ridges.— Your case of the limestone beds to my mind is the greatest difficulty on any mechanical doctrine ; though I did not expect ever to find actual displacement, as seems to be found by your shell-evidence.— I am extremely glad you have taken up this *most* interesting subject in such a philosophical spirit ; I have no doubt you will do much in it ; — Sedgwick let a fine opportunity slip away. — I hope you will get out another section like that in your letter ; these are the real things wanted.

Believe me Yours very sincerely
C. Darwin

[*Charles Darwin to Daniel Sharpe*]¹

Down, Farnborough, Kent.
Tuesday [1847]

My dear Sir

I do not know whether you look at the Edin. New Phil. Journal, but if not, do read a very short letter from Studer² at p. 186 of last Number. I have been pleased with it as it confirms my facts & views on the "foliation" of gneiss &c. being quite distinct from stratification : Studer does not seem to have perceived that foliation is only much developed cleavage.—

I often think over what little I was able to understand of your Paper³, & I anxiously look forward to read it in extenso.—

When non-cleavage [*drawing*]

beds intervene between

those with cleavage, have you ever noticed very carefully the lines of junction ? do the edges of the cleavage planes ever indent in ever so small a degree the bases or tops of the intercalated non-cleavage beds. If you have time to send me one single line, just inform me on this head.—

Believe me

Yours very truly
C. Darwin

HENRY TIBBATS STAINTON⁴

[*Charles Darwin to Henry Tibbats Stainton*]⁵

Down, Bromley, Kent
Oct. 20th [1855]

Dear Sir,

As I do not now collect or work on any order of Insects, it would, I fear be quite useless inserting my name.

¹ British Museum Add. MSS. 42240, ff. 21 and 22.

² B. Studer : "Remarks on the Geological Relations of the Gneiss of the Alps", *Edinb. New Phil. J.*, 1847, 42, 186.

³ D. Sharpe, *On Slaty Cleavage*, London, 1847.

⁴ Henry Tibbatts Stainton (1822–1892).

⁵ B.M. (N.H.) General Library, Autograph Collection.

The slip, however, is quite correct.

With much respect, I remain,

Dear Sir

Yours faithfully,

Ch. Darwin.

[*Charles Darwin to Henry Tibbats Stainton*]¹

Down, Bromley, Kent

June 11th [1860]

Dear Sir,

I take the liberty to send the enclosed.—I fear it is quite out of the question your being able to insert the whole in the *Intelligencer*; but perhaps you would oblige me by putting in the paragraph in which I ask for information on what kinds of moths the pollen-masses of Orchids have been found adhering.—My best & almost sole chance will be through the columns of the *Intelligencer*.

Dear Sir,

Yours faithfully

C. Darwin

(over

P.S. I have just received your extremely kind & to me most interesting note.—As you have Gardener Ch. I will not send the Duplicate copy, which I got to send to you, but think I will send it to Mr. Parfitt² & enquire whether he is certain that he recognises pollen-masses of Bee Orchis. The *green* pollen-masses I sh^d think were those of true Orchis.—Again I thank you sincerely. It is extremely curious the same moth having been found with pollen-masses in two parts of England. I c^d recognise pollen-masses of Butterfly Orchis & I daresay with a little care of most of our orchids.—

[*Charles Darwin to Henry Tibbats Stainton*]³

Down, Bromley, Kent

June 20th [1860]

Dear Sir,

I am very much obliged for your second note. I have had a very satisfactory answer from Mr Parfitt⁴, who is evidently a careful & conscientious observer.—

Would you *kindly* insert enclosed question in the *Intelligencer* & answer it yourself, if it lies in your power.—

Dear Sir,

Your truly obliged

C. Darwin.

[*Charles Darwin to Henry Tibbats Stainton*]⁵

Down, Bromley, Kent. S.E.

Feb. 21 [1868]

My dear Mr Stainton

Though you are so kind as to say that you will write again, I must thank you at once for your letter, which is of the *highest* value to me.—

¹ B.M. (N.H.) General Library, Autograph Collection.

² Edward Parfitt (1820–1893).

³ B.M. (N.H.) General Library, Autograph Collection.

⁴ See foot-note to previous letter.

⁵ B.M. (N.H.) General Library, Autograph Collection.

Your results are most curious & show what caution is requisite. Do you think it possible that breeding very young caterpillars under confinement & therefore not under strictly natural conditions could influence the sex. I do not recollect how soon the sexual organs are differentiated in Larvae. Perhaps Sir J. Lubbock¹ may know. It has been stated (but I cannot say that I quite believe it) by Knight² that heat will determine the sex in unisexual flowers.

I have had a note from Mr Trimen³ who advances the case of the *Lasiocampa quercus* a female of which taken at any time & brought out of doors will be surrounded by males; this looks very like an excess of males.—Have you noticed other such cases, viz of many males pursuing one female.—// I remember reading somewhere, I wish I knew where, an extraordinary account of males finding females at great distances & coming down chimney //—I think I have seen this with Butterflies. But then here comes another doubt—may not the same male serve more than one female? Or does male die after copulation? Do you know anything on this head? I think I will write to Dr Wallace⁴ of Colchester, who perhaps w^d excuse my writing to him.—I do not know to whom to apply in France, but the proportional numbers of male & female silk-moth has probably been observed there. The whole subject is very intricate, far more so than I anticipated, but I have often found that by patiently collecting facts, or supposed facts, in relation to various classes, a dim ray of light may be gained. I am getting the results of breeding race-horses—short-horns & greyhounds, tabulated on a large scale.—

I hope I shall not utterly weary you.

With cordial thanks

Yours very sincerely

Ch. Darwin.

[*Charles Darwin to Henry Tibbats Stainton*]⁵

Down, Bromley, Kent. S.E.

March 2 1868.

My dear Mr Stainton

Your letter has been wonderfully interesting to me, & what immense trouble you have taken in writing so many letters, the answer to which I have been very glad to see. The subject is extremely complex, & to me very important. I am having domesticated animals tabulated, & by patient enquiry, I hope to arrive at some degree of probability; certainty I fear is out of the question. With man alone we know positively that males are born in excess. Dr Wallace⁶ thinks that with *B. Cynthia* he certainly breeds more males than females. He seems to think that in collecting caterpillars any one w^d be more likely to collect those which w^d turn into females than those which w^d turn into males,

¹ Sir John Lubbock, afterwards Lord Avebury (1834–1913), F.R.S. (1858).

² Thomas Andrew Knight (1759–1838), vegetable physiologist, F.R.S. (1805).

³ Roland Trimen (1840–1916), F.R.S. (1883).

⁴ Dr Wallace of Colchester (unidentified).

⁵ B.M. (N.H.) General Library, Autograph Collection.

⁶ See foot-note to previous letter.

from the former being somewhat larger & thus more likely to be chosen, or unconsciously to strike the eye. This certainly seems not improbable; & if you are not wearied out, I sh^d like to have your opinion on this head; for it is a most uncomfortable anomaly that more free males sh^d be captured & more females breed under confinement.

I am sorry that my last note about the protective colouring of the brimstone & Orange-tip butterflies did not reach you in time; but my query, as far as the Brimstone is concerned is incidentally answered by Mr. Doubleday¹. I think I shall write to him about the courtship of butterflies. In the case of moths Dr Wallace seems positive that there is no courtship [*from here in Darwin's hand*], & that the female receives the first male which comes. If I am not mistaken I have seen several male Butterflies pursuing one female & I cannot but think that something more than mere chance must decide which shall be successful. So with the crowding *Lasiocampas*.

In truth I do not know how to thank you enough; & I will not give any more trouble.

Believe me

Your sincerely obliged

Ch. Darwin

I think I will write to Quatrefages² about silk-moths in France.

[*Charles Darwin to Henry Tibbats Stainton*]³

Down, Beckenham, Kent

Sept. 28, 1881.

My dear Sir

It was very kind of you to send me the Dahlia flower, which is curious & pretty, but analogous cases have been occasionally observed. Mr. Bree⁴, who half a century ago often wrote in Hort. & Nat. Hist. Journals, says that a Dahlia " bore two different kinds of self-coloured flowers, as well as a third kind which partook of both colours beautifully intermixed."—I could add other cases of such bud-variation.—

My dear Sir

Yours very faithfully

Charles Darwin

WILLIAM TURNER THISELTON-DYER⁵.

[*Charles Darwin to William Turner Thiselton-Dyer*]⁶

Down, Beckenham

April 22^d 1879

My dear Dyer,

The enclosed was raised from seed from Kew, under name of *Oxalis colorata*, & you were so kind as to say when it flowered you w^d give me

¹ Henry Doubleday (1808–1875), naturalist.

² Jean Louis Armand de Quatrefages de Bréau (1810–1892), zoologist, F.R.S. (1879).

³ B.M. (N.H.) General Library, Autograph Collection.

⁴ Charles Robert Bree (1811–1886).

⁵ William Turner Thiselton-Dyer (1843–1928).

⁶ B.M. (N.H.) Botany Department, Autograph Collection.

true name, as you did not know what this name signified.

Yours very truly
Ch. Darwin.

[In another hand :
Oxalis floribunda, Lehm !
fide N. E. Brown]

GEORGE ROBERT WATERHOUSE¹.
[*Charles Darwin to George Robert Waterhouse*]²
Down, Bromley, Kent
April 1 [? 1859]

My dear Waterhouse

I have looked through my collection of combs & have not a piece of drone cells ! I had plenty, & one choice piece, but some months ago I threw away a lot, & I suppose, I did not notice that this was a selected piece. Nothing easier of course, than to replace the piece. I am very sorry that I cannot answer your query. From memory I am convinced that the shorter diameter of Queen cells is larger than Drone cells.

I cannot see how the relation which you wish to establish will help you, as you have to account for so fine a gradation in the two sizes. I did indeed most carefully think over subject & came most deliberately to conclusion that distance at which each Bee stands from the others must be the governing element. How they instinctively judge this I cannot conjecture. I got comb and Bees from W. Indies solely for this object. I think you overrate the difficulty of this judgment of distance. The jumping spiders can judge distances accurately at which they spring on prey. One has only to perfect this instinct. I am convinced you are not right in speaking of Cell-construction as due to excretion *alone* ; the Bees can certainly build rough wall in proper position for any particular side of cell. They do not require to make whole cell or sphere, but begin to work at one face of the pyramidal basis alone. All that is required is for each Bee to stand in proper relation, position & distance with respect to the other Bees.

I venture to caution you (not from my own knowledge) about the mathematical part of the question ; for on the theory of spheres (& spheres must be used to make the pyramidal basis) not one angle or side of the hexagon touches the sides of spheres : the points of intersection lie quite within. I have had models made. The intersection of cylinders to make simple hexagon does not suffice as guide with respect to the intersection of spheres.

The tenacity with which your *Osmia* stuck to same position does not apply to Hive Bee for a score of different individuals work one after another at the very commencement of any one cell.

I much wish I could have answered your query.

My dear Waterhouse
Yours very sincerely
C. Darwin.

¹ George Robert Waterhouse (1810-1888), naturalist, Keeper of Mineralogy and Geology (1857-80), British Museum.

² B.M. (N.H.) General Library, Autograph Collection.

CHARLES WHITLEY¹.

[*Charles Darwin to Charles Whitley*]²

London [43 Great Marlborough Street]

October 24th [1836]

My dear Whitley

I was very glad to receive your letter, which I did the other day previous to my leaving Shrewsbury. It was a piece of high virtue on your part, being able to write a single line on such an occasion.

I most heartily congratulate you. Long may you live in your now perfect state. We poor batchelors are only half men,—creeping like caterpillars through the world without fulfilling our destination. Herbert³ did not call on the Saturday, as you told me that he would. As it was impossible for me from previous arrangements to have staid till that day, I left a letter for him, but which he has never received.— I am anxious to communicate with him and have not an idea how to manage it. I am at present waiting in London till the *Beagle* arrives at Woolwich to be paid off. After packing up all my my goods, I shall pay Shrewsbury a visit of a few days longer than my former one, and then return to Cambridge where I suppose I shall take up my residence for several months.— Ultimately I shall migrate to London.— I spent a few days with Henslow⁴ before coming up here & received a most cordial welcome from the few friends still living in Cambridge. Amongst those were old Heaviside⁵ & Matthews⁶: & when I return, we are to have a quiet little dinner. How I wish you were there to join us, but you, you lucky man, are enjoying far more cosy meals.— So much for my plans. When I shall be enabled to visit you at Durham Heaven only knows. I am at present at an utter loss to know how to begin the arrangements of specimens and observations collected during the five long years. All I know is, that I must work far harder than poor shoulders have ever been accustomed to do.— If you can muster time do write to me and direct Shrewsbury (if in a fortnights' time or 43 Great Marlborough St if earlier). In your last letter you told me nothing beyond the grand event. Pray write in more detail. Do not forget to give me old Herbert's direction. I wish I could see some early prospect of shaking you by the hand, and talking over old Cambridge days. Do you remember our very long walks, how pleasant they were, such nice quiet tranquil days never can return. Farewell My dear Whitley long may you enjoy your present happiness. Once again Farewell

Yours ever most truly

Charles Darwin

I fear my direction to you is not a very correct one, tell me if it is right.

¹ Charles Whitley (1808–1895). Subsequently Reader in Natural Philosophy in the University of Durham.

² British Museum Add. MSS. 41567, ff. 248 and 249. This letter is dated from the fact that 43 Great Marlborough Street was Darwin's address only from October to December 1836.

³ John Maurice Herbert (1808–1882).

⁴ John Stevens Henslow (1796–1861), botanist.

⁵ James William Lucas Heaviside (1808–1897).

⁶ Presumably Frederick Hoskyns Matthews (1778–?) of Shrewsbury School and Trinity College, Cambridge.

SAMUEL PICKWORTH WOODWARD¹.[*Charles Darwin to Samuel Pickworth Woodward*]²

Down, Bromley, Kent

9th 1860

My dear Sir,

I am very sorry to say that I cannot answer one of your questions. Von Buch's or Johnston's Phys. Atlas would probably state numbers of volcanos. I am nearly certain these are the best remarks of anybody's on activity of sub-aerial volcanos in Lyell Principles or Elements on Auvergne; I remember being astonished at their antiquity. Degradation would prevent any sub-aerial volcano in a recognizable condition being of excessive antiquity. St. Helena of which upper part at least is sub-aerial, struck me as of extreme antiquity. The Canary isles from littoral miocene shells must as a volcanic group be very old.

I entirely & absolutely disagree with Von Buch's elevation-crater-theory. Indeed I think it proved false. As there are so considered volcanic ash beds in the oldest formation, viz., Silurian, no doubt sub-aerial Volcanos have always existed, or at least *probably*, for on reflexion I do not suppose volcanic mud & ashes when stratified under water could be distinguished.

I am very glad to hear about your Brother.

I am sorry that I cannot answer your question.

In haste

Yours very truly

C. Darwin.

[*on back of letter*]:

The Cordillera were volcanic with islands since early part of Cretaceous period or older; yet I do not say that the volcanoes were sub-aerial—some streams [illegible] flowed in *not* deep water.

Iceland from Miocene plants must have been volcano since that period I suspect that this [is] oldest ascertained volcano.

UNIDENTIFIED³.[*Charles Darwin to*?]⁴

Down, Bromley, Kent

March 14th [1861]

Dear Sir

I am much obliged for your long letter, as I always like to know how naturalists view the subject. I feel not a shade of surprise at your entirely rejecting my views: my surprise is that I have been successful in converting some few eminent Botanists, Zoologists, & Geologists. In several cases the conversion has been very slow & that is the only

¹ Samuel Pickworth Woodward (1821-1865), naturalist.

² B.M. (N.H.) General Library, Autograph Collection.

³ Unidentified I.

⁴ British Museum Add. MSS. 37725, ff. 6-9. This letter was inaccurately transcribed and published by G. Sarton in *Isis*, 26, 1937, 336-40.

sort of conversion which I respect. I entirely agree with you that there is no more direct proof of variation being unlimited in amount than there is that it is strictly limited. — In a new & corrected Edit. of the Origin, which will appear in about a week or two¹, I have printed this as emphatically as I could.— I did not formerly explicitly say this (but indirectly in several places) because I thought it was obvious. The manner in which I wish to approach the whole subject, & in which it seems to me it may fairly be approached, I can best illustrate by the case of Light.— The ether is hypothetical, as are its undulations ; but as the undulating hypothesis groups together & explains a multitude of phenomena, it is universally now admitted as the true theory. The undulations in the ether are considered in some degree probable because sound is produced by undulations in air. So natural selection, I look at as in some degree probable, or possible, because we know what artificial selection can do.— But I believe in nat. Selection, not because I can prove in any single case that it has changed one species into another, but because it groups & explains well (as it seems to me) a host of facts in classification, embryology, morphology, rudimentary organs, geological succession & Distribution.— I have no space to discuss the many points alluded to in your letter.— I cannot see such perfection in structures as you do. In the new Edit. I have attempted to explain how it is that many new forms have not progressed to a higher grade of organization.

I did not allude to the very curious subject of “alternate generations”, because I did not, & do not yet, see, how it has any special bearing on my views.— I look at alternate generations, as not *essentially* differing from various stages in any one individual larva—a form of generation being merely added at some stage. Under this point of view I see no essential difference between alternate generations & metamorphosis : you, I perceive take some very different view.— I forget what Agassiz says on subject.— I quite agree with you that Agassiz’s Review is not in the least unfair². He misunderstands me a good deal.— His “categories of thought”, “prophetic types” & his views on classification are to me merely empty sounds. To others they seem full of meaning.

I received several months ago, & thank you for, a very curious pamphlet on representative form (or some such title) which interested me very much.—

With my best thanks, I remain

Dear Sir

Yours very faithfully

Charles Darwin

I am much pleased at & grateful for the sentence which you kindly copy from a recent letter from Agassiz.— I once saw him, & was charmed with him.—

¹ The third edition of *The Origin of Species* was published in April 1861, which enables this letter to be dated.

² The review by Louis Agassiz of *The Origin of Species* was published in *Silliman’s American Journal* for July 1860. It was reprinted in *Ann. Mag. Nat. Hist.*, 1860, 6, 219–32.

UNIDENTIFIED¹.[*Charles Darwin to*?]²

Down, Beckenham, Kent.

(London) Dec. 28th [1875]

My dear Sir

You will have heard that Ray Lankester³ who had done such excellent work in embryology has been black-balled at Linn. Soc. I hear from one of the black-ballers that this has been done to punish the Council for intending to remit his fees. Now this seems to me a most flagitious proceeding. I know not & care not whether the Council intends to act rightly or wrongly. The power has been given them & it is wicked to cast a stigma on a rising naturalist by black-balling him to punish the Council.— Those who think the Council have done wrong could have called a special meeting, or blamed it at the Anniversary, & taken away the power of ever again remitting fees.— I have consulted many men & all as yet have agreed with me. I have therefore most willingly consented to second (Huxley proposing him) Mr. Lankester for a second time (I knew nothing of the first proposal & he is not a personal friend, only an acquaintance) & he will be balloted for on Feb. 3^d at 8 o'clock. If you sh^d take the same view as I do, I earnestly hope that you will attend & endeavour to influence other members. It seems to me a case (& this is my whole motive) when one man of science ought to aid another & I sh^d be very sorry to see so disgraceful a stigma rest on the Linn. Soc.

My dear Sir

Yours very sincerely

Ch. Darwin

I laid case before a high Government Official to see how he w^d view it, & he used even stronger language than I have done. Pray forgive the ill expressed note, as I write in a forced hurry.

UNIDENTIFIED⁴.[*Charles Darwin to*?]⁵

Down, Beckenham, Kent.

March 27th 1882

Dear Sir

I am much obliged for your very kind offer. I am not well & have not strength to examine *Utricularia*, but think that I could try & look at the *Nitellia*. If it w^d not give you too much trouble, I sh^d much like to have a living plant. Will you kindly tell me how I c^d keep it alive. In what kind of water—whether there ought to be soil at the bottom of the vessel & about light.

Excuse brevity — Dear Sir

Yours faithfully

Ch. Darwin

¹ Unidentified II.² British Museum, Egerton MS. 3009.c., ff. 11 and 12.³ Edwin Ray Lankester (1847–1929), F.R.S. (1875).⁴ Unidentified III.⁵ British Museum, Darwin 46917.

[*Charles Darwin to*

?]¹

Down, Beckenham, Kent.

April 3^d 1882

Dear Sir

I thank you cordially for the beautiful specimens of the *Nitella* & for your letter of instructions. — I have roughly tried the effects of C. of *Ammonia* on the Chlorophyll grains, but I find stooping over the microscope affects my heart. The grains swell & then exhibit the contained particles of starch very clearly, & some of the grains become confluent, occasionally sending out prolongations. But my observations are hardly trustworthy. The grains do not seem to be so strongly affected as in some few other cases. The facts which you relate about the distribution of the *Nitella* are very curious ; & how little we know about the life of any one plant or animal!

Dear Sir

Yours faithfully & obliged

Ch. Darwin

¹ British Museum, Darwin 46917. This letter is in his handwriting throughout. He died on 19 April 1882.