

# Darwin conversation

THE ENDLESSLY CURIOUS LIFE AND LETTERS  
OF CHARLES DARWIN

**Complete texts of exhibited letters. Numbers correspond to case labels.**

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## Darwin the letter writer

### Case 1

#### 1 Letter to Caroline Darwin, 18 July 1836

[On board *Beagle*, bound for Ascencion]

July 18<sup>th</sup>.— 1836.—

My dear Caroline

We are at this present moment driving onwards with a most glorious tradewind towards Ascencion. I am determined to pay the debt of your most excellent correspondence; by at least writing to you all, as often as I can. I will leave this letter at Ascencion to take its chance of being forwarded. Before attempting to say anything else, I must disburthen my mind, of the bad news that our expected arrival in England, recedes, as we travel onwards. The best judges in the Ship entertain little hopes of it, till the end of October. The next three months appear infinitely tedious, & long, & I daresay the last three weeks, will be worse, as for the three closing days, they, by the same rule, ought to be intolerable. I feel inclined to write about nothing else, but to tell you over & over again, how I long to be quietly seated amongst you.— How beautiful Shropshire will look, if we can but cross the wide Atlantic before the end of October. You cannot imagine how curious I am to behold some of the old views, & to compare former with new impressions. I am determined & feel sure, that the scenery of England is ten times more beautiful than any we have seen.— What reasonable person can wish for great ill proportioned mountains, two & three miles high? No, no;

give me the Brythen or some such compact little hill.— And then as for your boundless plains & impenetrable forests, who would compare them with the green fields & oak woods of England?— People are pleased to talk of the ever smiling sky of the Tropics: must not this be precious nonsense? Who admires a lady's face who is always smiling? England is not one of your insipid beauties; she can cry, & frown, & smile, all by turns.— In short I am convinced it is a most ridiculous thing to go round the world, when by staying quietly, the world will go round with you.—

But I will turn back to the past, for if I look forward, I lose my wits, & talk nonsense. The Beagle staid at St Helena five days, during which time I lived in the clouds in the centre of the Is<sup>d</sup>.— It is a curious little world within itself; the habitable part is surrounded by a broad band of black desolate rocks, as if the wide barrier of the ocean was not sufficient to guard the precious spot. From my central position, I wandered on foot nearly over the whole Island; I enjoyed these rambles, more than I have done any thing for a long time past. The structure of the Is<sup>d</sup> is complicated & its geological history rather curious.— I have discovered a monstrous mistake, which has been handed from one book to the other, without examination. It has been said, that Sea shells are found on the surface of the land, at an elevation little short of 2000 ft. & hence that, this Is<sup>d</sup>. though possessing an entirely unique Flora, must have been raised, within a late period, from beneath the Ocean.— These shells turn out land shells! But what is very singular, they have ceased to exist, in a living state on the Is<sup>d</sup>.—

I heard much of old General Dallas & his daughters.— People speak very well of him—(as a well intentioned old goose).— He took much pains in improving the road & other public works, was most hospitable, magnificent, & popular.— The young ladies were the gayest of the gay.— Finally he was the last of the E. Indian Company's Governors, with an income more than quadruple the present.— Hence perhaps the lamentations at his departure.—

From St Helena, I wrote to Erasmus a long & a heavy letter all about myself, it was directed to the Wyndham Club.— I most earnestly hope Erasmus will not be wandering on the continent about the time of the Beagle's return; I am delighted he has taken a house, as he will more probably now be a fixture.— I shall really have so much to say, that I fear I shall annihilate some of my friends.— I s(hall) put myself under your hands; & you must undertake the task of scolding, as in years long gone past, & of civilizing me.— Oh for the time when we shall take a ride together on the Oswestry road.—

My dear Caroline I do long to see you, & all the rest of you, & my dear Father.— God bless you all—  
Your most affectionate | brother. Chas. Darwin.

P.S. I have kept this flap open in case of receiving any letters tomorrow when we reach Ascencion.—

*[Written in pencil on outer flap of cover:]* There is a Ship in the offing & this must go.— There are letters, but the bundle has not been opened.

DAR 223: 35

#### **4 Letter from Asa Gray to Joseph Hooker, 5 January 1860**

Jan<sup>y</sup>. 5/1860

My Dear Hooker

Your last letter, which reached me just before Christmas— has got mislaid during the upturnings in my study which take place at that season, & has not yet been discovered. I should be very sorry to lose it, for there were in it some botanical mem. which I had not secured. It will turn up, I trust. I remember you wrote of dear Lady Hooker being laid up with a bad leg. We hope it will not prove serious,— I fear it will be tedious.

The principal part of your letter was high laudation of Darwin's book.—Well, the book has reached me, and I finished its careful perusal 4 days ago!— And I freely say that your laudation is not out of place.

It is done in a *masterly manner*,—it might well have taken 20 years to produce it. It is crammed full of most interesting matter—thoroughly digested—well expressed—close, cogent—and taken as a system it makes out a better case than I had supposed possible.

D<sup>r</sup>. Wyman is just reading it—is struck with its ability,—but I shall know more what he thinks of it presently.

I gave a copy to a hard-headed friend of mine, of very impartial mind, familiar with physical science, thoughtful about general problems in Nat. Hist. but neither naturalist nor geologist. He is much impressed by it.

Agassiz—when I saw him last, had read but a part of it. He says it is *poor—very poor!!* (*entre nous*). The fact he growls over it, like a well cudgelled dog,—is very much annoyed by it—to our great delight—and I do not wonder at it. To bring all ideal system within the domain of pure science, & give good physical or natural explanations of all his capital points, is as bad as to have Forbes take the glacier-materials A. had long muddled over, and give scientific explanation of all the phenomena.—

Tell Darwin all this. I will write to him when I get a chance. As I have promised he & you shall have fair play here,—& Dana is in Italy, ill.— I must myself write a review of Darwin's book for *Sill. Journal*, (the more so that I suspect Agassiz means to come out upon it)—for the next (March) no.— And I am now setting about it (When I ought to be every moment working Expl. Expedition Compositæ, which I know far more about).— And really it is no easy job, as you may well imagine.

I doubt if I shall please you altogether. I know I shall not please Agassiz at all. I hear [one more] reprint is in press,—and the book will excite much attention here, and some controversy.

Your last letter had the address to *Cambridge* so blotted that the Boston Post office kept it a day or so to decypher. Else I should have had it in time to acknowledge in my last to Sir William. It answers the queries I asked, after the safety of the case I sent last summer, &c, &c.—

DAR 98: B20-1

### **5 Letter from the Misses Horner, [17 March 1837 - 28 December 1838]**

*The Botanists* present their best thanks to M<sup>r</sup> Darwin for his kindness in advancing them in their pursuit by sending them a book with such interesting plates and which they intend to study with great attention.—

*The learned Linguists* feel also grateful for M<sup>r</sup> Darwin's generous assistance.— Ki te kahore hoki he mahi. Friday then 2 Bedford Place

DAR 94: 1a

### 7 Postcard from William Thiselton-Dyer, 27 June 1878

*Colocasia antiquorum*, Schott = *Caladium esculentum*, Hort.

DAR 209.14: 188

### 8 letter to Hensleigh Wedgwood, 9 March 1871

Down | March 9<sup>th</sup>. 71

My dear Hensleigh,

I promise that I will not plague you with another letter. I have been particularly glad to get your last M.S, for I feared that we differed in toto.

We seem to agree about what may be called instinctive moral actions, as in impulsively saving a drowning man.

I think we also agree about the so-called self-regarding virtues, such as temperance; the breach of which, as in the case of Etiquette, I believe are regretted or repented solely from our appreciation of the approbation or disapprobation of our fellows; this appreciation depending (as I suppose you will admit) on our innate or instinctive sympathy, which seems to me a rather different emotion from love. Am I right in supposing that we agree about the self-regarding virtues & etiquettes? Every point of agreement is a satisfaction to me.

We differ on the causes of repentance or shame, when the social are mastered by the animal appetites, for in such cases I bring in the greater persistence of the former.— It would simplify matters much if I could persuade myself that this step or view might be omitted; and I do not feel so positive as I did. The following illustration will make me clearly perceive whether I clearly understand your view.— A man does not save a fellow-man from drowning, being prevented by a strong sense or instinct of self-preservation. When he reflects over his conduct, from the danger not being then present and imminent, while the social instincts are persistent, I believe that he feels dissatisfied with himself for having disobeyed the latter in not trying to save the man. This dissatisfaction does not imply any balancing of gratification and uneasiness, and may amount only to regret, or to repentance or remorse. I believe this would be the state of mind in our man, independently of any direct or indirect consideration or appreciation of the Approbation of his fellow-men. A man with a noble nature might repent not having tried to save the 'drownee', altho' from the risk being so extreme he might feel sure that no one wd blame him for not having made the attempt. Nor can I believe that his regret is due to habit of reflexion that it would have been highly praiseworthy, had he made the attempt. I suppose that you wd say at least in the former case of risk not being extreme that his repentance (or shame if you like) is due to his looking on his own conduct as on that of another man's, & he knows that he shd blame this other man, & so feels the blame himself; and blame is odious to us from being endowed with sympathy. So conversely with praise had he made the attempt. Is this your view?— A brief answer will suffice.—

Will you return me this letter as it will serve for a memorandum.

Possibly you might like to see the part between blue marks in the enclosed letter. Please return it— M<sup>r</sup> Johnson, the writer, is said to be a very clever man.

I have found your language book & dictionary very useful in my Expression work.—

Yours affect<sup>ly</sup>. | Ch. Darwin

DAR 88: 64-6

### **9 Second letter from Horace Darwin to George Darwin, 1 May 1876. Typed**

*[Text available through the Darwin Correspondence Project's 'Darwin family letters' collection on [epsilon.ac.uk](http://epsilon.ac.uk), a partnership of the Darwin Correspondence Project and others to publish 19<sup>th</sup> century science letters in a cross-searchable format]*

6 QUEEN ANNE STREET | CAVENDISH SQUARE | LONDON W.

MAY 1ST 1876

DEAR GEORGE—

I SHOULD HAVE LIKED TO HAVE BEEN WITH YOU WHEN YOU GOT MY FIRST LETTER , I THINK IT MUST HAVE ASTONISHED YOU SO MUCH .YOU MUST HAVE GUESSED BY THIS TIME THAT FATHER HAS GOT ATYPEWRITER' & A VERY NICE TOY IT IS . THE REASON HE GOT IT WAS HIS HAVING READ AN ARTICLE IN THE TIMES ABOUT IT ,WHICH A GLOWING ACCOUNT OF IT . FRANK & I WENT OFF TO SEE IT IN THE CITY ,& WERE SO MUCH STRUCK WITH IT THAT WE BOTH WANTED FATHER TO GET ONE AT ONCE & ESPECIALLY AS IT ONLY COSTS £21— . IT IS A LITTLE LIKE THE OLD WRITING BALL BUT YOU PLAY ON KEYS & IT IS NOT DIFFICULT TO LEARN TO DO IT SLOWLY AT ANY RATE ; IT ONLY CAME AT ABOUT 12 THIS MORNING , & I DONT DO IT MORE THAN 2 OR 3 TIMES AS SLOWLY AS WRITING I SHOULD THINK . IT IS THE NICEST TOY I EVER PLAYED WITH & I HAVE BEEN PLAYING WITH IT NEARLY ALL DAY . I ENCLOSE ONE OF THE ADVERTISEMENT AS I KNOW YOU WILL WANT TO GET ONE, & IF FRANK FINDS THAT HE CAN LEARN TO WRITE QUICKLY WITH IT I SHOULD THINK IT WOULD BE WELL WORTH WHILE FOR YOU TO DO SO , AS YOU WRITE SO MUCH. UNCLE RAS SAYS THAT YOU WILL INSTANTLY TELEGRAPH ON RECEIPT OF MY FIRST LETTER TO—KNOW WHAT ON EARTH HAS HAPPENED , BUT I DONT THINK THAT THIS LIKELY OF COURSE HE IS INTENSELY PLEASED WITH IT .BUT NO ONE CAN BE SO MUCH SO AS I AM . I THINK I SHALL HAVE TO GET ONE FOR MYSELF . FATHER HAS JUST GOT YOUR SECOND NOTE ABOUT THE EARTH.

YOURS EVER | HORACE DARWIN

DAR 258: 860

### **Replaced from October with [first letter from Horace Darwin to George Darwin, 1 May 1876.](#) Typed**

6 QUEEN ANNE STREET | CAVENDISH SQUARE | LONDON W.

MAY 1ST.— 1876

DEAR GEORGE—

I HAVE BEEN STAYING UP HERE ALL THE TIME WITH THE PARENTS, BECAUSE I HAVE BEEN RATHER SEEDY WHICH HAS TURNED INTO A SLIGHT ATTACK OF GOUT SINCE I HAVE BEEN HERE. IT WAS HARDLY WORTHY TO BE CALLED GOUT, & ONLY MADE ME FEEL RATHER SEEDY, BUT I AM MUCH BETTER NOW. FATHER HAS BEEN VERY WELL SINCE HE HAS BEEN HERE. I EXPECT YOUR CYLINDERS WILL BE DONE VERY SOON NOW, I LEFT INSTRUCTIONS WITH ONE OF THE PUPILS TO SEND THEM OFF AS SOON AS EVEN THEY WERE DONE. I EXPECT I SHALL BE GOING BACK TO ERITH IN A FEW DAYS THOUGH. I'M VERY GLAD TO HEAR THAT YOU THINK THAT YOU THINK THAT YOU MAY MAKE OUT SOMETHING ABOUT THE EARTH PROBLEM. I SHOWED MY DYNAMOMETER AGAIN TO MR. ANDERSON, & HAD LONG TALK WITH HIM ABOUT IT, HE CONSIDERED IT WELL & THEN SAID— 'THAT HE THOUGHT THAT IT WOULD MAKE A VERY GOOD SLOW SPEED DYNAMOMETER.' HE HAS GOT AN ORDER FOR ONE WHICH IS TO RUN AT A GREAT SPEED & HE SAID THAT HE DID NOT LIKE TO TRY MY PLAN ON ACCOUNT OF THE SPEED. BUT I THINK I HAVE PROVED THAT THE SPEED WILL NOT MATTER: I HAVE WRITTEN THE PROOF OUT & SHOWED IT TO HIM BUT HAVE NOT SEEN HIM SINCE HE HAS LOOKED OVER IT, I THINK THAT THERE IS JUST A CHANCE OF HIS MAKING IT YET. WHICH EXCITES ME VERY MUCH. UNCLE RAS IS VERY WELL NOW, HE HAD A VERY BAD ATTACK OF AGUE ONE DAY BUT BEFORE THAT HE WAS MOST REMARKABLY WELL, & SINCE THEN HE HAS BEEN VERY NEARLY AS WELL AS BEFORE I THINK

YOURS AFFEC. BRO. | H O R A C E D A R W I N .

DAR 258: 859

### **13 Postcard from Karl Beger, [12 February 1877], Hamburg**

Though no man of science but a simple bookkeeper, I have read and admired in the highest degree all your immortal works. My dear little wife, *who understands and loves you*, having presented me yesterday with a pretty boy, I have taken the liberty to give him your illustrious name. I most humbly beg to ask your kind permission, and promise to make him *an honorable man*.

Believe me, | Dear Sir, | Your humble servant | Carl Beger.

Hamburg, on Charles Darwins 70<sup>th</sup>. Birthday.

Charles Darwin Esq<sup>re</sup>. | FRS | England

DAR 201: 3

### **Wall Box**

#### **Printed form letter, 1860s - 1870s**

*Down, Beckenham, | Kent*

*MR. DARWIN is much obliged for the letter just received. Owing to the large number of communications which daily arrive, he regrets to say that it is almost impossible for him to do more than to acknowledge their receipt and express his thanks.*

DAR 133.1: 1

## Case 2

### 1 Letter to [W. H. Scott?], 16 November [1875?]

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

Nov 16

Dear Sir

I am much obliged to you for your kindness in sending me the very remarkable & trustworthy case of reason in the dog.—

Dear Sir | Yours faithfully | Ch. Darwin

MS Add 10409 [printer's block]

### 3 Letter to William Kemp from Thomas Walker, 15 February 1844

*[Text available through the 'Ruth Cramond' collection on [epsilon.ac.uk](http://epsilon.ac.uk), a partnership of the Darwin Correspondence Project and others to publish 19<sup>th</sup> century science letters in a cross-searchable format. Images and text also available on [Cambridge Digital Library](http://Cambridge Digital Library)]*

Cupar | Feby 15. | 184⟨4⟩

Dear Sir

As you will see from the papers I send you along with t(his) I have got into a sm⟨all⟩ controversy about Mr ⟨Wm⟩ Kemp and Tar burning. I could not possibly sit i⟨n⟩ silence and see an “Old Crony” as our Musselburg⟨h⟩ friend calls you, stript ⟨of⟩ his dues. I do not k⟨now⟩ who this Musselburgh m⟨an⟩ is, but he seems to hav⟨e⟩ some knowledge of both you and me. I sent y⟨ou⟩ a copy of the Sentinel last week, but in case you may not have got it, I send you another that you see the whole affair. I would thank you to return me the two Journals I herewith send you as I wish to keep them. You may do what you like with the Sentinel. Favour me with a letter at your convenience. It will not do to let Mr M<sup>c</sup>Gill monopolize all the praise. When are y⟨ou⟩ coming to Fife now? Th⟨e⟩ weather will soon be in trim for geologizing—

I have rather been complaining since I saw yo⟨u⟩ but am getting better again. I would be a great deal the better of a week among th⟨e⟩ hills with you. Hop⟨ing⟩ you are all well wit⟨h⟩ best respects to Minny | I am Dear Sir | Yours faithfully | Thomas Walke⟨r⟩

CUL MS Add 10252/33

### 4 Letter to William Kemp, 1 November [1843]

*[Text also available through the 'Ruth Cramond' collection on [epsilon.ac.uk](http://epsilon.ac.uk). Images and text also available on [Cambridge Digital Library](http://Cambridge Digital Library)]*

Down, near Bro⟨m⟩ley, | Kent | Nov. 1<sup>st</sup>.

Dear Sir

I have been detained from home, otherwise I sh<sup>d</sup> have sooner answered your last communication.—

I am sorry to say that M<sup>r</sup> Babington (who from the te(s)timony of several Botanists, is a first-rate judge in the genus *Atriplex*) pronounces the plant reared at the Hort. Soc. to be *A. angustifolium*; I enclose his le(t)ter, as you might like to see it.— I am quite p(u)zzled what to recommend you about publication,—b(u)t first let me say, that, from what I have seen (of) your ardent love of natural science, I would as soon believe myself capable of playing a hoax o(n) the public, as I would believe you to be so.— T(h)e most sceptical of the public w<sup>d</sup> only su(s)pect an accidental error. For myself I believe the seeds had been deposited, as you describe & that they produced the plants, you sent to Prof. H(e)nslow.— and I think I have shown by my (co)rrespondence, that I do not underrate the interest of the (most of line missing) seeds were sown, & with you, two species of *Polygonum*. *P. aviculare* & *P. convolvulus* (as Henslow informs me) & one of *Atriplex* came up, whereas at the Hort. Soc. another species or variety of *Atriplex* (for I sh<sup>d</sup> have told you, that the *Atriplex* reared by you is undoubtedly according to Henslow the *A. patula*) & a *Rumex R. acetosella*: therefore 3. genera, and 4 or 5 species have come up from 2 kind of seeds.— Moreover unfortunately the plants are just those, which might be expected to come up from garden soil. If the *Atriplex* had turned out a new species that alone w<sup>d</sup> have been good evidence; or even if the same variety of this genus had come up with you and at the Hort. Soc, there w<sup>d</sup> have been some evidence. I daresay the *Rumex* at the Hort. Soc was from a seed in the soil, but the Public (when all the facts are stated, which of c(our)se you would, wou(ld) I fear, say it is more probable that the seeds of all these plants were contained in the soil, than that such ancient seeds sh<sup>d</sup> retain their vitality.— I believe differently myself; but I declare I do not see, how you can make your case sufficiently clear, to justify its publication.— I take great blame to myself that I did not urge Prof. Lindley to plant the seeds in a circle or in a perfectly straight line, for in that case, or had you done so, the evidence w<sup>d</sup> have been good for the majority of the plants which came up, though even then not for every individual one.

Let me hear what you think of what I have said.— I have all your papers safe & w<sup>d</sup> return them to you at anytime— I assure you, I feel extremely disappointed at the prospect (of) your trouble being thrown away, & of so curious (& as I believe real) (a) fact being lost— I trust you will credit me, when I say, that I have done as much in endeavouring to obtain information for you, as I could if I had discovered the seeds myself; & that I have advised you to the best of my judgment.—

Believe me, dear Sir | Yours very faithfully | C. Darwin

MS Add 10252/18

## Wall

### Letter to the secretary of the Post Office, [1845 - 51?]

To the Secretary of the Post-office.

We the undersigned the principal inhabitants of the village of Down in Kent & its neighbourhood, respectfully request your attention to the arrangement for the delivery our letters. Although but a small place we receive an average from 50 to 60 letters & newspapers &c. daily.— A letter posted



by general post in London we receive not until after 9 o'clock the next morning, which is almost too late to act on that day. But our chief grievance is that a letter written in any part of Great Britain (except a few places in the South), say on a Monday, is not delivered here till past 9 on the Wednesday morning; such letters lying 23 hours at Bromley, only six miles distant. Our hopes for many months have been raised by constant reports of some improvement; but the present result & only change is that the Down Postman delivers all letters in Farnborough, & our delivery has been delayed a full half-hour.—

By a change granted us a few years ago, our letters leave this place at one o'clock, by which means they get to London in time to be sent out by the general post of the same day, & are delivered within London on that night; we most earnestly pray that this arrangement may not be disturbed.

Hoping that you will consider our case favourably | Sir | Your obedient servants

DAR 96: 6

## Anatomy of a letter

### Wall Box

#### Letter from [John Innes], [after 8 February – August 1855]

Hore. W S. | St Clements Rectory | Oxford—

No communication from Ainslie about water—

Probably I may have mentioned the pointer and setter.

Either M<sup>r</sup>. Tournay of Saltwood or Capt Johnson of Newington had a pointer bitch who appeared perfectly purely bred. being put to a thorough bred pointer dog she produced a litter of puppies which all appeared pure pointers but one which was a black and white brindled setter, with feathered legs and stern, appearing a thorough bred setter— I mention this one instance because I remember the setter well but I was told that in several litters this result occurred and that the setter was always distinctly indicated when pupped. I do not know where Capt Johnson is if alive— M<sup>r</sup>. Tournay I believe still lives at Saltwood— Lynedock Douglas Esq Hythe and Capt Douglas of the Down's Rifles would remember the circumstance and so probably would Gen<sup>l</sup>. and Col. King at Hythe and others.

Nothing is more common than for crossed dogs to take after one parent. The breeds commonly crossed are terriers with bull and pointers with fox hounds. there are often some of the first offspring which have but little sign of cross in their formation but continuing to breed with pointers and terriers the marks of the former cross frequently reappear in individuals

DAR 163: 5

### Case

#### 4 Letter from Fritz Müller, 1 and 3 October 1866

Desterro, Brazil | Octbr. 1<sup>st</sup>. 1866.

My dear Sir.



specimens the stamens and styli of the majority of flowers use to show the characteristic lengths of the form.

It is not very rare to find one or two of the white pollen-less anthers transformed into small leaflets, and I met with a single plant, (now in my garden), in which all the anthers are transformed into petala-like leaves, sometimes exceeding ten in number; the ovaries form five open leaves with green stigmas on the tip, but without ovula, and in the midst of these ovarial leaves there is a great number of petala-like leaves, among which sometimes some sepala may be distinguished.

Although the plant is extremely common (thousands flourishing in the very streets of our town) I have not yet been able to find a single seed-capsule. This sterility may be owing in part to the circumstance of the two forms but seldom growing mingled. You may find your way bordered, for a mile or more, by one of the two forms exclusively, which then suddenly is substituted by the other.

A second very common species of *Oxalis*, with small yellow flowers, is monomorphic, the styli reaching to the level of the longer stamens; this species produces plenty of seed-capsules.

Of a third species with fine crimson flowers, resembling in shape, size and colour those of *Linum grandiflorum*, I have as yet seen only one plant, in which the styli were intermediate in length between the two sets of stamens; although the stigmas of all the flowers were densely covered with pollen, I found no seed-capsules; thus I am inclined to think that it will prove dimorphic or trimorphic.

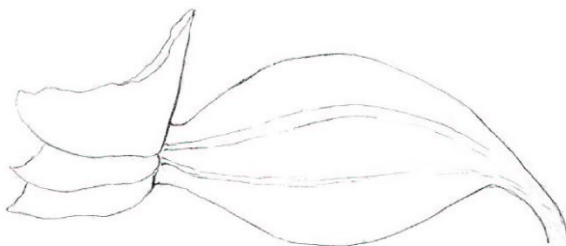
In short, I hope to have an opportunity of examining a fourth species of *Oxalis* with woody stem.—

To the list of dimorphic plants I may add a *Cordia*, of which I enclose a short-styled as well as a long-styled specimen; as soon as they are ripe, I shall send you seeds of either form.—

In my last letter (of Aug. 2<sup>d</sup>) I alluded to the circumstance of brightly coloured seeds remaining attached to the valves of the capsules after their opening. I have since found two more plants, in which this is the case. One is a twining Leguminosa, perhaps a *Rhynchosia*, with black and red seeds. The other is a tree, probably belonging to the Mimoseae, which after the opening of the seed-capsules presents a truly magnificent aspect, being covered over and over with large and elegant curls of pale yellowish silk, (the spirally contracted valves) beset with brilliant red pearls.

Octbr. 3. Yesterday I procured to calculate the number of seeds contained in a large capsule of an Orchid (*Maxillaria?*); the seeds weighed 4212

grains; I extended half a grain of the seeds into a narrow heap and by counting a small portion I found them to be 20661, so that the capsule would contain the prodigious quantity of 1.756.440 seeds!



Hoping that this will find you in good health I am dear Sir with the most sincere respect | Yours very truly | Fritz Müller

Do the aërial roots of all the species of *Catasetum* grow perpendicularly upwards into the air, as they do in our *Cat. mentosum*?

DAR 142: 99; DAR 157a: 103

### 6 Letter from Fritz Müller, 2 August 1866

Desterro, Brazil

August. 2. 66.

My Dear Sir,

Your kind letter of May 23<sup>d</sup> arrived here only July 24<sup>th</sup>.— In the meantime I could observe but a few specimens of one of our species of *Oxalis*; the length of the styli is very variable even in the flowers of the same plant, but whether the species is really trimorphic, I am not yet able to say.

In my last letter I gave you a short account of an Orchid having three fertile stamens. The species is not very rare and I have already seen numerous ears, all the flowers of which showed the same number of fertile stamens. In a closely allied species from Bahia, which I saw in the garden of a Frenchman, I was much surprised at finding but one fertile stamen as in other plants of the tribe. It is curious, that the number of fertile stamens while it furnishes an excellent character for dividing the Orchids into two main groups, in this case is not even of generic value.

Have you ever seen the flowers of *Notylia*? The stigma forms a very narrow slit, to which I found it impossible to cause the pollinia to adhere; at the same time the ovula have a very rudimentary appearance. Thus for some time I supposed the plant to be a male one, but afterwards I met with seed-capsules, the column of which had evidently borne an anthera, while the stigma seemed to have been of the same form as in the supposed male flowers. Besides I convinced myself that in some other Orchids the ovula are in a very rudimentary estate at the time when the flower is expanded and that only some weeks afterwards they become ready for impregnation. This is, for instance, the case with two self-fertilizing species of the tribe of *Epidendreae*; (one of them belongs to the genus *Isochilus*).— Of these species I can send you some drawings if you sh<sup>d</sup> desire it.—

The fine Orchid, of which I send you a drawing, seemed to me to be interesting by the two stigmas being widely separated and situated at the inside of leaf-like processes projecting beyond the anther.—

In your paper on the dimorphism of *Linum* you say that it had occurred to you, that possibly a species might be dimorphic in function, though not in structure. Some observations, which I casually made, seem to confirm this view and to prove that some species are completely barren or nearly so with pollen of the same individual plant. Thus I saw a large plant of an *Octomeria*; it had more than eighty monophyllous stalks, each with about a dozen yellow flowers; on most of the stigmas which I examined I found numerous (6–12) pollinia; but this plant yielded only two seed-capsules. Afterwards I have seen other plants, apparently of the same species, with numerous seed-capsules. I must add, that in the neighbourhood of the first plant I could not detect any other

plant of the same species.— A very large plant of a *Serjania*, widely spreading over a hedge, and as far as I know, about a mile distant from the next plant of the same species, (the same, from which the fig. 20 of my paper on the wood of climbing plants was taken) was covered for many weeks by thousands of flowers, which were visited all the day long by numberless insects, (—humble-bees, beetles, butterflies, etc.—); but only very few seed-capsules were produced in the last weeks and I suppose, from their appearance, that none of these capsules, (as yet unripe), will give good seed.— Last year I had raised some plants of *Eschscholtzia californica*; one of them began to flower about a month before the others, and in this time it yielded not even a single seed-capsule; but no sooner had a neighbouring plant expanded its first flowers, than germens of the former plant began to swell; the second plant was fertile from the beginning.—

*(one or more sheets missing)*

Seeds use to fall to the ground, as soon as the seed-capsules open and in this case they are commonly dark-coloured; if on the contrary, they remain attached to the open valvæ, in all the cases, I know, either the seeds themselves, or the arillus, or the interior of the valvæ are brightly coloured so as to attract the attention, which may carry the seeds to distant places. Thus the large valvæ of a *Tabernaemontana* are filled with a bright red pulpa;—the black and shining seeds of a *Paullinia* are half-imbedded into a white arillus and fixed to red valvæ,—and the seeds of a fine small tree related to *Acacia* or *Inga*, which also for some time remain attached to the valvæ, are black and white and visible at a great distance.—

*(half a page excised)* hundred miles beyond the actual sea-coast was once covered by a huge glacier!—?—

I am very glad to hear that Nägeli and my old friend Oscar Schmidt are with us.— My brother, Dr. Hermann Müller of Lippstadt, (Prussia) who is thoroughly conversant with the natural history of mosses, is collecting the facts bearing on the change of species, which these plants may offer.—

Wishing that this letter may find you in good health, believe me, dear Sir, with sincere respect very truly yours

DAR 76: B33, 33a; DAR 157a: 81, 102; DAR 142: 38

## The Beagle voyage: Learning for a lifetime

### Wall box

#### Letter from Sarah Owen [27 - 30 September 1831]

My dear Charles,

You see I am as good as my word, or rather M<sup>r</sup> Baker is, for I enclose the *promised* Pin, the hair is genuine, & I am much flattered in the idea that it is destined to accompany you round the world—

We all felt very melancholy after your departure on Sunday, I do not know what Woodhouse will do without you for so long, but I hope & trust we may both meet with success in our respective new careers, & live to meet here again *very very* often; remember your promise about N<sup>o</sup>.1, Belgrave S<sup>t</sup>. & pray think of me in the mean time, & write whenever you have an idle half hour. I

assure you my parting promise to you shall be most religiously kept, & you may expect a true & correct account from the Pen of the Sufferer herself—

I am so glad you have a short reprieve for the sake of your Family, though perhaps *you* are not so well pleased with the delay—

God bless you, my dear Charles, believe that whenever I may change my *title*, I shall always remain your very sincere & affectionate Friend | Sarah—

DAR 204: 61

## Case

### 2 Letter to Robert FitzRoy, [28 August 1834]

My direction is the Fonda Inglese | S<sup>t</sup>. Iago.

Thursday

My dear Fitz Roy,

I arrived at this gay city late last night, and am now most comfortably established at an English Hotel. My little circuit by Quellota and Aconcagua was exceedingly pleasant The difficulty in ascending the Campana is most absurdly exaggerated We rode up 5/6<sup>ths</sup> of the height to a spring called the Aqua del Guanaco & there bivouacked for two nights in a beautiful little arbor of Bamboos. I spent one whole day on the very summit, the view is not so pictur-escape as interesting from giving so excellent a plan of the whole country from the Andes to the sea— I do not think I ever more thoroughly enjoyed a days rambling. From Quellota I went to some Copper Mines beyond Aconcagua situated in a Ravine in the Cordilleras The major domo is a good simple hearted Cornish Miner— It would do Sullivan good to hear his constant exclamation “As for London—what is London? they can do anything in my country.” I enjoyed climb-ing about the mountains to my hearts content the snow however at present quite prevents the reaching any elevation— On Monday my Cornish friend and myself narrowly escaped being snowed in. we were involved in a multitude of snow banks, and a few hours afterwards there was a heavy snow-storm which would have completely puzzled us— The next morning I started for this place. I never saw anything so gloriously beautiful as the view of the mountains with their fresh and brilliant clothing of Snow— Altogether I am delighted with the Country of Chile— The country Chilenos themselves appear to me a very uninteresting race of people— They have lost much individual character in an *essay towards an approximation* to civilization My ride has enabled me to understand a little of the Geology—there is nothing of particular interest—all the rocks have been frizzled melted and bedevilled in every possible fashion. But here also the “con-founded Frenchmen” have been at work. A M: Gay has given me to day a copy of a paper, with some interesting details about the Geology of this province published by himself in the *Annales des Sciences*— I have been very busy all day, and have seen a host of people. I called on Col. Walpole, but he was in bed—or said so.— Corfield took me to dine with a M<sup>r</sup> Kennedy, who talks much about the Adventure & Beagle; he says he saw you at Chiloe— I have seen a strange genius a Major Sutcliffe. he tells me as soon as he heard there were two English Surveying Vessels at Valparaiso, he sent a Book of old Voyages in the Straits of Magellan to M<sup>r</sup> Caldcleugh to be forwarded to the Commanding Officer as they might prove of service— He has not heard, whether M<sup>r</sup> Caldcleugh has sent them to you— I told him I

would mention the circumstance when I wrote.— The Major is inclined to be very civil— I do not know what to make of him. He is full of marvellous stories; and to the surprise of every one every now & then some of them are proved to be true— My head is full of schemes; I shall not remain long here, although from the little I have yet seen I feel much inclined to like it. How very striking & beautiful the situation of the city is— I sat for an hour gazing all round me, from the little hill of S<sup>t</sup> Lucia. I wish you could come here to readmire the glorious prospect— I can by no means procure any sort of Map.— you could most exceedingly oblige me if you would get King to trace from Miers a little piece of the Country from Valparaiso to a degree south of R. Rapel—without any mountains. I do not think it will be more than ½ an hours work— I have some intention of returning to Valparaiso by the Rapel.— If you would send me this soon and half a dozen lines, mentioning, if you should know anything about the Samarangs movements; it would assist me in my schemes very much—

Adios, dear Fitz Roy | y<sup>r</sup>. faithful Philos. | C. D.

DAR 144: 115

### 3 Letter to Caroline Darwin, 13 October 1834

Valparaiso.

October 13<sup>th</sup>. 1834.

My dear Caroline

I have been unwell & in bed for the last fortnight, & am now only able to sit up for a short time. As I want occupation I will try & fill this letter.— Returning from my excursion into the country I staid a few days at some Goldmines & whilst there I drank some Chichi a very weak, sour new made wine, this half poisoned me, I staid till I thought I was well; but my first days ride, which was a long one again disordered my stomach, & afterwards I could not get well; I quite lost my appetite & became very weak. I had a long distance to travel & I suffered very much; at last I arrived here quite exhausted. But Bynoe with a good deal of Calomel & rest has nearly put me right again & I am now only a little feeble.— I consider myself very lucky in having reached this place, without having tried it, I should have thought it not possible; a man has a great deal more strength in him, when he is unwell, than he is aware of. If it had not been for this accident, my ride would have been very pleasant. I made a circuit, taking in St Iago. I set out by the valley of Aconcagua I had some capital scrambling about the mountains. I slept two nights near the summit of the Bell of Quillota. This is the highest mountain out of the chain of the Andes, being 4700 ft high. The view was very interesting, as it afforded a complete map of the Cordilleras & Chili.— From here I paid a visit to a Cornish miner who is working some mines in a ravine in the very Andes. I throughly enjoyed rambling about, hammer in hand, the bases of these great giants, as independently as I would the mountains in Wales. I reached the Snow but found it quite impossible to penetrate any higher.— I now struck down to the South, to St Iago the gay Capital of Chili.— I spent a very pleasant week there, receiving unbounded hospitality from the few English merchants who reside there.— Corfield was there also & we lived together at an inn.— St Iago is built on a plain; the basin of a former inland sea; the perfect levelness of this plain is contrasted in a strange & picturesque manner with great, snow topped mountains, which surround it.— From St Iago I proceeded to S. Fernando about 40 leagues to the South.— Every one in the city talked so much about the

robbers & murderers, I was persuaded to take another man with me, this added very much to the expense; & now I do not think it was necessary. Altogether it has been the most expensive excursion, I ever made, & in return I have seen scarcely enough of the Geology to repay it.— I was however lucky in getting a good many fossil shells from the modern formation of Chili.—

On my road to S. Fernando, I had some more hammering at the Andes, as I staid a few days at the hot springs of Cauquenes, situated in one of the valleys.— From S. Fernando I cut across the country to the coast & then returned, as I have said very miserable to Corfields house here at Valparaiso. You will be sorry to hear, the Schooner, the Adventure is sold; the Captain received no sort of encouragement from the Admiralty & he found the expense (of) so large a vessel so immense he determined at once to (give) her up.— We are now in the same state as when we left England with Wickham for 1<sup>st</sup> Lieut, which part of the business anyhow is a good job.— we shall all be very badly off for room; & I shall have trouble enough with stowing my collections. It is in every point of view a grievous affair in our little world; a sad tumbling down for some of the officers, from 1<sup>st</sup>. Lieut of the Schooner to the miserable midshipmans birth.—& many similar degradations.— It is necessary also to leave our little painter, Martens, to wander about y<sup>e</sup> world.— Thank Heavens, however, the Captain positively asserts that this change shall not prolong the voyage.—that in less than 2 years we shall be at New S. Wales.—

I find being sick at stomach inclines one also to be home-sick. In about a fortnight the Beagle proceeds down the coast, touches at Concepcion & Valdivia & sets to work behind Chiloe. I suspect we shall pay T del Fuego another visit; but of this good Lord deliver us: it is kept very secret, lest the men should desert; every one so hates the confounded country. Our voyage sounded much more delightful in the instructions, than it really is; in fact it is a survey of S. America, & return by the C. of Good Hope instead of C. Horn. We shall see nothing of any country, excepting S. America. But I ought not to grumble, for the voyage is for this very reason, I believe, much better for my pursuits, although not nearly so agreeable as a tour.— I will write again before sailing. I am however at present deeply in debt with letters. I received shortly since a very kind long one from M<sup>r</sup> Owen, which I will shortly answer.— Letter writing is a task, which I thoroughly dislike.— I do not mean writing to home: but to any body else, for really after such interval I have nothing to tell but my own history, & that is very tedious.—

I have picked up one very odd correspondent, it is M<sup>r</sup> Fox the Minister at Rio. (it is the M<sup>r</sup> Fox, who in one of Lord Byrons letters is said to be so altered after an illness that his *oldest Creditors* would not know him)

I forgot to thank Susan for her letter of May & Catherine for her pithy message “We do not write” because M<sup>r</sup> Owen does.— I must previously have acknowledged your long letter for the foregoing month.—

We are all here in great anxiety to hear some political news. A Ship sailed from Liverpool just after L<sup>d</sup> Greys resignation & we cannot guess who will succeed him.—

Give my best love to my Father & all of you & Believe me my very dear Caroline | Yours affectionately | Charles Darwin.—



#### 4 From Thomas Sutcliffe [28 August – 5 September 1834]

La Cordillera

The Hacienda la Calera belongs to the Late Presidente Sr. Fco Ruiz Tagle, who resides on his Estate, and is about six leagues from Santiago. It is well worth your while to pass by it—

Beluco belonging to the Marques de Larrayn is 6 leagues from la calera—and Aculeu is about 2 leagues from beluco, the lake is about 3 leagues from the houses of Aculeu.

a Mr Bruce formerly a master in H. M.s Navy lives about a league and half from rancagua, it wd be worth your while to visit him.

To Sr. Pedro Urriola comandante of the canton of rio claro, who resides on the estate of Sr. Fco. Valdivieso Sr I wd. advise you to visit, the estate is 3 leagues from Rancagua after you pass the cachapaul, rancagua is about 22 leagues from Santiago, and 15 leagues from San Fernando—I have merely scratched this croquis out by the rule of thumb, it may serve you as a reference, on the route you intend to take – the dotted lines mark the principal roads—

at Pelequin a road branches of towards the lake but I have only gone to it by Nancagua by the one dotted—

The mines of yaquil belonging to Zacarias Nixon Esqr. an american are half a league from Nancagua, and a road to the lake of Tagua Tagua passes by his house. Nancagua is about 6 leagues from Sn. Fernando, to the Intendent of the Province Dn. Feliciano Silva, who resides there I have given you a letter; also one to Zacarias Nixon Esqr.

DAR 35.2: 405

#### **6 Note from George Darwin to Thomas McKenny Hughes, 22 January 1897**

*From GH Darwin, Cambridge*

On Thursday two packing cases were despatched for you at Geolog. Museum containing Geolog. Specimens collected on voyage of Beagle. They are in canvas bags with numbers attached thereto & Frank is trying to find the corresponding catalogue

DAR 236: 7

#### 9 Letter from John Henslow, 31 August 1833

Cambridge

31 Aug<sup>t</sup> 1833

My dear Darwin,

I am afraid that I have been rather negligent in not writing sooner to announce the arrival of your last Cargo which came safe to hand excepting a few articles in the Cask of Spirits which are spoiled, owing to the spirit having escaped thro' the bung-hole— I am now in possession of your letter of last April, which has stirred me up to send you off a few books which I thought might

interest you, & I have (or rather *shall*) write to your Brother to recommend one or two more— The fossil portions of the Megatherium turned out to be extremely interesting as serving to illustrate certain parts of the animal which the specimens formerly received in this country & in France had failed to do. Buckland & Clift exhibited them at the Geological Section (what this means you will learn from the Report I send you)— & I have just received a letter from Clift requesting me to forward the whole to him, that he may pick them out carefully repair them, get them figured, & return them to me with a description of what they are & how far they serve to illustrate the osteology of the Great Beast— This I shall do in another week when I return again to Cambridge— for I am staying at present at Ely & am here merely on Saturday for L. Jenyn's duty tomorrow he having been unwell & advised not to take duty at present— I have popped the various animals that were in the Keg into fresh spirits in jars & placed them in my cellar— The more delicate things as insects, skins &c. I keep at my own house, with the precaution of putting camphor into the boxes— The plants delight me exceedingly, tho' I have not yet made them out—but with Hooker's work & help I hope to do so before long— I never thought of putting your name down to a Tablet we have been erecting to poor Ramsay's memory in Jesus Chapel till lately— As the list has not yet appeared I have ventured to do so for 21/- I propose having an engraving (I think I told you) from an excellent likeness which Miss Jenyns made for me—& this I shall let the subscribers to the Tablet have at whatever the cost price may be, about 10/ or 12/- probably— I am sure from your respect for R's memory I have not done wrong in putting down your name— The comet you speak of is *expected* in 1835, according to calculation—but it seems very doubtful whether the calculation is correct— The papers of course talk nonsense about it, but it is really something out of the ordinary cometical occurrences— M<sup>rs</sup> Henslow produced me a fine girl on June 23, the day before the Association met— It proved quite a breeding week with the Cambridge Ladies M<sup>rs</sup> Clark & M<sup>rs</sup> Willis being confined within a day or two of the same time— I long as much as you do to see the day when we shall be discussing the various events of your voyage together, but I hope also that there is much yet to arrive before you bend your way home again. Not but what I w<sup>d</sup>. have you return immediately if you are really tired out—but you remember how we used to talk of the certainty of many an annoyance that must arrive, & many a wish to be home again— If you propose returning before the whole period of the voyage expires, don't make up your mind in a hurry—but let it be a steady thought for at least a month without one single desire to continue—& if such an event should occur you may fairly conclude that you are sick of the expedition—but I suspect you will always find something to keep up your courage— Send home every scrap of Megatherium skull you can set your eyes upon.—& *all* fossils. Use your sweeping net well for I foresee that your minute insects will nearly all turn out new— (I must write on now to the end as I have transgressed the limits)— I have turned Entomologist myself this summer for my little girls who have started a collection of Insects & Shells—& make me work for them— Poor Stephens has just lost 400£ in a Law suit & we are levying a subscription to help him on with his Illustrations— I delight in your descriptions of the few animals you now & then allude to—

Believe me | affect<sup>y</sup>. y<sup>rs</sup>. | J. S. Henslow

DAR 97: B14 - 15

10 To Joseph Hooker, [13 or 20 November 1843]

Down near Bromley | Kent

Monday

My dear Sir

I had hoped before this time to have had the pleasure of seeing you & congratulating you on your safe return from your long & glorious voyage.

But as I seldom go to London, we may not yet meet for some time—without you are led to attend the Geological Meetings.

I am anxious to know what you intend doing with all your materials— I had so much pleasure in reading parts of some of your letters, that I shall be very sorry if I, as one of the Public, have no opportunity of reading a good deal more.— I suppose you are very busy now & full of enjoyment; how well I remember the happiness of my first few months of England—it was worth all the discomforts of many a gale— But I have run from the subject, which made me write, of expressing my pleasure that Henslow, (as he informed me a few days since by letter) has sent to you my small collection of plants— You cannot think how much pleased I am, as I feared they w<sup>d</sup> have been all lost & few as they are, they cost me a good deal of trouble.— There are a very few notes, which I believe Henslow has got describing the habitats &c of some few of the more remarkable plants.— I paid particular attention to the Alpine flowers of Tierra Del. & I am sure I got every plant, which was in flower in Patagonia at the seasons, when we were there.— I have long thought that some general sketch of the Flora of that point of land, stretching so far into the southern seas, would be very curious.— Do make comparative remarks on the species allied to the Europæan species, for the advantage of Botanical Ignoramus'es like myself. It has always struck me as a curious point to find out, whether there are many Europæan genera in T. del Fuego, which are not found along the ridge of the Cordillera; the separation in such cases w<sup>d</sup> be so enormous.— Do point out in any sketch you draw up, what genera are American & what Europæan & how great the differences of the species, are, when the genera are Europæan, for the sake of the Ignoramuses.—

I hope Henslow will send you my Galapagos Plants (about which Humboldt even expressed to me considerable curiosity)— I took much pains in collecting all I could,— A Flora of this archipelago would, I suspect, offer a nearly parallel case to that of St Helena, which has so long excited interest.

Pray excuse this long rambling note, & believe me, my dear Sir | Yours very sincerely | C. Darwin

Will you be so good as to present my respectful compliments to Sir W. Hooker.

DAR 114: 1

## Working from home

### Case

#### 1 From Thomas Huxley, 20 July 1868

July 20<sup>th</sup> | 1868

My dear Darwin

Kühne, Professor of Physiology, (and a monstrous clever fellow) has just been to see me, and he wants to know whether there is any possibility of his paying his devotions at the Shrine of D<sup>r</sup>. Darwin.

I have told him that that great Saint though always kind to worshippers is not always in a condition to be worshipped— In fact that the best incense occasionally gives him a sick headache

However I promised to make inquiries & let Kühne know

Tell me what I shall say, if you do not care to see him— If you do his address is 37 Baker S<sup>t</sup>.—unless you prefer to retain me as medium

MS Add. 10334

#### 6 Letter to Joseph Hooker, 26[- 7] March [1864]

Down Bromley Kent

Mar 26

My dear Hooker

Since receiving your pleasant letter of Feb. 9. I have daily been wishing to scribble to you in pencil, but have been unable from having had a good deal more sickness. We have had D<sup>r</sup> Jenner down to see me, who feels sure there is no organic mischief & thinks I shall some day get over the sickness. The last lot of plants are doing well & I am very much obliged for them. They are a great amusem<sup>t</sup> to me & I have one or 2 of them in my bedroom at a time; not that the subject is worth all the trouble I give it.

You once said that you tho<sup>t</sup> Veitch was a mere tradesman. Lately I ordered between 2 & 3£ worth of climbing plants from him. I told him that they were for observation as I begged him to choose growing plants. In answer he sent me more than I ordered & absolutely declined any payment, was not this very handsome, tho' in one sense rather a bore? I am so magnificent that I am thinking of building a large greenhouse & turning the present green house into a cool Stove. Do look how Nepenthes climbs? to which you alluded— You did not answer me about Vanilla but I suppose it climbs by rootlets & if so I do not care—

*Sunday morning—*

Hurrah! I have been 52 hours without vomiting!! I have had a capital letter from John Scott, but I grieve to hear that he has left Bot. Garden & says nothing about the cause or the future. I hope he has not quarrelled.— Pray tell me whether any steps have been taken about his Associateship.

Linn: Soc<sup>y</sup>. I earnestly hope it will not be forgotten— Have you settled for the Duke of North<sup>d</sup>. Man? It must have been a fearful responsibility.—

À propos to what Frankland quotes I sh<sup>d</sup> be very much obliged if you w<sup>d</sup> ask Tyndall when you next see him whether he supposes if only ½ the present amount of snow fell on the Alps, that the climate of Europe fell to that of Greenland, whether the glaciers w<sup>d</sup> not greatly advance? I see the importance of the fall of snow, but does not Frankland exaggerate its importance. F. ought to look into my journal for the extraordinary flexure in the snow line in S. Chile. What superb work Tyndall seems to be doing as I see in the Reader Blessings on the Ed. he gives me a weekly treat.

What a pity it is that Huxley & Falconer sh<sup>d</sup> make their attacks & squabbles so public! Jukes has risen greatly in my opinion from the matter & more especially from the spirit of his letter.

I have 1 or 2 little questions Is E. Blyth settled in Dublin? Is Owen's lecture at Exeter Hall published?

*Please tell me to what order Siphomeris lingua belongs* as I can nowhere find it? I enclose A. Gray's letter tho' remarkable for nothing but its niceness

yours affectionately | Ch Darwin | (a forgery—)

82 plants have now come up from the earth round the partridge's leg

DAR 115: 225

### **7 to John Lubbock, [3 September 1862]**

Cliff Cottage | Bournemouth

Wednesday

My dear Lubbock

I beg a million pardons. Abuse me to any degree but forgive me— it is all an illusion (but almost excusable) about the Bees. I do so hope that you have not wasted any time for my stupid blunder.— I hate myself I hate clover & I hate Bees—

In Haste to catch evening post | Yours sincerely | C. Darwin

DAR 263: 55

### **8 Letter from Margaret Wedgwood, [before 4 August 1862]**

The Manor | Llandudno

Dear Uncle Charles

Of 256 specimens of *Lythrum* gathered this morning from different plants, we find

94 with long pistil

95 — middle length pistil

69 — shortest pistil.

These plants were all in one large field or near it but tomorrow we will go if we can manage it in a different direction for more— We find it rather difficult in gathering to know what are distinct plants and what only offsets. At Criccieth & Aber we thought the different sorts usually grew in plots together mixed with a few of the others but here they are all together. We have found the *Hottonia*, and find the pistils of different lengths as I think you told us.

Your affec niece | M. S. Wedgwood

DAR 181: 64

### 9 Letter to Katherine, Lucy, and Margaret Wedgwood, 4 [August 1862]

Down

4<sup>th</sup>

My dear Angels!

I can call you nothing else.—

I never dreamed of your taking so much trouble; the enumeration will be invaluable. I will write this evening if possible & explain what I have very little doubt is the case with *Lythrum*, & which I am daily working to prove by most laborious crosses.—

But I write now to ask whether you will be more angelic than angels & send me in tin, not tightly packed, with *little* damp (*not* wet) moss (perhaps tied round stems??) 2 or 3 flowers of *both* forms of *Hottonia*: I much wish to measure pollen & compare stigmas.—

GoodBye | My dear Angels | C. Darwin

DAR 185:127

## **The state of things**

### **Wall**

#### Letter from Asa Gray, 10 November 1862

Cambridge, Mass

Nov. 10, 1862

Dear Darwin

Here is a new stamp for L. D.—tho' not postage.

And I shall put this in an envelope embossed with a 20 cent postage stamp. I have really nothing to write this week. I trust I shall receive to-morrow in time for foreign post, some copies of sheets of notices in *Nor. Amer. Journal*,—in which there are two articles upon which I wish your opinion.—

One of them is continuation of remarks on your far-famed Orchid-book.—with the substance of my notes on our species of Ophryd. & Cyripedium.

I am waiting for Capt. Anderson to come to this port that I may send you Cyripediums, &c—for your study next spring. If he does not come over in next Cunarder, I fear I shall have to consign my package to Kew.—where they may get out of the way before Hooker can turn them over to you. We are now wintry with precocious snow; but we still expect a short Indian summer.

I have to thank you for yours of the 16 Oct.—which has been lying a fortnight here. As you do not speak of your family, I conclude they are doing very well.

It is just as I thought about Rothrock's notes on Houstonia. All his projected experiments came to nothing, as I thought.—not well carried out.

It is refreshing to me that you find the Special Correspondent of the Times *detestable*.

Your comments upon our affairs always show such a good spirit, that you need not fear even my wife's "indignation".

We are sorry that you suffer in England; but you must blame the rebels for it, not us,—and your Manchester people should have looked earlier to India for cotton.

You dont see, as you would if here—the total impossibility of coming to any terms of peace with the South, based on their independence. Before that can be they or we must be thoroughly beaten. You can't be expected to see too,—what seems plain to me, that you English would give us no end of trouble, if we attempted a piece-meal existence. We must be strong enough to keep any Old-world power at bay. Then we shall behave pretty well, on the whole,—surely so when the North is dominant and is fairly treated. "Siezing on Canada". What do we want of Canada? When the South was aggressive and making Slave States, we often looked to the *peaceful* acquisition of Canada as desirable as a counterpoise— But when we had "changed all that"—and it is changed, a(nd) slavery limited, past all do(ubt,)—however the combat ends— we no longer have use or need of Canada. If we get set up again, we have work enough at home, & our hands full for years— we shall be strong for defence but weak for aggression. The ill-feeling to England will die out when we are well able to defend ourselves and our home interests.

It does seem that all England *wishes* us to be weak and divided,—perhaps that is good national policy. But the more that is so, the more necessary it is for us to vindicate our integrity, at whatever cost. Let us have it out now, even at the cost of 10 times what it has cost so far.

I never thought anything of American institutions for England. Aristocracy is a natural & needful appendage to Monarchy. You work out your own type—and you will liberalize fast enough,—and leave us to do ours. We'll make it do,—with some jangling.—

I wish we could be *shut up*, like the Japanese of old,—for 10 or 20 years,— —only with a weekly (ma)il from you and D<sup>r</sup>. Hooker, (Fa)re-well.— Ever Yours cordially | Asa Gray.

## Case

### 1 Letter to John Innes, 1 December 1868 (draft)

Dec 1. 68

Dear Innes

I write a line to ask you whether you intend to subscribe this year to the C. & C. Club, as we must immediately have our annual meeting.— I suppose that M<sup>r</sup> Robinson applied to you for your subscription for the Nat. School.— He has suddenly left us to stay for 3 months in Ireland, & as I did not anticipate anything of the kind, I passed over the school account to him, & know nothing about the subscriptions, & have *(5 words illeg)*.— The curate, whom M<sup>r</sup> Robinson has sent here does not appear any great *[acquisition]*. M<sup>r</sup> Horsman, now that he is known to have been a complete & *[premeditated]* swindler (for no other interpretation as it seems to me can be put on his conduct about the Organ) has done much injury in the Parish & some of the subscribers to the School were actually afraid to pay the subscriptions to M<sup>r</sup> Robinson apparently merely for being a clergyman; & what they will think now that he has gone off for 3 months, I know not.— As I fully believe that you are anxious to do all the good that you can to your Parish, I am sure you will allow me to say, that unless you can very soon make some fixed arrangement, so that some respectable man may hold the living permanently, great injury will be done here, which it will take years to repair, & what you will consider of importance the Church will be lowered in the estimation of the whole neighbourhood— Already so staunch a tory & *[church-goer]*, as old Mr Abraham Smith goes to dissenting chapel & *[propounded]* the doctrine so astounding as coming from him, that perhaps the disestablishment of the English Church wd be no bad thing.

I hope that you will *(illeg)* *[reflect]* over the state of things in the Parish, & excuse me for frankly telling you the state of things.—

DAR 96: 53

### 2 Letter from John Innes, 28 January 1870

*Hybrid Cow.— There is at present a hybrid cow among a herd at the home farm of Beaufort—a cross apparently between a deer and a cow. Except in the colour, which is black, the animal is identical in appearance with the one lately sold by Mr Maclean, V.S., to the proprietor of Wombwell's Menagerie.1 Strange to say, both animals were bred in or near the same parish—Ardclach—and in many respects bear a similar history. Mr Lawson, manager at Beaufort, bought the cow last autumn among a herd of cattle, and it has since been seen and admired by many. The hybrid cow in Wombwell's menagerie was calved at Lochindorb, and was exhibited by Mr M'Donald, Blervie, at the Forres Christmas Cattle Show in 1868.*

Dear Darwin

The above is from the Elgin Courant of this day 28th. Janry. 1870. I have not seen the animal. It is satisfactory to know that the former one, which I told you of before, and which seemed if not a hybrid, a wonderfully good imitation of one has gone to a menagerie where perhaps in life or death the truth of its origin may be tested. Some old red deer seems to have taken to erratic courses about the ancient haunts of the Wolf of Badenoch



Faithfully Yours | J Brodie Innes

DAR 167: 27

### **3 Letter from John Innes, 21 January 1871**

Milton Brodie

21<sup>st</sup>. Jan<sup>ry</sup> 1871—

Dear Darwin,

I was right glad to get your letter yesterday, and to see that you are jolly under trying circumstances, like the immortal Mark Tapley. And I am very glad too that you have no grudge against me for the share I had in sending you two such bad lots. I do not acquit myself, for I feel I ought to have stuck to my own post, or given it up at once to the Archbishop, and the result has made me very much lament I did not take a more correct view of duty.

Certainly you and I never were like to quarrel over our differences, thanks mostly to your most kind forbearance with some hot headedness &c. I am sometimes amused at the look of wonder which follows my statement in the midst of a Darwinian theory discussion. "M<sup>r</sup> Darwin is one of my very most valued and dearest friends". I always think so, and say so when occasion offers. Dear me! if some of your naturalist, and my ritualist friends were to hear us two saying civil things to each other, they would say the weather was going to change, or Paris to be relieved, both which I wish might happen— Nobody can tell what that mad fool may do or say, he can do no harm anyway, and I don't see how under any circumstances you could be had up for examination. I have not the least doubt that everything you said was true and only part of the truth, but you could not be called to prove or disprove. I may be, and I don't care in the least if I am. It would not bore me, and even if it did I should accept it as a penance for my bad care of you lambs in the choice of a shepherd. So have no hesitation in saying I am quite willing to be called if necessary. For your guidance, and that of your Solicitor I put in a sort of form, that is separated from gossip and rubbish what I could really say about the Arcades Ambo

I am glad to hear matters have been better of late. When we get a house built, and a Lady in it, all without asking for subscriptions, I hope the Parishioners will come forward liberally and restore and enlarge the Church

I am afraid our sermons are dull that is perhaps our misfortune, It is a wilful sin if we make them long as well. I cut mine down now to 10 or 15 minutes, and every body says they are "so glad when I preach because they are sure of a short sermon". Can anything in the world be ruder? but I bear it like a Christian.

With our united kindest regards to you all | Believe me | Dear Darwin | Yours faithfully | Brodie Innes

DAR 167: 28

### **5 Letter from Lydia Becker, 6 February 1867**

*Manchester Ladies' Literary Society.* | 10 Grove s<sup>t</sup> | Ardwick

Feb. 6. 1867.

My dear Sir

I return you—with more thanks than I know how to express, the two papers which you were so good as to entrust to my care. Will you have the kindness to cause me to be informed of their arrival—having once lost a book-post packet I shall feel a little anxious till I hear they are again in your hands—and this induces me to give you the little extra trouble involved in registering the packet—for which I must apologise.

I have transcribed portions of them, and made large copies of the diagrams— I hope this was not wrong—without your permission, but I thought, as they were printed—I might do so without impropriety.

The arrangements in *Lythrum* are indeed most marvellous. It sets one wondering whether different sized stamens in the same flower can ever be quite without meaning, and if there is any difference in the action of the pollen of the long and short stamens in didynamous and tetradynamous flowers. In the N. O. *Geraniaceae* it seems as if there might be some transition going on—for in *Geranium* each alternate stamen is smaller, and in the allied genus *Erodium* the alternate stamens have become sterile. Can it be possible that this genus was once dimorphic, and one of the female forms having by any means become exterminated, the corresponding set of stamens have shed away? If one of the forms of *Lythrum* were to disappear—two sets of stamens would be made useless to the species, and it is conceivable that they might then gradually become abortive.

I obeyed your directions about the paper on Climbing Plants and the insight into their extraordinary and regular movements was a new revelation to all of us. I made large copies of the diagrams and dived into my herbarium for specimens of each class of climbers, bringing up enough to make a goodly show. Luckily a collection of ferns from the islands of the South Pacific recently presented to me contained a specimen of one named in your paper *Lygodium scandens*. Till I read it I had never dreamed of twiners in this class, as none of our British ferns have the habit, but as the “march of intellect” seems to be the order of the day, even in the vegetable world, there is no telling what they may accomplish in time!

Our society appears likely to prosper beyond my expectations the countenance you have afforded has been of wonderful service, and I do hope that by becoming useful to its members it may prove in some degree worthy of the generous encouragement you have given us.

The ladies who had the privilege of listening to the paper desire to express their thanks to you for it, which I hope you will be pleased to accept.

Believe me to be | yours gratefully | Lydia E. Becker.

DAR 160: 155

### 7 James Torbitt's publication of Darwin's letters.

Down,

Apr. 14, 1876.

My dear Sir,

Prof. Ansted's letter which I return, is a very good one. The more I reflect on your scheme the more I believe it is the one plan for succeeding in getting a sound variety.

During the last 10 years I have been experimenting on crossing plants and shall publish the results in the autumn. The flowers of the genus *Solanum* do not produce nectar and are but little visited by insects, tho' I have seen some on the flowers of the potato. Nevertheless they probably do not get intercrossed so much as the flowers of most other plants. Therefore I would *strongly* advise you to intercross any two varieties, (*and the more they differ in all respects the better*) that is if you can get two varieties which are moderately free of the pest. I *know* that there is the strongest probability that seedlings raised from a cross of this kind would not only grow more vigorously, but would possess greater constitutional vigour, so as to be less liable to disease of all kind and death.

Hoping that you may be successful | I remain, my dear Sir | Yours faithfully | Ch. Darwin.

[Enclosure]

*"Athenenæum Club, London,*

*10th April, 1876.*

*Dear Sir—*

*... .. I have long felt, with you, that the continued repetition of the individual by buds, at least in plants which have the reproductive organs in different individuals, is a very undesirable thing. ... . With regard to the potato and the vine, I can well imagine that the attacks of disease by fungoid growths are greatly facilitated by constitutional weakness, incident to a constant multiplication of tubers, cuttings, or grafts. ... I have read your essay with great interest. ... Natural selection of the strongest, by natural destruction of the weakest, is supported by experience and common sense. ... If I can help you with any experiments I shall be very glad. ...*

*Yours very faithfully, | D. T. Ansted."*

DAR 148: 93; *Belfast News-Letter*, 22 April 1876, p. 2

### **8 Telegram from the Naples Zoological Station, 12 February 1879.**

*Handed in at the Naples 12.10.43 am Office at 3.55 .m. Received here at 4.35 .m.*

*From C J*

*To Charles Darwin | Beckenham Kent Engla*

The Zoological Stations of Naples and the naturalists diferent nations there assembled presents their warmest congratulations to the veteran of Modern Zoology on the occaison of this seventieth Birthday

DAR 172: 2

# Writing and re-writing Origin

## Large Case

### 2 1857 Outline of Species Theory, draft, September 1857 DAR 6: 56r

[On display is a copy kept by Darwin of the enclosure to the [letter to Asa Gray, 5 September \[1857\]](#). The transcription is taken from the version in the Archives of the Gray Herbarium, Harvard University (48)]

I. It is wonderful what the principle of Selection by Man, that is the picking out of individuals with any desired quality, and breeding from them, and again picking out, can do. Even Breeders have been astonished at their own results. They can act on differences inappreciable to an uneducated eye. Selection has been *methodically* followed in *Europe* for only the last half century. But it has occasionally, and even in some degree methodically, been followed in the most ancient times. There must have been, also, a kind of unconscious selection from the most ancient times,—namely in the preservation of the individual animals (without any thought of their offspring) most useful to each race of man in his particular circumstances. The “rogueing” as nurserymen call the destroying of varieties, which depart from their type, is a kind of selection. I am convinced that intentional and occasional selection has been the main agent in making our domestic races. But, however, this may be, its great power of modification has been indisputably shown in late times. Selection acts only by the accumulation of very slight or greater variations, caused by external conditions, or by the mere fact that in generation the child is not absolutely similar to its parent. Man by this power of accumulating variations adapts living beings to his wants,—he may be said to make the wool of one sheep good for carpets and another for cloth &c.—

II. Now suppose there was a being, who did not judge by mere external appearance, but could study the whole internal organization— who never was capricious,—who should go on selecting for one end during millions of generations, who will say what he might not effect! In nature we have some *slight* variations, occasionally in all parts: and I think it can be shown that a change in the conditions of existence is the main cause of the child not exactly resembling its parents; and in nature geology shows us what changes have taken place, and are taking place. We have almost unlimited time: no one but a practical geologist can fully appreciate this: think of the Glacial period, during the whole of which the same species of shells at least have existed; there must have been during this period millions on millions of generations.

III. I think it can be shown that there is such an unerring power at work, or *Natural Selection* (the title of my Book), which selects exclusively for the good of each organic being. The elder De Candolle, W. Herbert, and Lyell have written strongly on the struggle for life; but even they have not written strongly enough. Reflect that every being (even the Elephant) breeds at such a rate, that in a few years, or at most a few centuries or thousands of years, the surface of the earth would not hold the progeny of any one species. I have found it hard constantly to bear in mind that the increase of every single species is checked during some part of its life, or during some shortly recurrent generation. Only a few of those annually born can live to propagate their kind. What a trifling difference must often determine which shall survive and which perish—

IV. Now take the case of a country undergoing some change; this will tend to cause some of its inhabitants to vary slightly; not but what I believe most beings vary at all times enough for selection to act on. Some of its inhabitants will be exterminated, and the remainder will be

exposed to the mutual action of a different set of inhabitants, which I believe to be more important to the life of each being than mere climate. Considering the infinitely various ways, beings have to obtain food by struggling with other beings, to escape danger at various times of life, to have their eggs or seeds disseminated &c. &c, I cannot doubt that during millions of generations individuals of a species will be born with some slight variation profitable to some part of its economy; such will have a better chance of surviving, propagating, this variation, which again will be slowly increased by the accumulative action of Natural selection; and the variety thus formed will either coexist with, or more commonly will exterminate its parent form. An organic being like the woodpecker or misletoe may thus come to be adapted to a score of contingencies: natural selection, accumulating those slight variations in all parts of its structure which are in any way useful to it, during any part of its life.

V. Multiform difficulties will occur to everyone on this theory. Most can I think be satisfactorily answered.— “Natura non facit saltum” answers some of the most obvious.— The slowness of the change, and only a very few undergoing change at any one time answers others. The extreme imperfections of our geological records answers others.—

VI. One other principle, which may be called the principle of divergence plays, I believe, an important part in the origin of species. The same spot will support more life if occupied by very diverse forms: we see this in the many generic forms in a square yard of turf (I have counted 20 species belonging to 18 genera),—or in the plants and insects, on any little uniform islet, belonging almost to as many genera and families as to species.— We can understand this with the higher, animals whose habits we best understand. We know that it has been experimentally shown that a plot of land will yield a greater weight, if cropped with several species of grasses than with 2 or 3 species. Now every single organic being, by propagating so rapidly, may be said to be striving its utmost to increase in numbers. So it will be with the offspring of any species after it has broken into varieties or sub-species or true species. And it follows, I think, from the foregoing facts that the varying offspring of each species will try (only few will succeed) to seize on as many and as diverse places in the economy of nature, as possible. Each new variety or species, when formed will generally take the places of and so exterminate its less well-fitted parent. This, I believe, to be the origin of the classification or arrangement of all organic beings at all times. These always **seem** to branch and sub-branch like a tree from a common trunk; the flourishing twigs destroying the less vigorous,—the dead and lost branches rudely representing extinct genera and families.

This sketch is *most* imperfect; but in so short a space I cannot make it better. Your imagination must fill up many wide blanks.— Without some reflexion it will appear all rubbish; perhaps it will appear so after reflexion.— | C. D.

This little abstract touches only on the accumulative power of natural selection, which I look at as by far the most important element in the production of new forms. The laws governing the incipient or primordial variation (unimportant except as to groundwork for selection to act on, in which respect it is all important) I shall discuss under several heads, but I can come, as you may well believe, only to very partial & imperfect conclusions.—

## 6 Letter from Alfred Wallace, 2 July 1866

Hurstpierpoint, Sussex | July 2nd. 1866.

My dear Darwin

I have been so repeatedly struck by the utter inability of numbers of intelligent persons to see clearly or at all, the self acting & necessary effects of *Nat Selection*, that I am led to conclude that the term itself & your mode of illustrating it, however clear & beautiful to many of us are yet not the best adapted to impress it on the general *naturalist public*. The two last cases of this misunderstanding are, 1<sup>st</sup>. The article on "*Darwin & his teachings*" in the last "Quarterly Journal of Science", which, though very well written & on the whole appreciative, yet concludes with a charge of something like blindness, in your not seeing that "Natural Selection" requires the constant watching of an intelligent "chooser" like man's selection to which you so often compare it;—and 2<sup>nd</sup>., in Janet's recent work on the "*Materialism of the present day*", reviewed in last Saturday's "Reader", by an extract from which I see that he considers your weak point to be, that you do not see that "*thought & direction are essential to the action of 'Nat. Selection'.*" The same objection has been made a score of times by your chief opponents, & I have heard it as often stated myself in conversation.

Now I think this arises almost entirely from your choice of the term "*Nat. Selection*" & so constantly comparing it in its effects, to *Man's selection*, and also to your so frequently personifying *Nature* as "*selecting*" as "*preferring*" as "*seeking only the good of the species*" &c. &c. To the few, this is as clear as daylight, & beautifully suggestive, but to many it is evidently a stumbling block. I wish therefore to suggest to you the possibility of entirely avoiding this source of misconception in your great work, (if not now too late) & also in any future editions of the "Origin", and I think it may be done without difficulty & very effectually by adopting Spencer's term (which he generally uses in preference to *Nat. Selection*) viz. "*Survival of the fittest.*"

This term is the plain expression of the facts,—*Nat. selection* is a metaphorical expression of it— and to a certain degree *indirect & incorrect*, since, even personifying *Nature*, she does not so much *select* special variations, as exterminate the most unfavourable ones.

Combined with the enormous multiplying powers of all organisms, & the "struggle for existence" leading to the constant destruction of by far the largest proportion,—facts which no one of your opponents, as far as I am aware, has denied or misunderstood,—"*the survival of the fittest*" rather than of those who were less fit, could not possibly be denied or misunderstood. Neither would it be possible to say, that to ensure the "*survival of the fittest*" any *intelligent chooser* was necessary,—whereas when you say *natural selection* acts so as to choose those that are fittest it is misunderstood & apparently always will be. Referring to your book I find such expressions as "Man selects only for his own good; *Nature* only for that of the being which she tends". This it seems will always be misunderstood; but if you had said "Man selects only for his own good; *Nature*, by the inevitable "*survival of the fittest*", only for that of the being she tends",—it would have been less liable to be so.

I find you use the term "Natural Selection" in two senses, 1<sup>st</sup> for the simple preservation of favourable & rejection of unfavourable variations, in which case it is equivalent to "*survival of the fittest*",—or 2<sup>nd</sup>. for the effect or change, produced by this preservation, as when you say, "To sum up the circumstances favourable or unfavourable to natural selection", and again "Isolation, also, is an important element in the process of natural selection",—here it is not merely "survival of the

fittest” but, change produced by survival of the fittest, that is meant— On looking over your fourth Chap. I find that these alterations of terms can be in most cases easily made, while in some cases the addition of “*or survival of the fittest*”, after “*natural selection*” would be best; and in others, less likely to be misunderstood, the original term may stand alone.

I could not venture to propose to any other person so great an alteration of terms, but you I am sure will give it an impartial consideration, and if you really think the change will produce a better understanding of your work, will not hesitate to adopt it.

It is evidently also necessary not to *personify* “nature” too much,—though I am very apt to do it myself,—since people will not understand that all such phrases are metaphors.

*Natural selection*, is, when understood, so necessary & self evident a principle, that it is a pity it should be in any way obscured; & it therefore occurs to me, that the free use of “*survival of the fittest*”,—which is a compact & accurate definition of it,—would tend much to its being more widely accepted and prevent its being so much misrepresented & misunderstood.

There is another objection made by Janet which is also a very common one. It is that the chances are almost infinite again the particular kind of variation required being coincident with each change of external conditions, to enable an animal to become modified by Nat. Selection in harmony with such changed conditions; especially when we consider, that, to have produced the almost infinite modifications of organic beings this coincidence must have taken place an almost infinite number of times.

Now it seems to me that you have yourself led to this objection being made, by so often stating the case *too strongly* against yourself. For Example, at the Commencement of Chap. IV. you ask, if it is “*improbable that useful variations should sometimes occur in the course of thousands of generations*”;—and a little further on you say, “*unless profitable variations do occur natural selection can do nothing.*” Now such expressions have given your opponents the advantage of assuming that favourable variations are rare accidents, or may even for long periods never occur at all, & thus Janet’s argument would appear to many to have great force. I think it would be better to do away with all such qualifying expressions, and constantly maintain (what I certainly believe to be the fact) that variations of *every kind* are *always occurring in every part of every species*,—& therefore that favourable variations are *always ready* when wanted. You have I am sure abundant materials to prove this, and it is, I believe, the grand fact that renders modification & adaptation to conditions almost always possible. I would put the burthen of proof on my opponents, to show, that any one *organ structure* or *faculty* does *not vary*, even during one generation among all the individuals of a species,—and also to show *any mode or way* in which any such *organ &c.* does not vary. I would ask them to give any reason for supposing that *any organ &c.* is ever *absolutely identical* at any *one time* in *all the individuals* of a species,—& if not then it is always varying, and there are always materials which, from the simple fact, that “*the fittest survive*”, will tend to the modification of the race into harmony with changed conditions.

I hope these remarks may be intelligible to you, & that you will be as kind as to let me know what you think of them.

I have not heard for some time how you are getting on.

I hope you are still improving in health, & that you will be able now to get on with your great work for which so many thousands are looking with interest.

With best wishes | Believe me My dear Darwin | Yours very faithfully | Alfred R. Wallace—

C. Darwin Esq.

DAR 106: B33 – 8

**8 Letter to the editor. By M.B.W. In *The Field*, Vol. 21, no. 540, Saturday 2 May 1863, p. 416**

*ARE CATS WITH BLUE EYES INVARIABLY DEAF? Mr Darwin states broadly that “cats with blue eyes are invariably deaf.” A few days since I had an opportunity of testing the correctness of this assertion, in the case of a white cat with decided blue eyes, and can positively state that in this case there is no foundation for Mr Darwin’s statement. It would tend to clear up an interesting point in natural history if some of your correspondents would furnish the result of their observations on this point – MBW [In THE FIELD of Aug. 11, 1860, Mr B. F. Capel stated, in reply to a question in a previous number, that there was at that time, at Sproughton near Ipswich, a white cat with blue eyes, and that it was perfectly deaf. ED.]*

**10 Letter from John Rodwell, 31 October 1860**

Belle Vue House | Brox Bourne.

Oct. 31. 1860

Dear Sir/

I have purposely postponed writing to you again until I had an opportunity of making further observations upon my white cat. This I have now done and am able to state positively that although both her eyes are undoubtedly blue she is not in the slightest degree deaf. She has been several times put into a room apart with merely the door left ajar, and has always run immediately, at the summons of myself or of any of the servants, from an adjoining room in which it was impossible for her to see us. My friend D<sup>r</sup>. Hooper of Hoddesdon also assures me that though he has often known cats with both eyes blue to be stone deaf he has also known them to be as fully endowed with the faculty of hearing as any others. But I hope in the course of a short time to be able to report to you upon the state of a large number of white cats, as a friend of his, a lady residing not far hence, is a fancier of white cats and has a large number—of which I could only learn yesterday that some with eyes more or less tinged with pink are entirely deaf.

Now that I am speaking of cats I may mention that my late uncle the Rev. W. Kirby of Barham was a great Cat-fancier all his life, and that he was very careful to breed none but Tabbies, which under his care, I am afraid I must not use the word *selection*, as I do not know that he ever eliminated the smaller cats, though he certainly never would allow a white or Tortoiseshell on his premises, attained to a size I never saw elsewhere. He was extremely proud of his Cats, and had petted this one particular Breed for the nearly sixty years that he was Curate and Rector of Barham. From my childish days, till I became his Curate, I always recollect him with a cat on his Table, Knee or Shoulder. And I have no doubt but that careful Breeding had produced their unusual size.

It may perhaps be worth while to mention that about 1843 when I was Incumbent of S. Peter’s Saffron Hill, a large portion of the old Fleet Sewer (or River), said never to have been before



opened since the days of Q. Elizabeth, was exposed to view. I then saw several enormous Rats which had been taken thence by the workmen, and upon examination they all proved to be blind and almost entirely devoid of hair, and so ferocious were they that the workmen assured me they were deterred from entering the old parts of the Sewer as the rats would unquestionably fly at them. The rats which I saw were taken out at Holborn Bridge, and as there are three arches still remaining there of an old roman Bridge some sixteen or more feet below the present surface, it is *possible* that those rats may have been breeding there for ages, and if like the blind cave animals you mention in C. V. of the Origin of Sp<sup>s</sup>.—their progenitors lost the power of sight a 1000 years since, and lost as they would, I suppose, at the same time any great ability for migration, this would be a curious illustration of a part of your theory. All I can say is that some eight or ten Rats which I saw

DAR 47: 167-8

### **Small Case**

#### **3 Letter from George Harris, 3 March 1862**

20, Glo'ster Street, | Queen Square, | Bloomsbury, W.C.

Mar: 3/62.

Sir,

Being but a poor working man with a wife and four children to support, and further, being one of those "*thirsty souls*" who would drink deep at the fountain of science—when accessible, I have taken the liberty to obtrude upon you and to ask of you the favor of a presentation copy of your great work on the "*Origin of Species*" the price (12/-) being far above my purchasing means. (power)

As I am but a poor tailor, I am desirous not to ask the labor of others for nothing, and shall, in return, be but too glad to do work for the am<sup>t</sup>. "My soul thirsteth after Knowledge" is the apology I make for my obtrusion, and remain, Sir, | yours faithfully, | Geo: E. Harris.

C. Darwin, Esq<sup>r</sup>.

DAR 166.1: 107

#### **4 Letter to George Harris, 5 March [1862]**

Down Bromley Kent | March 5.

Sir

I have directed M<sup>r</sup>. Murray to send to you a copy of the "Origin", & I hope it may in some degree interest you.—

Sir | Yours faithfully | C. Darwin

Private collection

### 5 Letter from Thomas Maston, 5 February 1879

11 Jennetts Crescent | Westgate | Otley | Near Leeds

5/2/79

Dear Sir/

I hope you will not think me too bold in taking upon myself to write to you, begging of you a favour.

I am a stone Mason, and about 2 years ago I bought two of your works the “Origin of Species”, and the “Descent of Man” and I have read them, and studied them the most of this time, and struggled, in my humble way, to defend the theory therein enunciated, against that un-holy cant, which has been risen against it by a certain class of desperate theological thinkers in the hope of provoking ignorant laughter, to shame honest men into silence on this subject, choosing in this way to show their weakness, and to exhibit the truth strength of your conclusions.

I should like your later work i.e., the “Expression of the Emotions in Man and Animals”, but through slack trade for this last 2 years, and this most severe winter it is out of my reach now.

I should be very thankful for any help you can give me,

hoping will not forget me | I am Yours most | respectfully | Thomas Maston

M<sup>r</sup>. Charles Darwin, | M.A., F.R.S.

DAR 171: 88

## **Humans and other animals**

### 1 Letter from Thomas Huxley, 8 December 1874

4 Marlborough Place | NW

Dec. 8<sup>th</sup> 1874

My dear Darwin

Best thanks for Semper It is an interesting but monstrous ill-natured article (he doesn't attack me) and it serves him right that he has not only been completely anticipated by Balfour— but that Balfour's must (foot of page excised) shew that he has altogether blundered in his interpretation of the fact. Indeed the only value of the paper is that it confirms Balfour's statement as to fact

I will return the paper to you as soon as I have made some excerpts

It did us good on Sunday to see how well & (foot of page excised)

P.S. | I forgot to say that I have been making out wonderful things about *Amphioxus* His skull & brain are bigger in proportion (or at any rate longer) to his body than yours— and the theory of the vertebrate skull that I have been grinding at these seventeen years, is I believe just as clear proving at last the great break between *Amphioxus* & other fishes supposed to exist by us all up to this time—is pretty well bridged over & the relations of *Amphioxus* with the Lampreys are as clear as

mud in a wine glass— The skull consists of above 14 segments all trace of which is lost even in the embryo of the higher vertebrates

Three cheers for our great ancestor!— *Prot. Adam* (Haeckel)

DAR 103: 234–5

### **2 3 Charles Darwin's annotated copy of his 'Expression' queries and responses by Christian Ngqika enclosed with [letter from James Weale, 7 July 1867](#)**

*[Transcriptions combined]*

Answers written by Christian Gaika, Constable at Bedford, brother of the Chief Sandilli

*[(1.) Is astonishment expressed by the eyes and mouth being opened wide, & by the eyebrows being raised?]*

Answer to first question, Yes they do open their mouths and rise their eyebrows.

*[(2.) Does Shame excite a blush when the colour of the skin allows it to be visible? Especially how far down the body does the blush extend?]*

Second question Know there is no discolour of the face visible

*[(3.) When a man is indignant or defiant does he frown, hold his body and head erect, square his shoulders and clench his fists?]*

Third question yes when the indignation is much in them, but they do not square their shoulders.

*[(4.) When considering deeply on any subject, or trying to understand any puzzle, does he frown, or wrinkle the skin beneath the lower eyelids?]*

Fourth question yes and some times puts his hand to his chin, and pul his beard

*[(5.) When in low spirits, are the corners of the mouth depressed & the inner corner or angle of the eyebrows raised & contracted by that muscle which the french call the grief muscle?]*

Fifth question Know

*[(6.) When in good spirits do the eyes sparkle, with the skin round and under them a little wrinkled & with the mouth a little drawn back in the corners?]*

Sixth question Know no signe is seen.

*[(7.) When a man sneers or snarls at another, is the corner of the upper lip over the canine teeth raised on the side facing the man whom he addresses?]*

Seventh question Know he lifts his upper lip a little and shows his upper teeth and turns his head on the side of the one he is adressing

*[(8.) Can a dogged or obstinate expression be recognised, which is chiefly shown by the mouth being firmly closed, a lowering brow & a slight frown?]*

eigth question yes they do when fighting

*[(9.) Is contempt expressed by a slight protrusion of the lips & turning up of the nose, with a slight expiration?]*

ninth question Contempt is expressed by smiling and laughing

*[(10.) Is disgust shown by the lower lip being turned down, the upper lip slightly raised, with a sudden expiration something like incipient vomiting?]*

Tenth question yes, but not always.

*[(11.) Is extreme fear expressed in the same general manner as with Europeans?]*

Eleventh question yes the shaking of the body is much experienced and the eyes widely open.

*[(12.) Is laughter ever carried to such an extreme as to bring tears into the eyes?]*

Twelfth question yes that is their common practice.

*[(13.) When a man wishes to show that he cannot prevent something being done or cannot himself do something, does he shrug his shoulders, turn inwards his elbows, extend outward his hands & open the palms?]*

Thirteenth question yes they are then restless and look ashamed to keep their heads up.

*[(14.) Do the children when sulky pout or greatly protrude the lips?]*

Fourteenth question yes and some times shows dishonour to the one

*[(15.) Can guilty, or sly, or jealous expressions be recognised—though I know not how these can be defined?]*

Fifteenth question Guilt can be recognised by the eyes half open, and the chin to the breast, and some times by the movements in the body. jealous by the distemper shown to the party

*[(16.) As a sign to keep silent is a gentle hiss uttered?]*

Sixteenth question yes

*[(17.) Is the head nodded vertically in affirmation, & shaken laterally in negation]*

Seventeenth question yes

DAR 181: 41

#### **4 Letter from Frans Donders, 17 May 1870**

Utrecht, | 17 Mai | 1870

Dear Sir,

Je sens maintenant que j'aurais dû vous donner la triste nouvelle de la perte de notre fille, notre unique enfant, l'objet et le centre de notre vie, notre bonheur et notre espoir. Elle est décédée le 5 du mois de Mars, 5 Jours seulement après avoir donné la vie à deux enfans, qui, quoique bien faibles, lui ont survécu jusqu'ici. Nous vivons dans une tristesse qui semble ne pouvoir plus finir, et autant que mon gendre bien-aimé, le Docteur Engelmann, J'ai eu de la peine à rentrer dans le

laboratoire et à vaquer à nos occupations. Déjà dans les deux derniers mois de la grossesse, notre fille était tellement souffrante, que nous vivions dans une inquiétude continuelle. Sans cela le petit travail que vous m'avez fait l'honneur de me confièr, aurait été terminé depuis long-temps. Pourtant je venais justement d'y remettre la main, et vous le recevrez dans peu de jours. J'ai examiné avec quelque soin la circulation de l'oeil, et il ne me faut qu'un peu de disposition pour finir ma note à ce sujet que je desire publier dans mon Journal: en général je crois pouvoir confirmer les vues de Sir Charles Bell. Le Docteur Moore vous enverra la traduction anglaise de cet article. Quant à l'autre question, je ne pense pas que le rapport supposé existe. Dans ma lettre suivante, je formulerai ma reponse. Soyez sûr de l'avoir en mains dans 8 Jours après la reception de celle-ci, et avant ce temps Mr. Moore aura reçu un exemplaire imprimé de mon article sur la première question.

Veuillez agréer l'assurance de mon respect et de mon affection, | Dear Sir, | Votre bien devoué |  
Donders

[TRANSLATION]

Dear Sir,

I now feel that I ought to have given you the sad news of the loss of our daughter, our only child, the object and the centre of our life, our happiness and our hope. She died on 5th March, just 5 Days after having given birth to two babies, who, though very weak, have survived her up to now. We are living with a sorrow which it seems can never end, and just like my much-loved son-in-law, Doctor Engelmann, I have found it hard to return to the laboratory and to attend to our occupations. In the last two months of her pregnancy, our daughter was already suffering so much that we lived in constant apprehension. But for this the small piece of work you did me the honour of confiding to me, would have been finished long since. However, I have just turned my hand to it once more, and you will have it in a few days. I have examined the circulation of the eye with some care, and all I am lacking is the inclination to finish my note on this subject, which I want to publish in my Journal: in general I believe I can confirm the views of Sir Charles Bell. Doctor Moore will send you the English translation of this article. As for the other question, I don't think that the supposed relationship exists. In my next letter, I shall formulate my reply. You may be certain of having it in hand within a week of receiving this one, and by then Mr. Moore will have received a printed copy of my article on the first question.

Please accept the assurance of my respect and affection, | Dear Sir, | Your most devoted | Donders

DAR 162: 224

### **5 Letter from William Reade, 19 May 1868**

Conservative Club— | S<sup>t</sup>. James' Street

May 19 '68

Dear Sir

I start in ten days or so for the Gold Coast, with the intention of going to Coomassie the capital of Ashanti, and if permitted by the King (which is I fear not very likely) to the country beyond— It is

probable that after my return to the coast I may take up my residence for a time at Whydah or Accra—

I intend to collect plants for the Kew collection, D<sup>r</sup>. Hooker having provided me with the necessary materials, and zoological specimens of every kind— But it is my desire to collect philosophically: I therefore (having first spoken to M<sup>r</sup>. Bates about it) venture to write to you to ask whether you would desire me to make any special inquiries either as to the habits of animals or of the natives— Any instructions which you may honour me with shall be faithfully carried out as far as will lie in my powers, and if I remain on the coast a considerable time as is not impossible, the results shall be communicated to you from time to time— If you are too occupied to write, & would prefer to give me hints verbally I shall be happy to call upon you any day you may be pleased to appoint during the next ten days or so—

My merely superficial acquaintance with zoology will I am afraid prevent me from making any investigations of a very minute or elaborate kind, & of botany I have no knowledge— I am best capable perhaps & certainly chiefly desirous of making inquiries relative to the human race, & hope to be able to make measurements on a large scale— But I beg to repeat that I would give my best attention to anything which could aid in elucidating or affording evidence for those grand problems which you are engaged in expounding—

I remain | Yours obediently | W. Winwood Reade

DAR 176: 33

### **6 Letter from James Crichton-Browne, [6 June 1870]**

With reference to figures 7, 8, 9, 10 & 11. I would venture with great deference to submit that my observations lead me to believe, that awakened attention, especially if accompanied by surprise or wonder is expressed is expressed by the action of the occipito-frontalis, elevation of the eyebrows &c, but that strong, sustained concentrated attention is accompanied by contraction of the corrugators of the eyebrows.

Figures 34 & 23. An exceedingly tragic expression, apparently produced by the powerful contraction of the muscles of the eyebrows, with some elevation of the skin of the forehead & transverse folds. This expression if perfect would produce the so-called 'horse-shoe' on the forehead about which Sir Walter Scott speaks in 'Redgauntlet'. M<sup>rs</sup>. Scott Siddons the actress has the power of producing these lines on the forehead with singular precision. She tells me that all her family have been remarkable for this power. The lines referred are if I remember rightly well seen in Sir Joshua Reynolds portrait of the great M<sup>rs</sup>. Siddons as the Tragic Muse. My brother M<sup>r</sup>. Balfour Browne informs me that the last descendent of the Griersons of Largg (the Redgauntlets of Sir Walter Scott) prides herself on possessing the family peculiarity, the power of producing in a striking manner, the horse-shoe on the forehead.

Figures 16. 17. 18. The action of the pyramidalis nasi does not convey to my mind any idea of an expressive expression. It suggests rather *painful attention*. In cases of profound melancholia I have frequently seen it combined in persistent action with the corrugatores superciliorum, notwithstanding Duchennes statement that they are antagonistic.

Figures 19. & 20. The muscles of the eyebrows are constantly seen in energetic action in cases of melancholia. The lines due to the habitual contraction of this muscle are most characteristic of the physiognomy of melancholia, especially hypochondriacal-melancholia, in which grief & anxiety are felt respecting bodily health & conditions. Along with the contraction of the eyebrows in such cases there is a peculiar acute arching of the upper eyelid which I am at a loss to understand.

Figures 30. 31. 32. & 33. The action of the muscles included in this group is singularly well illustrated in a form of mental disease, well known as the General Paralysis of the insane. "In this malady there is almost invariably optimism, delusions, as to wealth, rank, grandeur &c,—insane joyousness, benevolence & profusion, while its very earliest physical symptom is trembling at the corners of the mouth & outer corners of the eyes. This is a well recognised fact. Constant tremulous agitation of the inferior palpebral & great zygomatic muscles is pathognomic of the earlier stages of general paralysis. The countenance has a pleased, self-complacent & benevolent expression: As the disease advances other muscles become involved but until complete fatuity is reached, the prevailing expression is that of *feeble* benevolence."

Figure 34. To my thinking this is not a grimace but a genuine expression—mirth suppressed by voluntary effort—as when chiding a child for a ludicrous offence.

Figure 38. Presents not the faintest trace of any lascivious feeling but rather contempt disgust, meanness.

Figure 43. The action of the triangularis ori is well seen in young children in whom the angles of the mouth are constantly depressed, as the preliminary of tears. Along with the pulling down of the angles of the mouth, there is some pouting of the lower lip.

Why does not Duchenne deal with other muscles, very influential in the expression of the emotions—such as those regulating the movements of the eyeballs, the buccinator (precisely analogous to the other facial muscles, in that it is attached to the lips &c.) used in laughter, & the masseter; which occasions the grinding & gnashing of the teeth, in extreme rage & despair.

DAR 161: 323

### **7 Letter from Henrietta Darwin, 21 March 1871**

Sea Grove | Bournemouth

March 21<sup>st</sup>

My dear Father

Thank you very much for your letter this morning— I am v. glad old Murray got up his courage for the extra 500 so as to put off the evil day of a 2<sup>nd</sup>. edit— £1470 is a splendid sum & now I hope you will be easy abt Murray's gains in spite of the 128£— I think some of those great novelists only get £3000—I can't remember who—& to think of that kind of book bringing in nearly as m. as ½ a novel is wonderful.

What you say abt my helping has pleased me v. m— The pleasure of doing it rewards me for any trouble I can take over & over again—but to have a say so much, & to feel that at any rate you think I can help you so really, is very sweet to me— The memorial you propose will be very precious to

me. I can't think all of a sudden what I sh<sup>d</sup>. like to have that will be appropriate & lasting— I want it to be something that will seem fitting in the nature of things— & something that I shall like for always— & so deep reflexion is required. It is very good of u to think of it—

Lena insists upon my going to church w her this afternoon—why I don't kno—praps she knows they are pretty hymns—at any rate I have had no choice given me— I shall perhaps be a day or two in London at C Place before Sat<sup>dy</sup>.—when I shall join u at Queen Anne— It seems as if I'd been away ages. I think you'll think me immensely better for my stay here— Edmund is very busy over the Descent & seems to find it very amusing reading—

What a funny Welchman your man must be for if u come from a hairy ape u are likely to be one & tis an argument for you— I haven't seen the 2<sup>nd</sup>. Spec— I'm rather surprised u've had no effect on Wallace. It seems to me his mind can't be so clear as u used to think it for I'm sure u are right. I've been rereading the old Physical basis & have at last worked out on paper my dissatisfaction with it. If he wasn't such a busy man & the article hadn't been worked threadbare & I was likely to see him, I sh<sup>d</sup>. like to see how neatly he w<sup>d</sup>. smash me into a cocked hat— They asked me what a Mission was. but I haven't got energy to tell it now & will give a full acc<sup>t</sup>. in London of a Mission & all its manifestations—

Edmund is still bad— I never saw him so bad at Cannes. Alice is pretty nearly all right again— It will be v. nice to see you all again—praps if I thk hard till then I shall kno what I sh<sup>d</sup>. like for my present the v. best of any thing.

Thank you again dear Father— your most affec | HED

DAR 275: 44

### 12 Letter from Oscar Rejlander, 30 April 1871

1, | *Albert Mansions, | Victoria Street, | S.W.*

Apr. 30/71.

My dear Sir

It is very difficult to get, at will—those expressions you wish— Few have the command or imagination to appear real— — In time—I might catch some— —So I have tried in propria persona— —even cut my moustache shorter to try to please you in the last batch—. The crying baby in the arms of the Boy you see I have had enlarged for you—and so can any other be treated.—

You ask for a Bill— — You cannot pay for good will

For all the photogr<sup>s</sup>. you have had I will lump it at £3.3.— And—for each plate that you may want to make use of I will charge only 10/.

I have worked thro' the two Vol<sup>s</sup> you pleased to present me with— — Whether I agree or not it has helped to enlarge my mind—and certainly my reason cannot believe in the creation of an Adult+ at any one time any more than of a plant jumping full grown into existence.

Yours very truly | O. G. Rejlander

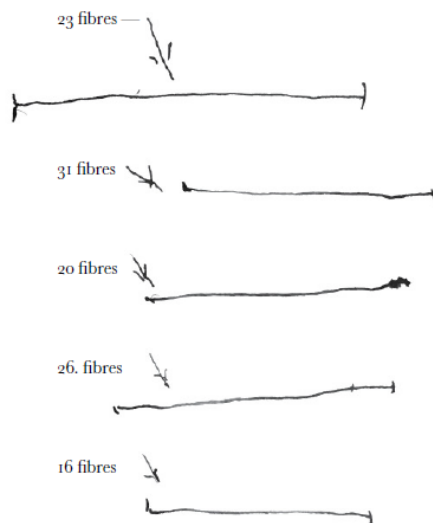


+“And the eyes were opened and the light did not effect them—and could at once discern all the dif. distances— And gastric juice was in the stomach but did not corrode the lining—and the liver & kidneys were active—on nothing— And the blood was there—injected?” — — — It’s getting absurd—

DAR 176: 115

## Plant or animal?

### 4 Letter from Francis and Amy Darwin, 8 August [1874]



Mürren | Sat Aug 8<sup>th</sup>

My dear Father

Thank you very much for yr little letter— I will explain the hieroglyphics above directly— What a fine beast-catcher *U. minor* must be. We have found *Parnessus* grass here & hoped it would be a fly catcher but it doesnt seem to be, it is not sticky & as far as a codrington will show has no glands. We have found lots of *Pinguicula*—chiefly *vulgaris*, some which looks like *Lusitanica* with a dash of *vulgaris* in it so perhaps Hooker is right about the variety; some looking like *vulgaris* in other respects but having a tinge of the *Lusitanica* purple— They seem not to catch insects nearly so much here. I rooted up a lot to count them but as Amy & I had spread them out to begin we were interrupted by an acquaintance whom we had to walk home with & now they have been scrummaged in a bit of paper & are not trustable— First we thought they didnt catch any but then we found some— I will try & make out accurately tomorrow—but one would say it was a very rare exception to find insects— The flies may have been washed off but I dont think so as the edges were well curled & so ought to have prevented all being washed off; also the leaves were bathed in secretion which doesnt look like a recent washing— But there has been much rain so perhaps this only shows how quick the secretion forms— That bending back of the leaves is certainly not a *P. Mortem* appearance— Amy has drawn one the minute as it came from the soil. I saw four with (I think) *Carex* seeds & some with heath leaves looked quite familiar—not *E. tetralix* tho’— The most curious thing is that they have such great big roots—which looks as if they didn’t get much animal

food— I think Amys only had roots of very few small fibres— I have counted them in 5 plants (the numbers are certainly understated.) the inklines show the length of longest fibre in each root— they are good stout fibres as you can see by the few I send— If I can make out that they dont catch flies—it will be fine— When we were out of a diligence walking over a bit of a pass, we found a big yellow Salvia with every flower bitten— do the bees bite the blue & red garden ones in England? We have rather wet weather here it is generally cloudy either morning or afternoon— There is an English Bentham here— just think he says the leaves are covered with “crystalline points”. I asked the daughter of the Hôtel whether they used Pinguicula for making cheese, but she said not & thought us amiable maniacs Wasn’t it fine Pagets offering me that post it makes it better that he examined me himself— We have been up a small mountain today & came down rather quick as it was raining & Amy is a bit tired— she has been very well all the time & will be all right tomorrow.

Your affectionate | secretaries | Frank | Amy

Amy tells me to say she thinks this impudent

DAR 58.1: 139 - 40

## **5 6 Results of experiments on digestion [from John Burdon Sanderson, 30 March \[1874\]](#) and associated envelope with Darwin’s annotations**

Experiments on the digestibility of certain preparations, sent by M<sup>r</sup>. Darwin.

The preparations were as follows:—

- 1 “Hæmoglobin”. This is a red substance insoluble in water, acids, alcohol, &c. It is therefore not hæmoglobin, but probably hæmatin, or a mixture of hæmatin with other bodies derived from blood.
2. “Globulin”. Probably a coagulated product from the crystalline lens.
3. “Mucin”. Probably acetic acid precipitate of some animal liquid containing mucin.

In order to test the digestibility of these substances each was subjected to the action of a digestive liquid containing 0.2 per cent of hydrochloric acid and about 1 per cent. of glycerin extract of stomach of dog.

It was first ascertained that this liquid was capable of digesting 1.31 of its weight of unboiled fibrin.

The results of the Experiments were as follows:

Percentage of fibrin digested		
		1.31
in one hour, as above stated		
Percentage of “Haemoglobin”	0.456	
"	" "Globulin"	0.141
"	" "Mucin"	0.296

It is to be observed that in each case the experiment consisted in subjecting an excess of the substance to be digested to the action of the digesting liquid. It is shown that each contains a certain proportion of digestible material. By other experiments it was ascertained with reference to the "haemoglobin" that the undigested residue yielded nothing even when the digestion was prolonged indefinitely

#### Digestibility of Chlorophyll.

Various experiments made both with the preparation sent and with fresh chlorophyll shewed that this substance is entirely indigestible.

#### Digestibility of recently precipitated globulin of serum of ox blood.

An experiment of the same kind as those above related with fresh globulin prepared by Schmidt's method, yielded 0.865 per cent. to the digestive liquid after digestion for one hour at 38° C. It is therefore considerably less soluble than fibrin.

#### Digestibility of Chondrin & Gelatin.

On this subject experiments have not been completed. I do not think however that it is possible to give any useful results, for neither gelatin nor chondrin can be obtained in a solid form otherwise than by the use of precipitants which alter their constitution in a degree which cannot be chemically defined. The result of this is that it is not possible to compare the action of the digestive liquid on products which stand in any definite relation to bodies actually existing in the organism.

#### Comparison of the action of Propionic, Butyric and Valerianic acids with that of Hydrochloric acid in digestion.

In these experiments a digestive liquid containing much less pepsin than was used in the former cases was prepared.

Exp. 1. Excess of unboiled fibrin was digested for four hours in a liquid containing 3.1 per ten thousand of glycerin extract, and 2 per thousand of hydrochloric acid.

Percentage of fibrin digested 4.079

Exp. 2. The same, with the exception that an equivalent proportion of propionic acid was substituted for hydrochloric.

Percentage of fibrin digested 0.563

Exp. 3. The same with butyric acid.

Percentage digested 0.835

Exp. 4. The same with Valerianic Acid—

Percentage digested 0.615.

Hence, the digestive powers of these acids as compared with that of hydrochloric acid are expressed by the numbers

16.5 (propionic acid)

24.7 (butyric acid)

16.1 (Valerianic acid)

the corresponding number for hydrochloric acid being 100.

There is therefore no relation between the order in which these acids occur in the series of fatty acids to which they belong and their digestive faculty.

*Darwin's annotations on envelope: 'Dr B. Sanderson On Digestion of Mucin Globulin &c with Hydrochloric Acid \* & on Power of Acids [del pencil]' blue crayon*

DAR 58.2: 64r; DAR 58.1: 59

### **7 Letter from Mary Treat, 13 December 1872**

Vineland, New Jersey, | Dec. 13, 1872.

Mr. Darwin:

Dear Sir,

Prof. Gray writes me that you have found the nerves in *Dionæa*. Good! And he asks me, in connection with himself, to make observations on *Drosera filiformis*, which I will gladly do.

As far as my observations extend, I do not consider this species so interesting as *D. longifolia*, or *D. rotundifolia*, although fully as *carnivorous* as the two latter, yet it captures only *small* insects which do not require any movement of the leaves to help confine them.

For some reason my plants did not work so well last season as the year before. Whether they were weakened by the unusually dry spring, or whether the locality from which I obtained them was not so good, or whether the fault may not have been somewhat with myself, I cannot say. For two months, commencing in early summer, almost my whole time and thought were concentrated on butterflies in the effort to control sex. The result of my experiments will appear in the *American Naturalist*.

My observations and experiments with butterflies, lead me to think that the theory of the Hive bee is not correct. I know that I shall meet with opposition, so the only way is to experiment. I have already engaged a Langstroth observing hive for rearing queens, and shall carry on these observations, as well as continue my experiments with butterflies the coming season.

Your theory is steadily gaining ground among the masses and thinking people of this country, Prof. Agassiz to the contrary notwithstanding. It is boldly advocated from an Orthodox pulpit in this place, and from the Unitarian pulpit we have had a series of discourses teaching the people your theory. Nothing brings out a crowd on Sunday, like the announcement that Darwinism is to be the theme. Surely the world moves!

Command me in whatever way you may wish observations made, on birds, insects, or plants, and I shall only be too glad to render assistance as far as in my power.

Accept my thanks for your courteous reply to my former letter, and believe me | Yours most sincerely, | Mary Treat.

**9 Letter from John Scott, 25 September 1872**

Calcutta | Roy. Bot. Gardens | 25<sup>th</sup>. Sep<sup>t</sup>. 1872

Dear Sir,

I had fully intended sending you some notes on the worm casts (enclosed in a Kew case) shortly after their despatch, I delayed in hopes to accompany them with others from Dr. King on the casts marked S. India. I am indebted to him for these. He was taken very ill however and has had to leave us altogether for a year or so at home. Since he left I have been in charge here; but I am scarcely I fear likely to keep it until his return: the appointment is one that the medical faculty claim and (I) believe they are doing all they can to get one of their body installed. Dr. King wrote strongly in my favour and I believe the Lieu<sup>t</sup>. Governor is also very favourable to me and not a few others high in office. I cannot hear what the Lieu<sup>t</sup> Gov<sup>r</sup>. intends doing; anyhow he has kept many in anxiety for the last two months and I in the meantime hold the appointment. I am thus kept very busy attending alike to my own duties and those of the Superintendent.— I shall not however delay longer in affording you the few observations I have been able to make here on worm workings.—

I am very pleased to hear th(e) two notes previously sent are of interest to you. But I must first correct one on the wo(rms) in the Rice fields. I know not how I could have made the slip to say that worms exist even in the *flooded rice-fields*. This is a serious mistake. As soon as the rains set in and the fields begin to get flooded all the worms betake themselves to the higher surrounding grounds: at least such places are greatly infested with them during the rains while in the subsequent cold and hot seasons (especially the latter) you will scarcely find one. I do not think they can live in the flooded rice-fields. (I) have been often surprised (by th)em with their quick return ( ) the latter after the rains (ha)ve ceased, when the crops are (fla)ttened and the lands dry. It is indeed wonderful to see so quickly the grounds studded with their Castings. One would almost be inclined to believe that they hibernate therein throughout the rains even as during the dry and parching weather the Land leeches and some of the smaller fish do. This however they can never do in lands full of water. This they possibly may do however: at no great depth in most of our rice-fields here occurs an impervious layer of clay. Can it be that these penetrate and remain more or less dormant in this throughout (the) rains. It is indeed difficult (to) understand how they can all reach the very limited tracts of dry land as the rains set in and so quickly as the low lands dry spread themselves so numerously over such extensive tracts. The eggs of worms are scarcely likely to survive three or four months submergence, and even should they do we have still the difficulty that full grown worms are abundant as well as those of smaller sizes.

1. With regard to the washing down of casts.— In the hot season, it is only in very moist and shady places as under trees and by the sides of tanks & watercourses that we find any Casts here. During the close of the rains and in the early part of the cold season you will find our lands very generally covered with them: especially so are sun lawns. They are I think on these far more serious pests than ever I saw them on lawns in Britain. About the close of the rains when they are working almost amongst plastic mud: the casts harden during the day and are very slowly denuded by rain. They are in general only rounded off and long stand up in little Knolls. On undisturbed ground you will find them thus standing until the hot season sets in when they crack and disintegrate more or

less by the dry heat. Showers (which we not unfrequently have) at this season wash them much: smoothing at least all their rough surfaces and indeed unless of a very clayey nature completely disintegrating them and then we have them as a more or less rounded disc.

During the cold season when the subsoil in which the worms work is but slightly moist every shower washes down—more or less—the casts. If very heavy you will find them quite levelled, when lighter as rounded discs. In the former case I have seen a more or less circular space five inches in diameter coated over: Casts reduced to discs I have frequently measured from three to four inches across. With regard to the washing of casts on slopes: This is very evident on any of the low banks we have here. It may be well to give you measurements of these: the following will illustrate. On a slightly inclined bank in the gardens here I marked and measured several casts and had them protected or rather enclosed. One was  $2\frac{1}{2}$  in. high by  $5\frac{1}{2}$  in circumf. (these I ought to tell you are rainy season observations on large grass-clad artificial mounds & consisting of a loamy-clay) and when washed down after several hours rain found an extended oval disc  $6\frac{1}{2}$  in. by  $3\frac{1}{2}$  inches: the latter being across where the cast originally stood and consisted chiefly of the rounded remains, with a very slight deposit on the three sides: all being carried down. I give you two more cases.— 1<sup>st</sup>. of one 2 in. high by  $4\frac{1}{2}$  in circumference disc extended downwards 5 inches from the site of cast scarcely at all above. 2<sup>d</sup>. cast 2 in. high by  $3\frac{1}{2}$  in circumference: soil carried downwards 6 inches below original cast.

Dr. King also tells me that the big casts sent you from South India are soon washed wholly down from the mountain sides during the rains. You will observe how those (which I sent you) are rounded off. These were collected during the hot season and had been chiefly disintegrated by the sun and rounded by light showers.

This leads to your second query and indeed partly answers it. I refer to the disintegration of the casts during the dry season. On fully exposed lands this is very marked and I have over and over again observed casts largely disintegrated and quite in a condition for dispersion by wind. In the hills in South India, Dr. King also observed those big casts in a similar state during the dry season. I am sure this is also the case in the mountains of Sikkim and indeed I suppose it must be so everywhere unless on exceedingly tenacious clays: seeing that it occurs in the strong clays of Lower Bengal.

3. I have oft during the last hot season when our worms apparently bore deepest (unless indeed they as I hinted might reside during the rains in the impervious layer of our rice-fields) and I have in no instance found them at more than  $2\frac{1}{2}$  feet. The soil there is quite moist [*illeg*] Bengal soil all through the hot season. I do not think the worms there however are at all active; as nearly every one which I have turned up had itself coiled up in the way one often finds those at home.— I may here note that some of the worm casts which I have measured this rain are much larger than any of those of which I sent you specimens: the following are examples:— 6 inches high by  $4\frac{1}{2}$  in circumf. 2<sup>d</sup>. 5 inches high by  $6\frac{1}{2}$  in circumf. 3<sup>d</sup>. 5 in. high by 8 in circumf. Generally they are from 3–4 or 5 inches high by 4 in girth. These are evidently cast up in a single night and those of the largest size noted above could not from the appearance of the clay be more than the work of two nights and this from a single worm. They are thus by no means mean agents in the transposition of matter. They also swarm everywhere almost in Bengal, and I presume elsewhere in India.

The casts sent you with the exception of those marked S. India are all from Lower Bengal.

You will observe the little pellets: these take the place generally speaking of casts in dryish grounds. Thus on our walks and under trees where the soil is dry and loose you will find the ground around the worm holes strewn with pellets of various sizes: very rarely do you observe casts as on wet grounds. In forests here generally speaking worms are less abundant than on open grounds. They affect them the most during the hot weather as might be expected. These pellets are washed away with every shower and disintegrate readily in the dry season. They vary as you will see by the specimens sent greatly in size.

4. The only query that remains I think regards worms drawing leaves into the mouths of their burrows. This they do here even as in temperate climates. This is chiefly however in the cold weather: in the mornings you everywhere meet with worm holes with leaves and twigs filling their mouth. During the rainy season I have rarely observed this. Pebbles I have never seen but I have had no opportunity of observing their habits in districts where these occur and here they would search in vain for such.

I do not think that I can give you any more information regarding these, but should anything have escaped me, or there be any other observations that you should like made it shall be a great pleasure to me to hear them. It is highly gratifying to me to hear from you that any little observations of mine are of value.

I am just now getting up a paper (very fully illustrated) on *palm-stem* structure. I have also one nearly finished on the structure of the stem of the Common Papaya—*Carica papaya*—I have also some interesting matter on sexual changes and reproduction of varieties in it. I am getting a *bisexual* race of it established.

I am exceedingly sorry to see by the papers that Dr. Hooker should have been so tyrannised over by Mr. Ayrton! it is pleasing to see however the general sympathy his case has evoked.

I am | your obliged & faithful svt | John Scott.

DAR 177: 121

**10 Section drawings, 'N. End of the Basilica', 'centre of the Basilica', from memorandum on Silchester enclosed with [letter from James Joyce, 15 November 1877](#)**

15 Nov 77

Memorandum on Silchester:— copied from the Journal of Excav<sup>t</sup>. 15 Nov 77

On Tuesday Nov 13. 1877 Mess<sup>rs</sup>. F. & H. Darwin went to Silchester with me when we were enabled to carry out in the course of three hours a very careful examination of several exposed vertical cuttings, from the herbage, directly down to the floors of R. Construction.

1<sup>st</sup>. Investigation.— House excavated in the Meadow, contiguous to the Spring. Two objects were in view. 1<sup>st</sup>. inspection of floors sunk toward the centre & away from the walls. 2<sup>nd</sup>. The texture of the mould overlying walls & floors.

General Aspect: This excavation presents at this date the following:— viz. a quadrangle in the centre, round 3 sides of which runs a corridor or ambulatory with tessellation left in patches upon its floors.— The ambulatory is on the N. the E. & the S.— none on the W. Upon the N. ambulatory

there abuts a range of Rooms, five (or more strictly, six,) in number, with floors of tesserae remaining in four. Examination was first directed to ascertain what amount of sinking or "sagging" had taken place where the tessellation was sufficiently complete to give data.

The S. ambulatory has remaining a piece of tessellation of large & coarse cubes (2) of tile, chipped to  $1\frac{1}{4}$  or  $1\frac{1}{2}$  inch each,)— A line was strained across tightly from the level of the herbage on the N. to that on the S. opposite, (slope of field surface  $3.40^\circ$ . from N to S.)— Depth of floor here below field surface—at the centre of the corridor  $2\text{ft}.10''$ . at the wall  $2'.2''\frac{1}{4}$ —which gives the maximum depth of sinking or sagging of this floor  $7\frac{3}{4}$  inches.— The height of the summit of what remains of the walls—  $2'$ ,  $1\frac{1}{2}''$  below the field surface at this part, w<sup>h</sup>. leaves only  $\frac{3}{4}$  inch of wall above floor.

The N. Ambulatory was next examined.

The floor of the N. Ambulatory is disintegrated, it contains some rough tessellation but not enough to be any guide as to the amount of sinking.— Wall separating this ambulatory from the quadrangle  $3\text{ft}.5\text{in.}$  under field surface line. This wall, very slightly higher than the ancient floor was. The Rooms were examined next. The floor of a narrow rectangular room about the centre of the range was selected because in the most complete condition. The tessellation covers the greater part of the floor, but is absent at the S. end. Here is a strongly marked depression toward the middle— so much so as to make it almost look as though a hypocaust were below the floor, but there is no ground to think this.— Observing the tessellation, it is fairly perfect in the centre but appears to cease just before it arrives at the walls on both sides. It almost has the look of having been removed for a breadth of 5 to 6 inches. It does not seem that this was the case,— there would have been no conceivable reason for taking away the tesserae from there, those in the centre being both easier to get, & more obvious to see, if any person wished to take them, but it does seem on the other hand as if the whole body of tesserae slid inward a little toward the depression in the centre—without breaking up.— (we omitted to take the exact depth of this hollow) The summit of the wall along the north side is  $2' 7''\frac{1}{2}$  below the field surface, & between the summit of the wall & the floor is a depth varying from  $1'.6''$ —to  $1\text{ft.}$  We examined the mould overlying this. The section was an excellent one, & showed exactly what was there. From the top of the wall to the herbage was  $31\frac{1}{2}$  inches.

This  $31\frac{1}{2}$  inches was made up of two distinct beds, the lower was  $24''\frac{1}{2}$  deep. It is dark brown mould, very thickly interspersed with small sized pebbles of waterworn gravel, & with bits of Roman tiles, mere fragments, apparently worn by the friction of water, or weathered, till the sharp edges of their fractures are gone. There was a much larger intermixture of pebbles & bits of tile in this mould than we expected. Above this  $24\frac{1}{2}''$  bed, is the surface mould in w<sup>h</sup>. the grass grows. This is homogeneous, free from pebbles, rich & dense of a dark brown colour, entirely without any fragments of broken tile— & about 7 inches deep.

The traces of worm casts were not so marked or so frequent here as we expected to have found them.

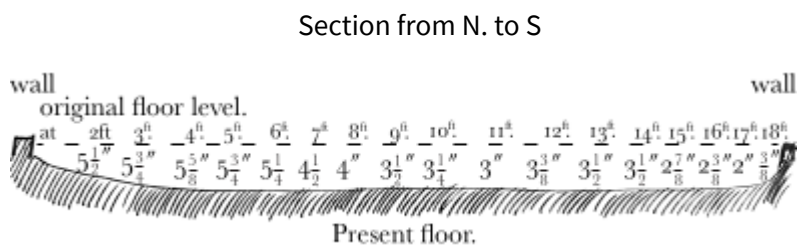
2<sup>nd</sup>. Investigation— In large excavation on the line of the Great N. Road, in the centre of the town— known as Block II.

General aspect.— a large Quadrangle—Ambulatories on the N. & S. both perfectly paved with tessellation in a perfect state, many rooms upon the outer side of these ambulatories, & along the west side also.— E side, no rooms open. Field surface found by a line strained tight above the centre of the S. ambulatory, from the herbage level at the W end to the herbage level at the E End.



Floor of South Ambulatory, tessellation quite perfect,  $11\frac{1}{2}$  inches at East end, below the field, &  $2^{\text{ft.}} 4\frac{1}{2}^{\text{in.}}$  at the west end, the field here sloping rather rapidly from W. to E. In this ambulatory there is very slight “sagging”— Examining the floor for worm casts, we found in a low part where water had remained & left a deposit of mud (very slight), very distinct worm casts, where the worms must have come up between the tesserae. Observing that here & there a tessera was loose, & a little higher in level, we removed these loose ones to look below, & found in each instance that a worm pipe existed underneath, where the loose tessera had been dislodged, & not unfrequently two pipes. This tessellation is formed not of tile work, but of hard sandstone, cubes ab<sup>t</sup> 1 inch—chipped to shape with a tool.

We next investigated by measurement the exact sinking toward a centre in a Room where the floor is quite perfect The Room we measured is that immediately next to the Red Wooden Hut,— & is at the Western End of the last named ambulatory—on the right as one advances up the corridor from E to W.



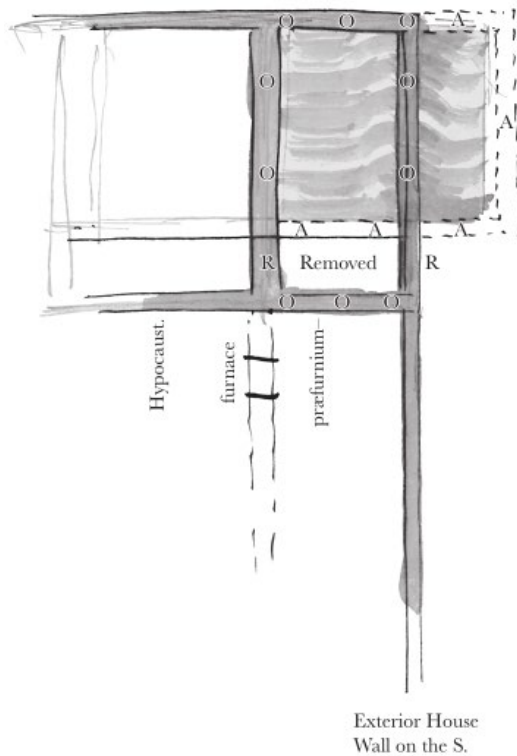
The maximum here is  $5^{\text{in.}}\frac{3}{4}$ , but it is obvious that the whole floor has sunk every where, more or less.

On this floor whilst we were examining it we found a worm which had just come up between two tesserae, nearly 7 inches long.— Memorandum entered here “worms seem to lift some of the tesserae completely out of their places.”— Yet there were very few worm casts visible on the face of the pavement.

Round the walls, in various places the tesserae have parted from the wall—looking as if removed—in other places they remain in situ close up to the line of the wall of the room.

We examined next the corner Room of the same house “Block II” at the S.E Angle.

This Room in the corner was enlarged. There was an insertion of a large hypocaust into the original ground plan. The hypocaust and the room containing the furnace were next to this corner chamber, the original room was enclosed within the original walls o . o . o . o, but the part of the room at R. R. was removed to give more space for inserting the hypocaust. In order to compensate the chamber, its area was extended beyond the house wall on the S. and an addition A. A. A. was made, encreasing the size on that side but altering the shape.



In carrying out this change they did not remove away the portion of original wall at the centre of the altered apartment but left it under the floor buried. The floor has subsided considerably on each side, & leaves now a hump or ridge to indicate where the buried wall is. There is no visible trace of a wall but the certainty of its presence was demonstrated by digging— it was found readily below the hump at 5<sup>ft.</sup> 4<sup>in.</sup> distance from the actual S. wall of the room.

3<sup>rd</sup>. investigation.— Object,— 1. to trace whether worms are to be found quite underneath the R. walls.— and 2. whether may be traced as forcing their way right up through the centre of such walls.

For this, we investigated the walls now in process of present exposure, at the excavation which is in progress. General aspect.— A large quadrangle of extraordinary magnitude & probably the quadrangle of some public place. Round it ambulatories—with suites of rooms, & hypocausts upon the western range.— Masonry very solid—walls perfectly sound & strong—depth of the foundations below floor levels, 4 feet of substantial flint & mortar, 1<sup>ft.</sup> 6<sup>in.</sup> thick throughout.

Taking the fractured extremity of a principal wall on the N.— & near the N.W. angle of the building, a sinking was made deep enough to get below the lowest course of flints.— A spadefull of earth being thrown up from exactly underneath the wall itself several worms of very large dimensions appeared. Two in particular, 8 inches long, & ¼ inch (or more) in diameter.

With a view to trace the presence of worms in the wall itself, if there, the lower flints were now removed, and the interior of the wall searched. It was soft & crumbly & appeared very moist. There was no doubt as to traces of worms being in the rotten mortar which was found in the middle of the wall. The mortar was not white, nor light coloured—it was quite dark, & largely mixed with mould. This was not its original condition, & the mould permeating it must be due to the action of

worms which carry mould with them into the very heart of the wall, when they pass between the flints.

But this investigation was at a fractured part, & the structure might be looser there in consequence. Therefore the investigation was repeated in two other parts of the same building, & in the instance of walls absolutely sound & good.

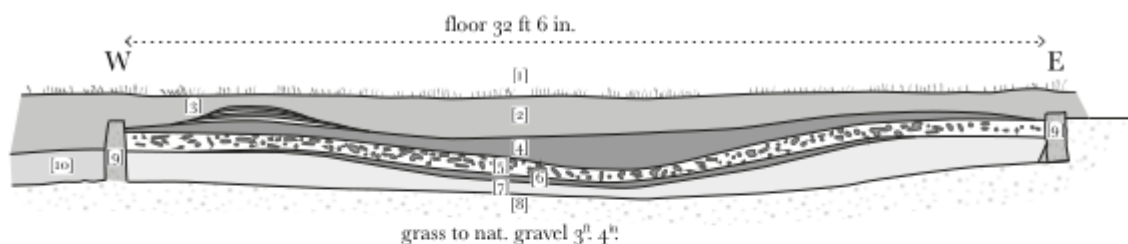
In the case of a wall upon the East side, now at this date in process of being exposed to view, a piece of wall was purposely opened for the first time to carry out this investigation. Worm casts & worm pipes were certainly found here in a fresh wall never exposed before. Mr Horace Darwin removed a flint of some size, & finding worm pipes he pursued these—one large pipe in particular, quite down into the very substance of this wall.

But being desirous of trying if possible more completely, I called D<sup>r</sup>. F. Darwin's attention to certain chambers in this building at its S. W. Corner, where the walls have been laid entirely bare below the floor levels, the whole floor & its sustratum being dug completely out, in order to exhibit the astonishing depth of the foundations. The walls go down quite 4 feet below the floors. The mortar here is perfectly hard, & looks quite fresh; With great effort a large flint was forced out of its place at a height of 12 to 14 inches from the bottom. It resisted strongly, seeming quite embedded in mortar as hard as cement, but once wrenched away, there appeared traces of worms work behind it in the inside of the wall within. There was, even here, dark mould mixed in the mortar of the middle part, & this mortar itself in places, instead of being hard & close as one w<sup>d</sup>. expect, was friable & could be picked away by the finger.

The last experiment gave me more surprize, & brought more conviction, than any before it. I should have said, and did say, that it was perfectly impossible such a wall could be penetrated by, or could contain, any earth worms.

J. G. Joyce. | 15 Nov 1877.

N<sup>o</sup> 1. Section in Basilica. Sep 27. 1866.

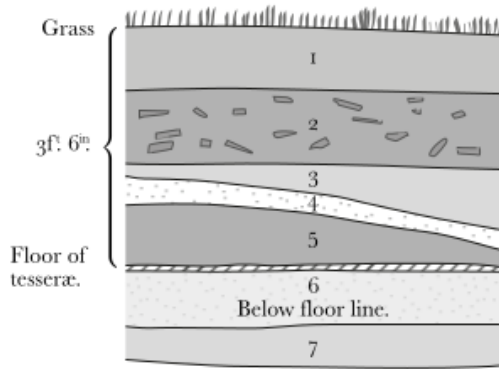


- [1] The surface of the field, with herbage growing.
- [2] Superficial mould, 1<sup>ft</sup>. 4<sup>in</sup> deep.
- [3] alternate seams ab<sup>t</sup> 1 inch each
- [4] Burnt wood, 10 inches deep at centre.
- [5] Mortar and Concrete with broken tiles. 6 inches deep
- [6] Burnt wood. 2 inches.
- [7] A bed of rubble and debris—mortar and gravel—a wreck but containing no tiles.— 6 inches at centre.
- [8] The natural gravel undisturbed in its own bed.
- [9] flint wall
- [10] floor of corridor

Section from E to W. of a floor in the Basilica at Silchester, in the Room named the *ærarium*<sup>5</sup> close to the Tribunal. This section was made on Sep 27. 1866, and beneath the layer of burnt wood on October 9 was found the bronze Eagle, the most interesting of all the relics discovered.<sup>6</sup>

Nº 2

Section in Building called "Block II." 8 aug 1865.

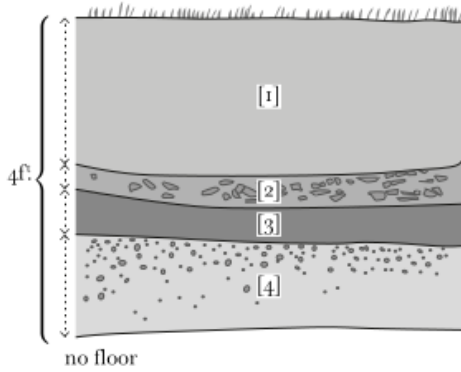


- 1 Grey loamy mould
- 2 Rich black mould quite different in colour from that over it. Contain broken tiles.
- 3 yellow clay, of variable depth.
- 4 Mortar, or Gypsum, white, perhaps wall plaster.
- X Dark rubble stuff, with mortar: 5.
- 6 rammed gravel, light col:
- 7. Natural gravel, in bed.

The meas<sup>t</sup> in inches of each bed is not preserved in the original entry, approximately they would be about as follows:

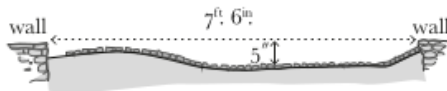
1 . . . . .	about 12 inches	}	Nº 2 probably is the "humus" from decayed timbers of a very heavy roof & broken tiles off the same.
2 . . . . .	" - 10 . . . . .		
3 . . . . .	" - 4, at least depth—varies.		
4 . . . . .	" - 6 . . . . .		
5 . . . . .	" - 10—varies.		
	—		
	3,6		

N<sup>o</sup> 3.  
Section in Block II. 22 nov 1864.

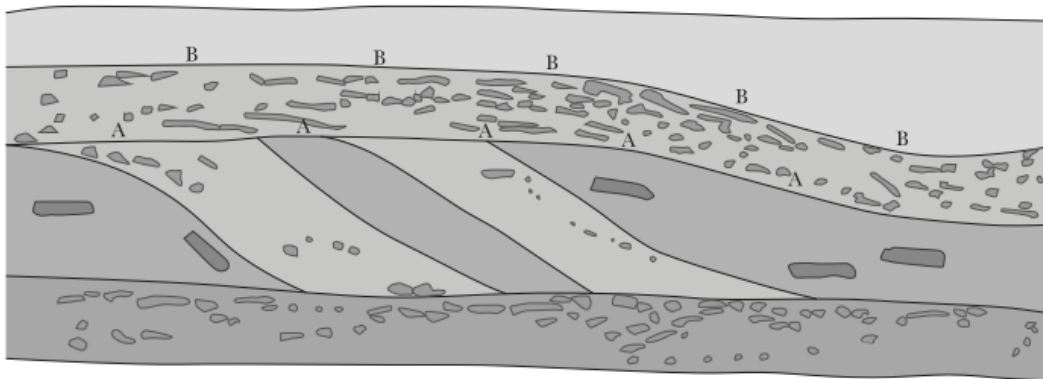


- [1] Rich mould 1ft. 10<sup>in</sup> average depth.
- [2] Broken tiles and rubble 4"
- [3] Jet black, decayed timber 6"
- [4] Deep gravel. The entry here does not say if this was made, or natural: probably it was artificial & the section did not reach the true natural bottom at 4 feet.

N<sup>o</sup> 4.  
Section of a floor in Block I. 2 nov 1864.



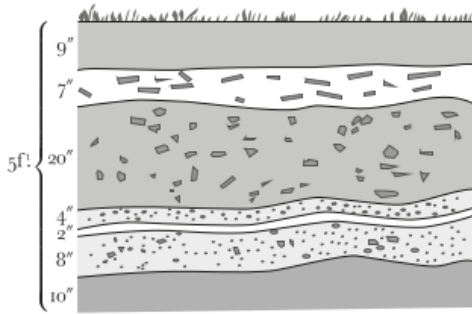
N<sup>o</sup> 5  
Section of a vertical cutting at the N. End  
of the Basilica.



Query. Are there blended in one ruin here the remains of two distinct destructions?— Below the line A. A. A is the deposit more ancient & part of w<sup>l</sup>. was cut away at top to level it?— Is the piece lying betwixt A A & B B B the residuum after the last destruction?

The grey deposit is much finer in grain than the rest.

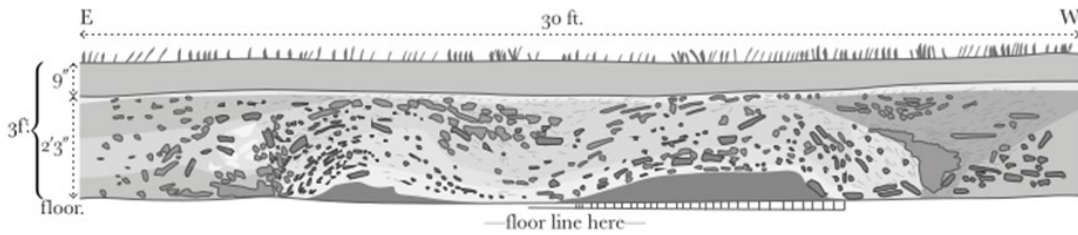
N<sup>o</sup> 6  
 Section to the depth of 5 feet taken at the centre  
 of the Basilica:— by its E. wall  
 May 7. 1870



surface mould.		9 <sup>in</sup>
Broken Tiles, large pieces, & mould of lightish colour	}	7 <sup>in</sup>
Fine grained dark rubbish, with scraps of tile— quite small in size: & different to those above.		20 <sup>in</sup>
Concrete. . . . .		4 <sup>in</sup>
perfectly white stucco.		2 <sup>in</sup>
made bottom. . . . .		8 <sup>in</sup>
dark & close substratum not natural—looks like debris of an older building—no tiles: fine grain—		10 <sup>in</sup>
		— 5.0

Traced from the original entry in the Journal of the Excavations,  
 J. G. J.  
 15 nov 1877.

N<sup>o</sup> 7.  
 10 sep 1868.  
 Section in the Room of the Basilica  
 called the Hall of Merchants.



This section shows, beneath a surface which was perfectly level before it was opened, an unusual amount of irregularity in the wreck of building below. The two black portions are charred wood, above these lies a line of nearly white material apparently stucco or wallplaster;—fine gravel, tiles, & mortar compose the rest of the mass, as high as the mould above. A floor of hard concrete (having here & there red tesserae on it) lies at a vertical depth of 3 feet below the grass. The mould is 9 inches.

Traced from the original entry.  
 J. G. J.  
 15 nov 1877.