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THE DARWIN LETTERS AT  
SHREWSBURY SCHOOL

BY

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## THE DARWIN LETTERS AT SHREWSBURY SCHOOL

Edited by SIR GAVIN DE BEER, F.R.S.

## INTRODUCTION

WITH the naïve innocence which was part of the charm of his child-like character, Darwin was less than fair to his old school, Shrewsbury. In his *Autobiography*, he wrote of it, 'Nothing could have been worse for the development of my mind than Dr. Butler's school, as it was strictly classical, nothing else being taught except a little ancient history and geography. The school as a means of education to me was simply a blank.' It is true that his Headmaster upbraided him for wasting his time on chemistry, with his brother Erasmus, in a shed at his home, and this probably rankled; but it seems that Darwin was incapable of appreciating the imponderables in the formation of his own mental equipment, for Edinburgh and Cambridge fared no better at his hands than Shrewsbury School: 'During the three years which I spent in Cambridge my time was wasted, as far as the academical studies were concerned, as completely as at Edinburgh and at school.' Worse still, he went on: 'Although as we shall presently see there were some redeeming features in my life at school, my time was sadly wasted there and worse than wasted.' How, in the state of ignorance that prevailed in 1818 as regards science in general and biology in particular, would Darwin have preferred to spend his time, and on what subjects? The only one that can be identified straightaway, and which Darwin himself always regretted, was mathematics.

It would be interesting to know how Darwin would have explained to himself the most curious fact in his intellectual evolution; how he became a scientist. When he sailed in the *Beagle* he was a rather ordinary well-to-do young man, with great courage and much horse-sense, no academic qualifications whatever, a love of riding and shooting, a great interest in collecting beetles and pebbles, and some familiarity with the techniques of collecting marine organisms and skinning birds. His interest had been awakened in natural history, first by reading Gilbert White, then by Robert Grant and other men at Edinburgh where, however, the instruction in what went by the name of science almost put him off the subject for ever, and lastly by John Stevens Henslow at Cambridge, who fostered his interest instead of

snubbing it as Grant had done, and introduced him to some other great men in Cambridge. But Darwin was still no scientist, and the confession which he made of his disappointment when Adam Sedgwick rejected the tropical shell which had been said to have been found in a quarry of glacial origin,<sup>2</sup> showed that he did not even know what natural science meant a few weeks before he sailed in the *Beagle*.

It has been claimed for Darwin by Professor Harold Fruchtbaum<sup>3</sup> that when he sailed in the *Beagle* he was 'one of the best-trained and most experienced all-round naturalists in England'. This was not the opinion of the man who knew him best, Henslow, who told him straight to his face that although he had recommended him to the Admiralty to sail in the *Beagle*, this was 'not on the supposition of your being a *finished* Naturalist';<sup>4</sup> and there was, and still is, a difference between the terms naturalist and 'scientist', a term which was not coined until 1834, in an anonymous review by William Whewell, on a parallel with artist, economist, and atheist, as Professor Sydney Ross has shown.<sup>5</sup>

So there remains the problem; how can it be explained that after five years in the *Beagle*, exposed to the close company of a bibliolater like Captain Robert FitzRoy, and scoffers like some of the other officers, but no man with whom he could discuss anything intelligently, and nothing to guide him but Lyell's *Principles of Geology* (which Henslow warned him *not* to believe), and Henslow's friendly letters<sup>6</sup> of encouragement and guidance in general matters (not to take offence at rudeness or ungentleman-like behaviour), young Darwin wasted no time at all, but quickly saw what was worth observing in phenomena of geology and natural history and what specimens were worth collecting, although he nearly missed the bull's-eye by not starting to collect specimens separately from each Galapagos Island. Even more important was the way in which, all by himself, he brought such rigorous powers of thought, criticism, and judgement to bear on his problems, that when he landed from the *Beagle* he had become the hardest-headed biologist in the world. Darwin was not unaware of this himself, for in one of his Notebooks,<sup>7</sup> shortly after his return, he wrote, 'Now that I have a taste for hardness of thought'. How did he acquire it?

The teaching of science is a subject on which there is still no agreement, some maintaining that progress in scientific research has been so great and is accelerating so fast, that no time is too soon or age too young to begin specializing in the study of science. Such a view is riddled with errors. One is its failure to recognize that natural science itself makes use of criteria (accuracy, consistency, *non sequitur*) for which there is no scientific

explanation or basis.<sup>8</sup> Another consists of the argument that because more is known, more must be taught. This mistaken policy leaves out of account the fact, to which Sir Peter Medawar<sup>9</sup> has drawn attention, that advances in scientific knowledge have resulted in welcome increases in the number of available general principles which provide broad frameworks; these must be taught, with the minimum of detail required to demonstrate, illustrate, and explain them. The result is that a mass of detail need not be taught at all; it will fall into place, later, when and if required. Before Darwin, there was no biology, only a string of facts, and he brought the first unifying general principle into the chaos: evolution. At the present time, although the amount to be discovered is even greater, there are numbers of principles and laws of wide validity, with the help of which programmes of research and of study can be drawn up, with great tidiness and economy of time.

Another lamentable error is the misguided attempt to split the intellectual endeavour of the western world into 'two cultures', so-called, thereby elevating on to the level of a sort of intellectual schizophrenia what is only a temporary, however regrettable, dichotomy in educational policy and practice. The use of reason, imagination, and creative powers of the mind are no different in the natural sciences from what they are in the arts. Descartes knew this very well, and nobody has given a better explanation of the working of the mind when it is producing ideas than Coleridge.<sup>10</sup> Nobody can really believe that full education can be achieved with nothing but mathematics, test-tubes, balances, dissecting dishes, seed-beds, and breeding-pens. And yet that is what Darwin seems to have wanted. Fortunately he did not have it, for he was himself an example of the prevalent if not widely recognized fact that the greatest men of science include those whose educational foundation was based on the classics.<sup>11</sup>

The chief aim of education, for any conceivable vocation, must be the teaching of how to think, and how to think straight. One of the time-honoured, and still not superseded methods of inducing the practice of accurate thought and critical judgement that follows from it, is by translating passages from English into a highly analytical and precise language like classical Greek or Latin. These languages are taught, not primarily for the enjoyment of their literatures, nor for the pleasures of etymological paper-chases, but for the purpose of learning to think in English. English is a very loose language in which a string of words can be made to mean quite different things, according to intonation (which cannot be read on paper) and to context (which usually means re-reading). This is why formulae and documents in English-speaking communities can be accepted by contending

parties who can be made to imagine that what they think they understand by the words of a text is identical with what the others understand. It is why the Church of England can accommodate the whole spectrum of belief from near-Catholicism to Evangelicalism. It is why policies of political parties are so widely acceptable that the number of these is minimal, in Great Britain and the United States, as compared with nations whose language is analytical and precise. It is not necessary to look further than France to see how the refinement of meaning of the language leads to dissent and the multiplication of parties. It is why industrial disputes are settled, until one side or the other finds that the meaning which it attached to the terms of settlement is not the same as that understood by the other. There may be a case for ascribing the deterioration in standards of accuracy and consistency discernible in England in recent years to the fact that most office staffs have not only had no training in Latin prose themselves, but have been taught by teachers who themselves have never had that training.

When does an English-speaking person first ask himself the question, 'What, *exactly* do I mean?' 'What *exact* meaning do I want to convey?' 'What do I want to say, and what do I want *him* to understand?' Until two generations ago, the general answer to this question was, when the schoolboy or schoolgirl was confronted with the task of turning a sentence in English into Greek or Latin prose. In this process, there is no room whatever for hesitation, uncertainty, or ambiguity; these languages are of such precision that complete certainty of meaning must precede the selection of translated words and syntax. The nail of meaning must be hit square on the head. This was the training to which Darwin was subjected, and it is impossible to resist the conclusion that it laid the foundations in his mind of an instrument which he used in an incomparable manner, for seeing the significance of important facts, and steering a straight course through an uncharted ocean of bewildering detail, with shoals of falsehood deposited by his predecessors right across his course. This applied not only to evolution and natural selection in wild and domestic plants and animals, man, and emotions, but to the elucidation of coral reefs, volcanic action, elevation and subsidence, foliation in geological formations, metamorphism of igneous and sedimentary rocks, pollination in orchids, primroses, and loosestrife, insectivorous plants, climbing plants, power of movement in plants which started the whole science of growth-hormones, the nature of hybrid-vigour, and the action of earthworms considered in terms of weight of soil sifted and lifted per unit area in unit time, and the biological effects of such action. *Arma virumque cano . . .*

Although Darwin misjudged his old school, it and its alumni have appreciated him. The Shrewsbury School Library possesses a small number of Darwin letters of the greatest interest, which the Governing Body has very kindly allowed me to edit and publish. I should like, at the same time, to thank the Librarian, Mr J. B. Lawson, for his inestimable help in this work. The provenance of these letters, where it is known, is given in each case; but mention must be made here of the generosity of Mr A. E. Gunther, O.S., son of R. T. Gunther, founder of the Old Ashmolean Museum of the History of Science, and grandson of Albert Günther, Keeper of Zoology in the British Museum. When Richard Owen died, many of his books and papers went to Albert Günther, including the copy of the *Origin of Species* which Darwin sent to him with a covering letter, and a draft of Owen's reply. All these Mr A. E. Gunther has presented to his old School, and they fill in many details in the sad story of Owen's failure to rise to the occasion of his (former) friend Darwin's triumph.

It will be noticed that among Darwin's most intimate Cambridge friends were Wranglers Herbert, Heaviside, and Whitley, and they may have been the origin of Darwin's later regret that he had never understood anything of the great leading principles of mathematics, 'for men thus endowed seem to have an extra sense'.<sup>12</sup>

The letters are arranged in alphabetical order of the names of the addressees, and in chronological order under each name. The notes which are essential for the editing of the letters follow each letter, in the same style as the previous publication of Darwin letters in *Notes and Records*.<sup>13</sup>

<sup>1</sup> *The Autobiography of Charles Darwin*, edited by Nora Barlow, London 1958, pp. 27, 58, 60.

<sup>2</sup> *ibid.*, p. 69.

<sup>3</sup> *Times Literary Supplement*, 5 October 1967.

<sup>4</sup> *Darwin and Henslow*, edited by Nora Barlow, London 1967, p. 30.

<sup>5</sup> Sydney Ross: 'Scientist: The Story of a Word', *Annals of Science*, 18, 1962 [1964], pp. 65-85.

<sup>6</sup> *Darwin & Henslow*, *op. cit.*, *passim*.

<sup>7</sup> Charles Darwin, Notebook on Metaphysics on Morals & Speculation on Expression (Notebook "M"), Cambridge University Library, Darwin, MSS. 125,

p. 34. (Notebook begun 15 July 1838, finished 2 October 1838.)

<sup>8</sup> Gavin de Beer: *Reflections of a Darwinian*, London 1962, p. 150.

<sup>9</sup> P. B. Medawar: *The Art of the Soluble*, London 1967, p. 114.

<sup>10</sup> Gavin de Beer: 'Other Men's Shoulders', *Annals of Science*, 20, 1964 [1965], p. 321.

<sup>11</sup> Sir D'Arcy Thompson; Sir Cyril Hinshelwood, Presidents of the Classical Association.

<sup>12</sup> *Autobiography*, *op. cit.*, p. 58.

<sup>13</sup> 'Some unpublished Letters of Charles Darwin', edited by Sir Gavin de Beer, *Notes & Records, Roy. Soc.*, 14, 1959, p. 12.

## JOHN MAURICE HERBERT

The identification of the addressee of the following letter with John Maurice Herbert (1808-1882) is based on four points: he was a 'dear old friend' of Darwin, as his son Francis relates in *Life and Letters*,<sup>1</sup> he was up at Cambridge at the same time as he, he was a member of St John's College, and Charles Thomas Whitley was Herbert's cousin; all matters which arise in the letter.

John Maurice Herbert came from Montgomeryshire. In 1830 he was Seventh Wrangler and became a Fellow of his College. Called to the Bar he became a County Court Judge for South Wales, and it is said that his decisions were so rarely appealed against or reversed that trial by jury in his courts became almost obsolete. It is not without interest to find Darwin such a close friend with a man of such high attainments in mathematics and in jurisprudence.

<sup>1</sup> *Life and Letters of Charles Darwin*, edited by Francis Darwin, London 1887, I, p. 165 & seq.

(Letter 1)

[Charles Darwin to John Maurice Herbert]

Down, Beckenham, Kent  
Dec<sup>r</sup>. 25 1880

My dear old Friend

I was glad to receive y<sup>r</sup> card, but I wish it had been accompanied by a note, telling me a little about yourself, how y<sup>r</sup> health and strength is & how you support y<sup>r</sup> solitary life.

I should also like to hear something about Whitley.<sup>1</sup>

Though we have no communication my memory often goes back to Cambridge days, & not long ago the scene of receiving the Microscope with the anonymous note came most visibly before my mind.

My youngest son Horace, now lives with his charming little wife, in Cambridge, & when I walked this summer through the Courts of S' Johns I thought of Van John<sup>2</sup> & old days.

Oh dear! life was then worth living, not that I have anything to complain of. My seven children have never given me a moment's uneasiness except on the score of health, three of them ailing, though not seriously, having inherited my poor constitution. They are good dear affectionate children & some of them will do good work.

My health is better than it used to be, but I live in a perpetually half knocked-up condition. I go on working at science, & in fact I am turned into a sort of machine for observing facts & grinding out conclusions,<sup>3</sup> & am never happy except when at work.

But I have written too much about myself. Do sometime let me hear something about yourself.

Farewell my old friend

Y<sup>rs</sup> ever sincerely

Chas: Darwin

My wife desires to be kindly remembered to you.

[None of this letter is in Darwin's handwriting.]

<sup>1</sup> Charles Thomas Whitley, see Letter 15, below.

<sup>2</sup> 'Van John' is also referred to in a letter from Darwin to William Darwin Fox of 1829 (*Life & Letters*, i, p. 176).

<sup>3</sup> This is a paraphrase of the statement in the *Autobiography*, 'My mind seems to have become a kind of machine for grinding general laws out of large collections of facts', *op. cit.*, p. 139.

#### HENRY JOHNSON

Henry Johnson studied medicine at Edinburgh where he matriculated in 1829, and therefore after Darwin had left that university. He was still in the Medical Register in 1883. Darwin's connexion with him was over his excavations of Roman remains at Wroxeter and his estimation of the ammonia present in worm-casts, which Darwin used in his book on vegetable mould and earthworms.<sup>1</sup>

<sup>1</sup> *The Formation of Vegetable Mould through the action of Worms*, published 10 October 1881.

(Letter 2)

[Charles Darwin to Henry Johnson]

Down, Beckenham, Kent  
Nov 14<sup>th</sup> 1880

My dear Johnson,

An overwhelming lot of letters have prevented me from thanking you sooner for your answer about a slope of the ground at Wroxeter; also your daughter for her very kind note. I hope before very long to hear again about my other bothersome questions. My heart & soul care for worms & nothing else in the world just at present!



But I write now to say that I have directed a new book by me on the 'Power of Movement in Plants' to be sent you, & it will arrive in about a week, & to add that it is unreadable except by a specialist. If you read Introduction & last Chapter you will know *whole contents*, except the evidence on each head. While correcting proofs, I bethought me of an old paper by you, but I had forgotten reference, & as far as my memory served me it was chiefly or exclusively on the movements due to difference in tension in different parts, as when a dandelion peduncle is split & [illegible] do not concern me. But if you have treated of other subjects, I shall be punished for my idleness.

I have written today seven letters & am quite tired, so Farewell.

Yours sincerely

Ch. Darwin.

#### MAXWELL TYLDEN MASTERS

The identification of the addressee of the following letter with Maxwell Tylden Masters (1833-1907) is based on the problems of peloria (concerning which other letters from Darwin to Masters have been published),<sup>2</sup> and on the references to his father, a horticulturist. William Masters (1796-1874), Maxwell's father, was a noted nurseryman. In 1869 Maxwell Masters published *Vegetable Teratology*.

The letter is believed to have been presented to the School by J. B. Oldham, O.S., but with the tentative identification of its addressee as John Jenner Weir, which is certainly incorrect.

<sup>2</sup> *More Letters of Charles Darwin*, edited by Francis Darwin and A. C. Seward, London 1903, I, p. 147; 2, p. 256.

(Letter 3)

[*Charles Darwin to Maxwell Tylden Masters*]

Down, Bromley, Kent  
Ap 25 [1860?]

My dear Sir

I beg you not to speak of 'apologies for intruding', when you are doing me the greatest kindness in giving me valuable information. I am heartily glad to hear of the papers in Linn: Journal, & then I shall see about peloria: I hope you will give any information which you may possess on the power of reproduction in peloria & other monsters. Of course you will naturally

compare monstrous to normal structures: as far as I have seen, such comparisons have generally to be instituted between members of *distinct groups* which to my mind greatly destroys the value of such parallelism in relation to the origin of species.

Unless your Father actually experimented on *mixed* pollen it is curious how he discovered the truth, which Gärtner has shown without doubt, namely that the stigma does select its own kind of pollen out of others; & more than that, for if the plant's own pollen be put on stigma within a certain number (forgotten by me) of hours after foreign pollen, all influence from the foreign pollen is completely eliminated. But then no facts are known showing that this holds with the pollen of different *varieties*: on the contrary some facts lead to the suspicion that the pollen of a *distinct variety* has a prepotent effect over a plant's own pollen. In the case of Hollyhocks, I have suspected from some facts that each variety preferred its own pollen; & I have in vain been searching for information from those who have raised many hollyhocks whether they find it necessary, in order to get the varieties pretty true by seed, to keep the several varieties in different parts of the garden.

Has your Father raised with care many seedling hollyhocks?

I observe that your Father is a strong believer in the rule that when variation has once commenced it goes on.<sup>1</sup> If at any time you were staying with your Father it would be a grand thing to get his reasons for this belief.

That is a curious case about the yellow Hyacinths. The laws of inheritance seem to be determined to puzzle everyone.

With sincere thanks My dear Sir

Yours sincerely

C. Darwin.

<sup>1</sup> This is a reflexion of Darwin's continued interest in the problem of the origin of variation in why once variation has started

under cultivation it continues although the conditions are then stable. *Origin of Species*, 1st edition, 1859, page 8.

#### RICHARD OWEN

Richard Owen (1804-1892) now takes his place beside the other recipients of letters from Darwin dated 11 November 1859, announcing the gift of a copy of the *Origin of Species*. The others were: Louis Agassiz, Alphonse de Candolle, Hugh Falconer, Asa Gray, John Stevens Henslow, Sir John Herschel, John Phillips, and Adam Sedgwick. Darwin wrote in the same sense to John Lubbock on 12, Alfred Russel Wallace on 13, and William

Darwin Fox on 16 November. Lyell, Hooker, and Huxley must have received advance copies, which were available in October.

Darwin's letter to Owen is written in the friendly tone which prevailed in all their correspondence since shortly after the return of the *Beagle*. The draft of Owen's reply is likewise cordial, but somewhat defensive, as if he were already reinsuring his position, and establishing a priority of method and principle. Owen must have written again to Darwin, to ask him for the reference to the passage in Hearne's Travels relating to the famous bear-and-whale passage, for Darwin wrote giving this information on 10 December.<sup>1</sup>

It would have been difficult, from this exchanged correspondence, to foresee the venom of Owen's anonymous review of the *Origin of Species*, his coaching of Samuel Wilberforce, Bishop of Oxford, or his disingenuous and disgraceful subsequent behaviour in regard to Darwin and his book.<sup>2</sup>

These letters were presented to the School, together with the original copy of the first edition of the *Origin of Species*, by Mr A. E. Gunther, O.S.

<sup>1</sup> *Notes and Records, Roy. Soc.* 14, 1959, p. 50.

<sup>2</sup> Gavin de Beer: *Charles Darwin*, London, 1963, p. 165.

(Letter 4)

[Charles Darwin to Richard Owen]

Down, Bromley, Kent

Nov. 11<sup>th</sup>

1859

Dear Owen

I have asked Mr Murray to send you a copy (as yet only an abstract) on the origin of species. I fear that it will be abominable in your eyes; but I assure you that it is the result of far more labour than is apparent on its face. If you honour me by reading it at all, I beg you to read it straight through, otherwise from being much condensed it will be unintelligible. I fear that my meaning will not be clear to anyone, without a considerable amount of reflexion. Whether I be in the main part right, as I honestly think myself to be, or wholly wrong, the old saying of magna est veritas et prevalebit is a good conclusion to all doubts.

Pray believe me

Yours sincerely

Charles Darwin

I am hydropathising at Ilkley Wells & shall remain here some weeks longer & hope to get a little health. Should you wish (which is not at all likely) to discuss by word of mouth or by letter any objections to my heterodox notions, I should be proud to answer them to the best of my power. But you are probably too much engaged to give up time to the question.

(Letter 5)

[Richard Owen to Charles Darwin: draft]

Reply Nov. 12 59 to Chas. Darwin FRS

I thank you in advance for your kind recollection of me, and shall welcome your work with the close & continuous perusal you recommend, but which it certainly would have had on its own merits. I am, and have been, disposed to believe in the operation of existing influences or causes in the ordained becoming and incoming of living species.

No attempt, therefore, to demonstrate the nature of such continuously operative creating forces can be 'heterodox', in any way, to my feelings. I have, indeed, received grave rebukes from some Masters in Philosophy for publishing my present state of belief in such terms as the subjoined extract from 'Palaeontology' in *Encyclo. Brit.* To which remonstrances I reply that I am free to test any propounded idea of a present operating creative i.e. species-making, force, by the conditions of any given species under immediate study & to express the result of my considerations on the equivalency or otherwise of the hypothetical cause to the effects produced & under review.

For the application of your rare gifts to the solution of this supreme question I shall ever feel my very great indebtedness.

#### ROBERT LAWSON TAIT

Robert Lawson Tait (1845-1899), surgeon and gynaecologist, had wide interests in biology, as the following letters show, with regard to comparative anatomy, insectivorous plants, animals' care of their young, and ferments. The devastating report of the referee on Tait's work in Letter 9 must have given Darwin much distress to communicate to him; nor is it known what body the referee worked for, but it may have been the Royal Society.

Other letters from Darwin to Tait are published in *More Letters* where he is obscurely referred to in the Index under 'Tait.L.'

The letters were presented to the School by D. M. Stewart, O.S., whose father's sister was Tait's wife.

(Letter 6)

[Charles Darwin to Robert Lawson Tait]

Down, Beckenham, Kent  
Oct 8 1871

Dear Sir,

I do not know anything about a supra-condyloid process in the humerus, yet I have a vague remembrance of having heard of it. But my knowledge of anatomy is quite insufficient to be trusted. If you publish on this subject I shd be grateful for a copy of your paper.

Dear Sir

Yours very faithfully

Ch. Darwin.

[signature only in Darwin's handwriting]

(Letter 7)

[Charles Darwin to Robert Lawson Tait]

Down, Beckenham, Kent  
Abinger Hall  
[alia manu: June 15 1875]

My dear Sir

Aldrovanda is rootless & floats freely & catches abundant prey in various parts of the world. If you will wait till my book<sup>1</sup> appears, I think you will find abundant evidence of absorption. Your separation of the ferments, seems a capital discovery. I have not strength to give evidence of absorption.

Yours sincerely

Ch. Darwin.

<sup>1</sup> *Insectivorous Plants* was published on of *Drosera*. *Aldrovanda* is a genus of 2 July 1875, and contains evidence of Droseraceae. absorption of insect prey by the tentacles

(Letter 8)

[Charles Darwin to Robert Lawson Tait]

Basset, Southampton  
Sept. 10 [1875]

My dear Sir

I can give you very little information on your subject. Earwigs are said, I believe, to take charge of their young, & I am sure that I have read that

even star-fishes do the same for their eggs, as everyone must have read of spiders. I rather think that I have referred to star-fishes in 2<sup>nd</sup> edition of *Descent of Man*. Other fish, besides stickle-backs, take charge of their young, of which I give fresh instances in 2<sup>nd</sup> edit. of *Descent*.<sup>1</sup>

Yours sincerely

Ch. Darwin.

<sup>1</sup> *Descent of Man*, 2nd edition, 1874, p. 162.

(Letter 9)

[Charles Darwin to Robert Lawson Tait]

Down, Beckenham, Kent  
May 5 [18]76

My dear Sir

I have at last heard about the Physiological Referee, but my informant was directed *not* to communicate to me the exact language, but only the sense of the Referee's report. I will not copy what my informant says:

'The referees report that the modifications which Mr L.T. has introduced into Brücke's process for isolating pepsin consist in neglecting certain precautions without which the method is useless. He relies on neutralisation for separating his *droserin*. This process can have no diagnostic value, seeing that innumerable substances would behave in this manner. The hygroscopic quality of his *azein* on which he insists is also unimportant since it is common to many derivatives of proteids, ex: gr: peptones. His method of determining the nature of the acid by comparative trials is valueless, because he has reduced them to a standard strength & Brücke has shown that different acids act equally effectively at *different* strengths. His method however of estimating the degree of acidity of the different acids is in itself defective in the absence of any evidence as to the purity & constant quality of the litmus used.'

I am extremely sorry to be compelled to convey the above information to you.

My dear Sir

Yours very faithfully

Charles Darwin.

[Signature only in Darwin's handwriting]

(Letter 10)

[Charles Darwin to Robert Lawson Tait]

Down, Beckenham, Kent

Jan 13 1880

My dear Sir

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

I was born on Feb. 12<sup>th</sup> 1809, so shall be 71 next Feb.

Pray reflect on what I have said about my grave, & till then,  
pray believe me

Yours very faithfully

Charles Darwin.

<sup>1</sup> The biographical notices of Darwin to which he was alluding were probably, Robert Hunt (1807-1887): *Biographical Memoirs of Men of Eminence* (in Hunt's proof copy of which Darwin's MS. sketch of his own life dated 3 May 1868 was bound up), *Notes and*

*Records Roy. Soc.* 14, 1959, p. 36; and R. Griffin & Co.'s *Comprehensive Dictionary of Biography*, London 1860, for which Darwin provided material in his letter of 29 January 1860, *Annals of Science*, 14, 1958, p. 94.

(Letter 11)

[Charles Darwin to Robert Lawson Tait]

Down, Beckenham, Kent

Feb. 13<sup>th</sup> 1880

My dear Sir

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

Accept my sincere thanks and believe me

Yours sincerely

Ch. Darwin.

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

(Letter 12)

[*Charles Darwin to Robert Lawson Tait*]Down, Beckenham, Kent  
July 19th 1880

My dear Sir

I have much pleasure in enclosing 25 £ for your Scientific Fund in Birmingham.

Yours very faithfully

Ch. Darwin.

(Letter 13)

[*Charles Darwin to Robert Lawson Tait*]Down, Beckenham, Kent  
Feb. 13<sup>th</sup> 1882

My dear Sir

I must write one line to thank you and M<sup>r</sup> Tait for your very kind note on my birthday.

I feel a very old man, &amp; my course is nearly run.

I remain

Yours very sincerely

Ch. Darwin.

## AUGUST WEISMANN

August Weismann (1834-1914) was a biologist of Darwin's own stature. His work on the continuity of the germ-plasm, his identification of chromosomes as bearers of hereditary factors, his recognition of regeneration as an adaptive phenomenon, and his search for the origin of evolutionary novelties in germinal variation, together with his acceptance of evolution, natural selection, and sexual selection, made him an ideal correspondent for Darwin.



[Charles Darwin to August Weismann]

Down, Beckenham, Kent

Nov. 10<sup>th</sup> 1879

My dear Sir

I am very much obliged for your kind present of your work on *Daphnia* with its admirable drawings & for your letter. As soon as I can find time (for I have two essays in German to read which bear on my immediate work) I will assuredly read your book, for there is to me always an extreme interest in hearing of adaptations in parts which appear to owe their structure to other causes.

The nature of the *Vanessa* in northern Siberia must have been particularly interesting & satisfactory to you. I have not heard for a long time from M<sup>r</sup> Meldola & do not know how the translation of your work goes on.<sup>1</sup> He has unfortunately very little spare time.

It is almost impossible to persuade English publishers to bring out translations of any scientific works, excepting such as bear on education; but I will not forget your wish, in which I heartily join, should any opportunity ever offer.

With good wishes, pray believe me

My dear Sir

Yours very faithfully

Charles Darwin.

<sup>1</sup> August Weismann: *Studies in the Theory of Descent* translated and edited with notes by Raphael Meldola: with a prefatory notice by Charles Darwin, 3 vols., London 1880-1882.

#### CHARLES THOMAS WHITLEY

Charles Thomas Whitley (1808-1895) was another of Darwin's intimate friends, who also was at Shrewsbury School. At Cambridge, where he matriculated in 1826, at St John's College, he became Senior Wrangler in 1830, and Fellow of his College in the following year. Clerk in Holy Orders in 1836, he became Reader in natural philosophy and mathematics at Durham, and Vicar of Bedlington, Northumberland, from 1854 until his death. An original member of the Surtees Society, he was one of the chief promoters of Bede College for the training of schoolmasters. In order to appreciate the following letter, reference must be made to the letter which

Darwin wrote to him<sup>1</sup> on 24 October 1836, congratulating Whitley on his marriage to his cousin Frances Whitley.

The letter was presented to the School by Lieut.-Colonel C. J. Cocks, O.S.

<sup>1</sup> 'Further unpublished Letters of Charles Darwin' edited by Sir Gavin de Beer, *Annals of Science*, 14, 1958, p. 111.

(Letter 15)

[Charles Darwin to Charles Thomas Whitley]

November 23rd [1838]  
Athenaeum

My dear Whitley

I have been very long in answering your kind letter, but now I have some news to tell you, which I am sure will please you, as holding out to me, the best chance of happiness, mortal man can boast of in this world. It can be nothing else, but that I am going to be *married*. The lady is my cousin Miss Emma Wedgwood, *you* will approve of marrying cousins: & if you knew what a good dear little wife, the lady will surely make, you would approve still more, & congratulate me heartily, on my good fortune.

We are to be married in the end of January, & intend living in London, at least for some years, until I have wearied the geological public with my newly acquired cacoethese scribendi.

I have a busy, & therefore a happy life before me, with a comfortable, though rather humble, fireside to spend my evenings, & the evening of my life by the side of, that is if I have the luck to live so long. My future wife is a very good musician, so that I [*tear*] to acquire first rate taste in music, & nothing less than symphonies of Beethoven shall go down.

I am glad to find from your letter that science is not so much at a discount with you as formerly: although I believe it was always rather more theoretically so, than practically. I am glad to find, you wondered properly at Glen Roy. I cannot get people to be half enough astonished at it. I saw nothing in my peregrinations to the Antipodes, nearly so curious in physical geography.<sup>2</sup> I do not doubt they are old sea-beaches: & many most curious inferences may be, I believe, deduced from the fact. This is a marvellous world we live in, & I never cease marvelling at it. But just at the present time I marvel more at the prospect of having a real, live, goodly wife to myself than at all the hundred wonders of the world.

I am very much obliged for your kind invitation northward. I fear it will be long before I shall be able to bend my steps that way: I often regret how little I see of my good old friends: but the recollection of them & of Cambridge days & of our long walks often gives me real happiness.

Heaviside<sup>2</sup> flourishes at Hayleybury: I was astonished the other day by being hailed by a Revd. Prof. of the College as *President*: which was explained to me by being informed that Heaviside had stated over what a noble club I had once presided.<sup>3</sup>

Good bye, my dear Whitley,

Most truly yr

C. Darwin  
The Revd Chas Whitley  
The University  
Durham.

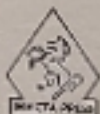
[C. Darwin Nov<sup>r</sup> 26<sup>th</sup> 1838]

<sup>1</sup> Darwin had visited Glen Roy in June 1838, and concluded that the Parallel Roads were marine beaches, results of a deep temporary subsidence of the land beneath the sea. He had seen irrefutable evidence of much deeper subsidences in South America. His conclusions were published in 'Observations on the Parallel Roads of Glen Roy, and other parts of Lochaber, with an Attempt to prove that they are of Marine Origin', *Phil. Trans.* **129**, 1839, p. 39. He refused to accept the glaciological explana-

tion of their formation, until forced to do so by Thomas Francis Jamieson in 1861 (*Notes and Records Roy. Soc.* **14**, 1959, p. 37).

<sup>2</sup> James William Heaviside (1808-1897), matriculated at Cambridge in 1826, Sidney 1827, Second Wrangler 1830; Fellow and Tutor of Sidney 1833, professor of mathematics at H.E.I.C. Hayleybury 1838-1857, Canon of Norwich.

<sup>3</sup> 'The Gourmet Club' (see *Life and Letters*, **1**, 1887, p. 169).



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