

Cambridge University Press
978-1-108-00347-6 - The Life and Letters of Charles Darwin: Including an
Autobiographical Chapter, Volume 3
Charles Darwin
Excerpt
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LIFE AND LETTERS
OF
CHARLES DARWIN.

——
CHAPTER I.

THE SPREAD OF EVOLUTION.

‘VARIATION OF ANIMALS AND PLANTS.’

1863–1866.

HIS book on animals and plants under domestication was my father's chief employment in the year 1863. His diary records the length of time spent over the composition of its chapters, and shows the rate at which he arranged and wrote out for printing the observations and deductions of several years.

The three chapters in vol. ii. on inheritance, which occupy 84 pages of print, were begun in January and finished on April 1st; the five on crossing, making 106 pages, were written in eight weeks, while the two chapters on selection, covering 57 pages, were begun on June 16th and finished on July 20th.

The work was more than once interrupted by ill-health, and, in September, what proved to be the beginning of a six months' illness forced him to leave home for the water-cure at Malvern. He returned in October, and remained ill and depressed, in spite of the hopeful opinion of one of the most cheery and skilful physicians of the day. Thus he wrote to Sir J. D. Hooker in November:—

“Dr. Brinton has been here (recommended by Busk); he

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does not believe my brain or heart are primarily affected, but I have been so steadily going downhill, I cannot help doubting whether I can ever crawl a little uphill again. Unless I can, enough to work a little, I hope my life may be very short, for to lie on a sofa all day and do nothing but give trouble to the best and kindest of wives and good dear children is dreadful.”

The minor works in this year were a short paper in the ‘Natural History Review’ (N.S. vol. iii. p. 115), entitled “On the so-called *Auditory-Sac* of Cirripedes,” and one in the ‘Geological Society’s Journal’ (vol. xix.), on the “Thickness of the Pampæan Formation near Buenos Ayres.” The paper on Cirripedes was called forth by the criticisms of a German naturalist Krohn,* and is of some interest in illustration of my father’s readiness to admit an error.

With regard to the spread of a belief in Evolution, it could not yet be said that the battle was won, but the growth of belief was undoubtedly rapid. So that, for instance, Charles Kingsley could write to F. D. Maurice :—†

“The state of the scientific mind is most curious ; Darwin is conquering everywhere, and rushing in like a flood, by the mere force of truth and fact.”

Mr. Huxley was as usual active in guiding and stimulating the growing tendency to tolerate or accept the views set forth in the ‘Origin of Species.’ He gave a series of lectures to working men at the School of Mines in November, 1862. These were printed in 1863 from the shorthand notes of Mr. May, as six little blue books, price 4*d.* each, under the title, ‘Our Knowledge of the Causes of Organic Nature.’ When published they were read with interest by my father, who thus refers to them in a letter to Sir J. D. Hooker :—

* Krohn stated that the structures described by my father as ovaries were in reality salivary glands, also that the oviduct runs down to the

orifice described in the ‘Monograph of the Cirripedia’ as the auditory *meatus*.

† Kingsley’s ‘Life,’ vol. ii. p. 171.

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1863.]

MR. HUXLEY'S LECTURES.

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“I am very glad you like Huxley’s lectures. I have been very much struck with them, especially with the ‘Philosophy of Induction.’ I have quarrelled with him for overdoing sterility and ignoring cases from Gärtner and Kölreuter about sterile varieties. His geology is obscure; and I rather doubt about man’s mind and language. But it seems to me *admirably* done, and, as you say, “Oh my!” about the praise of the ‘Origin.’ I can’t help liking it, which makes me rather ashamed of myself.”

My father admired the clearness of exposition shown in the lectures, and in the following letter urges their author to make use of his powers for the advantage of students:]

C. Darwin to T. H. Huxley.

Nov. 5 [1864].

I want to make a suggestion to you, but which may probably have occurred to you. — was reading your Lectures and ended by saying, “I wish he would write a book.” I answered, “he has just written a great book on the skull.” “I don’t call that a book,” she replied, and added, “I want something that people can read; he does write so well.” Now, with your ease in writing, and with knowledge at your fingers’ ends, do you not think you could write a popular Treatise on Zoology? Of course it would be some waste of time, but I have been asked more than a dozen times to recommend something for a beginner and could only think of Carpenter’s Zoology. I am sure that a striking Treatise would do real service to science by educating naturalists. If you were to keep a portfolio open for a couple of years, and throw in slips of paper as subjects crossed your mind, you would soon have a skeleton (and that seems to me the difficulty) on which to put the flesh and colours in your inimitable manner. I believe such a book might have a brilliant success, but I did not intend to scribble so much about it.

Give my kindest remembrance to Mrs. Huxley, and tell

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SPREAD OF EVOLUTION.

[1863.]

her I was looking at 'Enoch Arden,' and as I know how she admires Tennyson, I must call her attention to two sweetly pretty lines (p. 105) . . .

. . . and he meant, he said he meant,
 Perhaps he meant, or partly meant, you well.

Such a gem as this is enough to make me young again, and like poetry with pristine fervour.

My dear Huxley,
 Yours affectionately,
 CH. DARWIN.

[In another letter (Jan. 1865) he returns to the above suggestion, though he was in general strongly opposed to men of science giving up to the writing of text-books, or to teaching, the time that might otherwise have been given to original research.

"I knew there was very little chance of your having time to write a popular treatise on Zoology, but you are about the one man who could do it. At the time I felt it would be almost a sin for you to do it, as it would of course destroy some original work. On the other hand I sometimes think that general and popular treatises are almost as important for the progress of science as original work."

The series of letters will continue the history of the year 1863.]

C. Darwin to J. D. Hooker.

Down, Jan. 3 [1863].

MY DEAR HOOKER.—I am burning with indignation and must exhale. . . . I could not get to sleep till past 3 last night for indignation.* . . .

* It would serve no useful purpose if I were to go into the matter which so strongly roused my father's anger. It was a question of literary

dishonesty, in which a friend was the sufferer, but which in no way affected himself.

1863.]

SCIENCE IN THE COLONIES.

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Now for pleasanter subjects ; we were all amused at your defence of stamp collecting and collecting generally. . . . But, by Jove, I can hardly stomach a grown man collecting stamps. Who would ever have thought of your collecting Wedgwood-ware ! but that is wholly different, like engravings or pictures. We are degenerate descendants of old Josiah W., for we have not a bit of pretty ware in the house.

. . . Notwithstanding the very pleasant reason you give for our not enjoying a holiday, namely, that we have no vices, it is a horrid bore. I have been trying for health's sake to be idle, with no success. What I shall now have to do, will be to erect a tablet in Down Church, "Sacred to the Memory, &c.," and officially die, and then publish books, "by the late Charles Darwin," for I cannot think what has come over me of late ; I always suffered from the excitement of talking, but now it has become ludicrous. I talked lately 1½ hours (broken by tea by myself) with my nephew, and I was [ill] half the night. It is a fearful evil for self and family.

Good-night. Ever yours,
 C. DARWIN.

[The following letter to Sir Julius von Haast,* is an example of the sympathy which he felt with the spread and growth of science in the colonies. It was a feeling not expressed once only, but was frequently present in his mind, and often found utterance. When we, at Cambridge, had the satisfaction of receiving Sir J. von Haast into our body as a Doctor of Science (July 1886), I had the opportunity of hearing from him of the vivid pleasure which this, and other letters from my father, gave him. It was pleasant to see how strong had been the impression made by my father's warm-hearted sympathy—an impression which seemed,

* The late Sir Julius von Haast was, in 1862, Government Geologist to the Province of Canterbury. He was a German by birth, but had long been resident in New Zealand. He

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after more than twenty years, to be as fresh as when it was first received :]

C. Darwin to Julius von Haast.

Down, Jan. 22 [1863].

DEAR SIR,—I thank you most sincerely for sending me your Address and the Geological Report.* I have seldom in my life read anything more spirited and interesting than your address. The progress of your colony makes one proud, and it is really admirable to see a scientific institution founded in so young a nation. I thank you for the very honourable notice of my ‘Origin of Species.’ You will easily believe how much I have been interested by your striking facts on the old glacial period, and I suppose the world might be searched in vain for so grand a display of terraces. You have, indeed, a noble field for scientific research and discovery. I have been extremely much interested by what you say about the tracks of supposed [living] mammalia. Might I ask, if you succeed in discovering what the creatures are, you would have the great kindness to inform me? Perhaps they may turn out something like the Solenhofen bird creature, with its long tail and fingers, with claws to its wings! I may mention that in South America, in completely uninhabited regions, I found spring rat-traps, baited with *cheese*, were very successful in catching the smaller mammals. I would venture to suggest to you to urge on some of the capable members of your institution to observe annually the rate and manner of spreading of European weeds and insects, and especially to observe *what native plants most fail*; this latter point has never been attended to. Do the introduced hive-bees replace any other insect? &c. All such points are, in my opinion, great desiderata in

* Address to the ‘Philosophical Institute of Canterbury (N.Z.)’
 The “Report” is given in the *New*

Zealand Government Gazette, Province of Canterbury, Oct. 1862.

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1863.]

EVOLUTION IN FRANCE.

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science. What an interesting discovery that of the remains
 of prehistoric man!

Believe me, dear Sir,

With the most cordial respect and thanks,

Yours very faithfully,

CHARLES DARWIN.

*C. Darwin to Camille Dareste.**

Down, Feb. 16 [1863].

DEAR AND RESPECTED SIR.—I thank you sincerely for
 your letter and your pamphlet. I had heard (I think in one
 of M. Quatrefages' books) of your work, and was most
 anxious to read it, but did not know where to find it. You
 could not have made me a more valuable present. I have
 only just returned home, and have not yet read your work ;
 when I do if I wish to ask any questions I will venture to
 trouble you. Your approbation of my book on Species has
 gratified me extremely. Several naturalists in England,
 North America, and Germany, have declared that their
 opinions on the subject have in some degree been modified,
 but as far as I know, my book has produced no effect what-
 ever in France, and this makes me the more gratified by your
 very kind expression of approbation. Pray believe me, dear
 Sir, with much respect,

Yours faithfully and obliged,

CH. DARWIN.

C. Darwin to J. D. Hooker.

Down, Feb. 24 [1863].

MY DEAR HOOKER.—I am astonished at your note. I have

* Professor Dareste is a well-
 known worker in Animal Terato-
 logy. He was in 1863 living at
 Lille, but has since then been called

to Paris. My father took a special
 interest in Dareste's work on the
 production of monsters, as bearing
 on the causes of variation.

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not seen the *Athenæum*,* but I have sent for it, and may get it to-morrow ; and will then say what I think.

I have read Lyell's book. ['The Antiquity of Man.'] The whole certainly struck me as a compilation, but of the highest class, for when possible the facts have been verified on the spot, making it almost an original work. The Glacial chapters seem to me best, and in parts magnificent. I could hardly judge about Man, as all the gloss of novelty was completely worn off. But certainly the aggregation of the evidence produced a very striking effect on my mind. The chapter comparing language and changes of species, seems most ingenious and interesting. He has shown great skill in picking out salient points in the argument for change of species ; but I am deeply disappointed (I do not mean personally) to find that his timidity prevents him giving any judgment. . . . From all my communications with him I must ever think that he has really entirely lost faith in the immutability of species ; and yet one of his strongest sentences is nearly as follows: "If it should *ever* † be rendered highly probable that species change by variation and natural selection," &c. &c. I had hoped he would have guided the public as far as his own belief went. . . . One thing does please me on this subject, that he seems to appreciate your work. No doubt the public or a part may be induced to think that, as he gives to us a larger space than to Lamarck, he must think there is something in our views. When reading the brain chapter, it struck me forcibly that if

* In the 'Antiquity of Man,' first edition, p. 480, Lyell criticised somewhat severely Owen's account of the difference between the Human and Simian brains. The number of the *Athenæum* here referred to (1863, p. 262) contains a reply by Professor Owen to Lyell's strictures. The surprise expressed by my father was at the revival of a

controversy which every one believed to be closed. Prof. Huxley (*Medical Times*, Oct. 25, 1862, quoted in 'Man's Place in Nature,' p. 117) spoke of the "two years during which this preposterous controversy has dragged its weary length." And this no doubt expressed a very general feeling.

† The italics are not Lyell's.

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1863.]

'ANTIQUITY OF MAN.'

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he had said openly that he believed in change of species, and as a consequence that man was derived from some Quadrumanous animal, it would have been very proper to have discussed by compilation the differences in the most important organ, viz. the brain. As it is, the chapter seems to me to come in rather by the head and shoulders. I do not think (but then I am as prejudiced as Falconer and Huxley, or more so) that it is too severe; it struck me as given with judicial force. It might perhaps be said with truth that he had no business to judge on a subject on which he knows nothing; but compilers must do this to a certain extent. (You know I value and rank high compilers, being one myself!) I have taken you at your word, and scribbled at great length. If I get the *Athenæum* to-morrow, I will add my impression of Owen's letter.

. . . The Lyells are coming here on Sunday evening to stay till Wednesday. I dread it, but I must say how much disappointed I am that he has not spoken out on species, still less on man. And the best of the joke is that he thinks he has acted with the courage of a martyr of old. I hope I may have taken an exaggerated view of his timidity, and shall *particularly* be glad of your opinion on this head.* When I got his book I turned over the pages, and saw he had discussed the subject of species, and said that I thought he would do more to convert the public than all of us, and now (which makes the case worse for me) I must, in common honesty, retract. I wish to Heaven he had said not a word on the subject.

Wednesday morning: I have read the *Athenæum*. I do not think Lyell will be nearly so much annoyed as you expect. The concluding sentence is no doubt very stinging.

* On this subject my father wrote to Sir Joseph Hooker: "Cordial thanks for your deeply interesting letters about Lyell, Owen, and Co. I cannot say how glad I am to hear that I have not been unjust about the species-question towards Lyell. I feared I had been unreasonable."

No-one but a good anatomist could unravel Owen's letter ; at least it is quite beyond me.

. . . Lyell's memory plays him false when he says all anatomists were astonished at Owen's paper ;* it was often quoted with approbation. I *well* remember Lyell's admiration at this new classification! (Do not repeat this.) I remember it, because, though I knew nothing whatever about the brain, I felt a conviction that a classification thus founded on a single character would break down, and it seemed to me a great error not to separate more completely the Marsupialia. . . .

What an accursed evil it is that there should be all this quarrelling within, what ought to be, the peaceful realms of science.

I will go to my own present subject of inheritance and forget it all for a time. Farewell, my dear old friend,

C. DARWIN.

C. Darwin to Asa Gray.

Down, Feb. 23 [1863].

. . . If you have time to read you will be interested by parts of Lyell's book on man ; but I fear that the best part, about the Glacial period, may be too geological for any one except a regular geologist. He quotes you at the end with gusto. By the way, he told me the other day how pleased some had been by hearing that they could purchase your pamphlet. The *Parthenon* also speaks of it as the ablest contribution to the literature of the subject. It delights me when I see your work appreciated.

The Lyells come here this day week, and I shall grumble at his excessive caution. . . . The public may well say, if such a man dare not or will not speak out his mind, how can we who are ignorant form even a guess on the subject? Lyell was pleased when I told him lately that you thought that language might be used as an excellent illustration of deriva-

* "On the Characters, &c., of the Class Mammalia," 'Linn. Soc. Journal,' ii. 1858.