

Darwin's conception of the theory of natural selection

(After an unpublished letter of 1861)

With the kind permission of the Trustees of the British Museum I am able to publish below in extenso, the text of a letter by CHARLES DARWIN which has not yet been published. At any rate it is not included in any of the collections where one might expect to find it, to wit

FRANCIS DARWIN : *The life and letters of CHARLES DARWIN, edited by his son* (3 vols., London, 1887).

FRANCIS DARWIN and A. C. SEWARD : *More letters of CHARLES DARWIN* (2 vols., London, 1903).

HENRIETTA LITCHFIELD : EMMA DARWIN (1), *a century of family letters, 1792-1896, edited by her daughter* (2 vols., London, 1915).

The letter was presented to the British Museum in 1908 by the widow of THOMAS THORNTON, Esq. It covers eight small pages (18 1/2 × 12 cm.) in DARWIN'S own hand and bears the registration number Add. MS. 37725, f. 6. I reproduce below with the Trustees' permission the most significant page (p. 3). An extract from the letter was quoted in the *Guide to the exhibited MSS. Part I* (p. 73, British Museum, 1912).

The letter is dated March 14th but without mention of the year. That year was certainly 1861 for the following reasons. DARWIN refers to "a new and corrected Ed. of the *Origin* which will appear in about a week or two."

Now the first edition of the *Origin of Species* was published on Nov. 24, 1859, all copies (1250 in number) being sold on the first day. A new edition of 3000 copies was issued on Jan. 7, 1860 : this cannot be the one referred to, which appeared a

(1) That is, EMMA WEDGWOOD (1808-96), who was married to CHARLES DARWIN (1809-82) in 1839.

short time after the middle of March. The reference fits very well to the third edition (2000 copies) published in April 1861.

In the second place, DARWIN refers as to a recent publication to AGASSIZ's review of the *Origin* which had appeared in SILLIMAN's Journal on July 1860.

DARWIN's correspondent is unknown. He was probably an American botanist,—but not ASA GRAY, an old friend whom DARWIN addressed “My dear GRAY”. It would be interesting to determine his identity.

However the main value of this letter lies in DARWIN's appreciation of the theory of natural selection. The comparison with the wave theory of light is a very apt one, which I have not found elsewhere in his writings except in a letter which he wrote at about the same time (April 20, 1861) to FREDERICK WOLLASTON HUTTON (1836-1905), a geologist who emigrated to New Zealand a few years later (1866) and was there professor of geology and curator of the Canterbury Museum in Christchurch, N.Z. DARWIN wrote to him

“I am actually weary of telling people that I do not pretend to adduce direct evidence of one species changing into another, but that I believe that this view in the main is correct, because so many phenomena can be thus grouped together and explained. But it is generally of no use; I cannot make persons see this. I generally throw in their teeth the universally admitted theory of the undulation of light,—neither the undulation nor the very existence of ether being proved, yet admitted because the view explains so much.” (2)

This illustrates DARWIN's moderation and caution. As he wrote on June 6, 1863, to another young naturalist, the self-taught botanist JOHN SCOTT (1838-80) :

“I would suggest to you the advantage, at present, of being very sparing in introducing theory in your papers (I formerly erred much in Geology in that way) : *let theory guide your observations*, but till your reputation is well established be sparing in publishing theory. It makes persons doubt your observations.” (3)

The reader will excuse me for taking this opportunity of referring him to an earlier Darwinian paper “The discovery of the theory of natural selection” (*Isis* 14, 133-154, 1930), which contains facsimile reprints of the earliest publications of DARWIN and ALFRED RUSSEL WALLACE (1823-1913) on the subject.

(2) *More letters* (vol. 1, 184).

(3) *More letters* (vol. 2, 323; see also vol. 1, 217-8).

After his return from the "Beagle" voyage (1831-36), DARWIN lived for a few years in London and Cambridge; then on Sept. 14, 1842 he moved to Down where he spent the rest of his life,—forty years of quiet and almost uninterrupted work and meditation. Down House in Downe, Farnborough, Kent, is now a national museum open to the public, and the official residence of the secretary of the B.A.A.S. I made a pilgrimage to it on September 19, 1934 in the company of F. S. MARVIN and was deeply impressed. I would advise every historian of science who happens to be in London to drive to Down: this can be conveniently done within an afternoon. For more information see the descriptive pamphlet (*Isis* 16, 508; 23, 533 f.) or HOWARTH: *A history of DARWIN's parish* (Southampton, 1933; *Isis* 23, 534).

The letter reproduced below is dated "Down"—that is Down House, in Downe. Sir GEORGE BUKSTON BROWNE, who gave DARWIN's house to the B.A.A.S. never tires of pointing out the difference between Down (the house) and Downe (the village). This matters but little, but go to Downe and Down if you can!

GEORGE SARTON.

March 14th [1861]

Down, Bruly (?) (4), Kent

Dear Sir

I am much obliged for your long letter, as I always like to know how naturalists view the subject. I feel not a shade of surprise at your entirely rejecting my views: my surprise is that I have been successful in converting some few eminent Botanists, Zoologists and Geologists. In several cases the conversion has been very slow and that is the only sort of conversion which I respect (5). I *entirely*

(4) This word Bruly (?) is badly scribbled; should it read Bromley?

(5) DARWIN was perhaps thinking here of J.D.HOOKER and of CHARLES LYELL to whom he wrote on Nov. 23, 1859: "To have maintained in the position of a master, one side of a question for thirty years, and then deliberately give it up, is a fact to which I much doubt whether the records of science offer a parallel. For myself, also, I rejoice profoundly; for, thinking of so many cases of men pursuing an illusion for years, often and often a cold shudder has run through me, and I have asked myself whether I may not have devoted my life to a phantasy. Now I look at it as morally impossible that investigators of truth, like you and HOOKER, can be wholly wrong, and therefore I rest in peace." (*The life and letters of CHARLES DARWIN*, vol. 2, 229, 1887). On March 3, 1860, DARWIN enclosed in a letter to HOOKER a memorandum of the scientists who had already accepted his views. I quote it from the same book p. 293:

illustrate of the case of Light. —

The ether is hypothetical, as are its undulations; but as the undulatory hypothesis groups together & explains a multitude of phenomena, it is universally admitted in the true theory. The undulations in the ether are considered in some degree probable, because sound is produced by undulations in air. So natural selection, I think it is in some degree probable, or possible, because we know what artificial selection can do. — But I believe in nat. selection, wh

agree with you that there is no more [p. 2] direct proof of variation being unlimited in amount than there is that it is strictly limited. In a new and corrected Edit. of the Origin, which will appear in about a week or two, I have printed this as emphatically as I could. I did not formerly explicitly say this (but indirectly in several places) because I thought it was obvious. The manner in which I wish to approach the whole subject, and in which it seems to me it may fairly be approached, I can best [p. 3] illustrate in the case of Light. The ether is hypothetical, as are its undulations; but as the undulatory hypothesis groups together and explains a multitude of phenomena, it is universally now admitted as the true theory. The undulations in the ether are considered in some degree probable, because sound is produced by undulations in air. So natural selection, I look at as in some degree probable, or possible, because we know what artificial selection can do. But I believe in nat. selection, not [p. 4] because, I can prove in any single case that it has changed one species into another, but because it groups and explains well (as it seems to me) a host of facts in classification, embryology, morphology, rudimentary organs, geological succession and Distribution. I have no space to discuss the many points alluded to in your letter. I cannot see such perfection in structure as you do. In the new Edit. I have attempted to explain how it is that [p. 5] many low forms have not progressed to a higher grade of organisation. I did not allude to the very curious subject of "alternate generations" because I did not, and do not yet, see, how it has any *special* bearing on my views. I look at alternate generations, as not essentially differing from various stages in any one individual larva—a form of generation being merily added at some stage. [p. 6] Under the point of view I see no essential difference between alternate generations and metempsychosis: you, I presume, take some very different view. I forget what AGASSIZ says on the subject. I quite agree with you that AGASSIZ' Review (6) is not in the least unfair. He misunderstands me a good deal. His [p. 7] "categories of thought," "prophetic types" and his views on classification are to me merely empty sounds. To others they seem full of meaning.

I received several months ago and thank you for, a very curious pamphlet on representative form (7) (or some such title) which interested me *very much*. With [p. 8] the best thanks, I remain,

Dear Sir

Yours very faithfully

CHARLES DARWIN.

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

Four geologists: LYELL, ANDREW RAMSAY, J. B. JUKES, H. D. ROGERS. Four zoologists and palaeontologists: HUXLEY, J. LUBBOCK, L. JENYNS (to large extent), SEARLES WOOD. Two physiologists: CARPENTER, Sir H. HOLLAND (to large extent). Five botanists: HOOKER, H. C. WATSON, ASA GRAY (to some extent), Dr. BOOTT (to large extent), THWAITES. In all fifteen men, not a very large number.

(6) This refers almost certainly to LOUIS AGASSIZ' review of the *Origin of Species* in the *American Journal of Science and Arts of July 1860*. See *Life and Letters* (vol. 2, 330, and by index).

(7) I cannot be quite sure of the reading of these two words because I do not understand the reference.