



all straight through, as otherwise it will be unintelligible. Try not to condemn it utterly, till you have finished it and reflected on the recapitulation. Not that I am so foolish as to expect to convert anyone, who has long viewed the subject from an opposite point of view. I remember too well how many long years my own conversion took. The utmost which I hope, is that you may see that more can be said on the side of the mutability of specific forms, than is at first apparent. If indeed your own observations have made you at all sceptical on the subject, then my Book may produce some effect. Hooker, whose studies make him, I think, one of the most competent judges in Europe, has completely given up species as immutable creations. Pray forgive me for troubling you with this note, which does not require any answer & pray believe me

Dear Phillips

Yours very sincerely

CHARLES DARWIN.

We have no record of the reply which Phillips sent to Darwin, but on November 25th Darwin wrote to T. H. Huxley<sup>4</sup>, 'Phillips is cautious, but decidedly, I fear, hostile.' On the following day, he wrote to Phillips the second letter which is quoted below. The letter was written from Yorkshire, where, as Darwin wrote to Huxley in another letter<sup>5</sup>, he was 'hydropathising and coming to life again, after having finished my accursed book, which would have been easy work to anyone else, but half-killed me'.

ILKLEY WELLS HOUSE,  
OTLEY, YORKSHIRE.  
Nov. 26th [1859]

MY DEAR PHILLIPS,

Thank you for your note. Permit me to say one word about my Book. Though many facts in palaeontology may appear, or be really, opposed to my notions, & though my explanations may be quite fallacious, I earnestly beg you to consider whether a theory wholly false could explain, as it seems to me to explain, several classes of facts—as affinity of inhabitants of islands to nearest continent—the nature of the inhabitants of oceanic islands—the affinities & classification of organic beings & their arrangement in groups—the strange fact of a being of one group being adapted to the habits of another group—the facts of morphology or homology—Embryology & Rudimentary Organs.—If you think the theory of Natural Selection does *not* to a large extent explain these classes of facts, I have not a word to say.

Pray forgive me saying a word in favour of my offspring to one, whom I consider an important judge.

Yours very sincerely

C. DARWIN.

In a letter which is recorded<sup>6</sup> as having been written a few days later, Darwin recounts the following to Asa Gray, the American naturalist:

'By the way, I met the other day Phillips, the palaeontologist, and he asked me, "How do you define a species?"'

<sup>4</sup> *More Letters of Charles Darwin*, i, p. 130.

<sup>5</sup> *Life and Letters of Charles Darwin*, ii, p. 172.

<sup>6</sup> *More Letters of Charles Darwin*, i, p. 127.

I answered, "I cannot." Whereupon he said, "At last I have found out the only true definition, — any form which has ever had a specific name!"'

About a year passed before the third brief letter was written.

DOWN BROMLEY,  
KENT.

Nov. 14th [1860]

DEAR PHILLIPS,

Many thanks for your kind present of your work *Life on the Earth*. I have glanced at the part about the Origin & am sorry, but not surprised, to see that you are dead against me.

Pray believe me Dear Phillips

Yours very sincerely

CII. DARWIN.

On May 15th, 1860, John Phillips delivered the Rede Lecture at Cambridge under the title of 'The Succession of Life on the Earth.' This was subsequently printed in book form and this is the 'kind present' which Darwin acknowledges in his note. Just before the lecture, J. S. Henslow, Professor of Botany at Cambridge, sounded the note of expectancy to his son-in-law, J. D. Hooker,

'I don't know whether you care to hear Phillips, who delivers the Rede Lecture in the Senate House next Tuesday at 2 p.m. It is understood that he means to attack the Darwinian hypothesis of Natural Selection . . .'<sup>7</sup>

The first report of this section of Phillips's lecture was that he had 'treated it very fairly', and this comment by Henslow was quoted by Darwin to Charles Lyell,<sup>8</sup> but on November 20th he wrote to Lyell again,

'I have not had heart to read Phillips yet'<sup>9</sup>

and on January 15th, 1861, to Hooker:

'I quite agree that Phillips is unreadably dull'<sup>10</sup> while in reply to Asa Gray, who had sent a copy of the latter's review in the *American Journal of Science* of 'Life on the Earth', Darwin wrote:

'You rank Phillips's book higher than I do, or than Lyell does, who thinks it fearfully retrograde.'<sup>11</sup>

<sup>7</sup> *Life and Letters of Sir J. D. Hooker*, ed. L. Huxley (London, 1918), i, p. 512.

<sup>8</sup> *Life and Letters of Charles Darwin*, ii, p. 309.

<sup>9</sup> *Op. cit. supra*, p. 349.

<sup>10</sup> *Op. cit. supra*, p. 358.

<sup>11</sup> *Op. cit. supra*, p. 373.

To another correspondent, Leonard Horner, Darwin said:

'I would rather have been well attacked than have been handled in the namby-pamby, old-woman style of the cautious Oxford Professor.'<sup>12</sup>

These last-quoted words show how much Darwin's confidence had increased since he wrote the first letter to Phillips. His hypothesis of 'Natural Selection' had drawn a steadily increasing band of adherents and even the fierce opposition which had been aroused in certain quarters, as exemplified by the wordy battles at the British Association meeting at Oxford in June, 1860, between Bishop Wilberforce and Richard Owen and Darwin's friends, Huxley and Hooker, seemed to be preferable to the sympathetic scepticism of Phillips. To the latter, an orthodox palaeontologist of great field experience, there appeared many difficulties to the unreserved acceptance of the new theory and his attitude was expressed in his presidential address to the Geological Society in February, 1860:

'Geologists . . . have added, from the evidence which their peculiar studies yield, a definite origin of each natural group in time, the approximate duration of the life of many, and the epochs of termination of several. Thus every specific form is conceived to have sprung into being at a certain point on the globe, at a definite epoch of time; its existence is traced through provinces of space and through periods of time, so that it has a real physical history.

For those who adopt this view, the course of reasoning on the succession of life on the globe is clear and convincing. It is, however, not universally adopted; but the hypotheses which have been framed to replace it (which always involve the idea of indefinite change of form, structure, and habits) would not, if adopted, materially affect the conclusions of geology, or change the practice of naturalists. If it is by the course of progressive change from older types that new specific forms have arisen, there must have been for each of these a time and place when it began to manifest the new specific distinction. Geology needs not to discuss these hypotheses, sanctioned though they may be by eminent names, amongst whom our Darwin is pre-eminent for powers of generalisation operating on a large basis of personal observation. None of them appear to be wholly without a foundation of fact, though none of them can be held to penetrate more than a small way into the mystery of the origin of species. We may grant, with Lamarck, the inherent power of an organic body to undergo some change, or to effect some self-development, by reason of the intensive or abnormal exercise of its organs; we may allow to external conditions some influence in modifying the sensible characters of species, which is so boldly claimed by the author of the *Vestiges of Creation*; and we may agree with Mr. Darwin in his more practical view of the derivation of some specific forms of one period from others of earlier date by descent with modification. We may accept all this, and yet consistently retain the conviction that the changes which are possible by such causes are circumscribed within the many essential types of structure which appear to be a part of the plan of creation.'<sup>13</sup>

<sup>12</sup> *More Letters of Charles Darwin*, ii, p. 31.

<sup>13</sup> *Quart. J. Geol. Soc. Lon.*, 1860, vol. 16, p. xxxvi.

It is not the purpose of this note to comment on the views expressed in the quotations given but rather to give some insight into the background of ideas which existed when Darwin's book was published.

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