

of my scientific predictions relative to the existence or former existence—though within the memory of man—of a huge bird in New Zealand. I had a fragment of one of its bones three years ago and ventured to build it up into “a heavier bird than the ostrich but as big;” it turns out, however, to have been much bigger, and has excited, I think, more interest than anything that has occurred in my line. Dr. Buckland, to whom the bones of said bird were sent, and who has made them over to me, partly attributes his recovery to them. He sent me a note this morning which he had received from the Queen’s Master of the Household (Hon. Charles A. Murray), who says, after a compliment to me: “The Prince has read your letter with the greatest interest; he desires me to thank you in his name, and if any further discoveries should be made in elucidation of the mystery of this feathered monster, pray let me again have the pleasure of hearing from you and of communicating the information to His Royal Highness.”

In this month Darwin wrote on the subject of his work on ‘Coral Reefs’ to Owen. In this letter he refers to some preliminary papers of Owen’s on the ‘Archetype,’ afterwards developed into his classic on the ‘Archetype and Homologies of the Vertebrate Skeleton,’ which appeared in 1848:—

Down, Farnborough, Kent.

‘ My dear Owen,— . . . I am *much* pleased at your praise of my Coral volume, and am very glad you recommend it to the notice of voyagers. It would undoubtedly be far more suggestive to any one who will really attend to the subject, but for the generality, perhaps, the abstract in my journal would be the most [useful]. . . . I have lately read with *very great* interest all the parts which I could follow in your Report on Archetypes, &c. You may remember that I suggested explanations to the woodcuts. I am not a quarter satisfied yet. You may with perfect justice say you do not write for tyros ; but if ever you take compassion (and there is no other claim) on ignoramuses such as myself, you will in every woodcut give the name to every letter or number in your woodcuts, even if repeated 500 times, for just that many times will it make your work intelligible to the ignorant.

‘ Believe me,

‘ Yours very sincerely,

‘ C. DARWIN.’

It was in this month also that a box arrived from New Zealand containing a large assortment of the bones of the dinornis, of which he had already described the ‘shaft of a femur’ in 1837.

‘ On January 19,’ the diary records, ‘ we opened the long-expected box from New Zealand,

that the town should reap the full benefits of the water supply. For that purpose the supply must be directed, by combined sewage works, irrespective of private and public streets, so as to carry off the sewerage from every, even the humblest dwelling.' Owen concludes with a promise of some further remarks on the 'economical and profitable results of the water and sewage works viewed merely as an investment.'

In July, Charles Darwin wrote a letter to Owen on the subject of the Cirripecta.⁹ Their place in the system had occasioned considerable doubt and difference of opinion amongst zoologists, but Darwin's researches went far to settle the vexed question of their zoological position, and so the following letter may be found of interest:—

Down, Farnborough, Kent : July 17 [1854].

'Dear Owen,—. . . . I cannot tell you how much gratified I am at what you say about the Cirripecta. I really feel rewarded for more labour than you would readily believe it possible could have been bestowed on the work. I have, however, made a mess of it, for I got so frightened at the thoughts of all the *seaside* species, that I have not illustrated and given in nearly detail enough my anatomical work,

⁹ A well-defined natural group of marine invertebrate animals, commonly known as 'barnacles.' They are very widely diffused—in fact, there

are scarcely any seas without some of the species, as they frequently fix themselves on to floating bodies.

which is the only part of the work which has really interested me. I find the mere systematic part infinitely tedious. I can, however, honestly state that all I have said on the males of *Ibla* and *Scalpellum* is the result of the most careful and repeated observation. If I am ever proved wrong in it, I shall be surprised. But my pen is running away with me; it is your fault, for I have been so much pleased with what you say. Making out the homologies of the shell and external parts of Cirripedes, as I fully believe correctly (and I am glad to say that Dana admits the view), gave me great satisfaction. But I must not bore you with my triumph. I have been very seldom in London for the last year. When I was last there I called at the College to see you, but you were just gone out. Pray believe me, in a great state of triumph, pride, vanity and conceit, &c., &c., &c.,

‘Yours sincerely,

‘CHARLES DARWIN.’

In December 1854 Owen was offered and declined the chair of Anatomy in the University of Edinburgh, which was rendered vacant by the death of Edward Forbes. Writing to his sister (December 20), he says: ‘Poor Edward Forbes! There was never a scientific man whose unexpected death caused a more general or sincere regret. . . . I declined the offer to succeed him, as I was by no means sure that after fulfilling the duties of

following note written to Lyell, which is included by Francis Darwin in his 'Life' of his father:—

'How curious I shall be to know what line Owen will take! Dead against us, I fear; but he wrote me a most liberal note on the reception of my book, and said he was quite prepared to answer fairly, and without prejudice, my line of argument.'

After a meeting with Owen, Darwin writes him the following interesting letter respecting the 'Origin':—

Down, Bromley, Kent: December 13 (1859).

'Dear Owen,— . . . You made a remark in our conversation something to the effect that my book could not probably be true as it attempted to explain so much. I can only answer that this might be objected to any view embracing two or three classes of facts. Yet I assure you that its truth has often and often weighed heavily on me; and I have thought that perhaps my book might be a case like Macleay's quinary system.⁸ So strongly did I feel this that I resolved to give it all up, as far as I could, if I did not convince at least two or three competent judges. You smiled at me for sticking myself up as a martyr; but I assure you if you had heard the unmerciful and, I think, unjust things said of my book and to

⁸ 'An artificial attempt at a natural system of classification which soon became a byword among naturalists.'—*Dict. Nat. Biogr.*

me in a letter by an old and very distinguished friend you would not wonder at me being sensitive, perhaps ridiculously sensitive. Forgive these remarks. I should be a dolt not to value your scientific opinion very highly. If my views are *in the main* correct, whatever value they may possess in pushing on science will now depend very little on me, but on the verdict pronounced by men eminent in science.

‘Believe me,

‘Yours very truly,

‘C. DARWIN.’

In the early part of this letter Darwin says he is not able to hunt up some information for which Owen has asked, as his ‘notes for the latter chapters are a chaos.’ The ‘old and very distinguished friend’ Dr. Francis Darwin considers to be Adam Sedgwick.

If not ‘dead against’ the theory of Natural Selection, Owen at first looked askance at it, preferring the idea of the great scheme of Nature which he had himself advanced. He was of opinion that the operation of external influences and the resulting ‘contest of existence’ lead to certain species becoming extinct. Thus it came about, he supposed, that, like the dodo in recent times, the *dinornis* and other gigantic birds had disappeared. But he never, so far as can be ascertained, expressed a definite opinion on Darwinism, and