

us, that we cannot say why one native weed or insect swarms in numbers, and another closely allied weed or insect is rare. It is then possible that we should understand why one group of beings has risen in the scale of life during the long lapse of time, and another group has remained stationary? Sir C. Lyell, who has given so excellent a discussion on species in his great work on the "Antiquity of Man," has advanced a somewhat analogous objection, namely, that the mammals, such as seals or bats, which alone have been enabled to reach oceanic islands, have not been developed into various terrestrial forms, fitted to fill the unoccupied places in their new island-homes; but Sir Charles has partly answered his own objection. Certainly I never anticipated that I should have had to encounter objections on the score that organic beings have not undergone a greater amount of change than that stamped in plain letters on almost every line of their structure. I cannot here resist expressing my satisfaction that Sir Charles Lyell, to whom I have for so many years looked up as my master in geology, has said (2nd edit. p. 469):—"Yet we ought by no means to undervalue the importance of the step which will have been made, should it hereafter be found the general result of a series of men of science (as I fully expect it will) that the past changes of the organic world have been brought about by the subordinate agency of such causes as Variation and Natural Selection." The whole subject of the gradual modification of species is only now opening out. There surely is a grand future for Natural History. Even the vital force may hereafter come within the grasp of modern science, its correlation with other forces have already been ably indicated by Dr. Carpenter in the *Philosophical Transactions*; but the nature of life will not be seized on by assuming that Foraminifera are periodically generated from slime or ooze.

CHARLES DARWIN.

THE ANTIQUITY OF MAN.

10, Kent Terrace, April 20, 1863.

It is with regret that I find myself at issue with the Author of the "Antiquity of Man." I could have wished to have avoided any controversy on the subject, as I hope at some future period to have a fitting time and occasion for my own account of the inquiry; but there are portions of Sir Charles Lyell's letter to the *Athenæum*, of the 18th inst., in reply to Dr. Falconer's letter in the number of the 4th inst., which call for some brief notice on my part. I would most willingly have commented on the proofs of Sir Charles Lyell's important work had they been submitted to me before the publication of the first edition; not having had that opportunity, I found myself obliged to report to Sir Charles, when he wrote to ask me for a list of errata and corrections for the second edition, that "I raised objection to the tone and cast of some chapters, and that the corrections I might think necessary would involve more alterations than was practicable, or than could originate with me." I referred as an example to the Bedford case, "in which so many important geological questions hinge." Sir Charles in his reply informed me that, after referring to the published accounts of it, he did not see what he had to alter. It is possible that I may not have been sufficiently explicit. I should regret if it were so.

With regard to the particular case, I can only repeat the statement that I made to my friend Dr. Falconer, that the Bedford section was made out by me long before the period of Sir Charles's visit there; that the main features were pointed out by me to him on that occasion; and that I further brought a short notice of Mr. Wyatt's interesting discovery together with the first geological description of the section before the Geological Society, in March, 1851,—some what prematurely, possibly, my being part of a general inquiry, in which, as Sir Charles knew, I had been engaged for some years. I should have waited until I could have brought forward the whole subject (long unavoidably delayed by the limited measure of time I can take from active business avocations), but for its special bearing on the question of the Antiquity of Man, and the publicity given to this case. Only those

engaged in the study of the quaternary deposits, and who know how difficult it is to obtain definite facts, and how many days and years may be spent in examining ground which affords only negative evidence, can understand the importance of a good positive case like that of Bedford. I quite agree with Sir Charles Lyell in his observations about too frequent a reference to original authorities in a popular work; it may even be a question whether the general reader may not consider such references to authorities and to companionship already too frequent in "The Antiquity of Man." No doubt, as Sir Charles observes, the public generally are satisfied to learn from him his own conclusions in as few words as possible; but he must remember that he is also addressing a large scientific public, and that it is not a question of frequency; but of accurate reference that is contended for. I am satisfied that whatever may have been the intention of Sir Charles, his readers must form a very inaccurate idea of the important part taken for many years past by Dr. Falconer in researches connected with the antiquity of man, in the investigation of horse caves in general, and of the Brixham cave in particular, as well as of the relative part taken by the various geologists named by Sir Charles and by Sir Charles himself in other parts of the investigation. I have been greatly interested in the progress of the Brixham cave exploration, and can fully corroborate Dr. Falconer's account of it; and this misapprehension is another reason which makes me regret the delay in the publication of the final results.

Sir Charles Lyell is perfectly correct in saying that I have modified my views since the publication of my references (not memoirs) on this subject. But I would remark that the paper was read before the Royal Society in the month following my first visit to the Somme Valley and to Hoxne, and that in it I contented myself with a description of the ground and with the determination of the geological age of the deposits—points which remain unimpaired—and stated that I reserved my views on the theoretical questions for a future inquiry and separate paper as early as the end of three years. I brought these forth in a memoir, read before the Royal Society in March, 1852; and although my views had, I admit, been modified and matured, the main question of the post-glacial age of the beds was confirmed by various new sections; whilst, although feeling that the period concerned is one of very remote antiquity, I still adhere to the opinion I had before expressed, that the evidence does not carry man back in past times more than it brings forward the great extinct mammalia towards recent times.

One of the great charms of scientific inquiry lies in the free and intimate intercourse and interchange of ideas amongst men engaged in the same branches of research. In such intercourse, where each observer contributes his facts or his opinions, the starting-point of some of these must often be lost to view, and all men of science must, at times, have felt and experienced that, in the lapse of time, an unconscious process of greater or lesser mental assimilation unavoidably takes place. It is, therefore, only when certain limits are passed, albeit inadvertently, that any one would care or think fit to object.

Every geologist must feel indebted to Sir Charles Lyell for the philosophical spirit he has brought to bear in geological inquiry, and all must admire the untiring energy with which he has for years past investigated the phenomena he describes. Having studied with him in the field many of the complicated phenomena of the post-pliocene deposits, while I claim as my share of the work the detection and the interpretation of certain physical phenomena, I am free to acknowledge the pleasure I have received from his views on the causation of the various questions arising therefrom with a geologist so experienced and philosophical as Sir Charles Lyell.

JOSEPH PRESTWICH.

THE NEW ZEALAND MOAS.

April 22, 1863.

A paragraph is now going round the papers stating that, just before the mail left, one of the

most gigantic of birds, a Moa or Dinornis, and believed to be extinct, had been seen alive in New Zealand, and that an enterprising colonist had offered a reward of 500*l.* for its capture, dead or alive. The public seem to be divided respecting the amount of credence to be attached to the story; but the fact that a gentleman residing on the spot thought it worth while to engage a band of hunters would seem to show that there was, in his judgment, some probability on the very face of it. That some of the smaller species of Dinornis may still be alive is an opinion which even Prof. Owen, if I understood him rightly, entertains. If extinct, the Moas have become so probably in quite recent times—that is to say, since the occupation of New Zealand by the Maoris. This opinion, I think, may be supported by philological arguments, briefly stated in my Official Report on the Fiji Islands, presented to Parliament, May, 1862, and also in my "Viti," p. 383, where I said:—"Toa" is the Fijian form of the word "Moa," applied throughout Polynesia to domestic fowls, and by the Maoris to the most gigantic extinct birds (*Dinornis*, sp. plur.) disintegrated in New Zealand. The Polynesian term for birds that fly about freely in the air is *Mauu* or *Mannannu*; and the fact that the New Zealanders did not choose one of these, but the one implying domesticity and want of free locomotion in the air, would seem a proof that the New Zealand Moas were actually seen alive by the Maoris about their premises, as stated in their traditions, and have only become extinct in comparatively recent times."

BERTHOLOD SEEMANN.

SCIENTIFIC BALLOON ASCENT.

Buckingham, April 21.

In the *Athenæum* of the 11th inst. are detailed the observations I made on the sky spectra in the Balloon Ascent on March 31. They were so different from what I expected that I could not avoid coming to the conclusion, that they were of little value in consequence of the ascent having been made so late in the day. I therefore resolved that on the next ascent, which was to be made on was near the meridian, and that the spectrum examination should be a primary subject of investigation. The apparatus was the same as that used on the previous experiments. It was covered with black cloth to prevent any stray light falling on the prism, and whilst observing my head was also covered with black cloth. Between the hours of 11 A.M. and noon, I examined the solar and sky spectra with care. The sky was generally covered with cumuli, and there was a great mist. The solar spectrum extended from B to H nearly; and the sky spectrum from B to G, but there were quite its limiting lines.

We left the earth on April 18 at 1h. 17m. P.M.; within two minutes afterwards we were 3,000 feet, and at 1h. 23m. we were one mile high. The second mile was passed at 1h. 30m.; the third at 1h. 35m.; the fourth at 1h. 40m.; and the highest point was reached at 2h. 30m.—at the height of four and a half miles nearly. At 2h. 35m. we passed below four miles; the next mile downwards was passed at 2h. 40m.; and at 2h. 46m. we were two miles from the earth, which we reached at 2h. 50m. At 1h. 20m. looking close to the sun, the line G was very clear, as well as the two nebulous lines H, and the spectrum extended somewhat farther; many lines were seen. At 1h. 21m. at the red end of the sky spectrum near the sun, the line B was very clear, and many lines between B and F were visible. At 1h. 23m. the sky spectrum under and close to the sun extended from A at the red end to beyond H, the lines were beautifully defined, and I thought somewhat more numerous than as viewed from the earth. At 1h. 25m. the sky spectrum extended some little distance from the red end to reach to A, and scarcely to B; but there were many lines between these extremes. At 1h. 33m. on directing the slit to the sky far from the sun, the field of view was dark. At 1h. 37m. the balloon was revolving I had a beam of light from the sun, whilst looking at the red end, and all lines were clear up to A. At 1h. 39m. the sky spectrum directed to a point in the sky as near the zenith as the balloon permitted, and the spectrum was