

was the first to break off from the basal stem of the Giant Primates. The orang, although it has the giant size, has retained the brachiating or arm locomotion of the gibbon stock, his thumb and great toe have become vestiges; the process of shortening of the spinal column, which set in during the Hylobatian stage, has progressed, so that now the lower limbs are attached to the body one or two vertebrae higher than in man, the gorilla, and chimpanzee. It has retained a primitive arrangement of the air cavities of the nose and face, whereas man, the gorilla, and chimpanzee have the same elaborate arrangement of cells which differentiate them from all other primates.

The orang's lower limbs are in a state of retrogression—as opposite to human limbs as could be. The Aurignac man, which Prof. Klaatsch assigns to the orang stock, is remarkable for his narrow and long head, whereas the orang's head is the most rounded of all primate forms. The resemblance between the humerus of the Aurignac man and that of the orang is fanciful, in my opinion. If we may judge the basal orang stock from its modern descendants, the one thing we can be certain of is that it is the last of the Higher Primate stocks which is likely to give rise to the human race. On the other hand, the chimpanzee, and especially the gorilla, are evidently the descendants of a stock from which it is not difficult to suppose the primary human stock may have arisen. The tendency to a greater use of the lower limbs was evidently already present in that primitive stock.

The conclusion I reached in 1900 simply confirmed the statements made by Huxley in 1863.

Nothing is impossible in nature, but there are some things which are highly improbable. A multiple origin for a single species is one of the most improbable, and, so far as the human species is concerned, there is no need to suppose a multiple origin. Prof. Klaatsch's opinion of anthropoid apes throws an interesting light on his theory. He has reverted to a slight modification of the very ancient view of the anthropoids—that they are representatives of retrograde humanity. In Herr Bonin's words, Klaatsch regards the gorilla and the other man-like apes as "failed experiments of man." There is no scientific basis for such a statement—the gorilla fills its place in nature quite as satisfactorily as man.

This view of the nature of the anthropoids only affects us so far as it may help us to understand Prof. Klaatsch's theory of the "pan-anthropoid" origin of human races. If that opinion is well founded, the opinion that the Higher Primates were designed as experiments in "Menschwerdung," then, of course, it follows that the experimenter may have succeeded on several occasions, and that each of the primitive primates may have given rise to races of men. In reality, we are being again introduced to the old theory of design, and hence the statement in my last letter that Prof. Klaatsch's theory exceeded "the limits of rational speculation."

A. KEITH.

Royal College of Surgeons, England, February 4.

"In Forbidden Seas."

"D. W. T.," who writes a review of the sea-otter, or rather of a book called "In Forbidden Seas," in *NATURE* of January 26, tells us that he is not aware that any living naturalist has ever seen this animal in its natural state. Now, Captain H. J. Snow, who is the author of "In Forbidden Seas," is, from my point of view, a first-class field naturalist, who by his collections and observations has added considerably to zoological and geological knowledge. I may add that he is also a keen surveyor, and his maps of the islands in these "Forbidden Seas" were so far back as 1895 published for the use of sailors by our Admiralty. By the publication of these charts, the shortest routes between Vancouver and certain ports on the Asiatic coast have been freed from uncertainties and dangers. Canadian and other vessels crossing the North Pacific, in cases of emergency have new harbours of refuge which can be approached with comparative safety. Snow's charts show new rocks and shoals, take out others, adjust islands in longitude, indicate anchorages, tide rips, watering places, seal and seal-lion rookeries, and, in short, make the unknown known. Sailing directions go with the charts.

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By reason of their knowledge of these Forbidden Seas and our ignorance of them, in 1855 the Russian fleet was enabled to evade that of the French and English allies. H.M.S. *Rattler* was wrecked in these seas, and the Japanese man-of-war *Tabor* was totally lost. The disabling of several gunboats which have attempted to survey these islands, and the numerous wrecks of British and other schooners which are to be found along their shores, testify to the difficulties which surround the navigation of these waters before the advent of Captain Snow. The Royal Geographical Society were so impressed by the value of his work that they awarded him one of their annual grants, and approached the Lords of the Admiralty to obtain for its author substantial recognition. Had the work been carried out by one of our surveying vessels it would have cost this country many thousands of pounds. All that was learnt was to the effect that no rule existed for the payment for work of this description. At a subsequent date the Rt. Hon. Arthur J. Balfour was approached. Among the signatories to the petition I see the name of the president of the Royal Geographical Society on behalf of the council, the Admiral of the Fleet, Rudyard Kipling, and those of many other well-known persons. Captain Snow gave up his working tools and received no recognition. I know that captains and admirals of British ships, like commissioners sent out to study seal fisheries, have sought and obtained valuable information from Captain Snow.

JOHN MILNE.

Shide, Newport, Isle of Wight, January 30.

I AM surprised and sorry that Prof. Milne should think, as he seems to do, that I sought to belittle Captain Snow's achievements, for I not only based my article on the sea-otter upon Captain Snow's additions to zoological knowledge, but I also paid an unstinted compliment to Captain Snow's romantic and adventurous career. I mentioned briefly that Captain Snow had won the reputation of an authority on the geography of the Kuriles; but that brief statement, brief because I was not dealing with, and was, indeed, very imperfectly acquainted with, his geographical work, was necessarily inadequate. Prof. Milne has done proper justice to this part of Captain Snow's work.

As regards the valuable information that Captain Snow has given to persons charged with the inspection of the seal-fisheries, I can bear testimony of my own. Still better testimony can be found, for instance, in Dr. L. Stejneger's report of 1898 on the Asiatic fur-seal islands, for Dr. Stejneger not only draws his description of the Kurile seal-rookeries chiefly from Captain Snow, but pays tribute to his "invaluable additions to the authentic history of the Kuriles," and to himself as "a man of unusual ability, literary and scientific, for the profession he had chosen to follow."

D'ARCY W. THOMPSON.

An Apparently hitherto Unnoticed "Anticipation" of the Theory of Natural Selection.

IN Loudon's *Magazine of Natural History*, 1835, pp. 40-53, there appears an article entitled "An attempt to classify the 'Varieties' of Animals, with observations on the marked Seasonal and other Changes which naturally take place in various British Species and which do not constitute Varieties," by Mr. Edward Blyth. Certain passages contained therein seem to indicate that the principle of natural selection, or the survival of the fittest, was clearly understood by Blyth in 1835, and, further, that he recognised its application to artificial selection. Moreover, he demonstrates the idea of sexual selection in one of its bearings. I have therefore considered them of sufficient interest to be made public, as it appears they have hitherto escaped notice.

"When two animals are matched together, each remarkable for a certain peculiarity, no matter how trivial, there is also a decided tendency in nature for that peculiarity to increase; and if the produce of these animals be set apart, and only those in which the same peculiarity is most apparent, be selected to breed from, the next generation will possess it in a still more remarkable degree; and so on, till at length the variety I designate a *breed*, is formed, which may be very unlike the original type." . . . "It is worthy of remark, however, that the original and typical

form of an animal is in great measure kept up by the same identical means by which a true *breed* is produced. The original form of a species is unquestionably better adapted to its *natural* habits than any modification of that form; and, as the sexual passions excite to rivalry and conflict, and the stronger must always prevail over the weaker, the latter, in a state of nature, is allowed but few opportunities of continuing its race. In a large herd of cattle, the strongest bull drives from him all the younger and weaker individuals of his own sex, and remains sole master of the herd; so that all the young which are produced must have had their origin from one which possessed the maximum of power and physical strength, and which, consequently, in the struggle for existence, was the best able to maintain his ground and defend himself from every enemy. In like manner, among animals which procure their food by means of their agility, strength, or delicacy of sense, the one best organised must always obtain the greatest quantity, and must, therefore, become physically the strongest, and be thus enabled, by routing its opponents, to transmit its superior qualities to a greater number of offspring. The same law, therefore, which was intended by Providence to keep up the typical qualities of a species, can be easily converted by man into a means of raising different varieties; but it is also clear that, if man did not keep these breeds by regulating the sexual intercourse, they would all, naturally soon revert to the original type. Farther, it is only on this principle that we can satisfactorily account for the degenerating effects said to be produced by the much censured practice of breeding in and in. There would almost seem, in some species, to be a tendency, in every separate family, to some particular kind of deviation, which is only counteracted by the various crossings which, in a state of nature, must take place, and by the above-mentioned law, which causes each race to be chiefly propagated by the most typical and perfect individuals" (pp. 45-46).¹

On the suggestion of Prof. Cossar Ewart, the above quotation was submitted to Mr. Francis Darwin, who has kindly informed me that he agrees with my remarks in general, but is unable to state definitely the identity of the author.

In his introduction to the "Origin" Darwin notices several such "anticipations," but no reference is made to Blyth's name in this connection.

It seems indeed strange that Darwin should have been unacquainted with this article, and, what appears stranger still, that Blyth himself should have failed to direct attention to his paper, or that there should be no mention of these passages in either Darwin's or Blyth's correspondence. Mr. Francis Darwin has, however, indicated "More Letters," i., p. 62) that much of Darwin's correspondence with Blyth has not been forthcoming. This is to be regretted.

Curiously enough, in a letter to Lyell, Darwin says:—"Blyth says (and he is in many respects a good judge) that his ideas on species are quite revolutionised. . . ." ("Life and Letters," ii., 1887, p. 316.)

At this juncture the question naturally arises, viz., Is the Edward Blyth of the article the Edward Blyth of Calcutta? On turning to Grote's "memoir" (Journal Asiatic Soc. Bengal, August, 1875, part ii., supplement), we find (p. 5) that Blyth contributed to both Loudon's and Charlesworth's series of the *Magazine of Natural History* from the year 1833. From the titles of the various articles which appear under Edward Blyth's name in Loudon's *Magazine*, there is no evidence to indicate that all these contributions did not originate from the same writer. On Grote's evidence we are therefore justified in concluding that our author is the naturalist who afterwards made himself famous by his writings on, and profound knowledge of, the mammals and birds of India. Moreover, this conclusion is substantiated by our author's address, given in the same volume of Loudon in several instances as "Tooting, Surrey," and we learn ("Dict. National Biog., London, 1886, vol. v., p. 276, art. Blyth, Edward) that Blyth purchased a druggist's business at Tooting on coming of age.

Mr. J. Ritchie, of the Royal Scottish Museum, has suggested to me that Blyth, in 1859, may quite easily have forgotten what he had written twenty-four years

¹ The italics in this quotation are Blyth's.

previously, the more so as he failed in the true application of his "principle." The association of his ideas with those of Darwin would, therefore, be incomplete or entirely wanting.

Though Blyth seems clearly to have recognised the principle of natural selection, he fails in its true application in that he regards his "principle" as operating for the conservation rather than the progression of the type, whereas the two really go hand in hand, the one being a complement of the other in the successive stages of evolution. Moreover, proof of Blyth's inability to recognise the logical issue of his theory is exhibited in some of his remarks, which appear to disagree, or are incompatible with, one another. For instance, it is hard to reconcile the sentence commencing "Farther," and ending "breeding in and in," with some of his previous statements.

Blyth was a staunch supporter of Darwin's views, and his early theorisings are of interest in connection with his projected work on "The Origination of Species," which, however, was never completed, even in manuscript form (Grote, *loc. cit.*, p. xiv).

H. M. VICKERS.

81A Princes Street, Edinburgh, February 3.

The Sailing-Flight of Birds.

IN NATURE of February 2, Mr. Mallock remarks that the skimming of some birds near the surface of the waves, where the variations in the velocity of the wind are great, may be dependent only on the inequalities of a horizontal breeze, and that an upward current is not absolutely necessary. My own observations have led me to the conclusion that whenever a bird glides for any distance without losing altitude he is, no less than the soaring kite or eagle, utilising an upward current of air. But it is possible that Mr. Mallock may be thinking of the albatross, who is perhaps without peer in his power of profiting by the vagaries of the wind. Unfortunately, I have had no opportunities of observing the albatross, and from those who have I get very conflicting accounts, some maintaining that he will glide for long distances under conditions which make it almost certain that the wind is horizontal, others holding that, though he brings the art to greater perfection, he does nothing different in kind from what the gull, that hangs with outstretched wings over the stern of a steamer, is able to achieve.

My object in writing this is to urge any of your readers whose good fortune gives them opportunities of watching the albatross on the wing to make careful observations on this very interesting subject.

F. W. HEADLEY.

I AGREE with Mr. Headley that observations of the various conditions under which flight with fixed wings can be accomplished are desirable, but it is quite as important to determine the motion of the air in any particular case as to observe the behaviour of the bird.

In the case of a bird skimming close to the surface of waves, the action is presumably that sketched below. To



appreciate this properly, regard must be had to the vertical motion of the air in respect to time as well as to the wave surface. It is assumed that the speed of the wind is greater than that of the waves, and that the bird is flying to windwards. In these circumstances, the mean velocity of the air is less in the lee of each wave-crest than it is on the windward slope (indeed, when the waves are steep, the flow on the lee side may be reversed).

If a bird follows the course indicated by the dotted line, it gains, not only from the ascending current off the windward slopes, but also from the increased velocity it can acquire by dropping to a low level in the slower wind to the leeward of them.

The question of possible flight by variations of horizontal velocity has been treated by Lord Rayleigh and Mr. R. E. Froude.

A. MALLOCK.

IN the flight of birds, besides the change in the inclination of the wing planes noted by the Rev. R. Abbay in NATURE of February 9, there must surely be some movement either of the wing, tail, or body which takes the place of the screw of the aeroplane. The seagull, for