

tween the trout which respectively inhabited the two ponds ("British and Irish Salmonidæ," pp. 226-27, 1837). Will anyone undertake to affirm, after looking at the coloured plates, that these changes must necessarily have been due to selection? Again, in a recent communication to the *Field* (July 7), Mr. Day gives an engraving of a remarkable variation which is taking place in the gill-covers of trout which have been transported to New Zealand, and there "turned down" under nature. Premising only that, although this is a change of structure, there is no more adaptive meaning to be found in it than in those changes of colour above mentioned,¹ I will quote Mr. Day's remarks upon the subject: "It will be interesting to watch the changes occurring among these trout in their new home, and to observe whether these serrations are continued or merely temporary; for if they should become developed with time there would be still more reason for constituting them a new species than now exists among the various European races; while, should trout with serrated preopercles and interopercles be admitted as constituting a new species, we could now trace the process of development from its commencement, and show how such has been occasioned by transplanting our European trout to the warmer waters of the Antipodes."

Should it be objected that, as a matter of fact, the state of matters anticipated by Mr. Day has not yet arrived, my answer would be obvious—namely, *supposing that such a state of matters had arrived*, could the fact be reasonably held to annihilate the whole theory of natural selection? Yet this is what such a fact would necessarily do, if we hold it to be "a necessary consequence of the theory" that every species which exists, exists in virtue of having been "selected." If we have not here a *reductio ad absurdum*, I do not know how one can ever hope to apply that method.

Of course I am not disputing that in general there is a very great distinction between local varieties and good species in respect of peculiar adaptive characters. In other words, I have no doubt at all that probably the great majority of species have been originated by natural selection, either as the sole cause or in association with other causes. But the allegation which I am resisting is, that it follows as a necessary consequence from the theory of selection itself that every species must owe its origin to selection. And I have endeavoured to show that this allegation admits of being reduced to an absurdity. When Mr. Wallace, in the letter above referred to, expresses dissent from Mr. Gulick's view that species are frequently originated by the influence of isolation alone, he adds: "If this is a fact, it is a most important and fundamental fact, equal in its far-reaching significance to natural selection itself; I accordingly read the paper with continual expectation of finding some evidence of this momentous principle, but in vain." Now, supposing that Mr. Wallace had found the evidence which would have fully satisfied him, would he therefore have been logically required to abandon his own great generalization? Would he have been required to acknowledge, not only, as he says, a principle "equal in its far-reaching significance to natural selection itself," but a principle which altogether superseded that of natural selection? I say it is absurd to suppose that such would have been the case, and yet it must necessarily have been the case if it be "a necessary consequence" of his theory that all (if any) species are originated by selection.

It will be remembered that I am not arguing the biological question whether, or how far, species exist which do not owe their existence to selection; I am arguing only the logical question whether it is "a necessary consequence of the theory of selection" that they cannot. And I now submit that it no more follows from the selection theory alone, that "every variety" which becomes "a species" does so "in consequence of being in some one or more respects better adapted to its surroundings than its existing contemporaries, than it does that every variety which becomes a variety does so for the same reason. If the former statement is a statement of biological fact (which, for my own part, I do not believe), the fact is one that would stand to be proved inductively as a fact: it cannot be made good by way of logical deduction "from the theory of selection."

¹ In this connection, also, it is of great importance to remember that it is only twenty years ago since the trout in question were sent to New Zealand, and their fry liberated in the waters there; for the most ardent upholder of the theory of natural selection as the sole cause of specific transmutation will scarcely maintain that twenty years is long enough for survival of the fittest to effect a structural change of an "unknown" adaptive character in a long-lived animal with all the waters of New Zealand to spread over.

I have thus dealt with Mr. Huxley's criticism at some length, because, although it has reference mainly to a matter of logical definition, and in no way touches my own theory of "physiological selection," it appears to me a matter of interest from a dialectical point of view, and also because it does involve certain questions of considerable importance from a biological point of view. Moreover, I object to being accused of misunderstanding the theory of natural selection, merely because some of my critics have not sufficiently considered what appears to them a "paradoxical" way of regarding it.

GEORGE J. ROMANES.

How Sea-Birds Dine.

As I have ascertained that the following fact is not well known, I send you this account in the hope that it may be of interest to naturalists and to the general public. Anyone who lives in the Western Hebrides will have often watched on a calm day the sea-birds feeding with noisy clamour in the sea-lochs and about the numerous islands. This is especially the case in August, when the shoals of small herring are very plentiful. Some years ago, when in a sailing-boat off the west coast of Mull, I caught with a hand-net a dishful of these small fry as they swam along the surface of the water. Last year, noticing from a steam-launch the birds congregated in great numbers at one spot, the idea struck me to steam to the place and try to get a share of the birds' repast. The idea was at once carried out. I stood on the prow with landing-net in hand, and the launch was steered towards the birds. As we drew near, the banqueters flew away with evident dissatisfaction at the interruption, a few of the more greedy making their last hasty dives. In another moment we were at the spot, and I saw, to my intense surprise, about 2 feet under the surface, a large reddish-brown ball, 2 to 3 feet in length and 2 feet in depth. I made a frantic swoop with the net into the ball, and brought on deck half a pailful of the sea-birds' dinner. Even as we passed we could see the great living ball sinking and breaking into pieces. This year I and others have tried the same spot with great success. Sometimes the ball has sunk too deep to be reached; sometimes there was no ball to be seen; but on the most successful day I filled a pailful in three hauls. In September we saw no ball, because, perhaps, the fish had grown too large for the birds to manage. As far as I can judge, the *modus operandi* is carried out by the divers, who surround a shoal and hem them in on all sides, so that the terrified fish huddle together in a vain effort to escape inevitable destruction. The divers work from below and other sea-birds feed from above; and, as in some cases after the birds had been at work for some time I saw no ball, I suppose not one fish is left to tell the tale. I must leave to naturalists the real explanation of the matter; but I may mention that, when disturbed by the boat, the divers seem to come to the surface in a great ring round the scene of their feast. I may also mention that once, when the boat was still 300 or 400 yards away, the birds suddenly rose and whirled about with frightened screams. I wondered what could be the cause, until I saw the round back of a porpoise rolling lazily round at the exact spot, and then rolling back again. When we steamed past there was no sign of a ball. What two delicious mouthfuls for the porpoise!

COMPTON.

Loch Luichart, Ross-shire, N.B.

The Zodiacal Light.

MR. O. T. SHERMAN gives an interesting communication on the zodiacal light in *NATURE* of October 18 (p. 594), and asks for reference to any observations. He alludes to Cassini. The following extract from a letter by Cassini may not have come under his notice: "It is a remarkable circumstance that since the end of the year 1688, when this light began to grow fainter, spots should have no longer appeared on the sun, while in the preceding years they were very frequent, which seems to support, in a manner, the conjecture that the light may arise from the same emanations as the spots and facule of the sun." This does not quite tally with Mr. Sherman's notion that the maxima of the zodiacal light coincide with the minima of sun-spots. May it not rather be that, supposing sun-spots to be largely occasioned by increased influx of meteoric matter falling into the sun, which matter gets sublimed and repulsed to augment the materials forming the zodiacal light, therefore the maxima of the latter may then lag behind the maxima of the sun-spots.

HENRY MUIRHEAD.

Cambuslang, October 20.