

The Colours of Arctic and Alpine Animals

I MUCH regret that I have been too busy to reply to my friend, Mr. A. R. Wallace (NATURE, April 16, p. 552), till the present moment, but this delay, unavoidable on my part, is the less to be regretted, since it has given an opportunity for the interesting facts recently adduced by Sig. Lorenzo Camerano (NATURE, May 28, p. 77) to be taken into consideration. As Mr. Wallace, with that keen penetration so familiar to all who know him and his writings, goes to the root of the matter under discussion and raises a distinct issue, I will now beg permission to offer a few words in reply to both these gentlemen.

First, with respect to the physical side of the question, Mr. Wallace is perfectly correct in supposing that colour *per se* has no influence upon the radiating or absorbing powers of bodies as far as regards obscure radiation. But I would point out that in the present case we are not concerned with colour alone; we have not merely to consider whether black or white is the best radiator, but we have for comparison two surfaces, hair or feathers, as the case may be, having, as far as we know at present, the same structure, and differing only in colour. The question before us is whether this colour-difference in the same substance is associated with any difference in radiating or absorbing power, and the final answer can only be given by carefully conducted experiments. I may add that I have long been waiting for an opportunity of conducting the necessary investigation, and with aid that has been kindly offered from several quarters I hope before long to be in a position to arrive at some satisfactory conclusion. The form of experiment suggested by Mr. Wallace, although decidedly worth the trial, does not appear to me to be very safe, inasmuch as the natural structure and arrangement of the fur would be lost in the process of weaving into cloth. Mr. Wallace's strictures as to the use of artificial dyes are, however, quite sound, and in these I fully concur. I may further state that when this question was raised some years ago, I searched literature (although by no means exhaustively) to see whether any experiments had been recorded, and although many hundreds of observations upon the radiative and absorptive powers of different bodies have been made by various physicists from the time of Franklin downwards, I have not been able to find any experiment bearing directly upon the question under consideration.

The point to be decided is, not only whether dark hair or feathers are better radiators than white hair or feathers, but whether the radiative power of these white coverings is less for that particular kind of radiation which is most greedily absorbed by the substance (snow) among which the animals have to pass their winter existence. Till this problem is solved physically we have, as it seems to me, only the purely biological considerations to fall back upon.

Before passing on to the more strictly zoological side of the subject I should like to disclaim the notion to which Sig. Camerano's letter may give rise, that the radiative (as distinguished from the protective) theory of Arctic colouring is original as far as concerns myself. With respect to the white covering of the warm-blooded animals, this theory was, as far as I knew at the time, original when first broached in 1880; but Lord Walsingham afterwards showed that the same conclusion had been arrived at in 1846 by Craven, with whose name it should be more fairly associated. The application of this theory (in a reversed sense) to explain the melanism of Arctic insects is entirely due to Lord Walsingham, and as my friend Mr. Wallace is disposed to give the weight of his authority to this extension of the theory, there is no occasion to discuss this point further on the present occasion.

It now remains to point out some of the considerations which

have led me to the belief that the protective theory of white colouring is not wholly sufficient. Thus, among birds there seems to be a tendency among the falcons (*F. candicans*, *F. islandus*, &c.) to become white in high latitudes—a mode of coloration which does not appear to me to be of much use in such species. These birds, as far as I know, swoop down on their prey from above, under which circumstances the lighter colouring would be of no advantage in enabling them to approach their prey undetected; on the other hand, it can hardly be maintained that these birds are subject to any persecution which would cause their lighter plumage to be of protective value. When on the wing the back only would be seen by another bird hovering over the falcon, and it is noteworthy that this part of the falcons in question is darker than the under side. The same considerations apply to the snowy owl (*Nyctea scandiaca*). In many other birds, again, such as the plovers (*Charadrius pluvialis*, *Squatarola cinerea*, &c.) and various species of *Scolopacia* (*Tringa variabilis*, *T. subarquata*, &c.), the under side only changes to white in winter—a change which it is impossible to associate either with protection from foes or with predatory advantage. On the other hand, it seems not unreasonable to suppose (on the radiation theory) that the under side of the bird, being nearest to the snow-covered surface of the ground, would require the most protection. It is of interest also to bear in mind from the present point of view that many mammals are known to become white on the under side during winter. Thus, Surgeon-Major Leith Adams, F.R.S., states in his observations on the natural history of Eastern Canada¹ that "there is, moreover, a seemingly strong disposition for the lower parts of animals to become white in winter—i.e. the parts in closest contact with the snow; thus the under surfaces of the deer tribe are always whitest. And, as if from its habit of constantly digging among the snow with its snout in quest of food, we find the cariboo with a white patch on its lips and around the hoof, &c." Such facts as these cannot, as it appears to me, be explained on the protection theory; but if any connection exists between the mode of colouring of an animal and its external conditions of life, the theory of preventive radiation or even the direct action of low temperature on the formation of the pigment seems to be more applicable.

The objections raised by Signor Camerano, although supported by some interesting observations, are, I venture to think, somewhat wide of the mark. The writer, indeed, endeavours to bring within the scope of the radiation theory classes of facts which I for one should certainly never dream of attributing to this cause, even if it had been demonstrated on a sound experimental basis. There can be no question as to the truth of his concluding statement that the causes tending to modify the colours are of an extremely complex character. It is this very complexity, indeed, which renders it so highly important to thoroughly investigate any explanation which bears the stamp of truth, though perhaps applicable to but a very limited group of facts. In view of these difficulties, and bearing in mind the inexhaustible resources of nature in adapting organisms to their environment by apparently opposite means, it is not at all surprising that cases should exist which stand apparently opposed to the particular class of cases here dealt with. There are many conceivable ways of enabling an animal to struggle against a severe climate besides that of lightening the colour of its fur, and natural selection would take advantage of any and every means presented for securing this end. To say, therefore, that some animals become darker in winter (*Cervus mandarinus*), or that others do not change colour at all (*Rupicapra europea*, *Capra ibex*), is no real objection to the radiation theory, but simply an illustration of the principle that there are many ways of securing the same result. Thus, in the case of the two last-named species, Sig. Camerano himself states that there is a great difference in the thickness of the winter covering. Then, again, the statement that a more or less distinct seasonal change of colour is observable in many animals appears to me to have no precise bearing on the question—all that can be said from the point of view either of adaptation or climatic protection is that in such slight mutations we have given to us a hint as to the method by which the more striking seasonal changes have been brought about. We must regard such changes either as the incipient stages of a seasonal variation which could, if necessary, be worked up into a more perfect adaptation (*protective or climatic*), or as the vanishing remnants of a seasonal variation formerly important, but now useless. The facts that some animals which are not polar or alpine are *permanently white*, that the

¹ "Field and Forest Rambles," 1873, p. 124.

colours of some Alpine Coleoptera are brighter than those of the warmer plains, and that the species of small islands often show a tendency to melanism, are at present simply inexplicable, but, as far as I can see, do not tell for or against either theory. It would certainly be a strong case against the present view if any animal could be named which became white in winter and was not an inhabitant of a country subject to cold winters. As far as my knowledge extends no such species exists. The light colour of desert mammals is most probably due to predatory advantage—the melanism of desert insects mentioned by Sig. Camerano is, I must confess, a new fact to me, and not at all in accordance with my own limited experience. The strongest objection raised by Sig. Camerano is, perhaps, contained in the statement that in the birds of the Antarctic region black is much more prevalent than in those of the Arctic regions. It is unfortunate, however, that the writer adduces in illustration such countries as Australia and New Zealand, which certainly cannot be considered within the Antarctic region.

In conclusion I should like to emphasize that the theory of climatic protection is not, as Mr. Wallace appears to believe, *opposed* to the theory of adaptation. If my first letter gave rise to this impression, I will take the present opportunity of pointing out that the animal kingdom abounds with cases of what our German colleagues happily call "functional change" (*Funktionswechsel*)—that is, the conversion of a character (or function) originally acquired for one purpose to a totally new use. It is thus not at all improbable that a mode of coloration originally acquired as a climatic protection, may afterwards be found to be of adaptive value, so that climatic and natural selection would in such cases work together. I fully concede that many of the Arctic and Alpine species now derive such advantages from their white covering; the question is whether this colouring was originally acquired solely for this purpose, or whether climatic adaptation may not have had an equal or even a greater influence in its production.

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