

## LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

## Permanent Variation of Colour in Fish

A QUESTION of some interest is raised by a letter published by Mr. Saville Kent, in *NATURE*, vol. viii. p. 25. It is stated that a Plaice, now in the Brighton Aquarium, has "the posterior half of its under surface, usually white, coloured and spotted as brilliantly as the upper one; the line of demarcation between these two colours again, though sinuous, is most abrupt," and the writer proceeds to say that, on the Darwinian theory, this may be considered as a remarkable instance of reversion—"the *Pleuronectidæ* being derived from ancestors originally possessing bilateral symmetry, and an equal degree of coloration on each side."

First, as to the fact:—Examples of such colouring among the *Pleuronectidæ* are not very uncommon, and they occur most frequently in the Flounder (*Pleuronectes flesus*) and Plaice (*P. platessa*). Sometimes it is the upper surface which is thus affected—more or less of it being purely white. In a specimen now before me the colouring of the upper surface occurs upon the under one in numerous blotches of various sizes, and this mode of distribution is not uncommon. In every instance that I have heard of, the line or lines of demarcation, when they exist, are such as your correspondent describes, but, in extreme cases, no such line is present—the whole of one surface having uniformly assumed the colouring of the other. Such abnormal colouring may occur either upon the upper or lower surfaces; the fish in the former case being entirely white, and in the latter entirely brown.

The rationale offered by your correspondent, although engaging, is not unopen to criticism. For nothing can be more evident to Darwinists than that the colouring of the *Pleuronectidæ* has been acquired because of its protective adaptation to their peculiar form and habits. But it is difficult to see how such colouring could have conferred protection upon their free-swimming ancestors, so that, unless we make the highly anti-Darwinian supposition that the common progenitor was coloured in anticipation of the habits to be contracted by its offspring, there is only one hypothesis open to us, viz., that the unmodified progenitor adopted, through natural selection, the habit of lying on its side because of its original sandy colour. As this view, however, will be rejected by all who know how much easier colour is to modify than habit or structure, we are compelled to adopt the supposition, as being the most probable, that the coloration of the *Pleuronectidæ* is the result and not the cause of their form, and has, therefore, been acquired during the process of their flattening.

Although, however, we cannot, without gratuitous supposition, imagine that the unmodified ancestor of the group in question was coloured exactly like his progeny, there is still one other hypothesis by which atavism might be called in to explain such instances as that adduced by your correspondent. Whatever may have been the original cause of the flattening taking place, it is not likely that the initial variations (whether these were sudden and considerable, or gradual and slight), presented nearly so great a modification as that which we now observe. During these initial stages the partially modified individuals may have lain indifferently on either side, and so have acquired protective colouring on both. As the flattening, however, proceeded (from whatever cause), and the bones of the skull, etc., became more and more contorted, the new exigencies of the case might have caused the left side to be more and more used as a ventral surface, until its colouring, being of no further use, was allowed gradually to disappear. Upon this view the deviations from the normal colouring which now occur would be reversions, not to the bilaterally-symmetrical ancestors of the flat-fishes, but to their partially modified offspring. And, if this view were tenable, it might throw some light upon the otherwise inexplicable fact that some species of *Pleuronectidæ* are normally reversed—i.e. the left side instead of the right, constituting the upper surface—while in both kind of species individuals often occur which are reversed with reference to their specific type.

As however, this explanation is rather far-fetched, and, moreover, fails to account for the appearance of the partly white and the wholly white specimens above mentioned, it is best, I think, altogether to abandon the reversion theory.

Another, and, to my mind, a more probable one is open to us.

Accepting the occurrence of abnormally reversed fish as an unexplained fact, we might, *a priori*, expect that a cross between a normal and a reversed individual of the same species might present the appearance described in your correspondent's letter—the abrupt, though sinuous line of demarcation between the two colours, which always attends the occurrence of this variation, being precisely analogous to that which obtains in higher animals when piebald. Moreover, the abnormal coloration being of most frequent occurrence in the Flounder and Plaice—fish which are also the most frequently reversed—and the occasional appearance of the entirely white and entirely brown varieties, are just the facts we should anticipate were this explanation the correct one. Of course it may be objected that abnormal colouring is not of nearly so frequent occurrence as abnormal reversal, but when we remember how utterly ignorant we are regarding the causes which determine reversal in the *Pleuronectidæ*, and the blending or non-blending of colours in all animals when crossed, we should not lay too much stress upon this objection.

The truth or falsehood of this explanation would admit of easy experimental test on the part of the Brighton Aquarium authorities. Should they, however, undertake such, they must not rest satisfied with mere simple crosses, however numerous, but also try various complex and reciprocal ones. The piebald fish they possess should also be crossed with several normal and reversed Plaice. Should all their experiments prove unsuccessful, they would still be interesting as tending to throw us back upon the only remaining explanation, viz. that all these instances of abnormal coloration are independent sports, and so affording us by far the most striking of the many examples in the animal kingdom of the tendency towards bilateral symmetry which abnormal colouring frequently presents.

Dunskait, Ross-shire, May 15

GEORGE J. ROMANES

## Venomous Caterpillars

THE concluding words of Mr. H. S. Wilson's letter in your last number only reiterate the truth of a fact. Nearly all British entomologists who have collected *Lepidoptera* must have had painful experience of the irritation caused by the hairs of some one or other of our Bombyces that have very hairy larvæ. *Por-thesia chrysorrhæa* is the greatest delinquent in this respect; and some years since I suffered intense agony after collecting the pupæ of this species. The hairs of the caterpillar are woven into the cocoon and the web surrounding it, and I recommend anyone in search of a counter-irritant to rub his face and neck with his hands after collecting these pupæ. The result, although painful, will be edifying and admonitory. The hairs have no effect upon the harder skin of the palm of the hand and fingers; and I believe (with most entomologists) that their action is purely mechanical, i.e. they pierce the tender skin in multitudes. A precisely similar, though less severe, effect is caused by the hairs of some Boraginaceous plants, e.g. *Echium vulgare*. On the Continent the extreme irritation caused by the hairs of *Cnethocampa procionea* is well known; and the introduction of a brood of these larvæ into a drawing-room would probably be followed by effects similar to those caused by the king's "great flea" in Faust.

At present I consider that the existence of caterpillars actually venomous (i.e. with a poison-gland at the base of each hair) requires confirmation. There are some pachydermatous individuals upon whom the hairs of Bombyces have little or no effect. I am unhappily not one of those, but my mental hide repels the insidious attacks of romancers in Natural History.

Lewisham, May 16

ROBERT MCLACHLAN

BETWEEN the years 1857 and 1862 when stationed at Beloz, the capital of British Honduras, I made the acquaintance of a so-called venomous caterpillar, which was held in very great dread by the natives, who averred that "its bite always produced fever."

Knowing their superstitious habits, and that, as far as my knowledge of natural history went, there did not exist a caterpillar capable of producing a wound of any kind by biting, I resolved to test the truth of the assertion. Accordingly, and to the intense horror of the bystanders, I took one in my hand from a tree that was literally covered with them. It was about 1½ in. long, by ¾ in. thick, of a blue-grey tint, and in addition to the fine long hairs which clothed it, was armed with clusters of short spines. These clusters were formed into rows

and contained about a dozen spines each. After a careful examination, I came to the conclusion that they were most likely to be the seat of the venomous propensities attributed to the insect, so I struck the back of my right hand against them two or three times to see what would be the effect. They were very brittle, and broke off as they entered the skin. I thought no more about it till about an hour had elapsed, when I experienced in the wrist a dead pain which gradually extended to the arm-pit, followed by a swelling of the glands.

For the whole day the pain was sufficient to render my arm useless; hence I thought that there must be some poisonous secretion in the spines, for the irritation caused by fine points, even if barbed, would scarcely produce such an effect. The pain died away in the evening, unattended by any feverish symptoms whatever, for I was in excellent health at the time. Next day I examined several of the spines under the microscope; they were not barbed, but hollow, and under pressure emitted a colourless transparent fluid, to which I attributed the poisonous qualities which caused me so much pain. A. M. FESTING

#### The Demagnetisation of Needles.

It may not be generally known that magnetised needles, like those used in galvanometers and telegraphs, are easily and rapidly demagnetised in the neighbourhood of other magnets, when the fields of the two magnets are not coincident—that is, when their respective lines of force are not in the same direction.

A striking instance of this has just been brought to my notice. A tangent galvanometer used for taking daily readings of the escape of the current to earth upon wires, when they are disconnected at their terminal points, was found constantly and gradually to be losing its delicacy. This was traced to be due to the demagnetisation of the needle. The needle was re-hardened and even changed but with the same effect. The galvanometer was fixed near some Wheatstone's A B C instruments, which, being worked by magneto-electric currents, have powerful permanent magnets within them. The galvanometer was shifted to the other side of the office, when the effect entirely ceased.

Hence those who have delicate galvanometers should be careful to see that they are not kept in the field of permanent magnets, unless, as in the case of the mariner's compass, they are free to move in the direction of the lines of forces of the magnetic field in which they lie.

Southampton, May 20

W. H. PREECE

#### Microscopes—Information Wanted

I AM following up some investigations and experiments in which I require certain data, which, however, I cannot at present arrive at, not being in possession of sufficiently delicate and exact instrumental appliances. The information which I now desire to elicit from some more experienced observers than myself is of such importance as to be both useful and interesting to many of your readers, and I therefore crave your insertion of this communication. The information I require is all the more important as having a bearing upon many questions which are now attracting public attention, such as spontaneous generation, the initial stage and transitional forms of living organisms, also various researches in experimental physics, chemistry, &c. I desire to arrive at the following data:—

1. What is the estimated dimensions of most minute particles of matter which can be visible, under any circumstances or conditions, under the highest powers of the microscope? I leave out of consideration (under this head) the question whether such matter is living or dead, organic or inorganic, or in fact regardless of any of its properties whatever except its mere visibility as a minute portion of matter. Some observers speak of visible particles  $\frac{1}{1000}$ th and  $\frac{1}{2000}$ th of an inch diameter; this is surely near the limit.

2. What is the best or most accurate method of arriving at an estimate of the dimensions of such minute objects as are too small to admit of actual measurement by any of the appliances now in use? Every microscopist knows from experience that objects may be distinctly visible, not as a mere point, but having an appreciable diameter, and yet be too minute for actual measurement to any degree of accuracy.

3. Have the most recently constructed microscopic objectives, such as the  $\frac{1}{1000}$ th or  $\frac{1}{2000}$ th, any advantages over the  $\frac{1}{1000}$ th or  $\frac{1}{2000}$ th

inch objectives in the determination of the data above referred to? and have immersion lenses any advantage in this respect? I find some difference of opinion on this point. Some microscopists consider that a really first-class  $\frac{1}{1000}$ th with the use of deep eyepieces will enable us to see anything whatever which can be seen by any other objective of shorter focus. On the other hand, it is evident that a great number of the most experienced microscopists think otherwise; and from the very fact of their purchase of such expensive high powers, argue that such lenses are found to supply what other powers cannot accomplish.

It appears to me that there is too much of vague and indefinite assertion in regard to the comparative powers and qualities of microscopic objectives, and it is very desirable that some more definite results should be arrived at. With what precision and accuracy the results of astronomical observations are made! and taking into consideration that many of these results are obtained by different methods of observation, using different instruments, and by different observers, it is astonishing that the discrepancies and errors of observation are so small. It is generally admitted that the microscope is, to say the least, equally perfect; if not more so, than the telescope; and we should therefore expect a corresponding degree of accuracy in the results of microscopical observations. There are no doubt many who, like myself, have hitherto worked with only the medium and low powers, but wish to be possessed of the improved objectives of high power, but from want of sufficient information it is difficult to make a suitable choice. H. H.

Melbourne, Victoria, March 27

#### Arctic Exploration

THE story of the American Arctic Expedition under Mr. Hall is a wonderfully curious one; but are we justified, from what we have been told, in coming to the conclusion that the part of the crew of the *Polaris*, that has been rescued in so remarkable a manner, are "deserters?"

As far as I have understood the reports which have appeared in the papers, none of the rescued men have said they were deserters; and until we hear what those who remained on board the *Polaris* have to say, it appears to be unjust and reprehensible to bring so grave an accusation against men, possibly innocent.

Should it so happen that Mr. Tyson and his companions are deserters, can we put faith in the correctness of any part of their story?

There is certainly some mistake about the disposal of the six boats of the ship. As far as I can make out, only four, or at most five, are accounted for, namely, two abandoned in Smith Sound, and the two on the ice with Mr. Tyson, one of which was burnt for fuel, and the other, that in which they were when rescued, and which was taken on board the *Tigrits*.

May 31

JOHN RAE

#### The Westerly Progress of Cities

IN his work on the Atmosphere, M. Flammarion draws attention to a peculiarity in the habits of our large towns which everyone must have noticed. "The wealthy classes have a pronounced tendency to emigrate westward, leaving the eastern districts for the labouring populations. This remark applies not only to Paris, but to most great cities—London, Vienna, Berlin, St. Petersburg, Turin, Liège, Toulouse, Montpellier, Caen, and even Pompeii."

Having frequently remarked this "westing" in many English towns, I have lately written to several friends, asking for definite information on this point, concerning the town in which they are resident. With scarcely an exception the reply of each showed, to alter Bishop Berkeley's line a little, that:—"Westward the course of fashion takes its way." This is true, I believe, of Edinburgh, Dublin in former years at any rate, Glasgow, Birmingham, Leeds, Southampton, Bristol, and Liverpool and Manchester to some extent. No doubt many of your readers can very largely extend this list; it would be interesting to collect wide information on this question. For supposing it established as a general fact, what an excellent speculation to buy up land in the west of a rapidly growing town like Leicester or Bradford! Perhaps it is common to do so already.

Whence arises this tendency? It can hardly be an accident, nor can it be due to the direction of the river beside which the town may happen to be built, for in the towns named, many of