

only its first pair of legs are black and yellow, and these alone are stretched out conspicuously (see Fig. 2). The great differences between the attitudes of these two closely related moths, corresponding to the distribution of startling colours upon them, afford a very strong support to the theory of warning colours. Mr. Beddard might reply that they thus make prominent the unpalatable pigments that the enemies may first



FIG. 1.

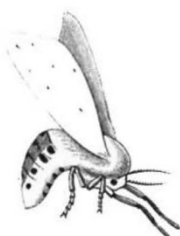


FIG. 2.

make trial of them upon a material which will ensure their ultimate rejection. But if the colour has not a meaning as such, there is no reason why this spot should be attacked in preference to any other part of the exposed surface; and the existence of the colour as a covering to the most vital parts seems to indicate that it acts as a warning away rather than in the reverse manner.

The fact that brightly coloured animals are frequently attacked does not seem to me to be a great difficulty. The really important point is whether the enemy remembers the attack, and is assisted in identifying the unpalatable species by its bright colours. Many experiments seem to show that this is so. Certainly Mr. Beddard will not assert that the majority of insect-eating animals fail to know and recognize a wasp without tasting it. Again, the question is really, as Mr. Titchener implies in his interesting communication, one of "comparative palatability"; and there is no doubt that insect-eating animals when sufficiently hungry will attack and sometimes devour insects which they would ordinarily reject. Furthermore, an animal which naturally prefers a varied insect food, and which is fed in confinement largely on other substances and partially on a monotonous insect diet, may be expected to be less scrupulous than it would be in the wild state. I may state, however, that the most intelligent insect-eating animals, such as the marmoset, hardly ever make mistakes; their suspicion being at once aroused by any trace of a warning colour.

It is well known that we chiefly owe the theory of warning colours to Mr. A. R. Wallace. My own conviction of its entire validity rests upon the results of a prolonged series of experiments, of which only a part has been published. I believe that I conducted these experiments fairly, that my mind was open, and that I had no personal bias in the matter at all, either in favour of or against the theory. And I can confidently make the same claim on behalf of others who have experimented in the same manner—such as Mr. Jenner Weir, Prof. Weismann, and M. Portchinsky. I may allude especially to the writings of the last-named authority, as they are the most important as well as the most recent contribution to the theory which we owe to Mr. Wallace.

I may also take this opportunity of replying to a very similar objection raised by some reviewers against my book on the "Colours of Animals, their Meaning and Use, &c." They point out that I have not alluded to Eimer's work on the comparison of the wing-markings of *Papilionide*, and they assume that his paper has, therefore, escaped my attention. But Eimer's paper has no bearing whatever on the value of colour in the struggle for existence, and this is the subject of my book, as anyone can infer from the preface, or even from the title. For this reason I was also compelled to omit reference to what I venture to regard as the far more important work of Weismann on the development of the colours and marking of caterpillars, and of Dixey on the wing-markings of *Vanesside* and *Argynniide*, as well as a very large proportion of my own work, which is a continuation of that begun by Weismann, and was, in fact, inspired by it.

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My friend Prof. Meldola has drawn my attention to a communication by Mr. F. E. Beddard in NATURE of November 26 (p. 78), in which the view is expressed that the brimstone butterfly (*Gonepteryx rhamni*) is rendered protected or unpalatable by the yellow pigment of its wings being due to a substance formed as "a urinary pigment," and that the coloration is "a consequence of the deposition in the integument of bitter pigments."

The following objections may be urged against the view that this coloration, said to be of the nature of a "urinary pigment," affords any protection whatever.

Gonepteryx rhamni itself has its female much paler than the male and of a greenish-white hue, whilst the wings in both sexes are of a leaf-like appearance, which can only be due to the process of natural selection, and can scarcely have been exercised in the direction of "protective resemblance" if the insect was already unpalatable by the "urinary" nature of the yellow pigment of its wings.

Yellow Lepidoptera have certainly no immunity from the attacks of birds; on the contrary, the scanty records we possess of these onslaughts go to prove that the contrary is the case. The late Mr. P. H. Gosse observed one of the greenlets (*Vierosylva calidris*) to pursue a species of *Terias* in Jamaica ("Birds of Jamaica," p. 194). In Southern India, Mr. E. L. Arnold found the principal victims of the green bee-eaters to be specimens of *Terias hecate* ("On the Indian Hills," vol. i. p. 247-48). Quite recently in the Transvaal I have observed the wagtail, *Motacilla capensis*, to pursue and devour the yellow Lithosiid moth, *Binna madagascariensis*.

But the facts of "mimicry" seem to effectually dispose of the supposition. In South Africa, the yellow black-margined *Papilio cenea* affords by its females the most striking examples to prove the non-protective value of this coloration; for the females respectively mimic those two well-known "protected butterflies," the blackish *Amauris echeria* and the reddish *Danais chrysippus*, whilst, to add to the negative evidence, the yellow male has been seen by Mr. Weale to become the prey of the flycatcher, *Tchitrea cristata*.

On the Amazons, Mr. Bates has long since shown that the yellow and black *Leptalis orise* mimics the markings—even to the colour of the antennæ and the spotting of the abdomen—of the protected or unpalatable *Methona psidii*.

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A Difficulty in Weismannism.

IN his communication of November 28 (NATURE, December 3, p. 102), Prof. Hartog asks us to believe that Weismann, in a letter from which he quotes, insists (1) that the Ahnenplasmas are "not completely unchangeable," and (2) that "each Ahnenplasma unit corresponds to an individual of the species itself; and if put under suitable trophic conditions would, singly, reproduce such an individual."

Assuming that thesis II. adequately represents the Freiburg Professor's latest views, and that a few sentences detached from their context are to be depended upon, we must, it seems to me, conclude, with Prof. Hartog, that he has unearthed an inconsistency, and, what is of more importance, shown that the shuffling process is not only unnecessary, but that a new significance must be found for it.

I am, however, still inclined to believe that hypothesis B is the one upon which Weismann has founded his theories of heredity and sexual reproduction. The hypothesis, however, should take account of the variability, slight though it may be, of the Ahnenplasmas. We agree to call the Ahnenplasmas Protozoan, simply because we have no conception of the *kind* and *amount* of the variation they have undergone since they parted company with the unicellular organisms in which they originated. We have no reason, however, to believe that the external causes which led to their variation in unicellular organisms are powerless to affect them now that they are localized in the reproductive cells of multicellular ones.

Prof. Hartog, moreover, while relinquishing the idea of the variability of the offspring of the lioness, endeavours from another point of view to attack Weismannism on the plane of hypothesis B. Is he, too, sceptical as to Weismann's adherence to hypothesis A, or does he simply wish to overwhelm the so-called disciples?

In either case, several objections may be made to his argument. In the *first place*, we object most emphatically to any