

truth, that, unless such objects are properly mounted, it is worse than useless to exhibit them to the public at all. They should be taken down and stowed away in drawers, or preserved in any other way that may be convenient for scientific study. Left in their glass cases, they are much more likely to repel than to attract the ordinary observer, for whose benefit the exhibition is intended.

Under such circumstances we cannot receive otherwise than with pleasure a treatise prepared with the view of teaching the true principles of the art of taxidermy and their proper application. The Royal Cabinet of Natural History at Stuttgart is well known to those who have visited it as one of the few institutions of this kind where real care and skill are exhibited in the mounting of the specimens, and no one can be more fitted than its energetic "preparateur" to give instructions upon a subject of which he has shown such perfect knowledge. Herr Martin has, moreover, obtained the assistance of several individuals who are fully qualified to assist him in his task, which appears to have somewhat of a comprehensive scope. In a former part of the present work, Herr Martin has treated of the various methods of collecting animals of all sorts in the field, and of preserving them for scientific purposes. The fact of a third edition of this former part having been already called for shows that the work has been appreciated by those or whose instruction it is designed. In the present section of his volume, Mr. Martin and his fellow-workers treat more especially of the processes to be performed in the museum itself, such as the modelling in plaster of beasts large and small, the formation of preparations of the internal organs, the making of skeletons, and the mounting of microscopical objects. Full instructions are likewise given upon every point connected with the practical working of a public museum, not only as regard the objects themselves, and the best mode of exhibiting them, but also in relation to the wants and requirements of the visitors that resort to such institutions.

P. L. S.

LETTERS TO THE EDITOR

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

Mimicry versus Hybridity

BEFORE attempting to combat the old theory under which Mr. Murray has taken refuge, in opposition to the theory propounded by Mr. Bates, I must first make a few remarks upon the different forms of mimicry to which the Lepidoptera are subject.

Mimicry may be divided into two heads, viz. :—the mimicry of one lepidopterous insect by another, and the mimicry of the vegetable kingdom, and of backgrounds generally, by Lepidoptera. As Mr. Murray doubtless refers to only the first of these heads when he speaks of hybridisation, I need not trouble the reader with any remarks respecting the second. Mimicry, then, between butterflies and moths, may again be divided into three sections : that which modifies both sexes, that which chiefly modifies the females, and, lastly, that which chiefly modifies the males ;* these variations of modification are all easily explained by the theory of protective assimilation variously adapted to the economy of the different modified species ; but it can in no way be explained by the theory of modification by hybridity. Mr. Murray speaks of hybridisation as if it were a thing recognised by lepidopterists, and of no uncommon occurrence, whereas it has, so far as I know, only occurred in the Heterocerous Lepidoptera, and only between species of the same genus ; there is, indeed, a case on record of a skipper butterfly and a burnet moth being taken *in coitu*, but no reasonable being could expect that any issue would result from such an union ; again, I maintain that if it were even possible for hybridity to occur between different sub-

orders, families, or even genera of Lepidoptera (which, by the way, is as likely as hybridity between a vulture and a dove or a horse and a rabbit), the offspring would inevitably be modified in structure just as much as hybrids between distinct races or species of vertebrates are ; they would moreover, if fertile, certainly revert to one or other of the parent stocks, which, however we do not find to be the case ; if the fertilisation of flowers and butterflies were the same, hybrids might be as common in the one case as the other, and the results attained might be more nearly alike ; but as butterflies are not fertilised through the transmission of pollen by external agencies, and as they seem to have a decided preference for mates belonging to their own peculiar species, hybridisation must needs be a thing almost unknown amongst them. Lastly, I need scarcely say that the fact of birds hunting by sight and not by smell only does not in any way destroy the argument respecting the favoured and non-favoured species of Lepidoptera ; the same thing may be said of lizards, frogs, dragon-flies, and spiders, which all of them persecute the order, and which all avoid not those insects only which have a peculiar odour, but those which, like the *Danaïnae*, *Heliconinae*, *Acarinae*, and others, have an acrid taste resulting from an offensive liquid which they exude from the body. I have been more fortunate than Mr. Scudder, inasmuch as I have frequently seen birds catch and devour the unprotected species upon the wing, whilst I have received abundant evidence both from scientific and non-scientific collectors respecting the perfect immunity which the *Danaïnae*, &c., enjoy from all kinds of persecution, whilst their less fortunate brethren come to an untimely end.*

British Museum

ARTHUR G. BUTLER

I AM rather surprised that Mr. Andrew Murray should have advanced his theory of mimicry being due to hybridisation, without adducing one solitary fact to prove that hybridisation between distinct families of insects ever occurs, or that, if it do occur, the offspring are fertile *inter se*. Mimicry is most frequent between very distinct families or higher groups, and often between different orders of insects. We may fairly consider that the "natural orders" of plants, as being the next well-marked groups above genera, are about equivalent to the families of insects, so that the analogy furnished by hybridisation among plants, on which alone Mr. Murray's theory is founded, wholly breaks down, unless he can show (which he has not done) that such hybridisation occurs between species of different "natural orders," or of well-marked groups higher than genera. It would be mere waste of time to discuss the details of a theory whose fundamental assumption is not only quite unsupported by fact, but is diametrically opposed to the almost, if not quite, universal fact that hybrids do not occur between species of different families or higher groups.

Mr. Scudder's letter contains some interesting and suggestive facts, and opens up a new field of investigation as to the immunity of certain species, in their egg or larva state, from the attacks of hymenopterous and dipterous parasites. It is, I believe, now stated for the first time, that the peculiar secretions which render the Danaidæ distasteful to birds not only extend to their larva and egg state, but act as a safeguard from the attacks of parasites. The objection that it would have been more advantageous for the larva than for the imago of the *Limenitis misippus* to mimic the *Danaï archippus*, appears to me to have no weight. We do not know, for instance, if such mimicry would be any defence against parasites who may be guided by smell rather than sight ; and from the frequent limitation of certain odours and secretions to whole genera or families, the variations necessary to produce them may be of rare occurrence.

The fact that *Limenitis misippus* and *L. ursula* are about equally plentiful is not at all remarkable, since there are species of all degrees of rarity in every extensive group ; but in this case it happens that both insects are mimickers, *Limenitis ursula* resembling the common N. American *Papilio philenor*, especially on the under side, which is exposed when the insects are at rest. This case of mimicry is not so perfect or so striking as the other, but that it is one is pretty certain, and there are several other

* The Hon. Mr. Justice Newton, who assiduously collected and took notes upon the Lepidoptera of Bombay, informed me that the *Charaxes Pasipho* of Westwood was continually persecuted by the Bulbul, so that he rarely captured a specimen of this species which had not a piece snipped out of the hind wings ; he offered one to a Bulbul which he had in a cage, and it was greedily devoured, whilst it was only by repeated persecution that he succeeded in inducing the bird to touch a *Danaïnae*, which he offered to it.

* An interesting illustration of this type of mimicry exists between the genera *Belenois* and *Mylothris*, the males of the African group of *Mylothris* being identical in colour with males of the genus *Belenois* (sub-family *Peirinae*).

instances in various parts of the world in which *Papilio* of certain groups are the objects of mimicry. Although Mr. Scudder has never seen a bird capture a butterfly, others have been more fortunate, and that they are thus captured very largely in the tropics is certain. It is not improbable, from the rarity of mimicry in the temperate zone, that the few cases which exist may have been produced under the more favourable climatal and organic conditions of the semi-tropical epochs anterior to the glacial period.

ALFRED R. WALLACE

The Difficulties of Natural Selection

The papers read by me before the Entomological Society "On the Relation between the Colour and the Edibility of Lepidoptera and their Larvæ" having been noticed and commented upon by Mr. A. W. Bennett and others in NATURE, I have deemed it desirable to offer a few remarks on the subject.

The object I had in making the experiments was to ascertain whether there could be proved to exist any relation between the colours of larvæ and their edibility.

The disciples of Mr. Darwin argued that the brilliant colours of so many male birds arose from sexual selection, and that the equally striking colours of flowers were but guides to insects, to enable them to distinguish, at some distance, the flowers from the leaves, and thus insure fertilisation by the interchange of pollen. Such reasons, however, were quite valueless to account for the bright colours of the asexual larvæ of many Lepidoptera, several species of which are banded and striped with blue, yellow, and red; colours which instead of concealing them by harmonising with the leaves on which they feed, are often in complete contrast with the u.

Now Mr. Wallace had a theory that these gaily coloured larvæ were uneatable by birds, and that their gay colours were protective, because if they were indistinguishable from eatable species, they would be seized by birds, and though rejected afterwards, would be so much injured that the probability of their becoming imagines would be very remote, even if they were not at once killed.

This I found to be the case; in my experiments extending over many years, and most carefully made with several species of birds, I have not met with one instance in which a strikingly-coloured larva was eaten. In most cases they were not even regarded when thrown into the aviary, although I had several birds always on the watch for the eatable species, with which I constantly fed them; while these latter were seized immediately they were seen.

The larva of the *Cucullia verbasci* is conspicuously coloured blue and yellow, and feeds without any attempt at concealment on several species of *Verbasum*. I placed the plants in the aviary, and fed the *Cucullie* upon them until every leaf was devoured, and the caterpillars gnawed holes in the stem; but not one was in the slightest degree injured, yet at the same time other larvæ were greedily eaten.

On the other hand, I found that all larvæ were eagerly eaten which have soft smooth bodies and dull colours, while the hairy larvæ are rejected entirely.

These eatable species are protected in various ways; some are nocturnal in their habits, descending to the ground during the day; some feeding on the under sides of the leaves; others arrange their bodies in a line with the shoots of the plants and look like a streak of the bark; some are of precisely the colour of the leaves, or even of the corolla of the plant on which they feed; others roll themselves up in leaves, the larvæ of the *Geometridæ* are often exactly like twigs, with the terminal and side buds imitated.

This latter resemblance is so complete that, after being thirty years an entomologist, I was deceived myself, and took out my pruning scissors to cut from a plum-tree a spur which I thought I had overlooked. This turned out to be a larva of a *Geometer* two inches long. I showed it to several members of my family, and defined a space of four inches in which it was to be seen; but none of them could see that it was a caterpillar. Surely this was a case of protective mimicry.

All the eatable larvæ agree in not moving when there appears the least danger, and very rarely moving at all during the day.

Even if there were no cases of protective mimicry in the larval states of Lepidoptera, I do not think that would be any argument against the existence of such in the perfect state. It appears to me rather that as so few specimens become imagines in proportion to the eggs produced, the more need is there that these few should survive.

I cannot, therefore, agree with Dr. Scudder in thinking that mimicry has been supposed to exist where it is least wanted, viz., in the perfect state of Lepidoptera. Nor can I coincide with Mr. Bennett that it is a matter of indifference to the supporters of the theory of Natural Selection whether twig-like caterpillars are eaten by birds or not. My point is that they are often so like twigs that they are passed over as such by insectivorous birds, and that the closer the resemblance the better their chance of escape.

I believe myself that Mr. Darwin's theory will survive, and even be benefited by, the criticisms of its opponents; but what I do dread is the injury it may receive from the false arguments of some of its illogical supporters.

Lest I may unwittingly place myself in the latter category, I will bring my remarks to a close.

J. JENNER WEIR

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Butterflies and Birds

A CORRESPONDENT in NATURE, Dec. 22, states that after fifteen years' experience in butterfly hunting, he has never seen one in a bird's bill. I was not aware the circumstance was unusual, for I have frequently seen the common sparrow chase and capture such butterflies as *V. urticae* and *P. rapæ*. It is quite a hare and greyhound affair, the butterfly often eluding for some time the swift pounces of its pursuer, so that the hunt is a long one.

T. G. B.

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Ceratodus Forsteri

SIR PHILIP GREY EGERTON presents his compliments to the Editor, and would esteem it a favour if he would insert the following paragraphs, from two letters recently received from Professor Agassiz, in an early number of NATURE. It will be gratifying to all men of science to know that the distinguished Professor has so far recovered from his late severe illness as to be able again to interest himself in scientific pursuits.

Oulton Park, Tarporley

"Cambridge, November 9

"I am slowly recovering, and find myself gradually returning to the ways of active life. As I wake anew to feel an interest in scientific pursuits, there is nothing for which I have a greater longing than the fossil fishes. If I could leave my house I would fly to you to resume the examination of your and Lord Enniskillen's collections. The recent discovery of Krefft has added fuel to the fire, and I feel the most intense desire to revise the facts bearing upon the relations of the Ganoids and Selachians in general, and more particularly those of the *Cœlocanthi*, to which, from the examination of the skeleton sent me by Krefft, I find his *Ceratodus Forsteri* belongs. It will no doubt turn out that the *Dipterini* are close relations. In this connection I am reminded of what you once wrote to me of the teeth of *Ctenodus*. Will you now have the kindness to give me all the particulars? I am having sections of the teeth of *Ceratodus Forsteri* and some of the fossil species made for comparison. I have little doubt already that this genus will turn out to be one of the most curious *synthetic* types (I call them) in the animal kingdom, exhibiting characters of Placoids (Selachians) in the teeth, Ganoids in the scales, their embryonic characters in the preservation of a dorsal chord, instead of distinct bony vertebræ, and finally hollow bones as in birds."

"Cambridge, Dec. 8

"I take it some of your naturalists will crow over what they will be pleased to call my stupendous mistake in referring the teeth of *Ceratodus* to the Selachians, when the fish proves to have large imbricated scales; and yet I never was more pleased than when I learned the fact, for it settles beyond dispute the existence in nature of types, to which I have long ago called attention, under the name of *synthetic types* (see my *Essay on Classification*), but of which naturalists have thus far taken little or no notice. When I described the teeth of *Ceratodus* as those of a distinct genus among the *Cestracionts*, I was led to do so by appearances which secured for this association the assent of all naturalists. As long as the fossil teeth only were known, nobody questioned the relationship. Owen himself, in his 'Odontography,' mentions the teeth of *Ceratodus* and their structure, and has not a shadow of a doubt that I am right in placing that genus near *Cestracion*; and now comes the discovery that *Ctenodus*, a genus also referred to the *Cestracionts*, is based upon the dental plate of a bony fish, closely allied to the one recently discovered by Krefft, and referred by him to