

discussion in the articles is based upon results of tests carried out by the author, and a valuable feature is a large table giving details of these results for 100 boiler plants, chiefly of the Lancashire type. Mr. Brownlie's figures indicate that the average net working efficiency of colliery steam-boiler plants is only about 55.5 per cent. By carrying out a re-organisation of such plants on modern scientific lines it is possible to obtain 70 per cent. efficiency, and Mr. Brownlie estimates that about 6,600,000 tons of coal per annum could be saved by the adoption of scientific methods and by more extensive use of refuse coal.

The 100 boiler plants tested have a total of 570 boilers, 500 of these being Lancashire, 2 Cornish, 37 egg-ended, and 31 modern tubular boilers. The average efficiency of the egg-ended boilers is less than 35 per cent., and there appears to be still a fair number of this type at work, in spite of it being hopelessly out of date. It is also of interest to note that the few modern tubular boilers installed are, on the average, giving no better results than the Lancashire boilers, which average 55 per cent. efficiency. This fact obviously indicates improper arrangements in the installation or bad methods of working, or both.

Another point of importance to which Mr. Brownlie directs attention arises from the Final Report of the Coal Conservation Committee, which states that "the policy of collieries has been to set free the best qualities of coal for the market, and to retain for colliery consumption the poorest quality. The returns show that the quantity of ash in some of the fuels used ranges from 50 per cent. to 80 per cent." Mr. Brownlie actually finds an average of 15.5 per cent. ash and coal of 10,500 B.Th.U. used at colliery boiler plants, and most people will support him in his statements that he has never heard of a case of 50-80 per cent. ash; that such instances must be rare; and that the statement in the report is most misleading. In actual fact, 52 per cent. of the coal employed at collieries is high-grade coal; of the remainder, 32 per cent. could be used economically in industry for steam generation, and only 16 per cent. is definitely unsaleable. The highest ash-content of this refuse coal was 35 per cent. Mr. Brownlie maintains that these results are typical of the colliery industry, and the idea that collieries burn chiefly refuse and unsaleable coal is a complete fallacy.

As a matter of fact, there are millions of tons of refuse coal lying unburnt at collieries, and a very large proportion of this refuse could be utilised for steam generation, as has been proved by Mr. Brownlie's firm on a number of colliery plants. The carrying out of this proposition would result in a very large saving in the coal consumption, even after ample deduction for the cost of extra boilers and plant necessary because of the low calorific value. A fair average price for the whole of the coal burnt on colliery boiler plants is to-day about 20s. per ton; making allowance for extra labour, plant, and depreciation, and taking 3 tons of refuse coal as equal in practice to 1 ton of saleable coal, the value of refuse coal to-day would be about 8s. per ton.

Mr. Brownlie's pamphlet is to be welcomed, partly on account of the strong case for reform presented in view of the need for national economy, and partly on account of the large number of test results which he gives in a form suitable for easy comparison. The pamphlet may be obtained from Messrs. Brownlie and Green, Ltd., 2 Austin Friars, London, E.C.2.

NO. 2606, VOL. 104]

THE BRITISH ASSOCIATION AT BOURNEMOUTH.

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY DR. F. A. DIXEY, M.A., F.R.S.,
PRESIDENT OF THE SECTION.

ONE of the results of the great war now happily at an end has been its effect upon science. On the one hand it has checked the progress of scientific investigation; it has done much to destroy international co-operation and sympathy; it has removed from our ranks, temporarily or permanently, many admirable workers. On the other hand it has acted as a great stimulus in many departments of scientific inquiry, and it has given the general public an interest in many scientific questions which have hitherto met with little recognition or encouragement from the people at large. It was perhaps inevitable, but at the same time, as I venture to think, rather to be deplored, that that interest has tended to concentrate itself upon applied more than upon abstract science; that it has been concerned chiefly with the employment of natural knowledge in devising and perfecting new methods of destruction. Terrible as is the power which the present-day engines of warfare have attained, it may be reasonable to hope that some compensation for the mischief and suffering which they have caused may eventually be found in peaceful directions; that the submarine, the aircraft, and even the high explosive may cease to be a terror to civilisation, and in spite of their past history may after all become agents in the advancement of the general welfare:

Hoc paces habuere bone, ventique secundi,

will, let us hope, be a legitimate reflection in later times. But for the true scientific worker, I think I may safely assert, the primary object of his studies is the attainment of knowledge for its own sake: applications of such knowledge may be trusted to follow; some beneficial, some perhaps the reverse. Still, whether they do or do not so follow is less a concern of the scientific man than whether his labours have resulted in a fresh advance into the realms of the unknown. I confess to some sympathy with the feeling which is said to be expressed in the regular toast of a certain scientific gathering:—"Pure mathematics, and may they never be of any use to anybody."

For genuine enthusiasm in the cause of science for its own sake, I think that we zoologists may claim a good record. We are by no means unmindful of the great benefits to humanity which have taken their rise more or less directly from zoological science. I need do no more than mention the services to medicine, great at the present and destined to be greater still in the future, that are being rendered by the protozoologist and the entomologist. We may look forward also to results of the highest practical importance from the investigations into the laws of heredity in which we are engaged with the co-operation of our allies the botanists. But what we are entitled to protest against is the temper of mind which values science only for the material benefits that may be got from it; and what above all we should like to see is a greater respect on the part of the public for science purely as science, a higher appreciation of the labours of scientific men, and a greater readiness, in matters where science touches on the common affairs of life, to be guided by the accumulated knowledge and experience of those who have made such matters the subject of constant and devoted study. If the war leads to any repair of the general deficiency in these respects, it

will to that extent have conferred a benefit on the community.

Regarding, as I do, my present position in this Section as a great honour and privilege, especially in view of this being the first meeting of the British Association to be held after the war, I hope I may be allowed a few preliminary remarks of a somewhat autobiographical character. As far back as I can remember, zoology has been a passion with me. I was brought up in a non-zoological environment, and for the first few years of my life my only knowledge of the subject was gained from an odd volume of Chambers's "Information for the People." But on being asked by a visitor what I intended to do with myself when I grew up, I can distinctly remember answering, with the confident assurance of seven or eight, "Zoology suits me best"—pronouncing the word, which I had only seen and never heard, as zoology. By the time I went to school, my opportunities had increased; but I soon found myself engaged in the classical and mathematical routine from which in those days there was little chance of escape. In due course I went to the University with a classical scholarship, which necessitated for the time an even more rigid exclusion of scientific aspirations than before. I mention this because I wish to pay a tribute of gratitude to the College authorities of that day, to whose wise policy I owe it that I was eventually able to fulfil in some measure my desire for natural, and especially biological, knowledge. After two years of more or less successful application to the literary studies of the University, I petitioned to be allowed to read for the final school in natural science. The petition was granted; my scholarship was not taken away, and was even prolonged to the end of my fifth year. This I think was an enlightened measure, remarkable for the time, more than forty years ago, when it was adopted. I only hope that we have not in this respect fallen back from the standard of our predecessors. The avidity with which I took up the study of elementary chemistry and physics, and the enthusiasm with which I started on comparative anatomy under the auspices of George Rolleston are among the most pleasant recollections of my youth. But from the force of circumstances, though always at heart a zoologist, I have never been in a position to give myself unreservedly to that department of biology; and even now, in what I must call my old age, I fear I cannot regard myself as much more than a zoological amateur. My working hours are largely taken up with serving tables.

What moral do I draw from this brief recital? Not by any means that I should have been allowed to escape a grounding in the elements of a literary education, though I think it quite possible that the past, and even the present, methods of school instruction are not ideally the best. My experience has led me to conclude that much of the time spent over the minutiae of Greek and Latin grammar might, in the case of the average boy, be better employed. But I do not agree that a moderate knowledge of the classics, well taught by a sensible master, is useless from any reasonable point of view. To those of my hearers who appreciate Kipling, I would call to mind the vividly truthful sketch of school life called "Regulus." Let them reflect how the wonderful workmanship of the inspired and inspiring Ode of Horace, round which the sketch is written, must have sunk into the mind of the apparently careless and exasperating "Beetle," the "egregious Beetle" as King calls him, to bear such marvellous fruit in after years. Beetle, as we all know, is no professional scholar, no classical pedant, but a man of the world who has not forgotten his Horace, and upon whose extraordinary literary skill those early school-tasks must have had, whether consciously or

not, a dominating influence. How else could he have written "Regulus"? "You see," says King, "that some of it sticks." So it does, if it is only given a fair chance; and in the skirmish between King the classical and Hartopp the science master, both right up to a point and both wrong beyond it, I give on the whole the palm to King. To revert to my own case. I do not regret a word of either the Latin or the Greek that I was obliged to read, nor even the inkling of the niceties of scholarship to which I got, I hope, a fair introduction. But I do think that I might have been allowed to start on scientific work at an earlier period, and that a good deal of the time spent, say, on Greek and Latin prose and verse writing, might in my case have been well spared for other objects.

To generalise what I have been saying. Start teaching your boy or girl on a good wide basis. Nothing is better for this than the old school subjects of classics, history, and mathematics, with the addition of natural science. In course of time a bent will declare itself. Encourage this, even at the expense of other studies desirable in themselves. But do not allow any one subject, however congenial, to usurp the place of a grounding in those matters which are proper to a general education. The time for specialising will come; and when it has arrived do all you can to remove obstacles, pecuniary and other. Do not hamper your historian with chemistry or your zoologist with the differential calculus. If they have a taste for these things by way of diversion or recreation, well and good. But let their action be voluntary.

This, however, is not a fitting occasion for propounding my views on the question of education, and it is time to turn to the immediate object of my address. And here I think I cannot do better than bring before your notice certain facts which have a bearing on the subject of insect mimicry; a subject which for many years past has engaged much of my attention. The facts on all hands are allowed to be remarkable. As to their interpretation there is much diversity of opinion; and indeed, until complete data are forthcoming, this could scarcely be otherwise.

In the first place let us glance at a certain assemblage of butterflies that inhabits New Guinea with some of the adjacent islands. These butterflies, though belonging to different subfamilies, present a resemblance to each other which is too strong to be accidental. Three of them belong to the Pierines, the group which includes the common white butterflies of this country; the fourth is a Nymphaline, not widely removed from our well-known tortoiseshells, red admiral and peacock. The resemblance on the upper surface between two of the three Pierines is not especially noteworthy, inasmuch as they present in common the ordinary Pierine appearance of a white or nearly white ground colour with a dark border somewhat broadened at the apex. But this, an everyday feature in the Pierines, is almost unknown in the very large subfamily to which our present Nymphaline belongs. Still, though sufficiently remarkable to arrest the attention of anyone familiar with these groups, the Pierine-like aspect of the upper surface of this Nymphaline, which is known as *Mynes doryca*, would not by itself have seemed to call for any special explanation. The resemblance would pass as merely an interesting coincidence. But the under surface of the three Pierines, known respectively as *Huphina abnormis*, *Delias ornytion*, and *Delias irma*, presents a striking combination of colour very unusual in their own group; and this peculiar character of the under surface is shared by the Nymphaline *Mynes doryca*. The "long arm of coincidence" could scarcely reach so far as this. Whatever might be said about the likeness seen from above, that the wings beneath should show

virtually the same unusual pattern in the Mynes as in the Pierines seems to call for some explanation other than an appeal to chance or accident. Moreover, with regard to the Pierines themselves, the two members of the genus *Delias* are, of course, fairly closely related; but the *Huphina* belongs to an entirely distinct genus, separated from *Delias* by many important structural differences. The two species of *Delias* perhaps depart less widely in aspect from their nearest congeners than does either the *Huphina* or the Mynes. The under surface of the *Huphina* is unexampled in its genus, but the upper surface is quite ordinary. The Mynes, as we have seen, stands alone among its nearest relatives not only in the character of its under surface, but also in the Pierine-like character of its wings above.

We will now turn to another assemblage, which presents us with the same problem from a somewhat different point of view. In south-eastern Asia, with certain of the adjacent islands, is found a genus of large butterflies, called by Wallace *Prioneris* from the saw-like front margin of the forewing in the male. More than fifty years ago it was remarked by Wallace that the species of *Prioneris* in several cases seem to mimic those of the genus *Delias*, and that "in all cases the pairs which resemble each other inhabit the same district, and very often are known to come from the same locality." The parallelism is even stronger than was stated by Wallace, for there is not a single known member of the genus *Prioneris* which does not resemble a species of *Delias*, so that *Prioneris* cannot really be said to have an aspect of its own. *Prioneris clemanthe* and *Delias agostina* form a pair inhabiting the Himalayas, Burma, and Further India. In the same region occur *Prioneris thestylis* and *Delias belladonna*, the striking similarity of which species, especially on the underside and in the female, drew the special attention of Mr. Wallace. A still more remarkable instance is that of *Prioneris sita* of southern India and Ceylon, the likeness of which to the common Indian *Delias eucharis* is spoken of by Wallace as "perfect"; while Fruhstorfer, a hostile witness, testifies to the fact that the *Prioneris* always flies in company with the *Delias*, and rests just like the latter with closed wings on the red flowers of the Lantana. *Prioneris hypsipyle* of Sumatra and *P. autothisbe* of Java are like *Delias egialea* and *D. criihoe* of the same two islands. Here again Fruhstorfer says of *Prioneris autothisbe* that it visits the flowers of the Cinchona, "always in company with the similarly coloured *Delias criihoe*." Wallace remarked on the close similarity between *Prioneris cornelia* of Borneo and *Delias singhapura* of the Malay Peninsula; in this case, it will be noted, the localities, though not far distant from each other, are not identical. But a *Delias* form which was unknown at the date of Wallace's paper has since been found in Borneo, and this latter butterfly, known as *D. indistincta*, is even more exactly copied by *P. cornelia* than is the *Delias* which first drew Wallace's attention. *Prioneris vollenhovii* of Borneo is a kind of compromise between *Delias indistincta* and, on the underside, *D. pandemia* of the same island, and it may be added that another Bornean Pierine, *Huphina pactolica*, is a good copy of *Delias indistincta*, therefore resembling also the Bornean *Prioneris cornelia* and *P. vollenhovii*.

The memoir, published in 1867, in which Wallace remarked on the parallelism between *Prioneris* and *Delias*, contains a noteworthy prediction by the same author. Speaking of *Pieris* (now called *Huphina*) *laeta* of Timor, he says that it "departs so much from the style of colouring of its allies and approaches so nearly to that of *Thyca* (*Delias*) *belisama* of Java, that I should almost look for an ally of the last species to be discovered in Timor to serve as its pattern." Thirty-

four years after the expression of this anticipation, Mr. Doherty discovered in Timor an ally of *Delias belisama* which at once suggests itself as the model from which the peculiar and brilliant colouring of *Huphina laeta* has been derived. Fruhstorfer, who is by no means friendly to the theory of mimicry, says of this *Delias*, which was named *splendida* by Lord Rothschild, that beneath it is "deceptively like *Huphina laeta*." But here comes in a curious point. The black forewing with its yellow apex and the orange-yellow hindwing with its scarlet black-bordered costal streak are present on the underside of both the *Delias* and the *Huphina*; but the latter butterfly possesses, in addition to these features, a row of scarlet marginal spots on the hindwing which are not to be found on the *Delias*. In spite of this discrepancy, the likeness is sufficiently striking. But from the same island of Timor, Doherty sent home another *Delias* which, besides resembling *D. splendida*, possesses a row of scarlet patches in the corresponding situation to those of *H. laeta*. In this latter *Delias*, however, named *dohertyi* by Lord Rothschild after its discoverer, the brilliant scarlet costal streak is completely absent. The *Huphina*, therefore, is more like either species of *Delias* than they are like each other, forming, as it were, a link between them. So that, adopting Professor Poulton's terminology, we may say that, if this is a case of mimicry, one form may possess at the same time the aposemes belonging to two distinct models. I will not now stop to discuss the bearing of this case on current theories, but will only remark that, granting mimicry, the whole assemblage, *D. splendida*, *H. laeta*, *D. dohertyi*, may be expected to gain advantage from the blending action of the intermediate *H. laeta*. This I think would happen whether *laeta* is a "Batesian" or "Müllerian" mimic, but the gain to the association in the latter case is certainly the more obvious.

This state of things would be sufficiently curious if it stood by itself. But it does not stand by itself. In Lombok, Sumbawa, and Flores there occurs another member of the peculiar group of *Huphina* to which *H. laeta* belongs. This butterfly, known as *H. temena*, resembles *H. laeta* in many respects; possessing on the underside of the hindwing a scarlet costal streak and a row of scarlet marginal spots like those of that insect. The forewing, however, differs from that of *H. laeta* in having its ground-colour not uniformly black, but divided between a dark shading to the veins, a dark submarginal band, and series of pale streaks and patches in the interspaces between the veins. The question at once suggests itself: Is there a relation between *H. temena* and one or more species of *Delias* corresponding to that between *H. laeta* and *D. splendida* and *dohertyi*? The answer to this question is in the affirmative. *Delias oraia*, together with *Delias sumbawana*, both species inhabiting the same three islands as *H. temena*, form with it an assemblage quite comparable with the former triad from Timor. Further, the points in which *H. temena* differs from *H. laeta* have their counterpart in the distinctions between *D. oraia* and *D. splendida* on the one hand, and *D. sumbawana* and *D. dohertyi* on the other. These points are chiefly, in the *temena* assemblage, the less definitely black-bordered costal streak, the more strongly-marked black bordering to the submarginal scarlet spots, and the diversely-coloured as compared with the uniformly black forewing of the Timor insects.

Again, in the island of Bali, *Huphina tamar* would seem to combine certain features of two species of *Delias* in a similar manner to the cases of *laeta* and *temena* just considered. The underside as a whole is reminiscent of *D. periboea*, a member, like *D. dohertyi* and *D. sumbawana*, of the *eucharis* or hypa-

rete group of the genus; while the red costal streak suggests the influence of a representative in Bali of the *belisama* group, like *D. splendida* and *D. oraia* in the other islands.

Finally, in the island of Sumba we have another member of this remarkable group of Huphinas. *Huphina julia*, the butterfly referred to, so closely resembles *Delias fasciata* of the same island, that even the sceptical Fruhstorfer is constrained to speak of it as a "faithful copy" of that insect. But here once more it is noticeable that one of the most conspicuous features of the Huphina is absent from the *Delias*. This time it is not, as in the case of *D. splendida*, the submarginal row of scarlet spots on the underside of the hindwing, but it is the scarlet costal streak that is wanting. *Huphina julia* was discovered by Mr. Doherty in the year 1887, and described in 1891. It is interesting, in the light of what is now known of the butterfly fauna of the Lesser Sunda islands, to read what Doherty has to say about the mimicry question in relation to the *Delias* and Huphina forms that have just been mentioned. Speaking of *H. julia*, he says, "If it stood alone, I should certainly suppose it to be a mimic of some form of *Delias hyparete* yet undiscovered in the island. But both *H. laeta* and *H. temena* require to be accounted for in the same way, and while it is possible that some Timorese *Delias* may resemble *H. laeta*, I feel sure that *H. temena* can have no such original. It must then be assumed that this group is less pressed by its enemies in the Timorian Islands, and has therefore been able to acquire more brilliant colours than its allies." So far Doherty.

Whatever may be the value of this last hypothesis, we have just seen that the supposed facts on which it rests are non-existent, for (1) the "form of *Delias hyparete* as yet undiscovered" has actually turned up in the person of *D. fasciata*; (2) it is not only possible, but actually the case, that "some Timorese *Delias* may resemble *H. laeta*"; (3) Mr. Doherty "feels sure that *H. temena* can have no such original," but *Delias oraia* and *Delias sumbawana* have just the same relation to *Huphina temena* as *D. splendida* and *D. dohertyi* to *H. laeta*. In view of these facts it may be not rash to suppose that the apparent absence of a model for the red costal streak of *H. julia* may hereafter be accounted for.

Of the three instances of possible mimetic association which have now been mentioned, I think that only one, viz. the first, has previously been treated in detail. The numbers of cases more or less similar to these three might be very largely extended, but for our present purpose it will be sufficient to confine our attention to those already given. It is probable that to some minds the facts adduced are simply curious coincidences, needing no explanation; but it can scarcely be wrong to suppose that to most students of nature the observed phenomena do call for some attempt at interpretation; and on a review of the evidence it seems clear that the geographical element must enter largely into any explanation that may be offered. On the whole, it is certainly the case that the forms which are supposed to be related by mimicry do inhabit the same localities; the continental *Prioneris*, for example, is like the continental *Delias*, and the island *Prioneris* recalls the island, not the continental, *Delias*. Moreover, we find the differences between the *Delias* of Timor, of Sumbawa and Sumba reflected in the associated Huphinas of the same islands. If it be granted that the geographical element is a factor, it is natural to inquire how it works.

It is no doubt true that external geographical conditions are occasionally capable of producing, whether directly or indirectly, a community of aspect in the animals or plants exposed to their influence. The pre-

valence of a sandy coloration in the mammals and birds of a desert, and of whiteness in the inhabitants of the arctic snow-fields, the spiny character so often assumed by the plants of arid regions, and the general dwarfing of the vegetation that grows close to the sea, may be given in illustration. At first sight these phenomena may seem to be of the nature of direct effects of the environment; quite possibly some of them are so, but I think few observers would deny that they are at least largely adaptive, being used for purposes of aggression or defence. Still, even if we allow the direct effect of the environment, as perhaps we may do especially in the case of the plants, can we frame any hypothesis of the action of geographical conditions which shall lead directly to the assumption of a common pattern in the case of the three or four butterflies from New Guinea? I confess that I am quite unable to do so. If the climate, or the soil, or any other geographical condition in New Guinea is capable of directly inducing so remarkable a combination of colour as we see in these Pierines and Nymphalines, why does it not affect other organisms in a similar way? Why do not other Pierines, for instance, closely related to *ornytion* and *abnormis*, share in the same coloration? And considering the characteristic aspect of the underside, which is supposed to be called into being by some unexplained condition peculiar to New Guinea, we may well ask, Why should its most conspicuous features belong in the one case to the forewing and in the other to the hindwing, and *vice versa*, the general effect being the same?

Fruhstorfer, we may note, does not feel these difficulties. "Many Pierids," he says, "present typical examples of that resemblance to other butterflies which has been named mimicry. The origin of this resemblance, however, is now explained by the supposition that the mimics were modified by the same (as yet unknown) influences under which the colouring of the models, mostly Danaids, developed." I think it will be generally agreed that this reference to "unknown influences" is no explanation at all.

It is necessary to take into account the fact that the resemblances of which we are speaking are independent of structural differences, being, in fact, merely superficial. This is a point which is capable of much wider demonstration than I am giving it to-day. But even from the instances now before us I think there cannot be much difficulty in coming to the conclusion that the resemblances are an appeal to vision. They are meant to be *seen*, though by whom and for what purpose may be open to question. Speculations as to recognition and sexual attraction may, I think, in these cases be put out of court; but there remains the theory of warning colours assumed in reference to the attacks of vertebrate enemies. From the fact that the most striking and most conspicuous of these common aposemes or danger-signals belong to the under surface—that is to say, the part chiefly exposed to view during rest—it may be inferred that the enemies to be guarded against are mainly those that attack butterflies, not on the wing, but when settled in repose. Both birds and monkeys are known to feed on butterflies, and there is a good deal of evidence as to their preference for one kind of food over another. I will not stop to give details, but anyone who wishes to study the evidence may be referred especially to the memoirs of Dr. G. A. K. Marshall, Mr. C. F. M. Swynnerton, and Capt. G. D. H. Carpenter.

If the warning-colour interpretation of these resemblances be the true one, we see at once why they are so largely independent of structure and affinity. Being meant to catch the eye, they ride rough-shod, so to speak, over inconspicuous features, such as venation; nor do they respect more than the nature of things

obliges them to do the ties of blood-relationship. Then, again, it is obvious why they occur in the same and not in widely different localities; in some instances, as we have seen, their bearers actually flying in company and frequenting the same flowers; for the common aspect, supposing it to be in any sense protective, would only take effect when the sharers in it were exposed to the attacks of the same body of enemies; that is to say, when they inhabited the same locality. And this would be equally true, whether the warning colours are shared between distasteful forms, or whether they are deceptively adopted by forms unprotected by inedibility; whether, in Prof. Poulton's terms, they are synposematic or pseudoposematic. I do not enlarge upon this part of the question, or upon the theories which are known under the names of Bates and Müller respectively, because these theories have been fully dealt with elsewhere, and I think I may assume that they are familiar to the greater part of my hearers. But that mistaken ideas as to what is really meant by protection and mimicry still prevail in some quarters, is evident from certain remarks of Fruhstorfer in dealing with the genus *Prioneris* which we have just been discussing. "Wallace," he says, "regards the 'rarer' *Prioneris* as a mimetic form of the 'commoner' *Delias*. But I cannot accept his view, since mimicry among the in all respects harmless Pierids appears no sort of protection, and, properly speaking, the smooth-margined *Delias* should rather copy the armed *Prioneris* if there is assumed to be mimicry at all." If anyone has no better knowledge than this of what is meant by the theory of mimicry, it is not wonderful that he should consider the subject unworthy of serious attention.

The warning-colour theory, then, gives a rational explanation both of the superficial character of the resemblances and of the geographical factor in their occurrence. But it obviously involves the reality of natural selection; and it is here that some are disposed to part company with the upholders of the theory. I have already referred to the fact that much positive evidence now exists both that butterflies are eaten and that preferences on the part of their enemies exist between one kind and another. I will only remark in passing that the objector on this score sometimes adopts an attitude which is scarcely reasonable, and, perhaps, on that very account is somewhat hard to combat. The kind of objector that I mean begins by saying that the destruction of butterflies by birds and other enemies is not sufficient to give play for the operation of selection. You beg his pardon, and produce evidence of considerable butterfly destruction. To which he replies, "Oh, they *are* eaten, are they? I thought you said they were protected." This is a good dilemma, but the dilemma is notoriously an unconvincing form of argument. If a reply be called for, it may be given like this: "Butterflies are either preyed upon or they are not. If they are, an opening is given for selection; if they are not, it shows the existence of some form of protection." The essence of the matter is that both the likes and dislikes of insectivorous animals, and the means of protection enjoyed by their prey, are not absolute, but relative. A bird that will reject an insect in some circumstances will capture it in some others; it will, for instance, avoid insect A if it can't get insect B, but will feed on A if nothing else is to be had; and it is probable that scarcely any insect is entirely proof against the attack of every kind of enemy. The relative nature of protection is readily admitted when the question is not one of mimicry or of warning colours, but of protective resemblance to inanimate objects. All degrees

of disguise, from the rudimentary to the almost perfect, are employed; the lower degrees are allowed to be of some service, and, on the other hand, a disguise that is almost completely deceptive may at times be penetrated. This consideration applies also to the objection that the first beginnings of mimetic assimilation can have no selective value. If the rough resemblance to an inanimate object affords some amount of protection, though that amount may be relatively small, why should not the same apply to the first suggestion on the part of a mimic of an approach to the aposeme or warning colour of its model? The position that neither kind of assimilation is of service is intelligible, though not common; but there is no reason why benefit should be affirmed in the one case and denied in the other. There are further considerations which tend to deprive this latter criticism of force; the fact, for instance, that a resemblance to one form may serve as a stepping-stone for a likeness to another; or, again, the existence of clusters, as they may be called, of forms varying in affinity, but embodying a transition by easy stages from one extreme to another. In a case of this sort the objection that may be felt as to two terms in the series arbitrarily or accidentally picked out is seen to be groundless when the whole assemblage is taken together.

Much attention has lately been given to the fact that of individual variations some are transmissible by heredity and some are not; under the latter heading would generally fall somatic modifications directly induced upon the individual by conditions of environment. Whether any other kind of variation belongs to the same category need not for the present purpose come into discussion. But with regard to the undoubtedly transmissible variations, or mutations if we like to call them so, there is, I think, a fairly general consensus of opinion that they need not necessarily be large in amount. A complete gradation, in fact, appears to exist between a departure from type so slight as to be scarcely noticeable, and one so striking as to rank as a sport or a monstrosity. And we know now that where the Mendelian relation exists between two forms, no amount of interbreeding will abolish either type; intermediates, when formed, are not permanent, and if one type is to prevail over the other, it must be by means of selection, either natural or artificial.

In view of all these considerations, I venture to think that there is no reason to dispute the influence of natural selection in the production of these remarkable resemblances. Other interpretations may no doubt be given, but they involve the ignoring of some one or more of the facts. It may fairly be claimed that the theories of Wallace, Bates, and Müller, depending as they do on a basis of both observation and experiment, come nearer to accounting for the facts than any other explanation yet offered. It will, of course, always be possible to deny that any explanation is attainable, or to assert that we ought to be satisfied with the facts as we find them without attempting to unravel their causes. But such an attitude of mind is not scientific, and if carried into other matters would tend to deprive the study of Nature of what, to most of us, is its principal charm. It is quite true that before the validity of any generalisation is accepted as finally and absolutely established, every opportunity should be taken of deductive verification. This has been fully recognised by the supporters of the theory of mimicry, and much has been done to test in this manner the various conclusions on which the theory rests. The verification is not complete, and perhaps never will

be, but every successive step increases the probability of its truth; and probability, as Bishop Butler taught, is the guide of life. Meantime it is, one may say, the positive duty of everyone who has the opportunity, to fill up, so far as is in his power, the gaps that still exist in the chain of evidence. Here is an especially promising field for naturalists resident in tropical regions.

Before concluding this address there are two points on which I should like to lay some special emphasis. One is the undesirability—I had almost said folly—of undervaluing any source of information or any particular department of study which does not come within the personal purview of the critic or commentator. "I hold," says Quiller-Couch, "there is no surer sign of intellectual ill-breeding than to speak, even to feel, slightly of any knowledge oneself does not happen to possess." This is a temptation to which many of us are liable; and falls, I fear, are frequent. It was a matter of sincere regret to me to find one of my most valued scientific friends speaking publicly of the Odes of Horace as a subject comparatively devoid of interest. I can only confess my utter inability to sympathise with my friend's point of view. If he had merely said, "Excellent as those works may be, I have other things to do than to attend to them," I could approve; but that is a different matter. The failing that I speak of is, unfortunately, by no means unknown among scientific men, and is perhaps rather specially prevalent when such subjects as those of my present address are in question. I can recall a very eminent man of science, no longer living, speaking with scarcely veiled scorn of those who occupied themselves with "butterflies in cases." This was in a presidential address to a section of this association. If so little respect is paid by a leader of science to work done in another part of the field, it is perhaps not to be wondered at that one of his Majesty's judges should speak of the formation of a great collection of butterflies—a most valuable asset for bionomic research—as the "gratification of an infantile taste." This or that collector may be an unscientific person, but it would be easy to show that the study of insects in general, and of butterflies in particular, is one of the most efficient of the instruments in our hands for arriving at a solution of fundamental problems in biology.

My second and final point is this: I have not hesitated to affirm my conviction of the importance in evolution of the Darwinian doctrine of natural selection. This necessarily carries with it a belief in the existence and general prevalence of adaptation. I am willing to admit that at times too much exuberance may have been shown in the pursuit of what Aubrey Moore called "the new teleology." "Men of science," it has been said, "like young colts in a fresh pasture, are apt to be exhilarated on being turned into a new field of inquiry; to go off at a hand-gallop, in total disregard of hedges and ditches, to lose sight of the real limitation of their inquiries, and to forget the extreme imperfection of what is really known." This is not the utterance of some cold outside critic, but of a great exponent of scientific method—no other than Huxley himself. It may be true of some of the wilder speculations of Huxley's date. I am by no means sure that there is not truth in it as applied to some of the developments of a later time. But however wide of the mark our suggested explanations and hypotheses may be, the net result of all our inquiries, after the gradual pruning away of excrescences and superfluities, will be a real advance into the realms of the unknown. We may feel perfectly assured that the objections so far brought against our own interpretations are null and

void, but we may yet have to give way in the light of further knowledge. "Let us not smile too soon at the pranks of Puck among the critics; it is more prudent to move apart and feel gently whether that sleek nose with fair large ears may not have been slipped upon our own shoulders."¹

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

BRISTOL.—Under the will of the late Dr. Joseph Wigglesworth, whose interest in bird life is widely known, his ornithological library passes by bequest to the University. This library of more than 1000 volumes, including finely-bound copies of the works of Gould, Seebohm, Dresser, Lilford, Levaillant, and other leading authorities, is probably one of the best in the kingdom. It will be housed in a separate room in the new University buildings, and will be kept up to date. Dr. Wigglesworth gave the residue of his estate to the University after his widow's death for the furnishing and maintenance of this special library. The advantage to a university of facilities for prosecuting specific lines of research can scarcely be overestimated. Situate, as is Bristol, in a district rich in birds, it is to be hoped that the studies to which Dr. Wigglesworth devoted so large a portion of the little leisure obtainable in a busy and fruitful life will be stimulated by a bequest which will serve to keep his own work in remembrance.

CAMBRIDGE.—Dr. A. E. Shipley, Master of Christ's College, has resigned the office of Vice-Chancellor and been succeeded by Dr. Peter Giles, Master of Emmanuel. During his period of office Dr. Shipley devoted himself consistently to progressive measures, and was most active in furthering schemes of scientific importance. He has had two years of very strenuous work under abnormal conditions, and members of the University are grateful to him for the devoted attention he has given to all matters affecting their best interests.

GLASGOW.—During the summer an unusually large number of university lecturers have been promoted to professorial chairs at Glasgow and elsewhere. Prof. Henderson, formerly assistant, and lately professor, at the affiliated Royal Technical College, has been appointed to the Regius chair of chemistry in the University; Dr. T. S. Patterson, Waltonian lecturer, to the Gardiner chair of organic chemistry; Dr. E. P. Cathcart, formerly Grieve lecturer, to the Gardiner chair of physiological chemistry; Dr. C. Browning, formerly lecturer in clinical pathology, to the Gardiner chair of bacteriology; and two other lecturers in the arts faculty have also been promoted to chairs in the University.

The Queen's University of Belfast has elected Dr. A. W. Stewart, lecturer in physical chemistry at Glasgow, to its chair of chemistry, and Dr. T. Walmsley, lecturer in embryology at Glasgow, to its chair of anatomy. Dundee University College (St. Andrews) has appointed Dr. F. J. Charteris, lecturer in pharmacy at Glasgow, to its chair of materia medica, and Dr. J. F. Gemmill, research fellow and formerly lecturer in embryology at Glasgow, to its chair of natural history. Dr. Shaw Dunn, lecturer in clinical pathology at Glasgow, has been appointed professor of pathology in the University of Birmingham. Dr. W. E. Agar, lecturer in zoology and heredity at Glasgow, has been appointed professor of biology in the University of Melbourne. Dr. Leonard Findlay, Gow lecturer in medical diseases of children, has also been

¹ Dowden.