

out in the sand, and relieve each other by turns at incubation. Le Vaillant purposely watched an ostrich's nest, and during the day saw four hens sit successively on the same eggs, a male bird coming late in the evening to take his turn at incubation." A little further on, I added: "Incubation lasts six weeks, the cock-bird taking his turn at sitting like the hens."

Your reviewer, still sceptical, replies: "The passage in Mr. Harting's book is based on the statement of Le Vaillant, whose observations, except when confirmed by later experience, are justly discredited by the best-informed naturalists of the present day, as he was notoriously so often unworthy of belief."

Permit me to point out that in making the statement above quoted, I by no means relied *solely* on Le Vaillant. I had before me the evidence of several modern observers on the subject, whose publications are referred to in my "List of Works quoted," at the commencement of my volume. At p. 189 I have alluded to the experiments made at San Donato, near Florence, in 1859 and 1860, by Prince Demidoff, who says that "the female ostrich began to sit as soon as the first egg was laid, and sat for three hours daily, leaving the male for the rest of the time."

At p. 196, quoting a report forwarded in 1873 by a resident of experience in South Africa to the Council of the Zoological and Acclimatisation Society of Victoria, who were then contemplating the introduction of the ostrich into that colony, I find this distinct statement: "The process of hatching is performed by the male and female sitting alternately, one keeping a vigilant look-out as sentry, as well as procuring food."

Again, in a Report by Dr. W. G. Atherstone of Grahamstown, based on observations made by himself and friends on different ostrich farms in the neighbourhood of Grahamstown, and quoted by me in *extenso*, the following passage occurs on p. 202 of my book:—"They sit alternately, the male at night grazing and guarding the females. During the daytime, the time of the male bird going on the nest varies during the period of incubation, as also does the time between the female leaving the nest and the male taking her place, the exposure and cooling being probably regulated by the temperature of the incubation fever at different stages."

In addition to the evidence of these observers I had before me the testimony of Mr. F. Denny of Grahamstown, which is too long to be quoted here, but which will be found embodied in an interesting note published in the *Zoologist* for 1874 (p. 3916); so that I felt perfectly justified in asserting in effect, as Mr. Romanes has done, that *the task of incubation with the ostrich is shared by both the sexes*. It would be easy to adduce further evidence on the subject if necessary, but I will not occupy space further than to observe that if your reviewer will turn to p. 107 of Douglass's "Ostrich Taming in South Africa," published by Messrs. Cassell and Co. in 1881, he will see a full page illustration thus lettered, "*Hen bird sitting*." From a photograph taken at Heatherton Towers."

Admirers of Le Vaillant will be glad to learn that in this case at least his assertions (to quote your reviewer) "have been confirmed by later experience," and are therefore not to be discredited.—I am, Sir, your obedient servant,

22, Regent's Park Road, N.W.

J. E. HARTING

After such a battery of evidence it seems almost needless to adduce more; but as the point is an interesting one to ornithologists, I shall briefly add some corroborative proof from other sources.

In the *Spectator*, besides referring to the above, I gave a reference to two articles published by Mr. E. B. Biggar on the ostrich-farms of the Cape Colony, and also to the recently published work by Mr. Nicols; from each of these sources I shall now quote brief passages. Mr. Biggar writes as follows:—

"Some will sit throughout with the most solicitous maternal instinct; . . . others manifest such anxiety, that when the hen has been a little late in taking her morning turn upon the nest, he has gone out, and, hunting her up, has kicked her to the nest in the most unmanly manner. Some are very affectionate over their young, others the reverse; thus do individuals differ even among ostriches. As a rule the cock bird forms the nest, sits the longest, and takes the burden of the work of hatching and rearing. Contrary to what has been currently understood, and what is still stated even in recent colonial accounts, the cock bird sits at night, not the hen. In this peculiarity the hand of Providence may be seen, for the worst enemies of the nest appear at night, and the cock, being stronger and braver, is better able to resist them; moreover, the feathers of the cock

being black, night sitting would not expose him to that exhaustion from the sun's rays which would ensue if he sat during the day; while at the same time the grey feathers of the female are less conspicuous while she sits during the day."—*Field*, August 21, 1880.

And again, "After turning the eggs over one by one with her beak, she will sit perhaps for hours with her head stretched flat and snake-like on the ground, and her body as motionless as a mound of earth. Occasionally, on hot days, she may be seen with her body lifted slightly out of the nest to admit a current of air over the eggs; and sometimes she will even leave the nest for two or three hours, till instinct tells her that the lowering temperature requires her return" (*Century*, January, 1883).

Mr. Nicols's work, entitled "Zoological Notes," repeatedly states that the hen bird assists the cock in the process of incubation, and on my writing to him to ask whether he had witnessed the fact, he answers that although he has not done so himself, a well-educated friend "who had passed some time in visiting ostrich-farms in South Africa" had done so; and, in answer to his express inquiry on the subject, wrote, "that the female took part in the task, though not nearly to so great an extent as the male," adding that he was surprised to hear there should be any question concerning a fact so well known to the ostrich farmers.

Lastly, having recently been to Florence, I took the opportunity of calling upon the superintendent and proprietor of the Zoological Gardens there, and obtained all the particulars of the case alluded to by Mr. Harting in the above letter as having occurred at San Donato. I found that two broods of young had been raised in successive years by the same pair of ostriches, and that on both occasions the female assisted the male to incubate the eggs: "que le male et la femelle couvent alternativement," in the words of the published report ("*Guide du R. Jardin Zoologique de Florence*," p. 81, 1868). Here, however, as in all the previously-mentioned cases, the fact which I stated in "Animal Intelligence" was apparent, viz. that the cock bird undertook the whole duty of sitting during the night.

Now when all this evidence is taken together it appears to me impossible to doubt that the female ostrich assists the male in the process of incubation. Yet from the fact of this evidence not having been clearly focused, an old error on the subject still appears to be prevalent. This error arose some twenty years ago from the observations of M. Noel Suchet (? or Suquet) on a pair of ostriches kept in confinement. Thus, in 1863, Dr. Selater wrote:—"We now know with certainty from the observations of M. Noel Suchet, Director of the Zoological Gardens at Marseilles, that the normal habits of the ostrich (as regards incubation) do not differ materially from those of its allies of the same family" (*Proc. Zool. Soc.*, 1863, p. 233); and Mr. Darwin, following the judgment formed by Dr. Selater, wrote in the "*Descent of Man*" (p. 479) that the male bird "undertakes the whole duty of incubation." Again, my reviewer in the *Spectator*—who, although curiously weak in his logic, appears to be strong in his ornithology—pins his faith entirely to this single observation of M. Suchet. Lastly, Prof. Newton in his article on "Birds" in the "*Encyclopædia Britannica*" (p. 771), relying, I presume, on the same observation, writes:—"A band of female ostriches scrape holes in the desert sand, and therein promiscuously dropping their eggs, cover them with earth, and leave the task of incubation to the male, who discharges the duty thus imposed upon him by night only, and trusts by day to the sun's rays for keeping up the needful fostering warmth."

Thus it appears that the influence of M. Suchet's observations has been very disproportionate to its merits, and has misled some of our principal ornithologists concerning the normal habits of ostriches.<sup>1</sup> Possibly Prof. Newton, with his extensive knowledge of the literature of such matters, and writing since the appearance of most of the counter-evidence which I have given, is cognisant of some other observations on which he rests his statement. But, if so, it becomes desirable that he should supply his references, as otherwise his statement appears to rest, as my reviewer in the *Spectator* would say, "simply on the survival of the old belief." GEORGE J. ROMANES

March 12

#### Difficult Cases of Mimicry

I HAVE received from Mr. Thos. Blakiston, of Tokio, Japan, a communication to the *Japan Mail* by himself and Prof. Alexander,

<sup>1</sup> I may observe that Mr. R. B. Sharpe, writing in "*Cassell's Natural History*" (vol. iv. p. 228), has not been thus misled, for he says distinctly that the cock and hen "relieve each other by turns."

commenting on my article in NATURE, vol. xxvi, p. 86, and pointing out some errors as to the estimated advantage derived by the mimicking butterflies. On referring to my article, I find that I have, by an oversight, misstated the mathematical solution of the problem as given by Dr. Fritz Müller and confirmed by Mr. Meldola, and have thus given rise to some confusion to persons who have not the original article in the *Proceedings of the Entomological Society* to refer to. Your readers will remember that the question at issue was the advantage gained by a distasteful, and therefore protected, species of butterfly, which resembled another distasteful species, owing to a certain number being annually destroyed by young insectivorous birds in gaining experience of their distastefulness. Dr. Müller says: "If both species are equally common, then both will derive the same benefit from their resemblance—each will save half the number of victims which it has to furnish to the inexperience of its foes. But if one species is commoner than the other, then the benefit is unequally divided, and the *proportional advantage* for each of the two species which arises from their resemblance is as the *square* of their relative numbers." This is undoubtedly correct, but in my article I stated it in other words, and incorrectly, thus: "If two species, both equally distasteful, resemble each other, then the number of individuals sacrificed is divided between them in the proportion of the square of their respective numbers; so that if one species (*a*) is twice as numerous as another (*b*), then (*b*) will lose only one-fourth as many individuals as it would do if it were quite unlike (*a*); and if it is only one-tenth as numerous, then it will benefit in the proportion of 100 to 1."

This statement is shown by Messrs. Blakiston and Alexander to be untrue; but as some of your readers may not quite see how, if so, Dr. Müller's statement can be correct, it will be well to give some illustrative cases. Using small and easy figures, let us first suppose one species to be twice as numerous as the other, *a* having 2000 and *b* 1000 individuals, while the number required to be sacrificed to the birds is 30. Then, if *b* were unlike *a* it would lose 30 out of 1000, but when they become so like each other as to be mistaken, they would lose only 30 between them, *a* losing 20, and *b* 10. Thus *b* would be 20 better off than before, and *a* only 10 better off; but the 20 gained by *b* is a gain on 1000, equal to a gain of 40 on 2000, or four times as much *in proportion* as the gain of *a*. In another case let us suppose *c* to consist of 10,000 individuals, *d* of 1000 only, and the number required to be sacrificed in order to teach the young birds to be 110 for each species. Then, when both became alike, they would lose 110 between them, *c* losing 100, *d* only 10. Thus *c* will gain only 10 on its total of 10,000, while *d* will gain 100 on its total of 1000, equal to 1000 on 10,000, or 100 times as much *proportional gain* as *c*. Thus, while the gain in actual numbers is inversely proportional to the numbers of the two species, the *proportional gain* of each is inversely as the *square* of the two numbers.

I am, however, not quite sure that this way of estimating the *proportionate gain* has any bearing on the problem. When the numbers are very unequal, the species having the smaller number of individuals will presumably be less flourishing, and perhaps on the road to extinction. By coming to be mistaken for a flourishing species it will gain an amount of advantage which may long preserve it as a species; but the advantage will be measured solely by the fraction of *its own numbers* saved from destruction, not by the proportion this saving bears to that of the other species. I am inclined to think, therefore, that the benefit derived by a species resembling another more numerous in individuals is really in inverse proportion to their respective numbers, and that the proportion of the squares adduced by Dr. Müller, although it undoubtedly exists, has no bearing on the difficulty to be explained. ALFRED R. WALLACE

Mr. A. R. WALLACE has been so good as to forward me the extract from the *Japan Mail* above referred to, together with his reply. The article in question bears the title, "Protection by Mimicry—a Problem in Mathematical Zoology." The authors, while admitting the broad principles involved in Dr. Fritz Müller's theory, fail to see why the advantage derived by the mimicking species, in cases where the latter is less numerous than the model, should be as the square of the relative numbers. They admit that "the ingenious explanation seems perfectly satisfactory," but the proportional benefit appeared to them exaggerated. Mr. Wallace has now, I think, cleared up the misunderstanding with reference to this part of the question,

but it may be of use in assisting towards the further discussion of the problem if I here give the simple algebraical treatment adopted in the original paper.

Let  $a_1$  and  $a_2$  be the numbers of two distasteful species of butterflies in some definite district during one summer, and let  $n$  be the number of individuals of a distinct species which are destroyed in the course of a summer before its distastefulness is generally known. If both species are totally dissimilar, then each loses  $n$  individuals. If, however, they are undistinguishably similar, then the first loses  $\frac{a_1 n}{a_1 + a_2}$  and the second loses

$\frac{a_2 n}{a_1 + a_2}$ . The absolute gain by the resemblance is therefore for

the first species,  $n - \frac{a_1 n}{a_1 + a_2} = \frac{a_2 n}{a_1 + a_2}$ ; and in a similar manner

for the second species,  $\frac{a_1 n}{a_1 + a_2}$ . This absolute gain, compared

with the total numbers of the species, gives for the first ( $A_1$ ),  $\frac{a_2 n}{a_1(a_1 + a_2)}$ , and for the second ( $A_2$ ),  $\frac{a_1 n}{a_2(a_1 + a_2)}$ . We thus have the proportion,  $A_1 : A_2 = a_2^2 : a_1^2$ .

With reference to Mr. Wallace's concluding paragraph, I may point out that the advantage of the mimic is "measured solely by the fraction of *its own members* saved from destruction." Thus, taking his last example, the species *c* saves only 1/1000 of its whole number, and *d* saves 1/10 of its whole number by the resemblance to *c*. The fact that these numbers stand to one another in the ratio of 1 : 10<sup>3</sup>, whilst  $c : d = 10 : 1$ , is a mathematical necessity from which I do not see how we can escape. As the numerical disproportion between the species increases, the advantage derived by the more abundant insect is practically a vanishing quantity; whilst, on the other hand, if the two species are equal in numbers, it is obvious that they both derive the same advantage, each losing only half the number that it would if there was no resemblance between them.

It must not be forgotten in considering the question of mimicry between two nauseous species that the foregoing calculations apply only to the case where the resemblance is perfect, *i.e.* so exact that the insects are absolutely undistinguishable by their foes. The initial steps may be hastened in these cases by the near blood-relationship of the species, and it is a remarkable circumstance that large numbers of species belonging to different distasteful genera have a close similarity of wing-pattern, although the distinctness of the genera has never been called in question. But the genera concerned, although distinct, are very closely related, and this is quite in accordance with the views here advocated.

The general question as to the persecution of distasteful butterflies by young inexperienced birds, &c., is certainly one on which much work remains to be done, and very great service could be rendered if naturalists residing in the tropics would undertake some systematic experiments in this direction. My friend, Mr. W. L. Distant, the author of the "Rhopalocera Malayana," has already given reasons in these columns (vol. xxvi, p. 105) for disbelieving in any such want of experience, and I have discussed this phase of the question with him elsewhere (*Ann. and Mag. Nat. Hist.*, December, 1882).

R. MELDOLA

#### On the Value of the "Neoarctic" as One of the Primary Zoological Regions

IN the *Proceedings of the Academy of Natural Sciences of Philadelphia* (December, 1882) Prof. Angelo Heilprin has an article under the above title, in which he seeks to show that the Neoarctic and Palearctic should form one region, for which he proposes the somewhat awkward name "Triarctic Region," or the region of the three northern continents. The reasons for this proposal are, that in the chief vertebrate classes the proportion of peculiar forms is less in both the Neoarctic and Palearctic than in any of the other regions; while, if these two regions are combined, they will, together, have an amount of peculiarity greater than some of the tropical regions.

This may be quite true without leading to the conclusion argued for. The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups, and geographical individuality. The absence of the