

the names Alunda, Arungo, Acumbe, not Balunda, &c. They tell us that the Alunda never pronounce the letter r, and that in writing the names Arungo, Moiro, &c., in which that letter occurs, they have adhered to the Maravi dialect. We thence conclude that for the names Rua, Moero, Lufira, &c., and perhaps for the initial Ba above alluded to, Dr. Livingstone is probably indebted to his Arab friends, who rest satisfied with a jargon, in some degree intelligible everywhere, and nowhere perfect.

Dr. Livingstone seems to be elated with the discovery that "the chief sources of the Nile arise between 10° and 12° S. lat., or nearly in the position assigned to them by Ptolemy, whose river Rhapta (?) is probably the Rovuma." Here two different problems are attempted to be solved at once—one touching the Sources of the White Nile, and the other, those of Ptolemy's Nile. With respect to these latter, it will be enough to observe that Ptolemy's Lakes of the Nile, two, in number, 8 degrees asunder, are placed by him respectively in lats. 6° and 7° S., but his graduation being defective, through an imperfect estimate of the length of a degree, the positions thus assigned to the lakes fall under true graduation, to 11" N., and 40" S. of the equator. Ptolemy's Lakes, therefore, have not been reached by the zealous traveller.

With respect to the sources of the Bahr el Abyad, they may of course be traced to the head waters of the Luapula, provided that the results of Capt. Burton's observations on the altitude of Nyanza and the character of its northern end are completely thrown aside. With a greater elevation, and an outlet through Speke's Mountains of the Moon, the waters of the lake may reach Egypt.

It is to be regretted that Dr. Livingstone missed the opportunity of viewing the highest mountain in this part of the world, now known only by a ridiculously exaggerated description; and also a most interesting point in the centre of Africa. The great town, Katanga, as described by the Arabs, is near the copper mines, where 75 lbs. of copper may be bought for 4 cubits of American sheeting. The town is larger than Roonda (the Cazembe's town), and has good bazaars; it stands on the Rafira (Luvira) which joins the Ruapura (Luapula). The people are peaceable, and kind to strangers. The people from Zanzibar learned the language almost immediately.

F.R.G.S.

[We give a map of the region recently traversed by Livingstone, showing its connection with the known points in this part of Africa. We owe this map to the courtesy of the officers of the Royal Geographical Society.—ED.]

CUCKOWS' EGGS

SCARCELY any bird has so much occupied the attention, not merely of naturalists, but of people generally, as the Common Cuckow of Europe, and (we might almost add, consequently) scarcely any bird has had so many idle tales connected with it. Setting aside several of its habits wherein it differs from the common run of birds, its strange, and, according to the experience of most persons, its singular mode of entrusting its offspring to foster-parents, is enough to account for much of the interest which has been so long felt in its history. Within the last twenty years a theory (which is, as I shall pre-

sently show, by no means a new one) with respect to an important fact in its economy, has attracted a good deal of attention, first in Germany, and latterly in England; and as this theory seems to be especially open to misconception, and in some quarters to have been entirely misunderstood, I shall endeavour to give an account of it in a manner more distinct than has yet (I think) been done; and to show that there is no good ground for believing it to be irrational, as some have supposed, and for scouting it as something beneath contempt.

It has long been notorious to oologists that the eggs of the Cuckow are subject to very great variety in colour, and that a large number of birds laying eggs of very different colours enjoy the doubtful advantage of acting as foster-parents to the young Cuckow. Now the theory to which I refer is that "the egg of the Cuckow is approximately coloured and marked like those of the bird in whose nest it is deposited, that it may be the less easily recognised by the foster-parents as a substituted one."

This theory is old enough, for it was announced and criticised nearly a hundred years ago by Salerne,* who, after mentioning that he had seen two Stonechats' nests, each containing eggs of that bird, as well as a Cuckow's (which was as blue as the others, but twice [?] as large), goes on to say that he was assured by an inhabitant of Solagne (a district in France to the south of Orleans), that the Cuckow's egg is always blue; and then comes this remarkable statement:—"As to the assertion of another Solognot who says that the hen Cuckow lays its eggs precisely of the same colour as those in the nest of which she makes use, it is an incomprehensible thing." Many of my readers will, I doubt not, be at once inclined to agree with Salerne.

Little attention seems to have been paid to this passage by succeeding naturalists; † but in 1853 the same theory was prominently and (I believe) independently brought forward by Dr. Baldamus, then editor of *Naumannia*, a German ornithological magazine, now defunct; so far as I know, however, it was not until April, 1865, that an article in the English ornithological journal, the *Ibis*, by Mr. Dawson Rowley, gave anything like an idea of it to the public of this country. Some months later (14th September) Mr. A. C. Smith introduced the subject to the Wiltshire Archæological and Natural History Society, and the paper he then read, having been since printed in the *Wiltshire Magazine* (vol. ix. p. 57), and elsewhere, has, with Mr. Rowley's article, made the theory very generally known. Mr. Smith also published, subsequently, in the *Zoologist* for 1868, a translation of Dr. Baldamus's elaborate essay; but this translation being unaccompanied by the coloured plate which illustrated the original, unfortunately fails to do justice to the Doctor's theory, for without seeing the specimens on which this is founded, or good figures of them, the evidence in its favour can scarcely be appreciated fully.

Dr. Baldamus's theory had been some time known to me, when in 1861 I had the pleasure of being shown by him his collection of Cuckows' eggs, and I can declare

* L'histoire naturelle, éclaircie dans une de ses parties principales, l'ornithologie, &c. Paris; 1767, p. 42.

† Montbeillard (*Hist. Nat. des Ois.* vi. p. 309) mentions it, but I am not aware of any one else who has done so, until M. Vian in the *Revue et Magasin de Zoologie* for 1865 (p. 40), referred to it, and from this reference I became acquainted with it.

that his published figures represent the specimens (sixteen in number) from which they are drawn, as faithfully as figures of eggs usually do, and that an inspection of the series convinced me that the belief he entertained was not groundless. All the eggs in question, some departing very widely from what I had been used to regard as the normal colouring, bore an unmistakable resemblance to those of the birds in the nests of which they were asserted (in most cases, I was assured, on very good authority) to have been found; while in some cases there was just enough difference between them and those they "mimicked," to show that it was far more unlikely that they should have been extraordinary varieties of the eggs of the species in question, than eggs of the Cuckow.

Dr. Baldamus's allegation therefore seemed to me to be in part proved. If the history of the eggs before me could be trusted—and I had no reason to doubt it, the fact of the likeness was in many respects self-evident, in others certainly not so striking, and in some perhaps questionable. In further corroboration of the theory also, there were the similar instances cited with much assiduity from foreign sources by Dr. Baldamus in his essay,* and one, apparently not known to him, but given by Mr. Blyth in Sir William Jardine's "Contributions to Ornithology" for 1850 (p. 69 *bis*, pl. 52). Another and very remarkable case had also come to my own knowledge. In the autumn of 1857 I had received from Mr. Tristram all the eggs collected by him in Algeria during the preceding season. When they were unpacked, it appeared that there were two more specimens of the egg of a large North-African Cuckow (*Oxylophus glandarius*) than I had been led by him to expect. On examination, I found that the first two eggs of this species which had been obtained by him so much resembled eggs of the Magpie of the country (*Pica mauritanica*), in the nests of which they had been found, that skilful oologist as he was, they had passed, even to his practised though unsuspecting eye, as those of the latter bird. Had I known then of Salerne's words, I should have exclaimed with him, "c'est une chose incompréhensible!"

Having said thus much, and believing as I do the Doctor to be partly justified in the carefully-worded enunciation of what he calls a "Law of Nature," I must now declare that it is only "approximately" and by no means *universally* true that the Cuckow's egg is coloured like those of the victims of her imposition. Increase as we may, by renewed observations, the number of cases which bear in favour of his theory, yet, as almost every bird's-nesting boy knows, the instances in which we cannot, even by dint of straining our fancy, see resemblances where none exist, are still so numerous as to preclude me from believing in the generality of the practice imputed to the Cuckow. In proof of this I have only to mention the many eggs of that bird which are yearly found in nests of the Hedge-Sparrow in this country, without ever bearing the faintest similarity to its well-known green-blue eggs. One may grant that an ordinary English Cuckow's egg will pass well enough, in the eyes of the dupe, for that of a Titlark, a Pied Wagtail, or a Reed-Wren, which, according to my experience, are the most common foster-parents

of the Cuckow in this country; and indeed one may say, perhaps, that such an egg is a compromise between the three, or a resultant, perhaps, of three opposing forces; but any likeness between the Hedge-Sparrow's egg and the Cuckow's, so often found along side of it, or in its place, is not to be traced by the most fertile imagination. We must keep therefore strictly to the letter of the law laid down by Dr. Baldamus, and the practice imputed to the Cuckow is not universally but only "approximately" followed.

Now, is it possible to give a satisfactory explanation of the process by which the facts alleged are produced? Dr. Baldamus assigns none. He lays down a number of aphorisms, most of which are very interesting, and, I believe, true; but they do not touch the question. A good many people who have only read hastily, and still more those who have to all appearance only read at second or third-hand what has been written on the subject, seem to imagine that the Doctor has wished to assert that the Cuckow can voluntarily influence the colour of her egg, so as to assimilate it to those already in the nest in which she is about to deposit it.* Dr. Baldamus, indeed, mentions such a supposition, but expressly says that he rejects it, and herein I think that nearly every physiologist will agree with him.

It will be admitted, I think, that Dr. Baldamus's inference as to the object of the practice being that the Cuckow's egg should be "less easily recognised by the foster-parents as a substituted one," is likely to be true. This being the case, only one explanation of the process can to my mind be offered. Every person who has studied the habits of animals with sufficient attention will be conversant with the tendency which certain of those habits have to become hereditary. It is, I am sure, no violent hypothesis to suppose that there is a very reasonable probability of each Cuckow most commonly placing her eggs in the nests of the same species of bird, and of this habit being transmitted to her posterity. Without attributing any wonderful sagacity to the Cuckow, it does seem likely that the bird which once successfully deposited her eggs in a Reed-Wren's or a Titlark's nest should again seek for another Reed-Wren's or another Titlark's nest (as the case may be), when she had an egg to dispose of, and that she should continue her practice from one season to another. We know that year after year the same migratory bird will return to the same locality, and build its nest in almost the same spot. Though the Cuckow be somewhat of a vagrant, there is no improbability of her being subject to thus much regularity of habit, and, indeed, such has been asserted as an observed fact. If then this be so, there is every probability of her offspring inheriting the same habit, and the daughter of a Cuckow which always placed her egg in a Reed-Wren's or a Titlark's nest doing the like.

Further, I am in a position to maintain positively that there is a family likeness between the eggs laid by the same bird, even at an interval of many years. I know of more than one case in which a particular Golden Eagle has gone on season after season laying eggs that could be at once distinguished by a practised eye from the eggs of almost any other Golden Eagle; and I know of one case

* I do not here enumerate them; they will be found in *Naumannia* for 1853, p. 317, note. The plate which illustrates the paper is in the volume of the same magazine for the following year.

* Thus Mr. Cecil Smith (not to be confounded with Mr. A. C. Smith, before mentioned) in a work published within the last few weeks, falls into this mistake ("Birds of Somersetshire," p. 265), after having stigmatised the Doctor's theory as "wild," which he well might if it had been as it is represented.

in which the presumed daughter of a particular Golden Eagle, remarkable for having produced eggs of very great beauty, has in two successive years laid eggs which unmistakably resembled those of her reputed mother in the brilliant character of their colouring.

Hence I am not afraid of hazarding the supposition, that the habit of laying a particular style of egg is likely to become hereditary in the Cuckow; just as I have previously maintained that the habit of depositing that egg in the nest of a particular kind of bird is also likely to become hereditary.

Now it will be seen that it requires but only an application to this case of the principle of "Natural Selection" or "Survival of the Fittest" to show that if my argument be sound, nothing can be more likely than that, in the course of time, that principle would operate so as to produce the facts asserted by the anonymous Solognot of a hundred years ago, and by Dr. Baldamus and others since. The particular *gens* of Cuckow which inherited and transmitted the habit of laying in the nest of any particular species of bird eggs having more or less resemblance to the eggs of that species, would prosper most in those members of the *gens* where the likeness was strongest, and the other members would (*cæteris paribus*) in time be eliminated. It is not to be supposed that all species, or even all individuals of a species, are duped with equal ease. The operation of this kind of "Natural Selection" would be most marked in those cases where the species are not easily duped, that is, in those cases which occur the least frequently. Here it is that we find it, for it has been shown that eggs of the Cuckow, deposited in the nests of the Red-backed Shrike, of the Bunting-Lark, and of that bird which for some reason best known to the donor bears the English name of "Melodious Willow-warbler," approximate in their colouring to the eggs of those species—species in whose nests the Cuckow rarely (in comparison with others) deposits her eggs. Of species which would appear to be more easily duped, or duped in some other manner—the species in whose nests Cuckow's eggs are more commonly found, I may have something to say in another paper.

ALFRED NEWTON

THE ORIGIN OF BLOOD-LETTING

THE flamingo in the gardens of the Zoological Society has recently been observed to vomit a red-coloured fluid over certain smaller birds kept with it; and it has been shown that this red fluid contains true blood-corpuscles, and inferred that the flamingo is in the habit of feeding its young by this ejection of a blood-stained "pigeon's milk" into their mouths. Further, the habit of the flamingo has been with great probability connected with the story of the pelican, which, as is well known, is stated to wound its own breast in order to feed its young with the blood. It is not at all improbable that birds so alike in their plumage and habitat as the pelican and flamingo should be confused in the way suggested by Mr. Bartlett, who, I believe, first observed the habit of the captive flamingo. The extravasation of *blood corpuscles* normally from the pharynx or œsophagus of such an animal is a matter of great interest. Mr. Lowne has a paper in the Journal of the Queckett Microscopical Club, in which he gives a full account of the case, having examined the bloody exudation microscopically.

To this the reader is referred; but I have something to add to it.

The connection of the flamingo with the classical story of the pelican's self sacrifice is increased in interest, since it appears that the red exudation of the hippopotamus is connected with an equally ancient and more important tradition—namely, the history of the origin of blood-letting. Before giving this tradition, I would mention that two years since, by the kindness of Dr. Murie, I obtained some of the red exudation of the hippopotamus on a slip of glass, and on examining it with the spectroscope, I did *not* obtain a blood-spectrum. Mr. Tomes (Proc. Zool. Society, 1857) described the microscopic appearances of the exudation of the hippopotamus, and stated that he found in it remarkable corpuscles with pigmentary granules, but not *blood corpuscles*. The folds of the skin in various parts of the body of the hippopotamus are coloured bright pink by a distinct pigment, and the same tint suffuses the darker parts of the skin. I believe it is this pigmentary matter which causes the red colour of the exudation of the hippopotamus, and that it is not a sweat of blood at all. The case of Mr. Jamrack's rhinoceros mentioned by Mr. Lowne may be otherwise. Mr. Lowne says that cases of blood-stained sweat from the skin of man are, though rare, well authenticated. This is perhaps true; but many apparent cases of such staining are due to the formation of a purpurate in the sweat, from the decomposition of the uric acid which it contains.

Now, with regard to the hippopotamus, it is important to note how popular tradition has attributed the origin of a very valuable medical art to a totally false inference on the part of Egyptian priests.

M. Milne-Edwards, in the 3rd volume of his "Leçons sur la Physiologie" (p. 3), has the following note:—"Homer, whose poems constitute a sort of encyclopædia of the science which the Greeks possessed about the ninth century before Jesus Christ, does not speak of bleeding: but if we are to believe an author of the fifth century, Stephanus of Byzantium, this operation was known to the surgeons of the army of Agamemnon. In fact, he relates that one of them, Podalirius, son of Æsculapius, and brother of Machaon, on the return from the siege of Troy, practised it on a patient whose cure obtained for him the sovereignty of the Chersonese. This would be the first case of blood-letting of which the remembrance has been preserved; and, on consideration of a fable reported by Pliny, I am induced to believe that this practice had taken its rise in Upper Egypt: in fact, this naturalist tells us that the hippopotami, when they become too obese, have the habit of piercing for themselves the vein of the thigh, by pressing against a pointed reed; and that these animals have thus taught physicians to practise analogous operations. Now, this account does not apply to the sea-horse (or *Syngnathus*), as the author of an estimable work on the history of medicine (Leclerc) supposes, but to the great pachyderm which inhabits the rivers of the interior of Africa, and which is found in Upper Egypt. It is evidently a fable: but this fable could only have reached us from Egypt."

M. Milne-Edwards was not aware of, at any rate does not refer to, the red oozing observed on the skin of the hippopotamus sometimes after emerging from his bath,