

I do not believe that we are necessitated by the far sea flight of birds, to assume a sixth sense. Is it not conceivable that birds are capable of keeping exactly the same direction of flight for many hours together, and so to fly somewhat like a shot ball or a steamship with rudder bound fast? From the physiological side, it might of course be objected that a very slight difference in the strength of the right and left wing-beats must cause a deflection from the original course, just as in the case of rowing without a steersman, a constant control by sight is necessary, if the right direction is not to be lost. To this might be replied, however, that birds are so accomplished in flight, and that we may assume they have an extremely fine muscular sense. Besides, they migrate mostly in company, and an error in flight of one bird will be easily corrected by the others.

But how do they hit the direction in flying away from the coast? They must be able to exactly measure the angle at which they ought to leave the land. Therein, of course, a quite small error would involve great deflections from the proper course, but do we know that this does not actually occur often enough? and may it not be supposed that in many cases corrections are made in the flight, as soon as any point of orientation again emerges in the circle of vision? So much we at least know, that even on land birds wander not infrequently. And it is at least not demonstrated in any one of the cases cited by Mr. Newton, that the birds referred to appeared on those islands every year, nearly at the same time and in the same number.

Mr. Newton adduces a second series of "facts" which seem to be against the sufficiency of the five senses; but are these really facts?

The young, scarcely three months old, of many of our birds, are said to pursue their flight southwards in autumn alone. Is that certain? and have we not here, perhaps, a too ready deduction of general rules from a few well-observed cases? Mr. Newton even says: "This seems to happen with nearly all the accipitres," &c. He quotes a letter from M. Gätke, stating that in July "Young starlings pass over Heligoland by hundreds of thousands without a single old bird accompanying them." I confess that I cannot regard this as a fact, but as a more or less probable conjecture; for M. Gätke, though an excellent ornithologist, could not possibly have inspected a hundredth part of these "hundreds of thousands" of starlings flying about.

I do not mean to assert that these or the other data are false; they may well be correct. I merely hold that we must guard against building far-reaching theoretical inferences on observations the general validity of which is not in the least demonstrated.

But even supposing that all these data are correct; further, supposing it certain, that these young birds, which go forth alone, also actually find the route of the species with the same certainty as if they had known it long before, would these facts be explained by the supposition of a magnetic sense? I think not. For in that case, what must have been born with the young bird? Merely this magnetic sense? i.e., the power of directly perceiving external direction in its own body? By no means. There must also be born with the young bird the consciousness of what angle to the magnetic meridian it must shape its flight at.

But much more than this. It has been long known that birds, so long as they are migrating over land, frequently alter their direction; hence, supposing the young bird to be guided by a magnetic sense, there must be born with it the tendency to fly (say) twenty miles at an angle of 45° to the magnetic meridian, then 100 miles at an angle of 27°, and so on. That this is a physiological absurdity, no one would deny.

For these reasons I hold that a special sense for direction does not exist in birds, and that the phenomena of migration, however wonderful they appear, yet cannot ultimately depend on magic (*Zauberei*), and in this Mr. Newton no doubt agrees with me. Hence, nothing remains but to try to explain these phenomena by the known physical and mental properties of birds; for there is no third course.

I shall be rejoiced if Mr. Newton succeed with this better than I.

AUGUST WEISMANN

Freiburg im Breisgau, March 31

THE editor having afforded me the opportunity of seeing the foregoing remarks, it will, perhaps, be convenient to the readers of NATURE that I should here add the comments I have to make upon them.

I deeply regret if my criticism of Dr. Weismann's treatise or

lecture be open to the charge of unfairness. I had no wish to misrepresent him, and I cannot see that I have been guilty of such an act—indeed, the wide publication of his theory would render any attempt to do so futile. As to his acceptance of Dr. Palmén's conjecture for "absolute truths," I must urge that he took no exception to any of them, while, in the case of his Bernacle or Brent Goose, he especially adopted (p. 27) that route X which I had particular reason to consider unfounded. I did not assert that Dr. Weismann spoke of birds flying over the sea at the height of 20,000 feet, though there seems no reason why some might not, if they can do so over the land; nor did I impute to him that they always keep land in sight. I had no need to declare my disbelief in Dr. von Middendorff's magnetic hypothesis, for I never met with any man that held it. I had spoken of it already elsewhere (*Encycl. Brit.* Ed. 9, iii., p. 769), and I considered it had been set at rest for ever by Prof. Baird in the article I cited. In like manner it seemed useless to disclaim any belief in the possession by birds of a "sixth sense" which is not common to ourselves and other animals. My only object was to show that Dr. Weismann's theory was inconsistent with certain facts, and nothing he has since adduced makes me think it otherwise. As to some of these "facts" he is incredulous, and I have no fault to find with his caution in this respect, but I am sure that the more he investigates them, the less he will be inclined to demur to them. I shall leave to the ornithologists of New Zealand the defence of those that relate to their cuckoos. Dr. Weismann will find in Mr. Jones's "Naturalist in Bermuda" (London, 1859) more than enough to justify my allegations in regard to the passage of *Charadrius virginicus* (not *hawaiiensis*) over those islands; indeed, it has long been notorious; and as to the plovers of the Sandwich group, I have not only to thank Capt. Long, R.N., for his confirmation (*supra*, p. 460) of my statements, but also Prof. George Forbes, who kindly informs me that when there, on the occasion of the transit of Venus, he shot scores of these birds, and that his friend Capt. Cator, R.N., of H.M.S. *Scout*, having sailed thence, was overtaken in mid-ocean by them, flying in a direct line for Vancouver's Island, on arriving at which he found they had already reached it. Concerning the "facts" relating to some young birds preceding their parents in migration, the more inquiries I make of well-placed observers the more satisfactory are the answers. For want of space I cannot here give the details, but I may just say that Mr. Cordeaux, who has been for many years a watchful observer of migratory birds on the Lincolnshire coast, has named to me nine species of *Limicola*, of which he has personally assured himself that the young migrate apart from, and invariably arrive earlier than, the old—thus fully bearing out Temminck's assertion, made nearly forty years ago. The case of our cuckoos, which I cited, is incontestable, and M. Gätke, I doubt not, will satisfy any scruples about his starlings in that book which we are expecting from his hands.

I will also take this opportunity of replying to Mr. Pringle's note (*supra*, p. 481). My chief reason for not referring to the matter of temperature was that we know too little of the power of birds to resist extreme cold to depend much upon it, and I thought I would not take up room by bringing in that question. Doubtless there is something in what he says touching the loom of land, but I fail to see how it will help very far, and especially in nocturnal flights.

ALFRED NEWTON
Magdalene College, Cambridge, April 20

Colour in Nature

I WISH to offer a few remarks upon Mr. Wallace's kind and appreciative review of my work on the "Colour-Sense" in NATURE, vol. xix, p. 501. Mr. Wallace attributes to me "many errors" and inaccuracy as to matters of fact; but I do not think the instances he alleges are sufficient to justify the statement. Had I said in every case what Mr. Wallace makes me say, I should, doubtless, have been misrepresenting facts; but it seems to me that in most of the passages to which he refers he has slightly misconceived my meaning. I should not attempt to oppose so distinguished a naturalist on points of biological inference, but I venture to defend the accuracy of my statements of fact.

1. "*Scissirostrum Pagei* does not 'belong to a family generally dull,' while it is itself decidedly dull-coloured." The first statement will be correct if we place *Scissirostrum* among the brilliant starlings; but Mr. Wallace himself, following Prince