

I have already trespassed too much on the columns of *NATURE*, and I shall conclude by saying that these instances are not thought extraordinary here, and that the belief that cattle and horses can find their way "straight" is firmly held by all bushmen. I have heard similar instances at Lake Torrens, the Darling River, and Maneroo.

I am aware that they do not affect the question as to how a cat finds her way home when conveyed shut up in a bag, but I conceive that they bear out the view suggested by Mr. Darwin, and with which my own experience coincides.

A. W. HOWITT

Bairusdale, Gippsland, Victoria, May 21

Ingenuity in a Pigeon

THE following facts (having been witnessed by myself) may, perhaps, be considered worthy of insertion in your journal, as bearing on the subject of "Perception and Instinct in the Lower Animals," which has lately been brought into such prominent notice.

On the Richmond road (Surrey), at about a mile from the town, stands an old roadside inn, yclept "The Black Horse," owned by one R. Ketley. Attached to the house are a number of domestic pigeons of various breeds, chiefly "Pouters."

Having occasion to wait for my pony to be harnessed at this inn a few years since, my attention was directed by a gentleman (a resident of the neighbourhood) with whom I was acquainted, to the strange conduct of one of these birds.

A number of them were feeding on a few oats that had been accidentally let fall while fixing the nose-bag on a horse standing at bait. Having finished all the grain at hand, a large "Pouter" rose, and flapping its wings furiously, flew directly at the horse's eyes, causing that animal to toss his head, and in doing so, of course shake out more corn. I saw this several times repeated; in fact, whenever the supply on hand had been exhausted.

I leave it to your readers to consider the train of thought that must have passed through the pigeon's brain before it adopted the clever method above narrated, of stealing the horse's provender.

Was not this, indeed, something more than mere instinct?

RICHARD H. NAPIER

Upton Cottage, Bursledon, Southampton, Aug. 13

The Origin of Nerve Force

I NOTICE in *NATURE* for July 21 a paper by A. H. Garrod, suggesting that nervous force has its origin in thermo-electric currents due to the difference of temperature between the surface and interior of the body. Without presuming to any opinion from the physiological point of view, I venture to mention one or two obvious difficulties.

Although, as the writer observes, "in cold weather the impulse to act is much more powerfully felt than in summer, when the air is hot, and therefore the temperature of the surface is higher," yet even 98° F. (the internal temperature of a healthy body) is not uncommon for the air in tropical climates, where the natives can undergo great exertions. But, according to the thermo-electric hypothesis, the nerve force must in this case be *nil*. Again, temperatures of 140° to 160° F. are easily sustained for a considerable time in the Turkish Bath. Under these conditions the direction of the current should be reversed; and, even supposing that positive and negative currents both acted in the same sense on the muscles and nervous ganglia, it would seem that there must be an instant of transition when the two should be balanced, and nervous force at zero, and the powers both of sensation and motion lost with it.

The thermo-electric theory is not required to explain the cases to which Mr. Garrod alludes. We have only to consider that the body must be kept at a constant temperature of about 98° F., while heat is being continually evolved internally by nervous and muscular action, to see that the surface of the body must be cooler than the interior in order to get rid of the superfluous heat without consumption of work in increased perspiration and evaporation. At high external temperatures there will naturally be disinclination to muscular exertion; not only because it produces heat which tends to upset the equilibrium of temperature, but because the force that would have been expended in it is consumed in increased action to get rid of the heat. That the exhausting effect of hot water is much greater than that of hot air is accounted for both by its greater conductivity and specific

heat, and still more because it checks evaporation, which is one of the most powerful outlets for waste heat. It must be familiar to everyone that rapid exhaustion is produced by immersion not only in hot water but in that of almost any temperature. Taking 70° as an average external temperature, we shall find that immersion in water at 30° would be quite as rapidly destructive of nervous energy as in that of 110°; and that while air of the latter temperature could be sustained by the naked body for long without inconvenience, that of 30° would be rapidly fatal unless the temperature was kept up by violent exercise.

Supposing the brain to be really colder than the blood, I shall be glad if some physiologist will inform me if this is not due to the consumption of heat in building up the complicated and unstable matter of the brain from the comparatively stable and simple constituents of the blood, and in this case, if there is any difference of brain temperature between times of rest and nutrition (sleep) and those of active exertion.

Knowing as we do that chemical action is constantly going on in the body and that electrical disturbance is an almost constant result of such action, it seems hardly necessary to look further for the source of nerve force, though we are in almost complete ignorance of the details of its production.

HENRY R. PROCTER

The Flight of Birds

YOUR correspondent, J. Guthrie (vol. viii. p. 86), has struck a note which will, I think, echo. The question he raises is one which has exercised more minds than one. It has been present to me individually almost ever since I was able to reason. The opportunities enjoyed by exiles, especially in tropical countries, for the study of the phenomenon of a body, poised in mid-air, with no apparent support, is considerable, owing to the boldness and number of kites and birds of that class. I have watched them from the point of view—figuratively speaking—of your Cape correspondent, scores of times, and sometimes under peculiar conditions; but I am unable to add anything *certain* to the bare statement that birds of prey *can* maintain a position of absolute apparent rest.

It is some fourteen or fifteen years since I first watched an eagle in a telescope, with a view to test an explanation—the same as that suggested by Mr. Guthrie—hazarded as a conceivable possibility by my father, long before. Since that day I have had innumerable opportunities for close watch—some of which I will describe—and never have I seen anything to support it.

Not to go back too far, as I must trust to memory, I was, two or three years ago, on the summit of a long-backed solitary hill, 500 or 600 ft. high, in the Coimbatore plains of Southern India. There was a light breeze blowing, and I saw an eagle stemming it, on the leeward side of the hill, which was steep. Sometimes he was within (say) fifty yards, and having a good glass at hand, I rested it on a stone heap, and watched him. It was frequently possible to see him thus, stationary in a motionless field of view, at an apparent distance of 10 or 12 ft. Not a feather quivered: the head was turned from side to side as he scrutinised the hill-side: occasionally a foot was brought up to the beak: the roll of the eye was perceptible: but otherwise he was *at rest* to all appearance. Of course the tips of the wings came in for a share of my scrutiny. They may have been quivering, but they looked as steady as those of a stuffed specimen. And here I may observe, that for this appearance to be compatible with an unperceived vibration, the position of rest must have existed alternately with successive excursions, and the time occupied by the latter must have been insignificant as compared with the duration of rest. I find it impossible to accept this explanation, even as a first step, and need not inquire how it would produce the supporting effects. The tail, I should mention, was not at rest. It was frequently feeling, as it were, the passing breeze.

It is to be understood that in the course of frequent changes of general position, I had the bird under examination from different directions—not always of course so favourably.

On another occasion I spent a fortnight on the summit of a peculiar hill in this neighbourhood, with nothing to do but recruit as fast as possible. The hill resembles a dish-cover at top, and being the resort of fugitives from the dust and drought and heat of Bangalore in April and May, who occupy every available dwelling on a very restricted space, there is plenty to attract the kites from far and wide, to say nothing of vultures. There are two or three kinds of kite, but for the present subject they are all the same—fine, powerful, bold birds, with a stretch of