The Explanation of Animal Behaviour

By REX KNIGHT

PROF. LLOYD MORGAN long ago formulated the principle that "we should not regard any instance of animal behaviour as the outcome of higher mental processes, if it can adequately be interpreted as the outcome of mental processes which stand lower in the order of mental development". This principle is but a special application of the canon of parsimony, which must be obeyed in all scientific explanation; and, when logically extended, it imposes upon us the duty of inquiring, in the first place, whether animal actions must be ascribed to any mental processes at all.

It happens, however, that, if we openly pursue this inquiry, we run the risk of being misunderstood; for many people will wrongly suppose that in suggesting a non-mental explanation of animal behaviour, we are thereby questioning the existence of animal minds. In my much-discussed communication to the British Association at York, for example, I criticised the view that some animal actions cannot be accounted for unless we regard them as caused by the animal's mental experiences; and, in effect, I suggested that the view is unjustified. This was my sole aim, and (since to question whether *A* is necessary to the explanation of *B* is not to question whether *A* exists) it did not imply that there are no animal minds. Moreover, to guard against misunderstanding, I was careful to state, both at the beginning and at the end of my paper, that it is probable, on evolutionary grounds, that animals have mental experiences corresponding to their neural organisation. Nevertheless, I was widely misunderstood, and the general discussion, which newspaper reports of my remarks has aroused, has mostly proceeded on the mistaken assumption that I questioned or denied the existence of animal minds.

Let it be clear, then, that when we inquire whether animal actions can be explained in non-mental terms, the question is not whether animals think, but whether their thoughts must be held to affect some of their actions. What we are asking is this: Even though there be animal minds, must we ascribe causal efficacy to them in order to explain how animals act?

When the layman seeks to produce some animal action, which, in his view, cannot be explained in non-mental terms, he is apt to refer, first of all, to examples of the differentiation that animal behaviour often exhibits. He tells of the cat that will sleep only on one particular cushion, of the dog that attaches itself more to one person than to others, or of some other animal whose behaviour is differentiated. Sir Edward Headlam, for example, wrote to the *Times* (April 5) about a hunter that neighed whenever it saw a pink hunting-coat; and in the same journal (Sept. 24) Mr. H. Boyd Collins told of a dog, which, when its master imperfectly simulated grief, acted in a way in which (so ran the suggestion) it would not

have acted if the simulation had been complete. It is urged that in all these cases the animal 'knows', and that its behaviour must be ascribed to conscious discrimination.

This conclusion, however, is unwarranted. The fact that an animal responds differently to two different stimuli may not be due to the animal's having mentally distinguished between them. A wireless set will respond differently to different wave-lengths; a flower will respond differently to light and to darkness; a decapitated earthworm, confined in a *T*-shaped tube, will keep away from that branch of the *T* which gives it a mild electric shock; and, if we put a drop of acid on a spinal frog, the brainless creature will scratch just the spot that we touch—and, if we increase the strength of the acid, it will scratch correspondingly harder. Since in none of these cases is there any question of mind, it is clearly illicit to argue that differentiation in behaviour must always be due to conscious discrimination.

A second group of actions, the explanation of which is said to require reference to animal minds, comprises those that are sometimes ascribed to animal memory. Under this head we hear of the cat that purrs when its mistress comes home after a holiday; of the (authentic) elephant that deluged the man who had given it, six weeks before, a sandwich with much cayenne pepper; and of the dogs, referred to by Miss Katherine Buck in the *Morning Post* (Sept. 9) whose death followed, and seemed to be caused by, the previous death of their mate or their master.

But 'memory' is an ambiguous word. We can remember what we did last Sunday, and this kind of memory does involve the mind and conscious recall. We can also remember how to skate or to swim, and here the mind need not enter at all—by saying that we 'remember' we mean merely that the results of certain past behaviour have been enregistered in our bodily system, or (in Pavlovian language) that certain of our inborn reflexes have been interlinked or otherwise conditioned by our environment. Now, no one will dispute that many animal actions are due to memory, in the second, non-mental sense. But can we be reasonably sure that, as Prof. Lloyd Morgan and Sir Arthur Thomson believe, animals enjoy conscious recall? Even if we can be sure of this, are there any animal actions which must be attributed to conscious recall rather than to its physiological counterpart? Clearly, the cat's purring on the return of its mistress demands no such explanation. It can be adequately accounted for by the purely physical traces which the cat's previous association with its mistress has left in its nervous system; and the same may be said of the vindictive elephant. Nor does the death of a dog as the result of the death of its mate or its master need to be ascribed to any mental

experience. After all, a dog will not long survive the absence of food; and, granted that the food-stimulus is one that the dog innately needs, surely the stimuli provided by mate or master may become acquired or conditioned necessities. What affects the bereaved animal may be, not the recollection of past events, but just the absence of the mate or the master from its present environment.

A third group of animal actions, which, it is said, must be due to mental experiences, contains those that arise in connexion with animal learning. Animals, of course, can be trained. Sheep-dogs, horses, elephants, and other animals, are being trained every day; and, under experimental conditions, there has been (to mention only a few examples) the training of Möbius's pike, Yerkes's turtles, Sackett's porcupines, Thorndike's cats, and a large number of colonies of maze-threading rats. But, great as is the practical and theoretical value of animal training, does it anywhere provide us with facts that require the ascription of causal efficacy to animal minds? The training in each case seems clearly to consist in that modification, or conditioning, of the animal's inborn responses which does not demand explanation in other than physiological terms. It exhibits the gradual elimination of wrong responses (wrong in relation to the particular training concerned), and the organisation of right responses into larger and larger units. But it does not necessitate retrospection or any other experience in the animal's mind. There is no need to hold that Pavlov's dogs, when they salivated in response to the ringing of a bell, must have been consciously remembering those earlier occasions when the ringing of the bell had been accompanied by the giving of meat; and all other animal training, being but an extension of this simple case, is similarly explicable in non-mental terms.

Animals, however, do not learn only when they are trained by human beings. They also learn, as we say, 'on their own'. These two forms of learning are, to be sure, not radically different. In both the animal's learning consists in the changing of its behaviour under the influence of environment, and the only distinction between them is that in the first the environment is more controlled by man than it is in the second. Still, it is useful to separate the two forms, for it is when animals learn 'on their own' that the efficacy of their minds is said to be especially obvious.

Many British Association critics of my paper have brought forward examples of this kind of learning. The *Times* has published letters from Mr. C. H. Roberts, who described (Sept. 29) how a dog, finding no one at home to fill its water-bowl, carried the bowl to the house of a neighbour; from Mr. Gilbert Coleridge, who told (Sept. 23) how a pony learned to lift a gate off its hinges; and from Lieut.-Col. W. Bunbury, who gave an account (Sept. 21) of a Cairn terrier's feints. Moreover, both in the *Morning Post* (Sept. 6) and in the *Veterinary Record* (Oct. 1), Sir John Moore

described how he fell one day in the hunting field and was hung up by the stirrup between his horse's legs, and how the horse stopped twenty or thirty yards from the fence and put its off hind-leg beside him.

Now, in all these cases we may accept the recorded facts without accepting the view, dogmatically laid down in each instance, that they can be due to nothing but animal reasoning. For my part, before accepting any such view, I should require to know much about each of the examples put forward—more facts about the reported occurrence, and about the past behaviour of the animal concerned. Nevertheless, I would say, even now, that the action of Sir John Moore's horse was not indisputably due, as he supposes, to "reason, memory, and other attributes of mind". The stopping of a horse, whose rider has become suspended between its legs, can quite well be explained in terms of reflexes; and I think a similar explanation would suffice in each of the other cases that have been brought forward.

If I seem here to be over-cautious in regard to the efficacy of animal minds, I have at least two reasons for my parsimony: first, there is abundant evidence that brainless animals can profit by experience—that even the naturally ganglionless starfish learns; secondly, my own observations of animal behaviour, both inside and outside the laboratory, have shown me how easy it is for animals to acquire an undeserved reputation for mental characteristics. In my Department in the University of Aberdeen, we have been making careful observations of animal learning under controlled, experimental conditions. In one of our experiments we observed the behaviour of cats learning to get out of a box which could be opened from the inside only by pulling a stirrup. Most of the cats did learn to pull the stirrup; and one in particular earned a great reputation for intelligence among visitors by the speed with which, after a time, it opened the box and got to the milk that was waiting outside. These casual visitors naturally supposed that the cat pulled the stirrup because it had consciously grasped the fact that its action would open the door. Such a supposition, however, must be discounted in view of the fact that, when the stirrup was moved, the cat still clawed the air in the place where it had been—and that, when placed beside milk outside the open box, the cat would often not touch it until it had needlessly run into the box and pulled the stirrup.

It seems, then, that, when we consider the main groups of animal actions which are said to be caused by animal minds, we find none which is clearly incapable of being explained in non-mental terms; and this was the view that I sought to express and to argue at York.

One word in conclusion. Some of my critics, notably Mr. Gilbert Coleridge (*Times*, Sept. 9) and Sir John Moore (*Veterinary Record*, Oct. 1), condemn the observation of animal behaviour under experimental conditions. In a scientific journal there is no need to rebut so perverse a

view; but I would make just two comments upon it. First, under experimental conditions we can observe the genesis of any particular action, and so avoid those false inferences to which observation of the action by itself might naturally lead. Secondly, under experimental conditions we can

compare the behaviour of normal animals with that of spinal or decerebrate preparations, and thus test such mistaken assumptions as that mind is required in order that an animal should respond differently to different stimuli, or be modified by its environment.

Sir John Leslie, 1766-1832

By Prof. D. F. FRASER-HARRIS

THE centenary occurs on November 3 of the death of Sir John Leslie, mathematician and physicist. Leslie was a native of the small town of Largo in Fife, where his father, a most intelligent man who had come from the neighbourhood of St. Andrews, was a cabinet-maker. Leslie's mother's name was Carstairs. When only thirteen years old, John was sent to the University of St. Andrews to study for the ministry of the Church of Scotland, but after six sessions there and one or two years at Edinburgh until 1787, he gave up the idea of the Church and devoted himself to the study of his favourite mathematics. His paper on "The Resolution of Indeterminate Problems" had the honour of being admitted to the *Transactions of the Royal Society of Edinburgh* when its author was as yet in his twenty-second year.

In 1793 appeared Leslie's translation of Buffon's "Natural History of Birds" in nine volumes which he had made for John Murray.

Unsuccessful as a candidate for a chair first at the University of St. Andrews and then at the University of Glasgow, Leslie retired to Largo, where he carried out these experiments which, when published in 1804 as "An Experimental Enquiry into the Nature and Properties of Heat", made his name famous throughout the world of science. In this now classical research, he employed his famous cube to study the relative absorptive and reflecting powers of a large number of materials. The 'differential thermometer' which he had devised some years earlier was now found exceedingly useful. This instrument has been described by an enthusiastic admirer as "one of the most beautiful and delicate that inductive genius ever devised as a help to experimental enquiry". The essay of 1804 won for Leslie the recognition of the Royal Society, the Rumford Medal of which was awarded him in 1805.

In the same year, Leslie was a candidate for the chair of mathematics in the University of Edinburgh, vacant through the translation of Playfair to that of natural philosophy. Leslie, whose testimonials included one from Sir Joseph Banks, was vastly better qualified in science than his only opponent, a minister of Edinburgh; but the clergy, professing to see in his essay 'an infidel note' because he had quoted approvingly some remark of Hume on cause and effect, opposed in the most vehement manner his election to the chair. All intellectual Scotland was in a state of ferment; the case was argued for two days before a court consisting of 180 members and by a majority of

only 12 at midnight on the second day was Leslie acquitted of heresy (May 1805).

In 1809 Leslie published his "Elements of Geometry" which saw four editions. In the following year he succeeded in freezing water by artificial evaporation. A quantity of water was placed alongside some sulphuric acid in the receiver of an air-pump from which the air was rapidly removed. This diminution of pressure caused a vigorous evaporation from the water, the vapour being taken up by the acid as fast as it was produced. Leslie's evident joy at seeing the crystals of ice forming in the water has been testified to by an eye-witness. Leslie is therefore the 'father' of refrigerators and all manner of appliances for producing low temperatures. Most of the pieces of apparatus used in these researches were made by Leslie's own hands, and are now carefully preserved in the University of Edinburgh.

In spite of having made these fundamental discoveries in the science of heat, Leslie believed to the last in the objective existence of cold, and, in expressed opposition to Wells, held that dew was the result of "cold pulsations from the azure sky".

In 1819, on the death of Playfair, Leslie was elected to the more congenial chair of natural philosophy.

Clerk Maxwell has told us that Leslie was the first to give the correct explanation of the rise of liquid in a narrow tube by capillary attraction.

Leslie, who had travelled widely and read as widely, was in 1820 elected a *correspondant* of the Paris Academy of Sciences. He was the author of a large number of essays on mathematical and physical subjects, one of the most valuable of which was his "History of Mathematical and Physical Science during the Eighteenth Century" in the seventh edition of the "Encyclopædia Britannica".

Probably Leslie's most celebrated student was Thomas Carlyle, whose ticket, duly signed for the class of mathematics, may still be seen in the Carlyle House, Cheyne Row. Leslie had a high opinion of Carlyle's mathematical powers, and he helped him to his first appointment (1814) as teacher of mathematics in Annan Academy.

In 1832, on the recommendation of Brougham, Leslie was created a knight of the Guelphic Order by King William IV in the illustrious company of William Herschel, Charles Bell and David Brewster.

As a bachelor of frugal habits, Sir John had an income much in excess of his needs so that he was enabled to purchase the estate of Coates near