

ANIMAL INTELLIGENCE: AN EXPERIMENTAL STUDY.¹

MANY are the writers on animal intelligence, but few have made comparative psychology a subject of scientific investigation by the methods of careful observation and of experiment under conditions allowing of some control. Right welcome, therefore, is Mr. Thorndike's experimental study, of which a brief preliminary notice appeared in NATURE a few weeks ago (vol. lvii. p. 372).

This careful research goes far to confirm the conclusion, to which the present writer has been led, that the method of animal intelligence is one of indiscriminating trial and error, of profiting by chance experiences, and one which depends on the establishment of direct associations—a conclusion which is in close accord with that reached by Prof. Wundt. Mr. Thorndike is, however, somewhat severe in his criticisms of previous writers in the same field, complains that they have made no observations of their own, and says that most of the books do not give us a psychology, but rather a eulogy of animals. "They have all been about animal intelligence, never about animal stupidity." One of the previous writers has, however, said: "And then, as Mr. P. G. Hamerton well remarks, we have to take into account the immensity of the ignorance of animals." Ignorance and stupidity are, of course, by no means synonymous. But it is the former rather than the latter that is so abundantly exemplified in animal life.

In many of his experiments Mr. Thorndike's method was as follows. Very hungry kittens were shut up in box-cages, 20 inches long by 15 broad and 12 high, and food was placed outside within the animals' sight. To get out the kitten had either to pull down wire loops placed in different positions in different cages, or turn a broad button, or press an ordinary thumb-latch, or push down a small platform, or simply pull a string stretched across the roof. These devices (each in its separate cage) were so arranged that on the fitting push or pull the door opened; and fish was the reward of success. In other cages two or three distinct actions on the part of the kitten were required before the door opened. In yet other experiments the kitten was released and fed directly she either licked herself or scratched herself. The object of the investigation was to watch and record the establishment of associations; and the results are plotted in curves, giving the time-intervals between imprisonment and escape in successive experiments.

The curves are far from smooth, as is indeed to be expected where the internal factors are necessarily somewhat inconstant, and where the difficulties to be overcome by the subjects are different in different cases; but they bear out the contention that the method of animal intelligence is to profit by chance experience, and is dependent on the gradual establishment of direct associations. I have endeavoured to extract from some of Mr. Thorndike's carefully plotted data a mean curve for the method of trial and error, and though it does not come out very well, it does serve to indicate that *gradual* sweep towards rapid and assured success, which would theoretically result on this method. In contradistinction to this the curve of rational procedure is quite different. I plotted some curves of this type a few months ago, after reading Dr. Lindley's dissertation on "A Study of Puzzles" (*Amer. Journ. of Psych.*, vol. viii. No. 4). They were for ordinary wire-puzzles, and show a *sudden* leap from failure to success when the trick of the puzzle was discovered and *understood*, and after that some slight improvement in rapidity of success as the manipulative details were mastered.

¹ "Animal Intelligence: an Experimental Study of the Associative Processes in Animals." By Edward L. Thorndike, A.M. (Monograph Supplement to the *Psychological Review*, June 1898.)

Passing reference may here be made to Dr. Lindley's interesting study above mentioned. He finds by observation that the method of the young child is largely that of the animal. Trial and error, chance success, and direct association are predominant. In older children, who are beginning to generalise the results of their experience, rational procedure based on a considered scheme or plan, makes itself more and more felt. Further observation on similar lines will serve to link such results as Mr. Thorndike's with the human psychology of the text-books.

To return to Mr. Thorndike's research. The conditions of his experiments were perhaps not the most conducive to the discovery of rationality in animals if it exist. The sturdy and unconvinced advocate of reasoning (properly so-called) in animals may say that to place a starving kitten in the cramped confinement of one of Mr. Thorndike's box-cages, would be more likely to make a cat swear than to lead it to act rationally. And he may further urge that where the string passes out of sight and the bolt is hidden from view, the opportunities of understanding the situation are excluded. All the kitten could think would be: here's something loose and unnecessary to the normal constitution of a box; I'll try that on chance. But although I do not deem Mr. Thorndike's method so conclusive for the anti-rationalist view as observation under more natural, and, I may add, more sympathetic conditions, yet the form of his curves affords no particle of evidence for reasoned behaviour.

We may pass over his experiments on dogs and chicks with the barest mention. They serve to support the same conclusions with some differences of detail.

When we come to his psychological explanation of the nature of the associations involved, I find much to agree with but somewhat to dissent from. Where he argues that animals form no free ideas, I am heartily with him. I have myself contended that they are incapable of analysing a situation. And if in interpreting the facts of observation one's language may seem to imply that the sight of an object and its taste are analysed out and then associated, this is due to the inevitable analytic form which the use of words entails. Animals, in my opinion, do not analyse in this way, and do not form "free" ideas. The utmost that we can allow is that certain elements in a complex situation may, under given circumstances, predominate in consciousness over others; and this, not through any process of abstraction, but from the interplay of the nature of the animal and the circumstances of the case.

But when Mr. Thorndike says that "the groundwork of animal association is not the association of ideas, but the association of idea with impulse," I for one, as at present advised, am not prepared to follow him. "Impulse," he defines as "the consciousness accompanying a muscular innervation apart from that feeling of the act which comes from seeing oneself move, from feeling one's body in a different position, &c." Now in the first place this involves the assumption that physiological innervation is accompanied by a specific form of consciousness here termed "impulse." The question is still *sub judice*. But there is, at any rate, much to be said in favour of the view that consciousness is directly stirred only by *afferent* nerve-currents, and that the innervation process is itself unconscious, though its effects are communicated to consciousness by an afferent back-stroke from the motor organs as they move. This alternative view should, I think, have been mentioned, at all events in criticising one who provisionally holds it. On this view the efferent impulse (apart from its effects) cannot be psychologically associated with anything, since it is physiological and unconscious. In the second place, to suppose that one who holds the impulse as such to be purely organic, holds also that "an animal whenever it thinks of an act can supply an 'impulse' to do the act,"

savours, to say the least of it, of improbability. In any case I do not recognise it as my own view. I hold as strongly as Mr. Thorndike that the efferent impulse (as an organic link) is a *sine quâ non* in every case of association in animal psychology, and that no animal can supply it "at will."

A very interesting series of experiments were made with a view to extracting an answer to the question, Do animals imitate? The question is not so easy to answer as it looks. No one with adequate experience can doubt that young birds and mammals perform actions which, from the observer's point of view, are imitative. The sight of an animal performing some simple action is the stimulus which prompts to the performance of a similar action. This I have termed "instinctive imitation." And this Mr. Thorndike would not deny to animals, though he would, I take it, deny (and not without psychological justification) its right to be spoken of as imitation, properly so-called. On this basis are founded the numerous cases of imitation by suggestion where the sight of an action performed is the stimulus to the performance of a similar action. A more complex case is that of the bird which, hearing certain sounds, is not only stimulated to make sounds itself (like a laughing jackass to which one whistles), but gradually to make its own sounds resemble those which afford the stimuli (like the parrot which "draws a cork"). Here it seems that the resemblance itself gives satisfaction—in any case the factor of experiential selection is introduced. In these cases imitation by suggestion is supplemented by a tendency to more exactly reproduce the sound which affords the stimulus—a tendency which seems to be based upon the innate satisfaction which accompanies the act of reproduction. Thus far, in my opinion, animals can certainly go; but even this, it may be urged, is only pseudo-imitation. True imitation is seen only where a being of set purpose copies a given model, not only reproducing, but intending to reproduce. And it is the presence of true imitation of this type which Mr. Thorndike's experiments were designed to test. They afford, however, no evidence of it. Cats were allowed to see others do the trick of the box-cage. But they themselves, when placed in the cage, took the usual time to effect their escape. Their exit was no quicker from seeing others get out by the performance of certain clawings or pushings. The experiments do not carry complete conviction to my mind, though I regard the conclusion to which they lead as probably correct.

Mr. Thorndike thinks it likely that the primates stand at a higher level in this respect than dogs or cats. "If it is true," he says, "that the primates do imitate acts of such novelty and complexity that only this out-and-out kind of imitation can explain the fact, we have located one great advance in mental development. Till the primates we get practically nothing but instincts and individual acquirement through impulsive trial and error. Among the primates we get also acquisition by imitation, one form of the increase of mental equipment by tradition." My own observations on imitation in monkeys are too few and inconclusive to justify more than a very guarded expression of opinion. I lean to the view, however, that there is, even in them, little evidence of true imitation of the higher psychological type; and that the observed facts may be accounted for by a great extension of "instinctive imitation" suggestion, and behaviour directly founded thereon. I hope Mr. Thorndike will put the matter to the test of well-devised experiment.

Several interesting problems connected with the psychological interpretation of animal behaviour are briefly discussed, but can only be mentioned here. Mr. Thorndike accepts the conclusion that in animals "memory" is simply what has been termed "reinstating," and involves no true localisation in time or space. "The

animal's self is not a being looking 'before and after.'" "Memory in animals, if one still chooses to use the word, is permanence of associations, not the presence of an idea of an experience attributed to the past." This is precisely the conclusion to which the present writer has been led. On the question whether animals are aware of the pleasure or pain that others are feeling, he says that the conduct of animals "would seem to show that they do not. For it has given us good reason to suppose that they do not possess any stock of isolated ideas, much less any abstracted, inferred or transferred ideas. These ideas of others' feelings imply a power to transfer states felt in oneself to another, and realise them as there." As thus stated I think his conclusion is correct, though he quotes me in an opposite sense. In my later discussion ("Introduction to Comparative Psychology," p. 320) I expressly exclude any such ejective transference.

In conclusion, some apology is perhaps demanded for reference to my own observations and conclusions in the same field of study. But it is well to preserve historical continuity in a topic, and it so happens that Mr. Thorndike's work has carried further and extended some of my own; and that his leading conclusions are in the main confirmatory of those which I have reached. In the general trend of our opinions we are perhaps more essentially in accord than, in some cases, he seems to suppose. Even our illustrations are sometimes closely similar; both utilising, for example, the consciousness of a man when he is playing tennis as illustrating the probable subjective condition of the conscious but not yet self-conscious animal. And this substantial agreement is not a mere personal matter. Were it such there would be no justification for drawing attention to it. It shows that the method of observation and experiment, on different but parallel lines, has led two independent investigators to results which are on the whole harmonious; and it affords some ground for the hope that comparative psychology has passed from the anecdotal stage to the higher plane of verifiable observation, and that it is rising to the dignity of a science. In any case Mr. Thorndike's research is one of no little value, and will, I trust, be supplemented by further investigations.

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THE FLORA AND FAUNA OF BRITISH INDIA.

NO portion of the earth's surface surpasses the British Empire in India in the wealth and importance of its vegetable and animal life. Not only is there no other equally large tropical area that has received the same amount of exploration from naturalists, but the territories and dependencies of British India comprise regions with a marvellous variety of climates, from tropical islands like the Andamans and hot plains like the Carnatic, to the snows of the Himalayas and the frigid plateaus of Tibet; whilst the rainfall varies from the "record" 600 inches or more on the Khasi hills to the meagre supply that occasionally damps the arid sands of the Sind desert, where, frequently, for years in succession, rain is unknown. The remarkable antiquity of the Indian peninsula, the greater part of which appears to have been land from the earliest geological times, adds greatly to the scientific importance of the fauna and flora.

Under these circumstances it is not surprising that the variety of plants and animals occurring in India should be very great. There is no other large tropical region with which comparison is possible, because, as already mentioned, there is none of which the natural productions are as well known. Europe (3,800,000 square miles) has more than twice the area of India (1,750,000 square miles), but it has a far poorer flora and fauna, only about 9500 flowering plants being known to occur against 14,500 Indian species; whilst British India and its dependencies contain more than twice as many