A REPLY TO "THE NATURE OF ANIMAL INTELLIGENCE AND THE METHODS OF INVESTIGATING IT."[1]

My first duty is to beg the reader's pardon for a certain personal tone in this discussion. As Professor Mills has mentioned Dr. Thorndike twenty-nine times in his article, this reply will of necessity contain the word 'I' oftener than one would wish.

There are two sorts of assertions in Professor Mills' article: first, a number of important objections to a certain method of studying animal psychology; second, a number of attacks on my 'Experimental Study of the Associative Processes in Animals."[2] The former I am glad to have the opportunity to discuss, because they should be of real interest to all comparative psychologists. The latter can be safely left to the judgment of anyone who has read the monograph itself, and will be taken up here only because that monograph has probably been seen by only a few of the many who have read the attack upon it.

Let us turn first to the important objections to my method of studying the formation of associations in animals. I say my method, because it seems likely to be thought of chiefly in connection with my experiments, though Lubbock used practically the same method with insects. It is, in fact, odd that Lubbock's recommendation as to insects was not sooner followed with mammals. He says, "In order to test their intelligence, it has always seemed to me that there was no better way than to ascertain some object which they would clearly desire, and then to interpose some obstacle which a little ingenuity would enable them to overcome" (Ants, Bees and Wasps, N. Y., 1896, p. 247). He used food as the 'object,' as I did, and interposed mechanical obstacles as I did.

Professor Mills' weightiest objection is that, when confined while hungry in such boxes and pens as I used, the dogs and cats were in a 'panic-stricken' condition and, therefore, temporarily lost their normal wits. Now, it is true that in many of the trials with cats and chicks, notably the first ten or twenty trials with each animal, there is often, as I fully noted, great violence and fury of activity. And this *might* be the result of mental panic, and so might be a sign of a loss of normal mentality. But the animals (the dogs and some of the cats) which did not display this excitement and fury did not display any variation in the results toward more intelligence. Nor did the animals

[1] By Professor Wesley Mills, pp. 262-274 of the May number of THE PSYCHOLOGICAL REVIEW.

[2] *Animal Intelligence*, Monograph Supplement, No. VIII., to THE PSYCHOLOGICAL REVIEW.

which showed certain results in the experiments of which confinement in small boxes was an essential feature show any variation from those results in the experiments (see pp. 87–91 and 96 of the monograph already cited) in which there was no excitement, no different activity from that shown all the time. In these experiments the cats were in the big cage which had been their home for weeks.

Furthermore, it seems unlikely that in the case of the animals which had already been the subjects of two or three experiments, and which had been in such boxes a hundred or more times, the violence and fury of activity could have been the result of fear or in any way a sign of its presence. For, as was stated in the monograph, such animals which have been made during a number of trials to crawl into these boxes which Professor Mills supposes were so disturbing to them, *habitually of their own accord went into them again and again.* Nor did they try to escape when I picked them up to drop them in. In the experiments in which I moved the animal's limbs, putting him through the movements, there was after from 0 to 12 trials no fear of my handling. (See p. 68 of the monograph.)

In short, all evidences of panic may be absent without any change in mental functioning, and the only cause of mental panic which would seem probable, namely, *fear*, was certainly not present in the greater number of the experiments. So I feel bound still to maintain the account given in the monograph, and attribute the animal's fury of activity not to mental panic, but to a useful instinctive reaction to confinement. It should be remembered that even in the midst of the utmost activity the cats would take instant advantage of any chance to escape which appealed to their instinctive equipment (*e. g.*, the widening of an orifice). It should further be remembered that the most violent animals did the most pseudo-intelligent acts. If any one of the eight or ten psychologists and biologists who saw the experiments in progress had seen signs of mental panic in the animals I should have inserted this discussion in the monograph. But I venture to think that if Professor Mills had repeated five or six of my experiments he would have discarded this mental panic objection.

The next important objection is that the surroundings were unnatural. I myself long since criticised my method on these grounds,[1] and I am and always have been ready to admit that an animal may be able to reason with certain data, to imitate certain acts, and yet be unable to reason with the data with which you confront him or imitate the act you present as a model. For that reason I chose varied acts,

[1] See *Science*, Vol. VIII., No. 198, p. 520.

very simple acts, trying each with different animals and making many of them approach very closely to acts common in animal life, and making others practically identical with acts which have been recorded as proofs of high mental ability in animals (vide the experiments with boxes C, D and G). We have seen that so far as the mere being in boxes is concerned the animals soon got used to it, did not fear it, and presumably could and did use their mental powers while in that situation. If Professor Mills had specified some particular situation as unnatural, and argued in concrete terms that its remoteness from the ordinary conditions of animal life made it unfit to call forth what mental functions the animal had, I should here either try to show that it was fit to call them forth or confess that from the animal's conduct in it no conclusion could be drawn save the one that the animal's mentality was such as was not aroused thereby. Even this one conclusion would be valuable. Even if we had to say, 'all that these experiments prove is that these circumstances will not cause the animal to manifest memory, imitation, etc.,' we should be saying a good deal, for the advocates of the reason theory have pretty uniformly given as evidence the reactions of animals to novel mechanical continuances.

Professor Mills does not argue in concrete terms, does not criticise concrete unfitness in the situations I devised for the animals. He simply names them unnatural. Moreover, it would seem that he makes this word face two ways. When talking of my experiments, he uses the word in the sense of novel, unfamiliar to the animal. When arguing that my conclusions are wrong, he uses the word in the sense of beyond the limits of their mental functions, abhorrent to their normal intellection. Of course, the former may be true and the latter false. The fact that cats are not ordinarily treated as mine were does not imply that my cats could not and did not come to be at home in the life I imposed on them to such an extent that they could use therein all the general intellectual functions they possessed. Professor Mills himself has based statements about the presence of certain mental functions on the conduct of a kitten in gaining a certain resting-place (in a bookcase, if I remember rightly), in spite of mechanical obstacles interposed. The situation here coped with is as 'unnatural' as that in a majority of my experiments.

The general argument of the monograph is used in all sorts of scientific work and is simple enough. It says: "If dogs and cats have such and such mental functions, they will do so and so in certain situations and will not do so and so; while, on the other hand, the absence of the function in question will lead to the presence of certain things and the absence of certain other things." To provide the 'certain situations' was the task my experiments undertook. It is mere rhetoric to damn the whole argument with a word, 'unnatural.' The thing to do is to show the error in the logic or the disturbing factor in each experiment, to repeat the experiment minus that factor, get opposite results, and so refute my claims. Dr. Kline has in one slight case gained results by the use of more 'natural' surroundings and his results agree with mine. (See *Am. J. of Psy.*, Vol. X, pp. 277–8.) I may say here that Dr. Kline has in this article treated of fear and novel surroundings as disturbing features in my experiments more discriminatingly, perhaps, than Professor Mills, and that this paper is intended to be an explanation which will satisfy his criticisms as well as those of the latter.

Observational records are, as I said in the review in *Science* which has already been quoted, of very great value; but the fact remains that the host of observations so far collected, including the large number of Professor Mills' own to which he refers on page 264, had not provided us with agreement about the presence of a single general function in animal consciousness that was in dispute. I tried, therefore, to devise situations in which the conduct of the animals might be really illuminating. It would seem that Professor Mills allows that if the experiments were only free from the disturbing factors we have been talking about, the conclusions reached would be probably true, for he does not criticise the logic of the deductions. Now these conclusions are so far reaching that I am reviled for even pretending to have made such important ones. But this goes to show just that the method will, if we can show that these factors are not present, or can modify the method so as to exclude them, get us somewhere psychologically. So my general plea for experiments in animal psychology is that they at least pretend to give us an explanatory psychology, and not fragments of natural history.

Finally, just as in experiments like mine you may miss the truth by some mistake you make in picking the circumstances, the situation to test the presence of a function, so in the mere observation of the habitual life of animals or the experimental regulation of their ordinary activities, you may miss the truth by mistaking instinctive for imitative acts, associative for rational acts, permanent associations for memories. For instance, Professor Mills offers in his article, as a proof of the presence of an imitative faculty, an act (p. 268) which might very possibly have been the result of the instinct to follow common to so many young animals, so far as one can judge from his account—

"a student of McGill University has communicated to me the fact that a kitten which could not be induced to jump over an object placed before it, did so only after seeing the mother do it, and after that there was no more trouble in getting it to perform the trick." We shall see that another observation, that of the dog and the tree, which Professor Mills quotes to refute me, may have suffered in the interpretation.

Of course, it is clear that the psychological story told by correct experimentation will not conflict with the story told by correct observations reported correctly at first, second or tenth hand. But I am not yet sure that any trustworthy observation about the interpretation of which there is general agreement, conflicts with the results of my observations under test conditions in such a way as to render necessary the presupposition that in them there was some vital flaw. Such refutation of them may come, but Professor Mills does not seem to have brought it.

So much in general defence of the methods I used. It may now be permitted to mention some matters of detail: Professor Mills finds in the printed report of my experiments signs of conceit and of lack of 'respect for workers of the past of any complexion.' For psychological interpretations of the sort given by Romanes and Lindsay I certainly had and have no respect, though, of course, I esteem them for their zeal. But I cannot see that the presence or absence of megalomania in me is of any interest to comparative psychology. The monograph in question was not a presentation of personal opinion, but of certain facts, the accuracy of which, and of certain impersonal inductions and deductions, the logic of which, should be attacked impersonally. The question is whether certain facts exist and what they mean, and does not concern the individual psychology of any person.

Professor Mills' humor in making believe that because I characterize Lloyd Morgan as the 'sanest' of comparative psychologists, I think of them all as insane (p. 263), seems a bit disingenuous in view of the fact that his article will probably be the sole source of information about my book to a large number of people. Of course, when I wrote 'sanest,' I meant sanest. Had I meant 'least insane' I should assuredly have so written. On page 264 our author says, 'He' (Dr. Thorndike) 'comes very near to the belief that they are automata pure and simple, though this he does not assert in so many words.' This, I may be permitted to say, is an absolute misrepresentation. In every associative process discussed in the book I find present as an important element, *impulses*, and impulse I expressly define as 'the consciousness accompanying,' etc. (p. 14). Again, I speak everywhere of the *pleasure* resulting from the attainment of freedom, food, etc., as stamping in the connection between sense-impression and impulse. So, also, I speak everywhere of the sense-impression as the starting-point of the mental association. As a fact, *mental* processes are mentioned throughout the whole discussion. The one place where I frankly offered opinion in addition to fact was where I also attributed *representations* to animals: 'my opinion would be that animals *do* have representations, and that such are the beginning of the rich life of ideas in man' (p. 77). Again, after an attempt to 'describe graphically * * * the *mental* fact we have been studying,' I say (p. 89): "Yet there is consciousness enough at the time, keen consciousness of the sense-impressions, impulses, feelings of one's bodily acts. So with the animals. There is consciousness enough, but of this kind."

On page 264 Professor Mills talks as if I were trying to answer the question as to whether the animal mind was comparable to the human mind, and to answer it in the negative for the sake of exalting the human mind above the realm of natural evolution. The reader of the monograph will remember that one of the results of the study was the attainment of a possible mental evolution of an entirely natural sort. I never tried to answer the question, 'How far does the mentality of a dog or cat equal that of man in general, genus homo,' for such a question seems to me fruitless. It is like asking how far is 2 like x. The mentality of man *in general* is an unknown quantity, has a lot of possible values and so cannot be well used as a measure of anything. Any answer to it will be partially false and partially meaningless. Whether cats infer and compare, whether they imitate as present day adult human beings known to psychologists do, whether they form associations minus impulses of their own, are clear, answerable questions. Such I tried to answer. To say or to prove that the human mind of Europeans of to-day comes by continuous evolution from the animal mind does not make the latter any higher, endow it with a single new function nor alter it one whit. The protozoa are not at all different from what they were before after we call them the ancestors of the vertebrates. And one is free, it seems to me, to find out about questions of descriptive psychology, as well as of morphology, without meddling with questions of classification.

On page 265 Professor Mills rebukes me for considering hunger the strongest stimulus to animals. Of course, I did not so consider it, and I am not aware of anything in the monograph which even looks as if I did.

Again, on this same page he misrepresents me by quoting a sentence without its context and, indeed, with comments which positively give a wrong notion of the context. The sentence is: 'the question of whether an animal does or does not form a certain association requires for an answer no higher qualification than a pair of eyes.' This sentence, as anyone may see by reading pages 5, 6 and 7 of the monograph, refers to the particular associations involved in learning to escape from boxes. And whether an animal does or does not learn to escape from a box certainly can be observed by anyone with a pair of eyes. And as the text clearly states, it was just because I did not wish to impose on any one my own opinions or even observations, because I wanted to use a method which any one else could employ and gain results which any one else could verify or refute, that I planned experiments which depended, so to speak, on impersonal eyes, eyes in general, for many of their results. I unhesitatingly affirm that so far as the facts of escape or non-escape and the time records (and the sentence concerns nothing else), Professor Mills or any one else would have kept just the same records as I myself did—that his eyes would have seen no more nor less than mine.

On page 267 I am accused of sacrificing particulars about facts for the sake of rhetoric, again on the basis of an entirely misrepresented quotation. On pages 38 and 39 of the monograph I say that henceforth I shall frequently use the word 'animal' or 'animals' when I mean to make statements only about the particular score of animals which were the subjects of my experiments, as "really I claim for my animal psychology only that it is the psychology of just these particular animals." After giving one reason for this verbal usage I add, "my second reason is that I hate to burden the reader with the disgusting rhetoric which would result if I had to insert particularizations and reservations at every step." Professor Mills quotes, omitting the first five words, and giving the impression that I generally omitted details so as to have good paragraphs or something of that sort, whereas the only 'particularizations' to which I objected were such as saying, Cats 1 (8–10 months), 2 (5–7 months), 3 (5–11 months) etc., up to cat 13; Dogs etc., etc., did not do so and so every page or two, when by means of this little note upon verbal usage the reader could on each occasion interpret the word 'animals' to mean "the particular animals which he observed, not necessarily all animals." The rhetorical excellence thus gained requires absolutely no sacrifice of fact of any sort.

If I were sure that Professor Mills would enjoy a bit of jocularity, I should reply to his explanation of the failure of my animals to imitate, by his own failure to imitate Professors James, Ladd, Hall and

Cattell, by saying that it was a good explanation, that they, like him, did not imitate because they could not. His whole discussion of my views on imitation should, in fairness, be accepted only after a careful reading of what the monograph said on that subject. There is room in this reply for only one more comment, on another matter.

To prove that dogs have memory in the sense of the ability to "refer the present situation to a situation of the past and realize that it is the same" (the meaning taken in the monograph), Professor Mills tells us of a dog which stopped at a certain tree, up which he had, months ago, chased a cat, "looked up and behaved otherwise in such a manner as left no doubt in my mind that he remembered the identical tree and detail of the whole performance." I suppose this description of the effect on Professor Mills, beginning with the words 'behaved otherwise,' means that the dog barked at or jumped at the tree, or behaved as he would if the cat were there. It must be confessed that to a hardened disbeliever the argument, "the dog remembered because he behaved so that I know he remembered," seems hardly scientific; but supposing that the description means what we have suggested, it still does not prove that the dog felt a memory of previous incident. At the table this morning I took hold of a cup, raised it to my lips and drank, acted toward the cup just as I did a month ago, but I had absolutely no memory in connection with the act. Indeed, if the dog really remembered the previous chase, he would have good reasons *not* to stop at the tree and act as if a cat were there. Let us suppose that Professor Mills and his dog were both out for cats; that they chased a cat to a tree; that the dog barked, etc., at the foot; and that Professor Mills, running up, shot his gun at the cat. Next month they come along toward the tree. Now, suppose that Professor Mills should run up and shoot his gun as he did the other time. Would we think he remembered his chase of a month before? No! we would think that he had gone daft, or had *forgotten* that the cat was there a month ago. Such an act would be the natural result of a permanent association between the sight of that tree and certain impulses, or of an ill-defined representation; but it would be one of the last things to expect as a result of a memory of the previous occasion.

This reply should close with an apology. Discussions of method and argument over results are likely to be less profitable and much less interesting than new constructive work. This reply was, however, necessary because of Professor Mills' eminence as an observer of animals, and because of the importance of getting at the truth about the possible disturbing influence of fear and novel surroundings in certain convenient and, if legitimate, illuminating experiments.

[Note.—On page 268 Professor Mills has put 'to the laws of nature' instead of 'to the laws of its nature,' which means something rather different.]

Edward Thorndike.

Western Reserve University,
Cleveland, Ohio.

NOTES ON AFTER-IMAGES.

Location of After-image.

The following *Experiment 1* was made while I studied at Princeton, January 26, 1895. With an ordinary students' stand-lamp, I closed the left eye, shaded it with the hand, and gazed steadily at the flame until an exceedingly strong image was secured. Then, closing this eye and likewise covering it with the hand, I secured a strong image with the left eye.

Then, with a large piece of cardboard the eyes were shaded from the lamp-light and the after-image of the right eye was projected upon the wall, which was of a light shade. While this image was complementing from green to red, and at just the time the red was well produced, that eye was closed and the image of the left eye was thrown upon the wall, which image was found to be green at the instant that of the right was red. In like manner, when the image of the left eye was complemented into red, and the image of the right eye was at that time found to be green. Opening and closing the eyes alternately, it was found that each eye had its own independent after-image.

Experiment 2.—Proceeding as before in securing the after-images opposite in color for the eyes, the left eye was closed and the image of the right eye was projected on the wall. When this after-image had changed to red I projected the after-image of the left eye upon that of the right, that of the left at that instant being green. The combined image appeared green. Upon closing the left eye, or upon shifting its image to the left so as to make two separate images, it was found that the image of the right continued to be red while that of the left was green. The reverse was likewise accomplished. With sufficiently strong images this shifting of images into and away from each other proved an exceedingly interesting and beautiful process.