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The Delayed Reaction in Animals and Children

A DISSERTATION

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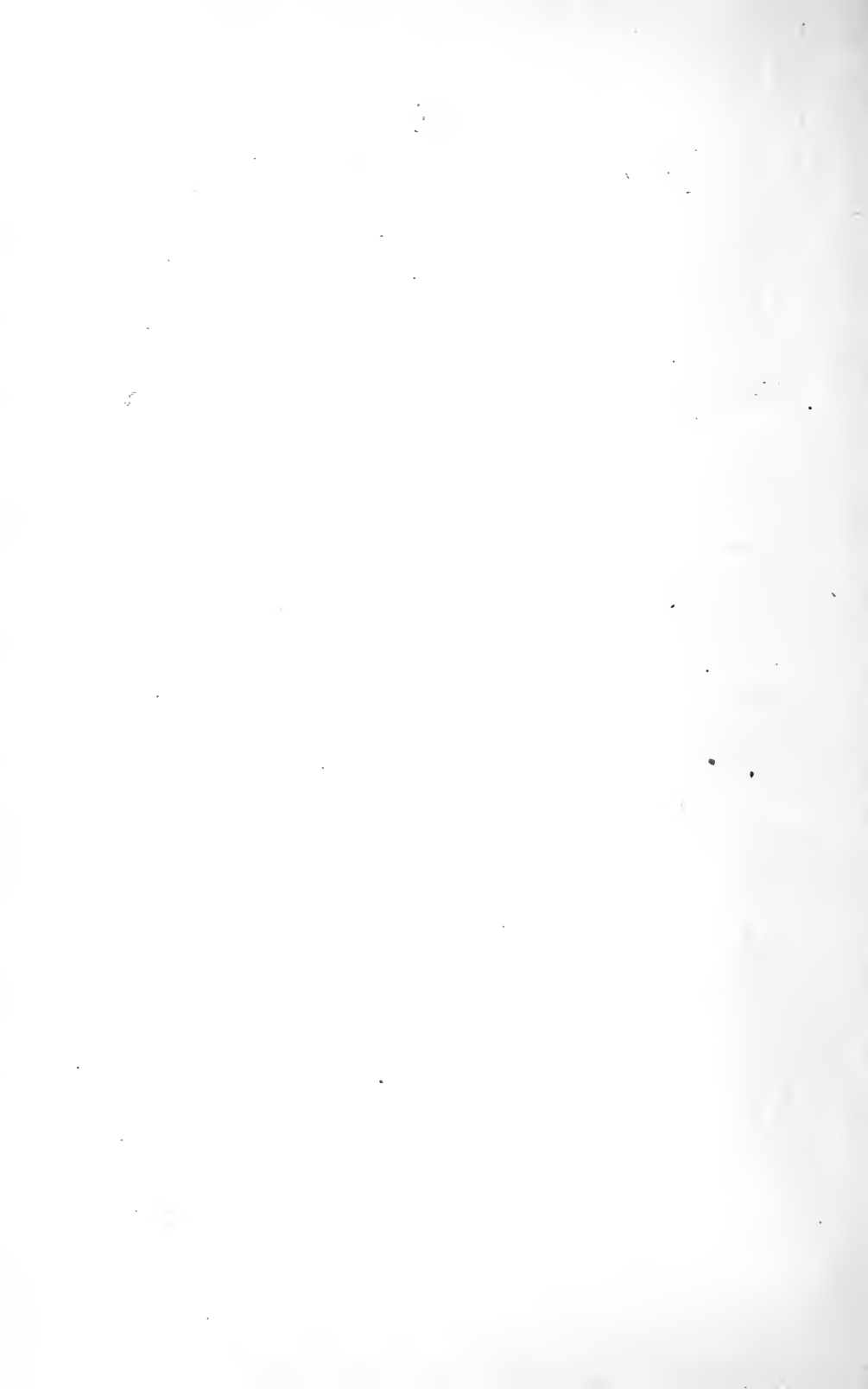
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## I. INTRODUCTION

The experiments in this monograph<sup>1</sup> aim at an analysis of typical mammalian behavior under conditions where the determining stimulus is absent at the moment of response. Associations were first set up between movements that led to food and a light which might be in any one of three boxes. Controls were used to make sure that the position of the light alone determined the reactions of the subject. Tests were then instituted in which the light was turned off before the reaction was made. The subject thus had to respond in the absence of the stimulus that hitherto had guided his reactions.

The nature of the present experiment may be further set forth by contrasting it with the following type of adjustment: A cat watches for a mouse and sees it appear at an open hole. The mouse vanishes before the cat can react, yet the cat goes over to the hole. There can be no question here but that the determining stimulus is absent at the moment of response, provided possible olfactory stimuli be neglected. Our experiment differs from this in complexity. If there were three holes that differed only in their several directions from the cat, and if in the past the mouse had appeared an equal number of times in all three holes, the conditions would be the same as in our tests. A selection between the three holes would need to be made on the basis of the immediately previous presence of the rat, if a correct reaction were to occur. If an animal *can* manifest behavior that does not lend itself to a "stimulus and response" explanation, this is one type of situation in which that behavior should appear. That, in fact, it is the situation *par excellence* for the eliciting of this behavior will, I believe, appear as this monograph progresses.

<sup>1</sup> Experimentation on the present problem was first begun in the University of Chicago laboratory by a graduate student, W. R. Hough. The following year the work was taken up and carried somewhat further by another student, H. B. Reed. Both students worked with white rats. Although in each case the results obtained were in strict harmony with those presented in this paper, in neither case were they conclusive. The chief value of the work lay in its suggestiveness. The apparatus used by Reed—Prob. Box D—is described below. The present investigation was carried on in the same laboratory from October, 1910 to April, 1912.

In the present experiments, two main factual questions arise: (1) How long after the determining stimulus has disappeared can an animal wait and still react correctly? (2) Does the animal give any behavior cues as to its method of solving the problem? If so, what are they? With these data given, there remains the task of interpretation. If a selective response has been initiated and controlled by a certain stimulus, and if the response can still be made successfully in the absence of that stimulus, then the subject must be using something that functions for the stimulus in initiating and guiding the correct response. Our investigation thus forces us to the consideration of the functional presence of a representative factor in the behavior of animals and children. Not only this, but the problem of the nature of this representative factor confronts us. Is it an overt motor attitude, or not? If not, is it sensory or imaginal, i.e., ideational?

In the interpretative study, I shall proceed on the assumption that animals are conscious. What the nature of this consciousness is, it will be the task of this paper to help determine. (If the reader does not choose to follow this line of interpretation, he may state everything in neurological terms without marring the significance of this discussion.) But *a propos* of the term "image" or "idea," let it be said once for all that wherever these terms are used by the present writer with reference to animal consciousness, they should be supplemented by the phrase "or functionally equivalent process." I use the structural term chiefly for the sake of its brevity.

## II. CRITICAL REVIEW OF HISTORICAL DATA

In the interpretative discussion at the close of the present monograph, we shall be confronted with the possibility that images or ideas<sup>2</sup> may have guided the reactions of the subjects. In that discussion, we shall assume that there is no necessity that psychology postulate such a representative factor save where successful reactions occur in the *absence* of the stim-

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<sup>2</sup> In the literature, it has been taken for granted that the question of the existence of images and that of the existence of ideas in animal consciousness is the same. I shall proceed on the assumption that images are centrally aroused processes, while ideas may be either peripherally or centrally conditioned. The essence of the idea is not its origin (or content), but its function. This point will be amplified in the final divisions of this monograph. The present statement will suffice for a definition of terms.



ulus (object) or movement that is represented. In the present historical section the attempt is made: (1) To present the main types of behavior that have been used as evidence of the existence of images in animals; and (2) to show that this evidence is inconclusive, because the behavior may be stated in sensori-motor terms. In the light of the assumption just made, there are two reasons why this behavior may be so interpreted: (1) The stimuli determining the reactions in question have not been adequately known; and (2) inasmuch as this is true, one has no right to assume the absence of the stimuli which are supposed to be represented. All of the behavior that is summarized in this section *may* have involved representative actors. The point of my criticism will be that they *need* not have involved such processes. It is not merely a question of the application of the law of parsimony, as that is usually stated. It is also a recognition of the fact that sensori-motor behavior is genetically the more fundamental form.

The following are the types of evidence to be considered: (1) Imitation; (2) use of tools; (3) dreams; (4) nature of the learning curve; (5) memory; (6) Thorndike's test: The immediate reaction to one stimulus before the appearance of a second which has always accompanied the first after an interval of a few seconds; (7) recognition; (8) learning by being "put through;" (9) rate of forgetting; (10) association by similarity; (11) reluctancy and expectancy of response; (12) varying means to the same end; (13) reactions to a temporal series of colors, and (14) Washburn's cat on the stairway.

#### 1. IMITATION

Thorndike<sup>3</sup> is an example of the comparative psychologist who interprets the highest type of imitation as requiring the presence of images or ideas. In his experiments, however, he found no evidence for the existence of this kind of behavior. The general character of the imitation experiment is well known. An animal that has failed to solve a certain problem is confined where it can see another work the mechanism and get food. It is then given an opportunity to make the desired reaction. If the trial succeeds or if there is a sudden improvement in the animal's behavior, it is said to have profited by

<sup>3</sup> Thorndike, E. L. *Animal Intelligence*. 1911, New York, pp. 76-108.

viewing the other's performance. Haggerty<sup>4</sup> found this behavior in well controlled tests with monkeys. He divides the stimulus for that imitative behavior into three parts: (1) A fairly simple mechanism; (2) the perception of another animal working at the mechanism, and (3) the perception of the other animal getting food. Haggerty does not discuss the theoretical side of imitation and argue for the existence of images. He simply presents the data. However, in view of the stand taken by Thorndike and others on interpretation, we shall nevertheless use material drawn from Haggerty's monograph for analysis.

The following criticisms tell against the use of this type of behavior as crucial evidence for images: The animal that does the imitating may make its improvement under the incentive of a social impulse rather than of the apprehension of relations. This receives confirmation by the fact that Haggerty's monkeys did better if the animal to be imitated was a stranger and thus aroused the imitator more strongly than a familiar animal would have done.<sup>5</sup> This criticism is a variant on the following one. (2) The fact that the sight of the other animal *being fed* was a part of the stimulus makes it at least possible that the imitator's attention was simply drawn very vividly to the spot to be attacked. Of course the more complicated the reaction to be made and the more exact the execution of this by the imitator, the better the argument for an ideational perception of relations. However, the general objection will always be valid that one can never say that the imitating animal was not guided solely by stimuli present to sense. The determining factor in the stimulus can not be said to be ideational. Once the animal's attention is vividly focused upon the objective sequence "pulling string—getting food," e.g., the necessary reaction may follow by association when the opportunity presents itself. Monkeys in particular have such an enormous repertoire of reactions that it is very tempting to analyze such imitative reactions as Haggerty presents on a purely "stimulus and response" basis. That author says of one of his typical experiments (Chute Experiment A): "In order to secure the food, the monkey must leap from the wire part of the cage to the chute, and, while

<sup>4</sup> Haggerty, M. E. Imitation in Monkeys. *Jour. of Comp. Neur. and Psy.*, 1909, vol. 19.

<sup>5</sup> Op. cit., p. 436.

holding to it, must thrust a hand up inside and pull the string, thereby releasing the small door in the top of the cage and allowing food which had been placed on it to fall to the floor. He must then descend to the floor to get the food." <sup>6</sup> *After the imitator's attention is focused upon the hole at the chute's end* due to the action of the imitatee, what is more natural than that it should jump to the chute? The hole has "caught its eye" before the jump, so the hand goes in almost reflexly, as reflexly closes about the string and pulls. Food falls and is eaten. If the animal is attentive to the movements during the performance, the chances are that it will henceforth succeed.<sup>7</sup>

## 2. USE OF TOOLS

A second argument for the presence of ideas is the use of tools by animals. In view of the fact that such behavior has at times been exclusively claimed for man, students of animal behavior have long sought for conclusive evidence on this question.<sup>8</sup> We shall take typical data presented by Hobhouse for consideration. This will also lead to an understanding of that author's attitude on the question of images. Hobhouse treats such behavior largely under the heading "Articulate Ideas." An example will best lead us to an understanding of his position. The animals used were monkeys, one a Rhesus and the other a chimpanzee. The latter had already learned to throw a rug over food placed at a distance in order to rake in the latter. He was then taught to substitute a stick for the rug. Quotations from the author will now indicate the animal's progress. "Next day, the chimpanzee learnt to use a short stick in order to reach a larger one, with which in turn he could reach the banana." <sup>9</sup> "One day I gave him a rope with a noose to throw over the box in place of his stick." (The banana was placed in a cigar box.) "I did not give him any hint, but he soon tried it in a vague way. He did not, however, understand the matter very well, for when he succeeded in getting the

<sup>6</sup> Op. cit., p. 355.

<sup>7</sup> Vide Hobhouse, L. T. *Mind in Evolution*. London, 1901, p. 202 for place of accident in learning.

<sup>8</sup> Lindsay, W. Lauder. *Mind in the Lower Animals in Health and Disease*. London, 1879, vol. I, chaps. 23 and 24.

Hobhouse, L. T. Op. cit., chap. 10.

Also many recent studies in imitation.

<sup>9</sup> Op. cit., p. 236.

rope round the box, he did not seem aware of his advantage, flung it away, went off for his shawl, and used it very successfully. I then tied a block of wood to the rope to assist in throwing it. He attempted this spontaneously, at first without success. Presently, however, he happened to pitch the block right into the box, which today was open, pulled it in, and got the banana. Notwithstanding this signal success, he never took to this trick."<sup>10</sup> "In the experiment to which I have already referred, when the box was tied to a rope, the further end of which was passed over a stanchion several feet from the cage, he failed, as I shall mention later, to find the right method, but was fertile in devising wrong ones. He would shake the rope violently, so that the banana would fall out of the box. He would then swing the rope to and fro, swishing the banana about from side to side, until by degrees it would come within his reach, in a way which I should have thought beforehand to be quite impossible."<sup>11</sup> "In these tests," says Hobhouse, "it was necessary that [the monkeys] should grasp how the stick and the food stood in relation to them; that they should get the stick at the food and beyond it."<sup>12</sup> "A form of 'analogical extension' is also strongly marked in the use of substitutes differing very widely in appearance and the manner of use from the object first employed."<sup>13</sup> These illustrations of the use of tools are examples and proofs of the existence in monkeys of what Hobhouse means by the *practical judgment*, where articulate ideas are employed.<sup>14</sup> The author explains articulate ideas as follows: "By a more articulate idea, is meant one in which comparatively distinct elements are held in a comparatively distinct relation."<sup>15</sup> The nature of the practical judgment is set forth as follows: "It is more than assimilation, because what is revived is an idea, a definite reference to something unperceived. It is more than association, because relation between the 'revived' idea and the given perception is an essential part of it, and it is less than analytic thought, because the relations involved are not dissected out as distinct elements in

<sup>10</sup> Op. cit., pp. 237-8.

<sup>11</sup> Op. cit., p. 238.

<sup>12</sup> Op. cit., p. 241.

<sup>13</sup> Op. cit., pp. 241-2.

<sup>14</sup> Op. cit., p. 269.

<sup>15</sup> Op. cit., p. 234.

consciousness.”<sup>16</sup> “The practical judgment is not independent of associations, for association supplies the whole of its material. But out of that material, it selects what it wants, and shapes it as required.”<sup>17</sup> This “material” that association presents is ideational as the quotation from page 117 indicates. It seems, though I would not be sure, as if Hobhouse assumed the existence of ideas and is only concerned with the possibility of their functioning in behavior. As a basis for this imputation, I point to his discussion of the association of ideas on page 114 and to that on pages 200-1. In the latter place, he makes clear (?) that what he does not wish to attribute to animals is the conscious analysis of the perceptual order. Such a distinction between existence and efficient functioning in behavior seems to be theoretically permissible.<sup>18</sup> Images, ideas *may* exist sporadically as has been claimed in the case of animal dreams. But comparative psychologists have neither the right nor the need to assume the existence of such processes save as that may be forced upon them by the evidences of behavior.

The question now is: Does the *use* of tools<sup>19</sup> such as described imply the presence of a centrally aroused factor? In the light of the above analysis of imitation, the reply is no. Even if Hobhouse's results be fully accepted without raising the question of careful controls, one need not accept his interpretations. The behavior may have been controlled entirely by sensory factors. As to the fertility shown by the animals in devising methods, (see above, p. 6), this was very probably but a random use of acquired co-ordinations. The use of the tools was acquired as any habit and the sight of the individual objects (ropes, etc.) aroused the *type of reaction* that had been taught. This shows a higher grade of intelligent adaptability in the animal than if it had been limited to the use of one object, but it does not prove the existence of a central conscious factor. It may be that such animals as the primates are able to give similar responses to different sensory stimuli on account of a factor of “hyper-excitability.” I hesitate to use such a term, but the general type of case held in mind is illustrated by the

<sup>16</sup> Op. cit., p. 117.

<sup>17</sup> Op. cit., p. 264.

<sup>18</sup> We shall see below (p. 9) that Morgan makes the same assumption.

<sup>19</sup> Hobhouse cites (op. cit., p. 258) only one well authenticated case of the making of a tool by an animal. The tool in this case was, as he notes, a very simple one.

animal that is supposed to react to a certain stimulus, but, being on the *qui vive*, reacts to anything that occurs at the proper moment. This behavior is familiar in human reaction experiments. If, now, we subtract the possible emotional disturbances (and I doubt whether that even is necessary), we have the type of case, I believe, that the experiments of Hobhouse present.

### 3. DREAMS

A third argument used for ideas is the supposed fact of an mal dreams. The usual criticism of this—to which I subscribe—is that the law of parsimony forces one to recognise that the interpretation of the facts may as well or better be physiological than ideational.

### 4. LEARNING CURVE

A fourth argument used in the discussion as to the presence of ideas is the nature of the learning curve. In 1898, Thorndike<sup>20</sup> presented data for cats which when plotted gave what the author termed a "gradual slope." "The gradual slope of the time curve,—shows the absence of reasoning." It represents "the wearing smooth of a path in the brain, not the decisions of a rational consciousness." There seems to be no doubt but that Thorndike meant that had *ideas* been present to guide the reactions that the latter would have succeeded within a few trials. Hobhouse would seem to agree with Thorndike that such a curve as the latter claimed to present was evidence of the absence of imagery. His criticism of Thorndike is to the effect that the former's curves are *not* gradual—"unless the slope of a church steeple is gradual."

The (apparently) common assumption of these writers has been questioned effectively both by Watson<sup>21</sup> and by Hicks and Carr.<sup>22</sup> The criticism of the latter authors is factual and is summed up as follows: "Our results indicate that any inference from such a general characteristic of a curve is not feasible, because we are dealing with a complex phenomenon due to several independently variable factors. Our results indicate that

<sup>20</sup> Thorndike, E. L. Animal Intelligence. *Psy. Rev. Mon. Supp.*, 1898, vol. 2, p. 45.

<sup>21</sup> Watson, J. B. Kinaesthetic and Organic Sensations. *Psy. Rev. Mon. Supp.*, 1907, vol. 8, pp. 23-4.

<sup>22</sup> Hicks, V. C. and Carr, H. A. Human Reactions in a Maze. *Jour. of Animal Behavior*, 1912, vol. 2, pp. 116-118.

the rational status of a group of animals can not be inferred from the slope of a curve in so far as this slope is dependent upon the number of trials or the relative rate of elimination. They indicate, moreover, that inferences as to intelligent status are legitimate in so far as the slope is determined by the factor of total values eliminated, but that the relation between the abruptness of slope and the degree of rational ability is just the inverse of that assumed by Thorndike and Hobhouse."

# 5. MEMORY

Arguments for the existence of ideas, have also been drawn from behavior purporting to be guided by memory—in the psychological sense. Let us use an example from Lloyd Morgan. The quotation of a few sentences will adequately represent his position when the Introduction to Comparative Psychology was written. "In the first place we may notice that the existence of memory is implied in the association of ideas; or rather in the occurrence of ideas at all." "If, therefore, animals have ideas at all—and if they have not we need not attempt to carry any further our investigations into zoölogical psychology—they must have memory, and there must be in them, as in us, some anatomical and physiological basis for what is popularly termed the retention of ideas."<sup>23</sup> By idea Morgan understands any centrally aroused conscious process. Of course Thorndike's results showed long ago that the presence of ideas in an animal requires vigorous proof rather than mere assumption. Now for a concrete example: "When I was at the cape I used to take my two dogs up the Devil's Peak, an outlying point of Table Mountain. There were several places at which it was necessary that I should lift them from ledge to ledge since they could not scramble up by themselves. After the first ascent they always remembered these places and waited patiently to be lifted up. On one of our first ascents one of them put up a young coney and they both gave chase. Subsequently, they always hurried on to this spot, and though they never saw another coney there, reiterated disappointment did not efface the memory of that first chase, or so it seemed. I think the last time I took them up must have been about three and a half years after the coney

<sup>23</sup> Morgan, Lloyd. Intro. to Comp. Psych. London, 1898, p. 117. Thus Morgan, as we noted for Hobhouse, seems to assume that ideas exist whether they function in behavior or not.

hunt: so long had the memory endured and the association remained uneffaced." <sup>24</sup>

This is a fairly well known example of the type of proof used by the "anecdotal psychologists." To some it may seem too trivial either for serious analysis or notice. But such a judgment is ill informed. We shall find similar arguments as late as Cole's paper on the Intelligence of Raccoons. The obvious criticism of Morgan's illustrations is that they may be simply cases of sensory recognition of the commonest kind. A further word will be said in connection with the criticism of Cole's work.

#### 6. THORNDIKE'S TEST

A great many of the experiments which Thorndike presents in his recent book on *Animal Intelligence*<sup>25</sup> involve more or less intimately the question of the existence of images. However, I shall limit my analysis to the case in which the problem of the fact of images is most crucially attacked. The case I choose is the famous one reported in the first monograph of *Animal Intelligence*. I shall term it the "hand-clapping test" with cats. Thorndike's own words are such an excellent example of scientific description that I shall quote them at length:<sup>26</sup> "The only logical way to go at this question and settle it is, I think, to find some associations the formation of which requires the presence of images, of ideas. You have to give an animal a chance to associate sense-impression A with sense-impression B and then to associate B with some act C so that the presence of B in the mind will lead to the performance of C. Presumably the representation of B, if present, will lead to C just as the sense-impression B did. Now, if the chance to associate B with A has been improved, you ought, when the animal is confronted with the sense-impression A, to get a revival of B and so the act C. Such a result would, if all chance to associate C with A had been eliminated, demonstrate the presence of representations and their associations. I performed such an experiment in a form modified so as to make it practicable with my animals and resources. Unfortunately, this modification spoils the crucial nature of the experiment and robs it of much of its authority. The experiment was as follows:

<sup>24</sup> *Ibid*, p. 118.

<sup>25</sup> Thorndike, E. L. *Animal Intelligence*. New York, 1911.

<sup>26</sup> *Ibid*, pp. 110-112.



"A cat was in the big box where they were kept (see p. 90) very hungry. As I had been for a long time the source of all food, the cats had grown to watch me very carefully. I sat during the experiment, about eight feet from the box, and would at intervals of two minutes clap my hands four times and say, 'I must feed those cats.' Of course the cat would at first feel no impulse except perhaps to watch me more closely when this signal was given. After ten seconds had elapsed I would take a piece of fish, go up to the cage and hold it through the wire netting, three feet from the floor. The cat would then, of course, feel the impulse to climb up the front of the cage. In fact, experience had previously established the habit of climbing up whenever I moved toward the cage, so that in the experiment the cat did not ordinarily wait until I arrived there with the fish. In this experiment

A=The sense-impression of my movements and voice when giving the signal

B=The sense-impression of my movements in taking fish, rising, walking to box, etc.

C=The act of climbing up, with the impulse leading thereunto.

"The question was whether after a while A would remind the cat of B, and cause him to do C before he got the *sense-impression* of B, that is, before the ten seconds were up. If A leads to C through a memory of B, animals surely *can* have association of ideas proper, and probably often *do*. Now, as a fact, after from thirty to sixty trials, the cat does perform C immediately on being confronted by A or some seconds later, at all events before B is presented. And it is my present opinion that their action is to be explained by the presence, through association, of the idea B. But it is not impossible that A was associated *directly* with the impulse to C, although that impulse was removed from it by ten seconds of time. Such an association is, it seems to me, highly improbable, unless the neurosis of A, and with it the psychosis, continues until the impulse to C appears. But if it does so continue during the ten seconds, and thus get directly linked to C, we have exactly a representation, an image, a memory, in the mind for eight of those ten seconds. It does not help the deniers of images to substitute

an image of A for an image of B. Yet, unless they do this, they have to suppose that A comes and goes, and that after ten seconds C comes, and, passing over the intervening B blank, willfully chooses out A and associates itself with it. There are some other considerations regarding the behavior of the cats from the time the signal was given till they climbed up, which may be omitted in the hope that it will soon be possible to perform a decisive experiment. If an observer can make sure of the animal's attention to a sequence A-B, where B does not arouse any impulse to act, and then later get the animal to associate B with C, leaving A out this time, he may then, if A, when presented anew, arouses C, bid the deniers of representations to forever hold their peace."

First as to the data obtained, Thorndike's results indicate only the magnitude of the interval between two stimuli which association can bridge. Using his symbols, B and C have been associated before this experiment was begun. The hand clapping, A, now precedes B by ten seconds. At the end of from thirty to sixty trials, the cat climbs up at A rather than waiting until B appears. Now must we assume either that the "A" neurosis, and hence the "A" psychosis, persists or that A has resulted in the central arousal of B? Not at all. There is good evidence to show that association in animals can bridge an interval of ten seconds and more. Nearly all behavior experiments cover at least ten seconds from the beginning of the test to the acquisition of food. Yet it is necessary that the first and the last of the test be associated in order to provide a motive for the complete reaction. In no case—Thorndike's not excepted—is the ten seconds a sheer gap. (Thorndike did not describe the behavior of the cats during the interval, although he did refer to it.) The animal is reacting during the interval. Motor attitudes at least are present to fill the gap. An animal as high in the scale as a cat could certainly form this simple association between a sound and a single reaction within sixty trials. Moreover, it is to be remembered that only two cats succeeded within this time. Two others were tested for one hundred and thirty-five trials, but uniform reactions were not secured. The situation would have been quite different had there been two or more signals, "A's," and as many different

responses. But even under such conditions, one would still be studying the association of stimulus and response. This would remain true as long as it was the *feeding* and not the animal's *reactions* that were delayed. On the other hand, if both the feeding and the reactions were delayed after the stimulus had been given, then *if there were such states as ideas* one would expect them to function here. Furthermore the need for ideas would increase with the number of different stimuli and reactions.

Just a word now concerning Thorndike's formula for the study of imagery. The fact that he himself was unable to carry out experiments in conformity with it, and that none have been carried out since his attempt, does not speak very well for the formula. I must confess my own failure as yet to perfect a technique by which the formula might be applied to animals. In order to associate A and B, it will be necessary that they be followed by a reaction x. A, B and x are now associated. B and C may now be linked together through a second reaction y. Even granting the ideational character of reactions carried out according to the formula, one would have to know the following facts concerning the above test: (1) Did the animal discriminate between A and B, between B and C, and between A and C? If the first and second discriminations were not made, A and B or C would have been directly associated through x or y. If A and C were not discriminated, the association B-C would have been useless. Difficulties such as these lead me to believe that the goal aimed at is unattainable. In fact, Thorndike states that the formula is valid only when B arouses no impulse to activity; this is the essential weakness of the formula, for one can never be certain of the absence of these intervening mediating motor tendencies. In fact their presence is extremely probable. It cannot be too often reiterated that structural psychology has no place in the study of animal behavior. One must speak in terms of function. It is impossible to tell whether an *image* is present or not. The most that one can ever say is that some process other than overt motor activity is present which functions as an image might in human consciousness. This amounts to an acceptance of Hobbhouse's statement (although I do not feel that he always limits himself to this) that the ideas we deal with are

"practical ideas," understanding by this a *function* which does for animals that which practical ideas do for human behavior."<sup>27</sup>

#### 7. LEARNING BY BEING "PUT THROUGH"

Perhaps the most important and best known piece of work on the presence of imagery in animals is that by Cole on the Intelligence of Raccoons. Let us consider the evidence which Cole presents. The argument derived from an animal's learning a problem by being "put through" may be analyzed first. Cole writes in particular reference to Thorndike, saying "It would seem that nine-tenths of the experimental evidence for the absence of ideas in dogs and cats comes from their inability to learn from being put through."<sup>28</sup> Again, "If inability thus to learn is evidence against the presence of ideas, then ability to do so should be equally strong evidence for it."<sup>28</sup> In an earlier paper,<sup>29</sup> I have discussed some aspects of this problem in the light of experiments carried out upon the white rat. This phase of the question need not be gone into more fully here. Whether or not it seems probable, from a speculative point of view, that an animal must use "free impulses" or images in order to learn from being "put through," we need not consider. My contention in the paper mentioned is that the data so far at hand do not warrant conclusions as to the presence of imagery. Furthermore I indicated that the behavior could be explained better in other terms. Now with reference to that type of experiment in which the problem learned is that of working latches rather than climbing into boxes, I believe the data presented by Cole are conclusive, as far as the facts are concerned. Some raccoons at least appear to learn by being "put through." Whether all raccoons would do so is, of course, quite another matter. But given the fact, it does not follow that one must necessarily interpret it as an evidence of the presence of images. Cole seems to have carried over this interpretation rather uncritically from Thorndike. The entire process can be adequately stated in sensational terms. Certain

<sup>27</sup> Hobhouse, L. T. *Mind in Evolution*. New York, 1901, p. 283. That Hobhouse does not limit himself strictly to this may be seen by reading the first few sentences on p. 284.

<sup>28</sup> Cole, L. W. Concerning the Intelligence of Raccoons. *Jour. of Comp. Neur. and Psych.*, vol. 17, 1907, p. 249.

<sup>29</sup> Hunter, W. S. A Note on the Behavior of the White Rat. *Jour. of Animal Behavior*, vol. 2, 1912.

stimuli, x, y, and z, e.g., are made prominent by directing the animal's attention to them. These stimuli occur in connection with one another and with certain movements, kinaesthetically reported, and are followed by the acquisition of food. What could be more natural, then, than that the cognizance of the stimuli should set off the associated movements? The behavior noted by Cole bears out this contention. All four raccoons, both the pair put through and the pair not put through, solved the problem of escaping from the box (No. 4) by working the fastenings at one trial in one manner and at another in another fashion.<sup>30</sup> This need mean no more than that several responses, as opposed to a fixed series, might follow upon certain stimuli. This would be a higher type of behavior, to be sure, than where only one response was given, but it would not therefore involve a new type of conscious process. "If the act which he (the raccoon) is put through is the one which will remain the easiest and the most convenient for him throughout the tests, irrespective of his position in the box, he will never vary from it. If not, he will employ your act when his position makes it convenient and he is looking at the latch you began with."<sup>31</sup> We are not told whether the raccoon learned which was the easiest way by trial and error or not. But it is to be inferred from the behavior of raccoon No. 2 that such was the case.<sup>32</sup> The behavior thus described is interesting, but entirely inadequate as far as the presence of imagery is concerned. It may well be that "animals which, so far as we know at present, are utterly unable to learn save by innervating their own muscles" are devoid of ideas, without its following that if this type of learning is present, the animal possesses imagery. Hence assuming the facts that Thorndike and Cole present to be unquestionable, it need only follow that the raccoon exhibits more complex sensori-motor behavior than the dog and the cat, and not that it shows a new type of behavior, i.e., a type of behavior involving the functional presence of a representative factor.

Cole adduces further evidence for the presence of imagery.<sup>33</sup> These may be listed as he himself presented them: (a) Recognition of objects; (b) forgetting; (c) variability; (d) associa-

<sup>30</sup> Op. cit., p. 243.

<sup>31</sup> Op. cit., pp. 245-6.

<sup>32</sup> Op. cit., p. 246.

<sup>33</sup> Op. cit., p. 251, ff.

tion by similarity; (e) reluctance and expectancy; (f) varying means to the same end, and (g) reactions to colors presented in a temporal series. (c was treated under 7, above.) Most of these arguments can be dismissed summarily. It should never be forgotten that although almost any type of behavior *may* involve imagery, the comparative psychologist is seeking for behavior whose explanation *requires* the assumption of such a function, even under the law of parsimony. Is the determining sensory stimulus present or absent at the moment of response? If it is present, why *should* the animal use a representative factor? These are the questions that every investigation as to the presence of images in animal consciousness must face.

#### 8. RECOGNITION

It seems to me extremely obvious that the fact of the recognition of a food bottle need not be interpreted as presupposing imagery. In fact it is hard to understand how imagery would function in such a situation! Recognition of this type does not necessarily imply memory or the dating of an experience in one's past. On this basis all animals must be granted the possession of images.

#### 9. RATE OF FORGETTING

The fact that some of the raccoons forgot the solution of the boxes after an interval of three days does indeed indicate, as Cole claims, that automatisms had not been set up. But one must not infer therefore that images were involved. There is no factual support for the assumption that imaginal forgetting is more rapid than sensory. The same is true of the variable nature of the raccoons' behavior discussed above. This very probably indicates a high order of adjustive ability on the sensori-motor level, but not necessarily an "imaginative" adjustment. Mere variability of response is present in all animals. Do all animals, then, possess images?

#### 10. ASSOCIATION BY SIMILARITY

"Association by similarity," or the fact that a raccoon will attack a certain fastening even after its location in the box has been changed, when contrasted with the activity of cats<sup>34</sup>

<sup>34</sup> Cole, L. W. Op. cit., p. 253.

and rats<sup>35</sup> that attack the old position rather than the old fastening, proves for raccoons only the superior importance of "objects,"—or the form, size and quality aspects of the stimulus,—over kinaesthetic space controls, i.e., the position aspects of the stimulus. In addition, it should be noted that Davis in his study of raccoons<sup>36</sup> obtained data of the opposite nature. His animals would claw at the spot where the fastening had been. But aside from all this, I see no reason why "association by similarity" should not be purely perceptual and hence be simply a type of recognition. As a matter of fact, all animals have responses (instinctive reactions for example) that are applied to classes of objects. Someone also has well said that animals, in cases like the present ones, simply *fail to see the difference* between two objects and hence react as though the two were the same.

#### 11. RELUCTANCY AND EXPECTANCY

Is the "reluctancy" or the "expectancy" which appears to be manifested in an animal's behavior toward a difficult and an easy box respectively to be taken as evidence of the presence of imagery? Cole, e.g., says "no one who saw the animals resist being put into a box failed to credit them with a rather distinct memory of the difficulty of escape."<sup>36a</sup> By "distinct memory" Cole undoubtedly means an imaginal process. But *do* the facts prove this? Is the case not perfectly amenable to a "stimulus and response" explanation? The raccoon has associated a certain box with a certain displeasure until the presentation of the box arouses immediately the negative reaction. The raccoon *may* have had images of his previous experiences, but the facts do not prove it. One does not *need* images to explain this behavior any more than to explain a child's refusal to take a second dose of bitter medicine.

#### 12. VARYING MEANS TO THE SAME END

The data presented by Cole under the heading "varying means to the same end" are just as inconclusive as that presented above, although they are more suggestive. We have here the activity of four raccoons directed toward entering a

<sup>35</sup> Richardson, Florence. A Study of the Sensory Control in the Rat. *Psych. Rev. Mon. Supp.*, vol. 12, 1909, p. 38, e.g.

<sup>36</sup> Davis, H. B. The Raccoon: a Study in Animal Intelligence. *Amer. Jour. Psych.*, vol. 18, 1907, p. 470.

<sup>36a</sup> Op. cit., p. 253.

box containing an apple. The raccoons were accustomed to reaching through a hole in the top of the box in order to procure the fruit. When a block with a steeple in it was placed in the hole, one raccoon immediately clawed out the block and ate the apple. "She seemed to work as if actuated by a thought of the apple in the box. It was not done by random clawing, nor could she smell or otherwise perceive the piece of the apple in the box."<sup>37</sup> We are not informed *why* the animal could not smell the apple. The fact that the fruit odor was in the room will not suffice. But even assuming this to have been controlled, we need not attribute an image of the apple to the animal. Habit got the raccoon to the hole and started her paw, and the contact (?) of the staple initiated the claw reflex. This plus the pleasurable results associated with the box are sufficient to explain the activity. In a slightly different experiment, the animals crawled through a hole in the top of the box in order to procure the apple. I now quote Cole: "The box had no bottom and instead of resting directly on the floor it rested on a row of bricks. Removing one of these made an opening under the lower edge of the box through which the raccoon might crawl. The opening in the top was now closed and nailed fast. No. 1 was freed, went to the top of the box and tried to claw out the block. He then walked about the room, then tried the block again. He then went to the opening made by removing the brick, stopped a moment, then crawled in."<sup>38</sup> To argue that this means *image* of apple is certainly naïve, at least. Could the raccoon not sense the apple when its nose was within a foot (see description of box 18, op. cit., p. 215) of it? Again where the animal climbed up and over a roll of poultry wire in order to descend into the box, the possibility of the presence of imagery is only suggested, not proved. The opening into the box, as well as the odor of the food, was there impelling the raccoon to approach. What more natural, then, than that the animals should climb the wire and thus reach the food. Such behavior is what would be expected of raccoons that lived in a wire cage. The case of raccoon No. 4 is somewhat different. With the box which possessed two openings, he went directly into the lower of the two at the

<sup>37</sup> Op. cit., pp. 254-5.

<sup>38</sup> Op. cit., p. 255.



first trial. What influence his starting point in approaching the box had upon his success, we are not told.

Logically, the position taken by Cole in his illustrations would require him to argue that ideas are present wherever "motor excess" in learning occurs. There a sensorially reported situation calls out in succession the animal's repertoire of instinctive and habitual acts. This is a variation of means toward the attainment of an end, and is on a par with the "variability of response" argument discussed above.

### 13. REACTIONS TO A TEMPORAL SERIES OF COLORS

It is only fair to Cole to note that his main emphasis does not rest upon the above data, but upon a series of tests that he made with colors presented in a temporal series. Three colors, white, blue and red, were placed upon three levers which in turn were secured by a single pivot on the back side of a board one foot high. When the colors were presented in the order W, B, R, the animal was to secure food by climbing upon a box. When R, R, R was given, no reaction should be made. Now since the terminal stimulus was identical in each case, Cole argues, the only means by which the animals could react discriminately is by remembering what colors of the series had preceded. The fact that the raccoons clawed up the cards from behind the screen, reacting only when the proper one appeared, was also used as evidence of images. Believing that these tests were almost absolutely uncontrolled and that the interpretations were invalid as far as the data presented were concerned, I set two graduate students, F. M. Gregg and C. A. McPheeters at work upon this problem. Their purpose was: (1) to duplicate as nearly as possible Cole's results under adequately controlled conditions, and (2) having set up the discrimination, to determine and not to assume its basis. Their results will soon appear in the *Journal of Animal Behavior* under the title *Some Reactions of Raccoons to a Temporal Series of Stimuli*. I shall only note here that they found: (1) that discrimination was not based upon the cards—in fact the discrimination was not even visual, and (2) that practically the entire discrimination was made on the basis of the first lever and not on the basis of all levers as Cole assumed. (Since all levers were influential, in Cole's opinion, it had been necessary to

assume that the first two were represented imaginally when the last was presented.)

The criticisms on Cole's entire work as outlined above reduce to these: (1) The facts are either inconclusive or irrelevant. And (2), there is no evidence of adequate controls. On the positive side, the work suggests that the raccoon is more intelligent than the dog and cat, but it does not determine wherein this superiority lies.

#### 14. WASHBURN'S CAT ON THE STAIRWAY

There is one other type of behavior that deserves mention. Again, it was not and need not be interpreted as necessarily involving the presence of imagery. The illustration follows: "A cat, indeed, once observed by the writer, did behave as a human being would do to whom any idea had occurred, when, on coming into the house for the first time after she had moved her kittens from an upper story to the ground floor, she started upstairs to the old nest, stopped halfway up, turned and ran down to the new one. But errors of interpretation are possible at every turn of such observations."<sup>39</sup>

This is an excellent illustration of the type of argument that would use "hesitation" and "wavering" as an evidence for the presence of ideas. It is a mode of behavior that is found almost everywhere in animal studies. A rat, e.g., hesitates at a division point of the maze and finally selects the right pathway, or it runs *half* the length of a blind alley and then turns back. Was it guided by an ideational representation of the movements to be made and their consequences? Not necessarily. Accidental stimuli may have initiated the new reaction and any conflict present may have been resolved on a purely sensory-motor level. The experimental technique for the control of such reactions will be discussed below (see p. 74).

There is very little that needs to be said in the way of a summary of this historical review. All of the arguments for the presence of imagery in animals that we have examined have been found inconclusive. It is not that the various types of behavior may not have involved a representative factor. The point is that this possibility is nowhere proved necessary. The fault does not lie in the exhaustiveness of the data. The vari-

<sup>39</sup> Washburn, M. F. *The Animal Mind*. New York, 1909, p. 272.

ous methods have been made to yield ample returns for this purpose. The crux of the matter is that the methods themselves are inadequate for the solution of the problem. Let me re-emphasize the fact that if comparative psychology is to postulate a representative factor, it is necessary that the stimulus represented be absent at the moment of response. If it is not absent, the reaction may be stated in sensori-motor terms. But in order to know that the stimulus is absent, it is first necessary to determine carefully what the stimulus is. None of the methods reviewed, I believe, meet these requirements. Whether the tests presented in this monograph do or not, the reader himself may judge.

### III. NOTES ON THE ANIMALS AND CHILDREN TESTED

Four classes of re-agents were used in the experiments whose description is to follow: white rats, dogs, raccoons (*Procyon lotor*) and children. A few words descriptive of these subjects will not be amiss.

#### 1. RATS

Twenty-two rats were used during the entire course of the experiments. Five of these were normal adults and were used only in preliminary tests in which the purpose was the perfection of a method. The remaining seventeen (normal) were all started in the experiments when approximately four weeks old. All were vigorous, healthy animals whose records may stand as typical.

#### 2. DOGS

The two dogs tested were mongrels in whom the rat terrier strain was dominant. They were very bright and intelligent looking, very active, playful and affectionate,—indeed they seemed to possess all the qualities that are attributed to intelligent dogs in countless anecdotes. This was the unanimous testimony of many observers. The two dogs, Blackie and Brownie, both females of the same litter, were secured from an animal dealer when they were small puppies and were started on the preliminary tests at about the age of five months. They were usually kept in a kennel out of doors and remained in excellent condition during the experiments. Of the two, Brownie was the more aggressive and, to the ordinary observer, appeared possibly the more intelligent.

### 3. RACCOONS

Four raccoons, two males—Bob and Jack—and two females—Betty and Jill, were tested. Bob and Betty had been pets and were secured from their owner when about five or six months old. Jack and Jill were caught in the woods when about two and a half months old. Preliminary experiments were started almost immediately with all four. The raccoons were and remained in perfect health throughout the experiments. The only physical defects possessed by any of them were the cataracts which developed in Bob's eyes about a month after his purchase. Although this interfered with his accurate vision of objects and resulted in his colliding frequently with them, he was able to distinguish such changes in brightness as were necessary in the experiments. This fact was demonstrated conclusively by many control tests which will be described later. Of the four, Betty was the quietest and most timid. She was the least promising subject among them. Further facts about these animals are given in the appendix.

### 4. CHILDREN

Five children were used in the course of the present tests: two boys, Hd and L, and three girls, F, M, and H. H, Hd and L were each approximately six years old. M was about eight years old; and F, about two and a half. Hd and L were in kindergarten work, and M and H were in the graded schools. The indications were that they were children both of normal ability and of normal intellectual advancement for their ages. F was a bright little girl and made an excellent subject. All of the children were more or less timid at first; but this was overcome, in all save possibly H's case, before tests were begun. Particular pains were taken with F. The experimenter was in her company a great deal, and by the beginning of the tests was a gladly accepted play-fellow.

## IV. APPARATUS AND GENERAL METHOD

The plan of box A is presented in Fig. 1. (This box was used for the raccoons.) The box is made of  $\frac{1}{4}$ " boards and is  $2\frac{1}{2}$ ' high with doors  $7\frac{1}{4}$ " wide and 13" high. The light stimulus came from 3 c.p. 8 volt miniature carbon lamps, so wired that they might be switched on one at a time. The current was obtained

from a 220 volt lighting circuit and was passed through a lamp rheostat before reaching the discrimination box. The release box R was raised by means of a cord passed over a pulley in the ceiling and back to the experimenter at E. The first release box had glass over the top and sides. The right and left faces of the box were  $12'' \times 15\frac{1}{2}''$ . The front was  $7\frac{1}{2}'' \times 15\frac{1}{2}''$ . With this release box the distances to the entrances of the three light boxes (L) were unequal. Those at the sides were each  $19\frac{3}{4}''$ , while the distance straight in front was  $20\frac{1}{2}''$ . These various unequal-

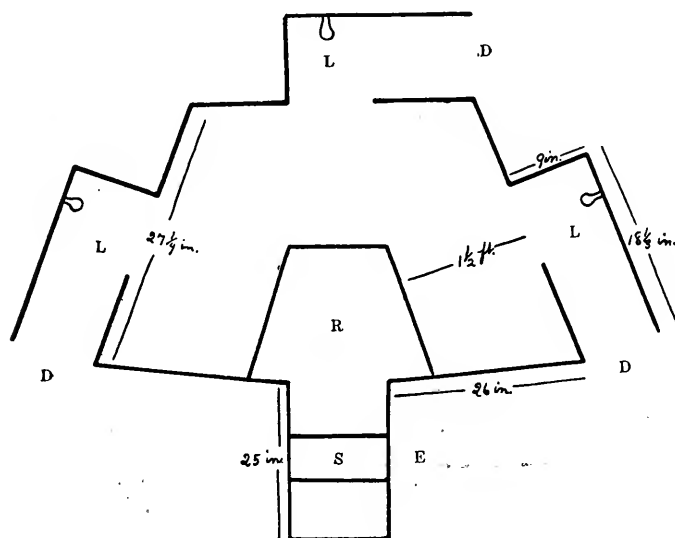


FIGURE 1. Ground plan of Box A

ities were due to two causes: (1) The box had been planned originally for a different type of test and was only later arranged for the present experiment. (2) The release box, being covered with glass would have been too heavy to handle had it been made larger. During the course of the experiments, another release was made. This one was covered with wire of  $\frac{1}{2}''$  mesh. Its sides were  $14\frac{1}{2}'' \times 15\frac{1}{2}''$ ; and its front was  $9\frac{1}{2}'' \times 15\frac{1}{2}''$ . The distances to the light boxes were now equal and of the dimensions indicated in Fig. 1.

Sliding doors were placed at the points marked D in the figure. They were controlled by strings which ran from them,

through screw-eyes on the top of the box, to the experimenter at E.

A 16 c.p. light was suspended about four feet from the floor over the center of the apparatus. Its intensity was diminished approximately by two-thirds by wrapping the bulb in cloth. The three light boxes were covered in order to prevent light from entering them from above. Part of the entrance box leading to the release, R, was covered by the switch-board, S, and the remainder by paste board. This prevented the animals from watching the experimenter.

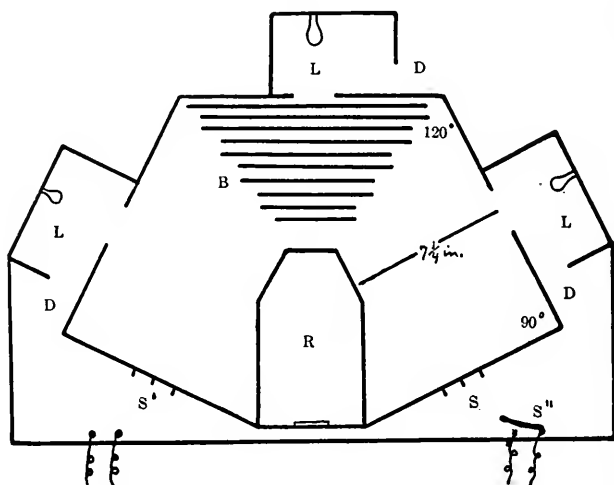


FIGURE 2. Ground plan of Box C. D, exit door.

Box B was, in principle, like Box A. It was used in testing the dogs. Its dimensions differed from those of A. Instead of being 90 degrees, the extreme lateral angles were 75 degrees. The angles on each side of the middle light box were about 145 degrees as opposed to 127 for Box A. Box B was only 2' high, and the length of its entrance box was 1' 4". The release box was covered with wire, and each face was 1' x 10". The distances from the release boxes to the light boxes were each 1 1/2'.

Fig. 2 is a ground plan of Box C. This box was used for rats. In addition to the data there given, the following points should be noted: The release box, R, was fastened by hinges

so that when it was raised the three faces cleared the floor practically the same distance. The faces and the front half of the top of R are covered with glass; the rest is of wood. The doors leading into the light boxes are 4" x 3". Those leading out of the light boxes, called exit doors here, are 3" x 2½". The switches at S' are for the lights which are of the same intensity and wiring arrangement as those in the two preceding boxes. The switches at S turn the current into any or all of the paths from the release box to the light boxes. This current is obtained from a dry cell and is passed through the primary coil of a Porter inductorium. The strength of the current passing into the prob-

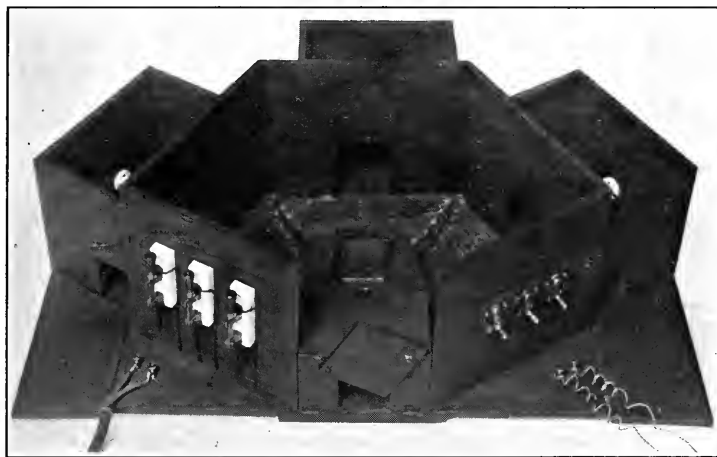


FIGURE 3. BOX C

lem box was regulated so that the animals never became frightened by severe shocks. The current was never strong enough to cause the animals to squeak and only rarely did they attempt to jump over the strips. The brass strips from which the shocks were obtained (only one group, marked B, is shown in this figure), were very thin and lay flat on the floor of the box. Before being tacked down, they were given an acid bath which destroyed their lustre but left their conductivity practically unaffected for my purpose.

Fig. 3 should give a clear presentation of the essentials of this box as well as of the others used in the course of this research.

Box D, also used for rats, is similar to Box C in all save two respects: (1) It was not wired for punishment. (2) The doors leading from the light boxes could be closed with wooden slides. The use of these slides was discontinued shortly after the experiments began. Pieces of wire mesh were then used. These admitted the light and thus offered less opportunity for the animals to tell which box was open and which was closed.

The apparatus (Fig. 4) which was used with children was constructed on the same principle as that described above for the other problem boxes. Three boards, each one foot square,

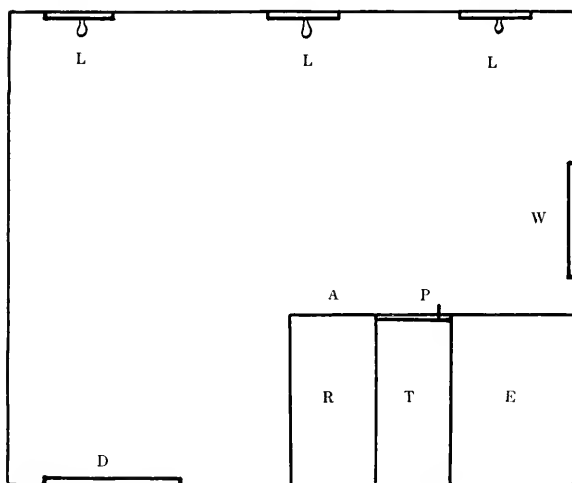


FIGURE 4. Ground plan of apparatus used with children. D, door; W, window

were placed against the wall of a room 12' x 14'. The middle square was seven feet distant from the release box, R. On each of these boards was mounted: (1) A 4 c.p. miniature electric light, L; and (2) just below the light, a push button. All three boards were painted black and were exact duplicates the one of the other. The front, A, of the release box was a lever which could be raised by a handle at P. The experimenter usually sat at E. T is a table which held: (1) The candy used as a reward in the tests; (2) the switches regulating the lights and the buzzer; and (3) the buzzer. C is a curtain which hid the experimenter from the subject's view when the latter had



left the release box. At first this curtain was also continued between R and T. Subsequently, this was found to be both inconvenient and unnecessary and its use was discontinued. The apparatus was wired so that any light could be turned on at will and so that any push button could be connected with the buzzer. Moving the switches was done without the subject's knowledge. The light was always turned on over the button that rang the buzzer. The child's problem was to find this button at the first trial when the light was on (in the learning series) and then (in the delayed reactions) after the light had been turned off for a certain interval of time.

The general method of experimentation was as follows: The animal to be tested was put in the release box, R, of problem Box C, for example. If, now, the lighted box is the one on the left, the exit doors of the others are closed and the switches at S are so set that if the experimenter close the switch at S', the animal will receive a shock if it steps on either the strips leading to the box on the right or on those leading to the one in front. The light is then turned on in the left box. The animal is released after five seconds, the time being taken with a metronome. A careful, detailed record is kept of the direction in which the animal is oriented<sup>40</sup> when released and of just where it goes after being released. In the case of the animals used in Boxes A, B and C, they should go straight to the lighted box, out through the exit doors and back to the entrance of the release box where they are fed. The rats used in Box D were fed a small morsel of bread and milk at the exit doors of the lighted boxes. Theoretically the olfactory control was not so good here as where no food at all was given in the light boxes. Practically, there was no difference. The rat was given only a bite, so almost no food fell on the floor; all the boxes were used an equal number of times; and all were frequently washed out. Whatever odor was present was so distributed as to afford no appreciable basis for discrimination. The results obtained with these rats, when compared with those where the olfactory control was better, support this statement. In any event,

<sup>40</sup> Orientations are spoken of as right and wrong, irrespective of whether an animal may be said to depend on them as cues or not. When the orientation is "right," the animal is headed toward the proper box. Any other orientation is "wrong."

olfactory inequalities would persist after the light was turned out and would aid in delayed reactions only if associated with the light. More attention will be given to this possibility later in the discussion.

After the animal had been trained until it chose the lighted box almost perfectly, delays were begun. The light was turned off just as the animal reached the box. This was called the first stage of delay. At the second stage, the light was turned out when the animal was half way to the box. At the third stage, the light was turned out just as the experimenter *started* to raise the release box. Here there was a genuine delay, although a small one. The first two stages served primarily to adapt the animal emotionally to the sudden change from light to darkness. The rats and dogs usually ran so fast that their momentum was sufficient to carry them into the box when once they were started toward it. In any case they only needed to continue in the direction in which they were going. This, however, was not the case with the third stage. *The light was put out before the animal started.* Throughout these three stages, the animal was released promptly at the end of five seconds. From this stage on, where the animal was detained one or more seconds after the light was out before being released, it was obviously necessary to let the animal see the light before this was turned off. Occasionally, the interval thus required was more than five seconds. In these higher stages of delay, I always waited until I felt sure that the animal had seen the light, and then turned off the current while the animal was still oriented toward the source of light. Record was kept of any change in the orientation which an animal made after the light was turned off. How detailed these records were will be seen in the section on experimental results.

The delays were gradually increased in length until one was found at which the animal failed. They were then decreased until the animal was again making a high percentage of correct choices, when the intervals were again increased. An animal was thus tested twice for the limit of its ability to delay with the backgrounds surrounding the entrances to the light boxes all similar the one to the other. When this limit was found, the wall of the box about the entrance to *c* was covered with white cardboard; that about *b*, with a black; and that about

a,<sup>41</sup> with a medium gray. If the animal's limit of delay was no better or was worse with this arrangement than before, the animal was dropped from the experiment. If the limit were better, the different backgrounds were removed and the similar ones used again. The limit of delay with these was then re-determined. If this third limit were greater than the second, the effect of training could be evoked as an explanation of the fact. But if it were markedly less than the second, the only cause could be an association between the backgrounds and the lights. The significance of this type of association will be dwelt upon in detail later in this paper.

One more point in general method remains to be considered. This is the question of what percentage of correct choices shall be taken as sufficient to justify further increase in the interval of delay. With three discrimination boxes, pure chance would lead the animal to make  $33\frac{1}{3}\%$  of correct reactions out of a long series of presentations. But series used in experimentation are very rarely long enough for chance to operate as theory demands. Besides there are various other influences which enter in to determine an animal's behavior above and beyond the influence of the stimulus proper to the test. One such influence is the position factor. Try as I might to eliminate this, most of the animals acquired at various times during the experiments more or less pronounced preferences for certain boxes. And these preferences varied from animal to animal. Sometimes they were so strong that the regular tests had to be stopped until the habit could be broken up. In the light of this, although on the whole each box was presented to an animal an equal number of times, in any one stage of the tests such an equality might not be present. Thus an equality of percentages among the various animals, in this case, would not mean that they knew the problem equally well. Again hesitations and waverings must be noted in estimating how well the animal is grounded in his appointed task. Further statements concerning the value to be attached to the percentages appear later in the paper (pp. 44, 46 and at intervals thereafter.) Considerations such as these render it highly inadvisable to lay down a rigid standard as to the number of trials and the per-

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<sup>41</sup> a, b, and c will be used in this paper to designate the right, middle and left light boxes respectively.

centage of correct reactions to be required of all animals. As will be seen in the following experiments some animals were advanced from stage to stage when but 70% of correct reactions were made. Others were detained on one stage until 85% or 95% were made. Sometimes an animal was given 5 or 10 trials, sometimes 150 or 200 trials on one stage of delay. One general fact, however, should be noted: The first set of rats, Bob and Betty during their first year's work, and H, L, M, and Hd of the children were rushed from stage to stage as fast as was at all feasible. The emphasis here lay upon what the subjects *could* do with their native equipment. With the other subjects used and with Bob and Betty during the second year's work, a larger number of trials were given on each stage of delay and the advance in the length of the periods was more gradual. The emphasis here was placed on what the subjects could *learn* to do.

## V. EXPERIMENTAL RESULTS

### 1. TESTS WITH ANIMALS

*A. Learning the Association.* (a) Rats.—The method of experimentation used by Hough and Reed was retested with the result that the general method outlined above was adopted. These tests were made upon five normal adult rats, Nos. 18–22.

The regular experiments may be divided into two sets: (A) Those in which reward only was employed; and (B) those in which both punishment and reward were used. Nine rats, Nos. 1–9 were tested in the first set and eight, Nos. 10–17, in the second set. Of the rats in A, Nos. 1, 3 and 8 were in poor health, and their records will not be considered.

Table I gives the number of trials required by the rats of Set A to learn the association between the light and getting food. These rats were given 5 trials daily. All save No. 6 learned the association. Of the last 50 trials given this animal, only 40% were successful; while of the entire 800, 54% were correct.

TABLE I

Rat	No. of trials on learning
No. 2	176
No. 4	175
No. 5	505
No. 6	800
No. 7	361
No. 9	280

The rats of Set B were tested as follows: Nos. 10, 11, 14 and 17 were given 5 trials daily. Nos. 12, 13, 15 and 16 received 10 trials. All of these animals learned the association. The number of trials required for this learning is given in Table II. The

TABLE II

Five trials		Ten trials	
Rat	No. of trials on learning	Rat	No. of trials on learning
No. 10	165	No. 12	440
No. 11	160	No. 13	250
No. 14	200	No. 15	220
No. 17	175	No. 16	480

results here indicate: (1) That the use of 5 trials favors rapid learning more than does the use of 10 trials, and (2) that rats tested with 5 trials daily under conditions of reward and punishment will learn the association more nearly in the same length of time than will rats given the same number of trials but tested with reward only. The use of punishment, while it may not shorten the length of time required by an "intelligent" rat to learn an association does hasten the learning in the case of the "dullards" such as Nos. 5, 6, 7 and 9. Rats will put forth a maximum effort under punishment when they would not do so under other conditions. These results are in harmony with the work of Hoge and Stocking.<sup>42</sup>

That the influences on time of learning exercised by the number of trials and by punishment are not due to preferences for the light or for the dark room boxes is indicated by Table III which gives the number of times each rat chose the

TABLE III

Rat.....	No. 2	No. 4	No. 5	No. 6	No. 7	No. 9
No. of choices of light box..	23	21	13	18	23	38
Rat.....	No. 10	No. 11	No. 14	No. 17		
No. of choices of light box..	20	28	28	32		
Rat.....	No. 16	No. 15	No. 13	No. 12		
No. of choices of light box..	24	30	21	27		

<sup>42</sup> Hoge, M. A. and Stocking, R. J. A Note on the Relative Value of Punishment and Reward as Motives. *Jour. of Animal Behavior*, 1912, vol. 2.

lighted box during the first 50 trials. The data given for the rats of each group were obtained under similar conditions (for our present purpose) and are strictly comparable. The only cases of marked preferences occur with rats Nos. 5 and 9. No. 5's preferences for the dark boxes will help to account for his slow learning; but it would seem that No. 9 should have learned very rapidly owing to its marked preference for the light box. With the exception of these two rats, all the animals were on an essential par as to preferences. Hence the differences noted in their learning periods must have been due to the different conditions under which they worked. Accidental individual variations are not the causes of these differences because of the number of rats used.

(b) Dogs.—The two dogs, Blackie and Brownie, were given 10 trials daily in Box B under conditions of reward only. The former animal was given 560 trials on the association. Of these, 408 trials or 72% were correct. Brownie was given 650 trials on learning the association. Of these, 396 trials or 60% were correct. Ninety-five per cent of the last 100 trials were successful. The relatively poor showing made by this dog was due to the acquisition of habits that interfered with the proper reaction. Twice during the learning series, extra tests had to be given in order to break up a discrimination now against the right and middle boxes, now against the left and middle boxes. In general, it may be said of both dogs that their rate of learning is no better than that of the rats. Indeed it is much worse than any of the rats tested with reward and punishment and 10 trials daily. The only rat that learned the reaction that did not do much better than the dogs was No. 5. I would suggest that the difference is due to the dogs' "helplessness" when deprived of cues from the experimenter. The following paragraph elaborates this point.

There are several interesting points relative to the dogs' behavior in the learning series that deserve mention. Indeed to a large extent, they are typical of their behavior throughout the entire experiment. When the dogs were first put into the problem box, instead of attempting to get out, they merely sat down and howled. They were out of sight of the experimenter, it will be remembered, and when placed in such conditions seemed quite helpless. They gradually overcame this timidity,

helplessness or lonesomeness but, as will be seen in the delayed reaction tests to be described below, they never revealed a high order of resourcefulness. We shall have further occasion to see that their behavior was almost, if not entirely, on a par with that of the rats. In learning the association, e.g., the dogs would go back and forth between some two boxes for many trials before investigating the third box. Often they would stop and look around in an apparent attempt to find the experimenter. This was never done by the rats and raccoons. Again, the brown dog fell into the most absurd habit of going out of the release box, turning entirely around to the left and then going to one of the boxes. This accomplished no end that I could determine, yet it was persisted in for some weeks.

(c) Raccoons.—Bob and Betty were each given 10 trials daily. Jack and Jill received 15. Reward only was used, although running into the wrong box and having to back out constituted no little punishment here as was also the case with the dogs. The experimenter was practically out of sight of the animals all of the time and absolutely so in certain control tests. It will be remembered that the raccoon tests were made in a room illumined by a single light suspended above the problem box. (See p. 24). This left the remainder of the room dark and thus helped conceal the experimenter.

Bob required 120 trials to learn the association. Of these, 93 trials or 77% were correct. Of the last 50 trials, 96% were successful. Betty was given 340 trials in the learning series. The work was not properly controlled for her until the last 120 of these trials. She formed the habit early of looking into the light boxes and seeing whether or not the wooden exit doors were closed. This difficulty was finally obviated by using coarse mesh wire for the doors. These could not be sensed until the animal practically touched them. Of Betty's last 50 trials on learning, only 79% were correct. These tests should have been continued longer. Jack was given 540 trials on learning. Of these, 428 trials or 79% were correct. Of the last 150 trials, 98% succeeded. Jill received 825 trials during the learning period. Of these, 631 trials or 76% were correct. Of the last 150 trials, 99% succeeded. (The experiment box had wire exit doors when Jack and Jill were tested.)

Although the nature of the learning process is not our specific problem, the differences in the total number of trials required for learning by Bob and the last two animals are so great as to invite comment. The conditions under which the records were obtained differ in the following points: (1) Jack and Jill were younger than Bob by at least a year. (2) Work was begun with them in July and with Bob in (the preceding) October. This brought him nearer the period of hibernation when his appetite would begin to fail. (3) Jack and Jill received more trials daily and also were tested for more days. Considering the fact that when the experiments ceased Jack and Jill were younger than the other two raccoons were when the latter started and yet had accomplished as much, I do not believe that age was an important factor in determining the reactions. However, the actual fact of an age difference remains. The second point should have little explanatory force, since the animals were always eager to work. As to the third point, one would expect this factor to work in a direction opposite to that which the results indicate. However, such was not true with the rats, and it may be that here also the lesser number of trials daily is more favorable to rapid learning. The fact of a possible difference of preference for light and dark remains to be considered. Of the first 50 trials, Bob made 56% correct. Of the first 45 trials, Jill made 22% correct. Jack made 48% successful responses out of the first 45 trials. Age differences, differences of brightness preference and differences in the number of daily trials are possible explanations for the varying lengths of the learning records. Innate (?) individual abilities must also be recognized. (Betty's results are not considered because of their unreliability at this point of the experiments.)

Summary.—The only comparative statement with reference to the learning periods of the different groups of animals that the facts warrant is this: The raccoons Jack and Jill and both dogs belong together in the class that learned most slowly. The second class with reference to speed of learning is composed of the rats. Bob is in a class by himself. He learned more rapidly than any of the other animals. Further comments upon this aspect of the learning process will be deferred to the section on children.



*B. Maximal Intervals of Delay Attained.* (a) Group differences as to maximal delay.—The only conditions of experimentation that varied from animal group to animal group were these: (1) Number of trials daily. Rats 2, 4, 5, 6, 7 and 9 (Set A) and rats 10, 11, 14 and 17 (of Set B) were given 5 trials daily. Rats 12, 13, 15 and 16 (of Set B) received 10 trials. The two dogs were given 10 trials daily. Of the raccoons, Jack and Jill were each given 15 trials and Bob and Betty 10 trials daily. (2) The rats of Set B were tested with reward alone. As was mentioned above, the dogs and raccoons were very much discomfited by having to back out of the light boxes when a wrong choice had been made. This was even more true in our tests than in most discrimination work owing to the small size of the light boxes.

TABLE IV

	Rat	Maximal delay	Per cent of correct response
Set A	2	1 sec.	64
	4	1 sec.	52
	5	3rd stage	52
	6	did not learn association	
	7	3 secs.	56
	9	10 secs.	72
Set B	10	1 sec.	76
	11	1 sec.	64
	*12	1 sec.	72
	*13	4 secs.	88
	14	3 secs.	80
	*15	1 sec.	86
	*16	1 sec.	50
	17	1 sec.	37

Set A tested with reward.

Set B tested with reward and punishment.

Those rats that are marked with a \* received 10 trials daily. All the others received 5.

Table IV gives the maximal delay attained by the rats. The percentages are computed on a basis of 25 trials. It will be seen from this that only four rats of the fourteen tested reached intervals greater than one second. The percentage of correct behavior for one of these (No. 7) was only 56. Rats Nos. 4, 11, 15, 16 and 17 were tested with a choice of two boxes as opposed to three. This was done only after the animals had

more or less completely broken down in their reactions to one box. Tests with two boxes were never made on any subject, unless the animal lost the cue to one box. It was this box that was dropped from the series. The maximum delays made by the rats under these conditions were increased. Nos. 11, 15 and 16 each made 5 secs. at from 76 to 90%. This must mean that it is easier to use two cues than three. Further comments will be added below (see p. 39).

Of the dogs, Blackie made 10 secs. at 76% for 30 trials. She then lost the cue to one of the boxes and was unable to react successfully to that one. When tested with two boxes she finally made a delay of 5 mins. for 5 trials at 80%; 86% had been made at 3 mins. for 30 trials. Brownie was tested only with three boxes. She made a delay of 1 sec. for 50 trials at 90% and 2 secs. for 70 trials at 68%. Both dogs were tested for eight months. The results should be representative of what dogs can do under the present conditions.

Of the raccoons, Jack made 14 secs. at 70% for 30 trials. Tested with two boxes, he finally made 85% at 20 secs. for 40 trials. He was tested for eight and one-half months. Betty made 82% at 7 secs. for 50 trials. Tested with two boxes, she reached 10 secs. at 86% for 30 trials. She was tested for one year and three weeks. Jill was tested only with three boxes. She made 3 secs. for 45 trials at 93%. The tests extended over a period of seven months. Bob made 90% for 10 trials at 15 secs. Tested with two boxes, he reached a delay of 25 secs. for 20 trials at 90%. Bob was tested steadily for a year and five months.

For fear that some critic would urge that the animals could not have delayed longer than they did even though the light had been constantly present, control tests were made as follows: The animals were taken at the stage when they had just broken down at some (for them) long interval of delay and were held in the release for one minute with the light on. When they were released, the light was still left on. The results show that the animals made a very high percentage of correct behavior. Their failure to make correct long delayed reactions must have been due, then, to their inability to use some cue by which to guide their reactions after such an interval and not to mere restlessness caused by the long confinement.

Appendix A contains tables that show all the regular tests given to the raccoons Bob and Jill and to rats 4 and 16. The data are there given in the order in which they were obtained. They are typical of the results of all the animals.

There is so much individual variation within the three groups of animals whose results have just been given that any exact correlation between length of delay and groups of animals is unwise. It is to be borne in mind moreover that it is not the length of delay but the methods of reaction after delay to which the greater importance attaches. This topic will be discussed later. To give some further idea, however, as to the relation between the groups Table V gives the longest and the shortest delay made by each class of animals. This table ignores the different conditions under which the delays were obtained. It presents the maxima and minima of the best reactions that the individual animals of a group were able to make under the present conditions.

TABLE V		
Subjects	Min. delay	Max. delay
Rats	Either no learning or 3rd stage	10 secs.
Dogs	2 secs.	5 mins.
Raccoons	3 secs.	25 secs.

(b) Effect of size of release upon interval of delay.—The experiments so far described were all made with the small release described in the section on apparatus. This release confined the animal's activities to a small part of the apparatus. It was thought if a release was used which would give the animal the freedom of the whole interior of the box, that not only might the maximal interval of delay be increased, but the animal might reveal more clearly its method of solving the problem,—indeed it might even develop a new and higher type of behavior in response to the more complex situation. These latter possibilities, we shall consider below.

The new release that was used fitted just inside the openings into the light boxes. It was made of a continuous piece of wire netting, thus making possible the simultaneous presentation of all three boxes. It was now possible for the animal to go over to the door of the lighted box and wait there during the interval of delay. The light was always left on until the animal had reached a position immediately in front of the lighted box.

Preliminary tests insured an absence of emotional disturbances in the reactions.

The animals tested under these conditions were: Rats 13, 15, 16 and 17 (No. 17 was the only one tested with a choice of three boxes); both dogs (Brownie alone was tested with a choice of three boxes); raccoons Jack, Jill and Bob (Jill alone was tested with three boxes).

Table 6 gives a comparative statement of the animals' abilities with large and small release. Both records for each animal are for discriminations with the same number of boxes. It will be seen from this that rat No. 13, the dog Blackie and the raccoons Jack and Bob are the only ones that failed to delay longer with the large release. This fact can be readily explained in the case of Jack and Blackie. The experimenter was primarily interested in discovering whether any new mode of behavior would appear under these conditions. When this question was answered, tests were discontinued with all the animals although in the case of Jack and Blackie the interval of delay was being steadily increased. The increase in delays made by the other animals is to be correlated with the changed conditions of experimentation. This point will be amplified when the detailed behavior of the animals is discussed.

TABLE VI

Animal	Small release		Large release	
	Delay	Per cent of correct reactions	Delay	Per cent of correct reactions
Rat No. 13	4 secs.	88	3 secs.	95
Rat No. 15	1 sec.	86	6 secs.	86
Rat No. 16	1 sec.	50	9 secs.	82
Rat No. 17	7 secs.	68	1 sec.	37
Blackie	5 mins.	80	1 min.	80
Brownie	2 secs.	68	6 secs.	96
Jill	3 secs.	93	7 secs.	80
Jack	20 secs.	85	15 secs.	88
Bob	25 secs.	90	20 secs.	76

(c) Effect of backgrounds of different brightnesses upon the interval of delay.—When the apparatus for the animals was constructed, every effort was made to secure qualitative similarity in the three light boxes. The backgrounds surrounding the entrances to the light boxes were all of the same brightness. During the course of the tests, the attempt was made to deter-

mine whether the animals *could* form an association between the lights and some constant marked differences in the external environment. In order to test this, three backgrounds of widely differing degrees of brightness were used as described above in the section on method. The animals tested under these conditions were: Rats Nos. 2, 4, 5 and 7 of Set A and Nos. 10, 12 and 16 of Set B; the dog Brownie; and the raccoon Jill.

The results of this test were entirely negative. No case was found where the animal made use of the different backgrounds as cues for guiding its reactions. One or two of the animals increased their interval of delay under the new conditions. However, when the old conditions of similar backgrounds were replaced, the long intervals of delay still continued. This indicated that the improvement was due to practice.

(d) Effect of number of boxes upon delay.—As stated above in section (a), whenever an animal's reactions broke down upon a particular box, that box was dropped from the tests and the discrimination confined to the other two. The following animals were tested under these changed conditions: Rats 4, 11, 15 and 16; the dog Blackie; and the raccoons Jack, Bob and Betty. The results are given for convenience above under section (a). Here it may be noted that all the animals delayed longer with two than with three boxes. This can be readily explained on the basis of the complexity of the problem. The maximum delay for any animal is decided by the accuracy of its response. With two boxes, this accuracy was increased and hence the maximum delay recorded was greater.

(e) Effect of other conditions upon delay.—The results do not indicate certainly any effect of punishment or of the number of trials upon the length of the interval of delay.

The results of this section indicate that the following factors influence the maximal amount of delay: (1) Different groups of animals; (2) size of release, and (3) the number of light boxes used. The following factors do not influence the amounts of delay: (1) Punishment and reward; (2) number of trials daily, and (3) backgrounds of different brightnesses.

*C. Methods of Reaction After Delay Used by the Animals.*—There are three different methods of delay which might have appeared and in point of fact did appear in our delayed reaction experiments: (1) The animal may maintain an orientation

of all or part of its body during the interval of delay, i.e., it may keep its head or even its whole body pointing toward a certain box. (2) There would be the negative side of this, where the experimenter could detect no orientation cues used by the animal. In this case, no observable part of the animal's body would remain in a constant position. (3) The animals might rely upon position in the box for their cues, i.e., they might actually go nearer to one box than to the others and then wait to be released. (4) Any combination of these three methods might occur. The discussion of methods 1 and 2 (orientation and non-orientation cues) will be combined and will be followed by a consideration of method 3. The actual existence of method 4 will be considered as occasion demands.

(a) Orientation of whole or part of body.—In addition to what was given above in the section on method concerning orientation, it will be well to make such additional comment here as will indicate clearly the nature of the data secured. Great pains were taken to insure accuracy and consistency in the recording of orientations. Needless to say in such prolonged tests as the present ones, the experimenter soon becomes expert in deciding whether an animal's movements are to be interpreted as a change in orientation. Before an animal has been tested long, the experimenter can pick out a certain range of movement and call this the orientation toward a certain box. The animal (dcgs excepted) was in constant motion, but so long as its activity was directed toward any one face of the release box, the orientation was recorded as unchanged. There would seem to be some chance for doubtful cases when the animal was pointed halfway between any two boxes. These cases were never counted as changes in orientation. Record was kept not only of the body position, but of whether *any* observable part of the animal remained in a constant position during the delay. Further note was taken of the gross amount of the loss of orientation—i.e., whether the animal turned clear around or not; and if not, then how far around—and of just which reactions were preceded by apparently identical orientations. The data were recorded quickly and easily by the use of symbols.

Every rat at the moment of release, went in the direction of his bodily orientation in 99 cases out of 100. (At times this

was not true because of position habits formed by the animal.) (For the present purposes, it makes no difference whether the reaction was correct or not.) The data supporting this statement are so overwhelming that they need not be given here in detail. The rat, when put into the release box during the delayed reactions, oriented *immediately* to the light with its entire body and began a series of attacks on that side of the box in an effort to get out. This attempt was kept up until the animal was released, whereupon it went to the box that was straight in front. Experiment served only to lengthen the period during which they would attack any one side of the box. These statements hold true for all rats.

Both dogs were dependent upon orientation for the guidance of their successful reactions. They only differed from one another in the length of time during which they could maintain a certain orientation. The dogs differed from the rats in that the determining cue was the direction of the *head* rather than of the *body*. For the sake of concrete material illustrative of this type of reaction, I shall give a summary of typical reactions made by Blackie. Of 770 trials, given during a period of two months, on delays less than 3 secs. long, 141 were unsuccessful. On 116 of the 141, the dog had the wrong orientation at the moment of release and followed it. On the remaining 25 reactions, the dog failed to follow its orientation and was wrong. On 8 trials the dog had the wrong orientation at the moment of release, i.e., was not headed toward the proper box, and yet reacted correctly. However, only 3 of these trials were with a 2 secs. delay and may have been due to chance. The remaining 5 trials were at the second stage of delay where the light was on until the animal was halfway to it. Obviously, these 5 reactions signify no great ability. These results indicate that just to the extent that the dog was able to hold the proper orientation during the delay, just to that extent it was capable of reacting correctly.

Let us take another typical set of results from the same dog obtained on delays from 15 secs. to 5 mins. extending over 30 days. Two hundred and eighty-five trials in all were given of which 37 were incorrect. In all 37 trials, the dog had the wrong orientation and followed it. Only once did she have the wrong orientation and react correctly. When Blackie entered the

release box, she would turn clear around and then, before the final breakdown came, would lay down facing the light. Occasionally she would be distracted by some accidental noise inside or outside the laboratory. In the great majority of these cases, she only turned her head. A few times she got up and turned clear around. But in any event, if she did not recover the proper orientation, the reaction failed. If she could have lost the orientation either completely, or almost so, and then have returned to it and have reacted correctly, the fact would have been strong evidence for believing that the dog recognized the proper orientation when it was reinstated. But taking the last 125 trials as typical, and classifying the instances where orientation was lost, recovered and a correct reaction made, it is found that 33 times the orientation was changed but slightly and three times completely changed.—By a slight change is meant that the dog turned her head and not her body. By a complete change is meant that the animal turned completely around.—Of the three cases of this latter behavior, one was very probably due to chance. The other two occurred on the second day of the 5 min. delays when the final breakdown was beginning. Moreover, the orientation that was lost and recovered,—in one case at the end of 36 secs.; in the other at the end of  $4\frac{1}{2}$  mins.,—was the orientation toward the box at the right. The reaction is thus not significant of some higher process, for on the following day the box at the right was the only one to which the animal responded. The recovered orientation, therefore, most probably indicates solely the growth of the habit that, on the following day, resulted in the complete disintegration of the reaction.

It is interesting in this connection to trace the change that occurred in Blackie's behavior from the beginning to the end of the experiments. It has already been noted how "helpless" and inactive both dogs were when the tests were started. After the work had progressed for several weeks and Blackie had become quiet and attentive, she would stand in the release box on all four feet and occasionally paw the wire in the direction of the light. A little later, she sat on her haunches during the retention in the release, but still clawed at the wire in a very calm manner. Toward the last of the tests described above, when the delays were growing rapidly longer, she lay flat down



on the floor with her head pointed, usually, toward the light. But she did not lie quietly. The long delay seemed very trying. Blackie would whine, wiggle her body and pat the floor with her fore paws in a fever of impatience,—yet never change the alignment of her body. Many times the dog held her orientation almost until the last second of delay and then if, when the release came, her head was (apparently) not more than half an inch to the right of its earlier position, she went in that direction and consequently went to the wrong box. Surely this is weighty evidence against the functional presence of any higher processes. Everything points to the conclusion that Blackie's reactions were determined by the orientation of her head at the moment of release.

It has just been indicated that the maintenance of an orientation either of all or a part of the body was necessary in the case of the rats and dogs, if their reactions were to succeed. Such was not the case with the raccoons. Each of these animals could react successfully when the wrong orientation was held at the moment of release and when, so far as the experimenter could detect, no part of the animal's body remained constant during the interval of delay. The evidence in support of this generalization is perfectly conclusive. It is only possible—and necessary—to present typical cases here. I use Jack as an illustration, although he did not delay as long as Bob.

In Jack's first 800 trials of delays, 77 were wrong. In 7 of these 77, the raccoon had the wrong bodily orientation, but did not follow it. On 12 trials, he had the right orientation, but did not follow it. On 58 trials, he had the wrong orientation and followed it. Of the 723 correct reactions, 167 were made starting with wrong orientations.

In the following 1066 trials, 149 reactions were incorrect. This group of trials extends from a period of 8 sec. delays at which 75% was made through a series at 11 secs. where 91% was made. Forty-one errors were made when the wrong orientation was held but was not followed. Thirty-seven were made when the right orientation was held. Seventy-one were made when the wrong orientation was held and followed. Out of the 917 correct trials, Jack reacted 309 times correctly when his orientation was wrong.

It is interesting to note that almost one-fourth of the last

mentioned errors were made despite the fact that the proper orientation was being held. This is a type of reaction that almost never occurred with the rats and the dogs. It would seem to indicate that this raccoon is less dependent on gross motor attitudes than the other animals. But the most significant behavior is that of reacting correctly when the wrong orientation was held at the moment of release. From the first set of figures above, it will be seen that of 232 trials when the wrong orientation was held, 167 or 71% were correct. The second set of figures shows that of 421 trials when the wrong orientation was held, 309 or 73% were successfully carried out. Such a high percentage places the results above the possibility of explanation by chance. Again, the fact that this type of behavior dominated for several days at a time indicates that something more than chance was manifesting itself. At 8 secs. delay, e.g., Jack made 10 correct reactions in one day, starting with wrong orientations. The following two days had 7 each; and the following two, 4 each. The next day the delay was increased to 9 secs. and 8 correct reactions with wrong orientations were made. Eight, 6, 4, 8 and 6 were the records of such reactions for the following days. The next day after these, 10 secs. delay was given. Here 9 correct reactions were made starting with wrong orientations. These are fair samples of the prevalence of this type of reaction.

In order to emphasize further the fact that Jack did not need to keep *any* observable part of his body in a constant position in order to react correctly, I shall give one day's record in detail (Table X). Often only the raccoon's hind feet—or one of them—remained constant. The interval of delay in this illustration is 10 secs. N.c. means no part of the body kept constant. H.f.c. means hind-feet constant. Rt.h.f.c. means right hind-foot constant. If the letter designating the orientation held at the moment of release is in *italics*, the animal oscillated between two faces of the release. If in addition the letter is starred, the animal oscillated from one to another of all three faces of the release. On the 3rd and 5th trials, Jack was distracted by some noise during the delay. A concrete statement of what these symbols mean in one or more instances should make the record perfectly clear. On the first trial, the light was turned on in the right-hand box. It was turned off and the

animal held for 10 secs. During this delay, Jack turned toward all three boxes. No observable part of his body remained constant. At the instant of release, he was headed toward the proper box, a, and reacted correctly. On the 5th trial, Jack was oriented toward the middle box, b, at the moment of release. He went to a, then to b, then to c and back to the experimenter for food. The reaction had failed. If to the large number of correct trials made with wrong orientations be added the correct trials made with correct orientations that had been completely lost and then refound, the number of reactions significantly different from those of the rats and the dogs reaches relatively huge proportions.

TABLE X

Raccoon	Orient. when released	Light box	Behavior	
Jack	*a	a	a	n.c.
	*a	a	a	h.f.c.
	*a	a	a	h.f.c.
	*a	b	b	rt.h.f.c.
	*b	c	abc	n.c.
	*b	c	c	rt.h.f.c.
	c	b	b	n.c.
	*c	a	a	n.c.
	b	c	c	h.f.c.
	*a	c	c	n.c.
	b	c	c	h.f.c.
	a	b	b	rt.h.f.c.
	*b	a	a	h.f.c.
	b	b	b	n.c.
	*a	b	ab	rt.h.f.c.

Two other types of reaction remain to be indicated in the above table, (X). Orientation b was held 6 times; orientation a, 7 times, and c, twice. Yet different correct reactions followed from the same orientations. Jack was oriented to c once and went to b correctly. The next trial he had the same orientation and went to a correctly. In neither case had he seemed to keep any part of his body constant. In the 4 correct responses to the light box b, the corresponding orientations were as different as possible. Twice Jack was oriented to a and once each to b and c, yet each time the correct reaction was made. In other words, the *same orientation* did lead to *different reactions* and *different orientations* to the *same reaction*. These, again, are types of behavior never met with in the rats and dogs. This behavior occurred so frequently with Jack and with Bob—

less frequently, however, with Jill and Betty—that it must be described as a genuinely new type of reaction in these experiments and not as the result of chance or of the foreknowledge of what box was to be presented. (This latter possibility was adequately ruled out by controls.)

Table XI gives a numerical statement of the importance of orientation for the rats, dogs and raccoons. As far as this factor is concerned, the animals within each class were on a par with one another. For this reason, I give the results for typical subjects and not an average for all members of a group. The data given for each animal are calculated from comparable groups of 800 delayed reactions each.

TABLE XI

	Rat No. 14	Dog Blackie	Raccoon Jack
No. of reactions in accordance with orientation..	698 trials or 87%	760 trials or 95%	614 trials or 76%
No. of reactions not in accordance with orientation.....	102 trials <sup>43</sup> or 13%	40 trials or 5%	186 trials or 24%
No. of reactions not in accordance with orientation that succeeded.....	15 trials or 14%	5 trials or 12%	167 trials or 89%

This table indicates plainly the similarity of the behavior of the dogs and rats as well as the wide divergence of the raccoons from the other two groups of animals. The rats and dogs almost never reacted in opposition to orientation. When they did do so, the number of their successes was a negligible quantity. That orientation was a strong factor with the raccoons is evidenced by the 76% of reactions that followed it. This fact makes the 89% of correct reactions starting from wrong orientations of great significance. The reactions succeeded in opposition to strong orientation influence. This statement is supported by the facts above noted. It was shown there that one of the raccoons (a typical one): (1) Made different correct reactions from the same orientation, and (2) made the same correct response from different orientations. Additional evi-

<sup>43</sup> The large percentage of reactions not in accordance with orientation made by the rat, when compared with those made by the dog, is due to the acquisition of a habit of holding orientation b and reacting to box a, i.e., to a position habit.

dence will be presented later when the effect of the size of the release upon the methods of delay is considered.

One other line of evidence which points to the uniqueness of the raccoon's behavior should be noticed here, viz., the growth of the methods of delay during the course of the experimentation. Did the rats and dogs rely upon orientation from the beginning of the tests? Was the non-orientation cue natural with the raccoons or acquired under the stress of circumstances? An examination of the records reveals the fact that from the beginning of the learning tests the rats and dogs reacted in accordance with orientation. There was no development of a new method as the delays were increased in length. There was simply an improvement in the facility with which the animal maintained its position for a given length of time. When the orientation was lost, it was no easier to react correctly at the last of the experimentation than at its beginning. In the case of the raccoons also there was no initiation of a new mode of response. There were, on the average, as many reactions per day during the learning period that did not follow the orientation as there were per day during the delayed reactions. The improvement that took place as the experimentation proceeded was in the accuracy of responses not in accordance with orientation, when these responses were made after a long delay. "Non-orientation reactions," therefore, seem to be *natural* with raccoons and thus seem to differentiate their behavior from that of the rats and dogs.

(b) Position in the box.—The data considered under the above title are those indicating the effect that the large release box had upon the methods of delay used by the various animals. It will be remembered that the large release gave the animals the freedom of the interior of the box and thus permitted them to take up a certain position in the box as well as to maintain a certain bodily orientation.

Four rats, both dogs and all of the raccoons save Betty were tested with this large release. Only three animals had their methods of behavior essentially modified: Rats 17 and 16 and the raccoon Jack. In no case, however, did an animal that had depended upon orientation for its cue begin the use of a non-orientation factor. What the modifications were will come out in the following descriptive summary.

An analysis of the behavior of the four rats when tested with the large release, brings to light the following facts: Save in four cases, No. 17 went in the direction in which his body was pointed at the moment of release. *This was true regardless of his position in the box.* Nos. 13 and 15 always went in the direction of body orientation. Hence position was only effective as an aid to the retention of body orientation. During the last week of the tests with this large release, No. 17 manifested an exceedingly interesting type of behavior. He needed to keep *neither position nor orientation constant in order to react correctly.* I could find no observable body cue by which the problem was solved. The first day of this particular week, 3 of the 10 trials were of this type, but for several days no more instances were noted. Then they were again in evidence until the end of the work. The *correct* reactions made in this manner never exceeded 3 per day. That I was much astonished at the sudden appearance of this type of behavior in the rat goes without saying. To think that at the end of eight months' steady work, the animal should suddenly adopt a new mode of behavior! It was not many days, however, before the accumulated data made clear the explanation. *The rat always turned to its right and entered the first box that was encountered.* Thus the animal could be at the middle box, but with its nose slightly to the right of the door to this box, and when released it would whirl and go to the box on the right. In the same manner, the rat might have its nose just to the right of the door to the left box when the release was raised and turn and enter the middle box. The behavior of rat No. 16 differed from this only in the direction of turning. This rat always turned to the left and entered the first box that it came to. From these data, it is obvious that the animals were using motor cues to guide their reactions. Their behavior was practically, if not entirely, automatic.

There is no need to detail the results obtained with the dogs when the large release was used. Both animals always went up close to the release in front of the lighted box and then waited until released. Each followed the orientation of his head regardless of which box was nearest.

Among the raccoons, Jack's change of behavior was a shift from the use of a non-orientation cue to a position cue. The

change occurred just at the close of the tests with the large release. His usual behavior as well as his new form may be set forth as follows: Before the new release was used, Jack was being tested with boxes *a* (the right one) and *c* (the left one) only. When the large release was put on, the tests were still confined to these two boxes. This was necessary in order that the results be strictly comparable. Of the 510 trials given under these conditions, 56 were made: (1) In which no observable part of the animal's body remained in a constant position; and (2) in which the animal's position in the box was wrong at the moment of release. In every one of these 56 trials, i.e., the animal was both in front of the wrong box and headed away from the right one when the instant of release arrived. Twenty-six of the 56 were reacted to correctly. Chance will account for this number of reactions as far as the mathematics of the problem are concerned. However, from the point of view of the observer, the majority of the reactions looked like anything but chance behavior. This was especially true when Jack reacted correctly and yet was pointed directly away from the proper box. There was a directness and sureness about his reactions that hardly savors of chance. I do not urge these instances on the reader as evidence of non-accidental reactions. I simply note the fact of their presence and the impression that they made upon me. It is well to remember in addition, though, that since orientation was such a strong factor in determining Jack's reactions (see above, Table XI) that any responses in opposition to this must be given great weight.

Owing to the fact that only two boxes were used in this series and that they were located far apart, the raccoon had every incentive to rely solely upon his position in the apparatus for the reaction cue. This he soon did to a large extent. In every case where his position was constant (and correct) and his orientation changed, he reacted successfully. In other words, position was the determining factor. When this became evident, the experiment was stopped. *The raccoon had shifted the basis of his response so that I could detect its nature by observation.* Jack's behavior at this point was thus on a par with the dogs and rats. Had the tests been continued, all that could have been expected was the perfection of a habit of staying near the proper box. This did not appeal to me as a profitable goal of endeavor.

For further confirmation of the statements made above concerning the general methods of delay, I shall present the raccoon Bob's record with the large release in some detail. Three hundred and sixty trials were given on two boxes with this release. Of the 27 reactions made with the *wrong position and correct orientation*, 19 were correct. Of the 91 reactions made with the *right position and wrong orientation*, 81 were correct. Sixty-three times the reactions were preceded by positions and orientations that were both wrong. There are two possibilities here: (1) Orientation and position may favor the same box; (2) they may favor different boxes. Forty of the 63 were reactions of class 1, i.e., were initiated by orientations and positions that favored the same wrong box. Fifteen of the 40 were successful. In other words, although both position and orientation favored the same wrong box, Bob was able to overcome the handicap and make 37% correct reactions. The remaining 23 reactions were of class 2. Of the 23, 16 were successful. Where position and orientation were both wrong, but did not combine to favor the same box, Bob made 69% correct reactions. Of the 7 reactions of class 2 that failed, 5 were in accordance with position and 2 in accordance with orientation, i.e., 5 times the animal went to the box favored by position and the remaining times to the one favored by orientation.

In order that the reader may have a perfectly concrete presentation of the reactions with the large release, I will quote a day's record from the diary. The reactions were made at 8 secs. delay and with the exit doors from the light boxes, *b* and *c*, open. In the records obtained with the large release, some new symbols were used in describing the data placed in the

TABLE XIV  
EIGHT SECONDS DELAY WITH LARGE RELEASE

Animal	Orient.	Light box	Behavior
Bob	b a-2	b	b h.f.c.
	b	c	c n.c.
	b	b	b h.f.c. (walked to <i>a</i> and back)
	b c-2	b	b n.c.
	c	c	c
	b	c	bc n.c.
	b	b	b
	c b- $\frac{3}{4}$	c	c n.c.
	b	b	b h.f.c.
	b	c	c n.c.



"orientation" column. The first reaction, e.g., is to be read as follows: The light was turned on in *b*, the middle box. Bob was held for 8 secs. during which time he pivoted on his hind feet, swinging his body along in front of *b*. At the moment of release he was still in front of this box, but was oriented with his nose in the corner halfway to *a*, the box on the right. During the delay at the fourth reaction, no part of the raccoon's body was constant. He walked over in front of *a* and when released was in front of *b*, but his body was pointed to the corner half way to *c* at the moment of release. At the instant of release for the eighth reaction, Bob's body was pointed three-fourths of the way toward *c*. This brought his nose within a few inches of the entrance to that box, yet the reaction succeeded. In the tenth trial, as in the second, position and orientation both favored the same wrong box. In each case the reaction was correct.

The net result of the data presented here for Bob is an almost entirely conclusive proof of the statement that he does not wholly depend either upon bodily orientation or upon position in the box for the cues determinant of the subsequent reaction. The 37% made with the reactions of class 1 is very significant when one does not lose sight of the fact that both orientation and position were here *combined against* any other factor leading to a correct response. In class 2 where such a combination was not present, the percentage is conclusive as to the presence of some non-observable cue. This is but further data confirmative of that already presented above for Jack with the small and large release.

In view of the generally negative results obtained with the large release, as far as developing new and higher types of behavior is concerned, some one may say: (1) That if all of the animals had been started with the large release instead of with the small one, they would not have been so likely to develop gross motor cues to guide their reactions, and (2) that it is not surprising that no new type of behavior appeared after the animals were firmly in the grip of habits developed in the small release. To these criticisms I can only reply that any experiment must exhaust first one method and then another. Time did not permit the use of both methods here.

## 2. TESTS WITH CHILDREN

A. *Method of Experimentation.*—Details of the method of experimentation here used may be presented as follows: When the subject was first brought into the room, the following instructions were given: We have a little game in here which we are going to play. You will stand in here, (I indicate the release box). When I release you by raising the gate, (I illustrate raising the gate), you are to go and push one of the buttons over on the wall. One of these buttons will make a noise. If, after I raise the gate, you *push the noisy button first*, I will give you some candy when you come back to me. But if you go first to some button that isn't noisy, then you must try again before you get the candy. So, you see, the game is to *push the noisy button first* and so get candy. Do you see now how we are to play the game? Run over there and push on some of the buttons. See, sometimes they make a noise and sometimes they don't. (I switch the buzzer on and off for all the buttons. The lights are not on yet, nor has the subject's attention been called to them.) Now let's try the game and see if you can *push the noisy button first*. (The child is placed in the release box. The light is switched on over the noisy button. The child is held 5 secs. in the release box before being set free.) These instructions were memorized by the experimenter; and, although parts were repeated several times to the subject, nothing that is not given above was told to the child.

It was found that with all save F, 6 preliminary trials were sufficient to familiarize the children with the apparatus and to overcome their timidity. (H's exception will be noted below.) F was given 35 trials extending over three days on the preliminary work of learning that the noisy button meant candy. By the end of that time the association was firmly established.

No fixed number of trials per day was given. The amount of the day's work was adjusted to the child's disposition and to the length of delays. No set number of trials was given at any stage of delays. In other words much the same method was used as for the first year's work with the raccoons, Bob and Betty. Delays were increased continuously until an error was made. At this point, they were either decreased at once or continued at their existent value for several trials before further change was made. The experimenter believes that so flexible

a method, when properly checked by careful observation of the subject so that the task is changed in constant sympathy with the subject's apparent needs, is excellently fitted to bring out what the subject *can* do naturally as opposed to what it can be *trained* to do. Of course the effects of training cannot, and need not, be eliminated. However, they are not so great in the method outlined above as they would be were many trials given at each stage. The difficulty here is the same as that mentioned when discussing the records for the raccoons viz., where only a few trials are given, the critic has a better chance to claim that the results are due to chance. Our conclusions will seek to avoid this criticism. But in the last analysis, the themselves must be their own justification.

*B. Are the Results Obtained from Animals and Children Comparable?*—In the light of the foregoing method and of careful observation of the children, the following points suggest themselves as the essential considerations in a relative estimate of the conditions under which the children and the other animals worked: (1) Fear.—This was overcome in the animals by the preliminary training; in the children, by kindness and cheerfulness on the part of the experimenter and by the child's examination of the apparatus as described above. (2) Motive.—Hunger and punishment insured a maximum of effort on the part of the animals. Candy, words of praise from the experimenter and a desire to excel its companions incited the child to do its best. (3) Knowledge of the reaction desired.—The rats, dogs and raccoons had to learn everything by themselves. (a) The preliminary series acquainted them with the fact that there were three exits to the problem box and possibly also with the fact that only one of these would be open at a time. (b) In the regular learning tests, these animals had to associate the light and the open box in such a manner that the light became the sign of the open box. (c) In the delayed reaction tests, again, they had to learn that the open exit was always in the box which had been most recently lighted. If we turn now to the children, we find the following situation: (a) They were told of the push buttons which for their problem corresponded to the exits of the other experiment boxes. Where the animals had had to learn the fact of only one open exit by trial and error, the children were at least aided by being told

that only one button would make a noise. (b) In the regular learning series, the children had to acquire the association between noisy button and light on their own initiation. It must be remembered that the experimenter *never* mentioned the word light and never directed the subjects' attention to the lights as long as the experiments continued. (c) In the delayed reaction, also, the children were thrown on their own resources in the working out of the problem. (4) Treatment during the delay.—During the interval of delay the rats, dogs and raccoons were usually left strictly alone. Only in a few control tests was any effort made to distract them. However, uncontrollable noises occasionally intervened and disturbed the dogs and the raccoons. During the period of delay, the children were entertained by the experimenter by means of stories, the drawing of pictures and, in a few of the long delays, by gifts of candy. In his opinion, these distractions made the experiment more difficult, although it is true that impatience and fretting on the part of the subject were largely eliminated. It would thus seem that the only objection to this method is that possibly the distractions served to urge the child to form a "purpose to remember" sooner than would otherwise have been the case. I cannot deny this as a possibility. It may have occurred with the girl M. But from the fact that the others remained impatient and complained of the delay until late in the experimentation, I do not believe they realized before that time that the problem was to see how long they could remember. Questioning on this point at the close of the experimentation confirmed this. (5) Possibility of the problem being talked over by the subjects.—This, of course, has no bearing on the case of the animals, but it presents a fairly serious possibility with respect to the children. The possibility is all the greater because of the fact that four of the children lived in the same neighborhood. Although recognizing this, I feel that it played very little part in the experiments; for where it might have been effective, the tests continued but ten days. M was old enough to do as she was told, unless severe temptation came her way. Such a possibility, however, was prevented by cold weather and school keeping her away from the other children. As will be pointed out later, talking could not have influenced H's conduct because she knew all about the experiment. F was under

the experimenter's control and never saw the other children. Hd and L differed so in their attitudes toward the problem and in their general behavior, that I cannot well believe they talked about the problem when together. Further evidence why it is improbable that the children planned with one another how to work the problem is that there was a very keen rivalry as to who should have the most candy beans at the end of the day's work. When the work was finally dropped, the children said that they had not talked the problem over with one another. In the light of these considerations, I believe that the children and the animals worked essentially on a par, so far as extra-individual influences are concerned, throughout both the regular learning series and the delayed reaction series. I say "essentially on a par" because the social influence due to the presence of another member of the same species was operative in the case of the children while it did not appear with the animals.

*C. Learning.*—A few words will suffice to describe the trials given on learning the association between the lights and the noisy button. With all subjects save F the light was first turned on at *a*. (For F it was put on at *b*.) On this first trial all the subjects failed. F went to *a*, then walked past *b* to *c* and thence back to *b* and rang the buzzer. All the others went first to *b* and then to *a*. This may have been due to the fact that *b* was the nearest of the buttons. F was the only one that made an error in this series after the first trial. The others learned the association in one trial. Out of the 16 trials on the first day, F failed on 9. The first test was missed the second day and none the third day. In other words, no errors were made after the 17th trial.

*D. Differences Between the Learning of Animals and Children.*—Some of the difference between the above data and that for the animals is undoubtedly due to differences in attitude toward the problem, although the conditions were so arranged that this should have been at a minimum. Five rats learned the association in from 160 to 176 trials. The two dogs, it will be recalled, required more than 500 trials. Bob received only 120 trials. These figures present the number of tests after which no errors were made. Over against these figures, 46 should,

stand as the largest number of trials given on learning to any of the children, and this to F, the youngest.

I believe that the main factor that would make for non-comparability in these results is that of brightness preference. It is not known what the value of this factor was for the children. But leaving this possibility aside, it is to be noted that the above records fall into two well defined groups; those for the animals and those for the children. If the difference here is correlated with grades of intelligence,<sup>44</sup> one may well ask why no such differences appear between the several classes of animals. The problem is all the more interesting when it is pointed out that there seems no certain correlation between the ability to learn the association and the ability to delay when one considers the various groups of animals. The dogs and the rats used the same method in delay, yet Blackie delayed longer than any rat. Jack and Bob used methods of behavior in delay quite different from the other animals and their delays were far longer than those of the rats and of Brownie. Furthermore two of these raccoons delayed about the same period of time, yet varied greatly in their times for learning the association. Among the children, matters would seem to be different. They learned rapidly and in delays reached relatively long periods of time by what seemed the same method used by the raccoons. The following answers suggest themselves with reference to the question put just above: The present data indicate: (1) Either that the different grades of intelligence among the animals were not great enough to be registered in the learning rates although the grades between animals and children were sufficient to manifest themselves; or (2) that the association can be learned with a type of process that will not suffice for long delays where the orientation is not maintained. If this latter alternative be correct, it would seem that the children used the method required for the last mentioned type of delay in their learning period. The raccoons, on the other hand, used the same method

<sup>44</sup> V. C. Hicks and H. A. Carr <sup>45</sup> find no such correlation of the number of trials taken in learning a *maze* and the grade of intelligence of the subjects. The time is not ripe for a statement of the type of learning present in maze problems when compared with the present test. However, the data presented by the above writers and by myself would indicate that the two problems do not involve the same means of solution, at least to the same extent.

<sup>45</sup> Hicks, V. C. and Carr, H. A. Human Reactions in a Maze. *Jour. of Animal Behavior*, 1912, vol. 2, p. 101.

in learning the association that the other animals did, but a different method when it came to delays. There may thus be a difference in intelligence due to varying abilities in the use of some one instrument of adjustment or due to varying abilities in the use of different instruments of adjustment. Blackie and the rats would illustrate the first case; the children and raccoons in comparison with the other animals would illustrate the second. So much for the possible meaning of the learning times of the different animals and children

*E. Delayed Reactions.*—M, age 8 years, was given 38 trials on delay. The first 2 only were at intervals less than 1 sec. The others ranged from 1 sec. to 28 mins. and extended over 7 days. Fifteen trials were on intervals of 10 mins. and over. Only two errors were made, one at 5 mins. and one at 20 mins. The latter was probably caused by ill-humor. Longer delays might doubtlessly have been secured had it been desired. The only attempt was as follows: On four days (not consecutive ones), M was asked which button she pushed last the day before. Three times she answered correctly.

M did not need to keep any part of her body constant during the delay. On the trials involving more than 10 mins. delay, she was sent out of the room and put with the other children. During the delays she conversed freely with the experimenter. Any "purpose to remember" that the subject formed was formed on her own initiative. Great care was taken—and I believe effectively so—to insure that no suggestions be secured from extraneous sources. As early as the 15th trial on delays, M volunteered the information that she remembered where the light was in order to push the proper button. Several times she reacted correctly and then volunteered that she had *guessed* which button to push.

Several extra trials were given in which the problem was changed. The light was now placed successively over two different buttons, the last button being the noisy one. This problem brought out a type of reaction very often seen in the animals. M would start toward one box and then turn and go to another. In the regular series, hesitation occasionally occurred and M wavered between two boxes.

Hd, aged 6 years, was given 47 trials on delays. Only the first two were less than 1 sec. long. The others ranged from

1 sec. to 33 mins. Fourteen trials involved intervals of 10 mins. or over. A total of 15 trials was given with intervals of 4, 5 and 6 mins. in length. In the 47 trials, 10 errors were made. Two of these came in each of the delays of 4, 5 and 6 mins. One came at 11 mins.; 1 at 12 mins.; and 2 at 20 mins. Hd, therefore, had no trouble in remembering the solution of the problem until the intervals of delay reached 4 to 6 mins. Then the difficulties that arose were mastered and did not reappear until the periods 11 to 12 mins. and 20 mins. were reached.

Hd did not need to keep any part of his body constant during delay. He and the experimenter exchanged stories continually. This subject differed from M in the number of errors and in the greater frequency with which information was offered. The former I attribute to difference in ability; the latter, to natural garrulousness. The restraint during delay was a great source of annoyance to Hd. He complained a great deal because of it. My diary notes contain many such passages as the following: When light was turned off, he said: "Why hold me so long, I may forget which it is." The subject evidently realized that he was to remember where the light had been. Many times during the delay, Hd would stop conversing and say "O, I know which one it is" and would then point—not always correctly—to the button he had in mind. Instances of wavering and hesitations preceding acts of choice were noticed with this subject.

L, age 6 years, was given 41 trials on delay. Only one of these involved an interval less than 1 sec. long. The remainder were from 1 sec. to 25 mins. in length. Twenty-one trials were on delays from 4 to 9 mins. long. Nine were on delays over 10 mins. in length. Seven errors were made in the total 41 trials. All of these came in delays of 4 mins. or over. Two were at 4 mins. and one each at the following intervals: 5, 7, 8, 15, 17 and 25 mins. L, as Hd, found most difficulty in the period around 5 mins.

L did not find it necessary to keep either his body or the direction of his attention constant in order to solve the problem. He, too, conversed freely with the experimenter during delays. L would often watch for the light out of the corner of his eyes. After it appeared, he would apparently pay no further attention to the problem until released. L was less demonstrative



than Hd and did not remark so often during the delays that he still knew the proper button. Like Hd, he was impatient during the short delays at the first and the long delays at the last of the experiments. At times he wavered and hesitated in his reactions, saying that he had forgotten or that he wasn't sure. (In such cases, of course, the experimenter gave no cue to the solution.)

Each of the three subjects whose records have been discussed formulated his own "purpose to remember." H was given trials under the same conditions as the others, save that she was told the purpose of the experiment. She was told that the light would be over the noisy button. When delays were begun, she was told to be sure and remember where the light had been.

H, age 6 years, received 15 trials on delays. Only the first two involved intervals less than 1 sec. in length. The others varied from 10 secs. to 35 mins. One error was made at 21 mins. At this trial, H walked half way to *b*, paused for 5 secs., wavered as though to go to *c*, but finally pushed *b*. She then returned to the release and was told to try again. Again she wavered at a point half way to *b*, but this time she went to *c*. The light had only been turned on the one time. I did not talk to H during the first 12 delays. She was always told before the light went out to be sure and remember, but this was all. In the subsequent trials, every effort was made to distract her during the periods of delay. The results obtained with H, when compared with those for Hd and L, indicate that it is an aid to the subject to have the "purpose to remember" expressly formulated for him.

F, age 2½ years, was given 507 trials on delay. Of these, 30 delays were less than 1 sec. long. The other 477 trials ranged from 1 sec. to 1 min. Of the 477, 143 were wrong. The following table (XV), gives the relative distribution of these errors. It does not give the delays in the order in which they were given to F. The table simply summarizes the number of trials and errors at each stage. An advance was never made from one stage to another until at least 80% correct reactions were made for at least 5 successive trials. The only interval that F did not finally master was 1 min. This is surprising when such a high percentage of correct reactions occurred at 50 secs. On each of the two days when F was tested for the 1 min. delays,

TABLE XV

Delay	No. of trs.	Errors	Delay	No. of trs.	Errors
1 sec.	23	4	15 secs.	65	19
2 secs.	4	0	20 "	40	8
3 "	5	0	25 "	22	5
5 "	38	10	30 "	22	11
6 "	9	0	35 "	18	2
7 "	35	9	40 "	23	10
8 "	3	0	50 "	19	2
10 "	85	37	1 min.	45	26
12 "	10	0			

the first one-third of the trials were at 50 secs. Reactions were perfect. The last two thirds of the day's work was at 1 min. and both times she fell below 50%. There were no known extraneous factors to cause this. The conditions were as near ideal as possible. The method adopted with F, after the first three days, was one of slow advance from stage to stage. At the higher delays, each day's work was begun—as just illustrated—with the longest interval that had been mastered. Some 5 trials were given here and only if the reactions were perfect was an advance made. I believe that entire dependence can be placed upon the results obtained. F was the only one of the children that did not reach a delay of at least 20 mins.

F was distracted continually. She needed to keep neither her attention nor any part of her body constant in order to react correctly on delays up through 50 secs. She often hesitated and wavered in making the choice of buttons. It is interesting to note in this connection that this wavering, hesitant behavior was only noted in the case of the raccoons and children. Every individual of these gave many examples of it. This is significant when it is borne in mind that these subjects used apparently the same method in solving the problem. If that statement is too broad, at least it may be said that they agreed in *not* using gross motor orientation exclusively as did the rats and dogs. This is not the very usual type of behavior described by Yerkes for the dancing mouse. He says: "I have at times seen a mouse run from one entrance to the other twenty times before making its choice; now and then it would start to enter one and, when half way in, draw back as if it had been shocked. Possibly merely touching the wires with its forepaws was re-

sponsible for this simulation of a reaction to the shock." "The above reaction, noted by Yerkes, and other similar cases in the literature are reactions to present stimuli. Where the mouse ran to the entrance of the box, but did not enter, the explanation undoubtedly lies in the inability of the stimulus to set off the proper reaction. As Yerkes suggests, the stopping of the animals half way to the box was probably due to the contact with the wires. The same general type of behavior described above for the raccoons and children occurred during certain periods of time with my own rats and the dogs. In every case, however, it was due to a habit of turning around when released. This habit was executed in the same fashion no matter which was the proper box to choose. As opposed to the tests where either present objective stimuli or habit are involved, the reactions of the children and raccoons were, *as far as could be determined*, perfectly spontaneous, i.e., determined by intra-organic conditions that varied for the different boxes. These cases belong in the same class as that of Miss Washburn's cat described above (p. 20).

F's father, a trained psychologist, informed me that the child was just reaching the stage where her memory for objects and events had begun to take on definite form. When brought in from a ride in the park or a visit to a friend, she could very seldom remember the details of the event, indeed not more than half the time could she remember the gross fact of having been somewhere. This occurred even with what were to F very interesting experiences. Sometimes, it is true, the difficulty lay with the lack of control of language; but this was not always the case. After her playmate had gone for some time, if F was asked who had been there, not only could she frequently not tell, but at times she was bewildered even by the suggestion that anyone had been to see her at all. On the other hand, some cases were noted where F remembered an event for several days. Phrases, also, that she had heard but once were often spontaneously repeated for the first time several days later. The present tests thus found the child in a very important stage of mental development. Definite memories, of the adult human kind, were still hanging in the balance with the chaos of the preceding period. As far as the lengths

<sup>46</sup> Yerkes, R. M. *The Dancing Mouse*. 1907, New York, p. 130.

of delay go, F ranks approximately half way between the other children and the raccoons. As to apparent method of work she—and the other children too—is superior to the raccoons in that orientation played no discernible rôle in her reactions.

## VI. THEORETICAL CONSIDERATIONS

### 1. THE CUES ESSENTIAL FOR SUCCESSFUL DELAYED REACTIONS

After the above presentation of experimental results, there remains the important task of determining as nearly as possible just what cues the subjects used in their reactions. The following paragraphs of this section will set forth in detail the various possibilities of interpretation and indicate what seem to me to be their relative validities.

Let us first formulate in a general way the conditions that a reagent will have to meet in the solution of the present problem. He is confronted with three boxes which offer as many known possible points of egress. One of the boxes is lighted. In the course of a series of experiments, the reagent has learned to go through, or to the lighted box in order to reach food. In time the stimulus will set off the reaction practically automatically. Approximately at this period, I begin to turn off the light before the subject reaches the box. It is possible that for several stages of delay the box which has been lighted remains light for a short interval of time due to the presence of after-images in the subject's eyes. But as the delays increase in length, there will come a point at which the problem shifts from "go to the lighted box" to "go to the one of three dark boxes that was most recently lighted." If this shift comes before the 1 sec. delay, it will be less likely to involve relearning. If, e.g., the change comes at the second stage, the momentum of running will make it easier for the animal to continue into the dark box than to turn and seek another. With some rats, the results indicate that the change did come at this stage; for when the light was turned out, the animals stopped short and went into another box, even though that too was dark. With the other animals and the children, it was impossible to tell just when the shift came. The problem having once changed, however, the question now is how long after the boxes all become dark can the subject pick out the one which was lighted most recently. Our special problem concerns only the solution of this

second problem. During the learning series, the light stimulus acquires the power of releasing muscular activity applied in a certain direction. Just what direction this shall be is determined by the spatial location of the light. A significant meaning here attaches to the objective stimulus, i.e., the light plus the definite location. (By the term meaning, I imply nothing more than the fact that a certain stimulus evokes a certain reaction under conditions that are not usually described as involving mere habitual or reflex activities.)

As soon as the problem shifts to a choice of one of three similar boxes, i.e., as soon as a stage is reached where the determining stimuli are absent at the moment of reaction, then it is necessary, I assume, if the reactions are to succeed, that the subject develop substitutes which shall take the place of those stimuli as carriers of the needed meanings. In other words, the substitutes must *fulfill the function* of the previous stimuli in arousing the three appropriate movements. The substitutes may secure this power either through association with the light during the learning series, or during the delayed reaction tests themselves through a process of trial and error. In view of the fact that delayed reactions did succeed under the present conditions, there can be no question as to the *existence* of the substitutes. Our next problem is that of determining their nature.

The substitutes or cues that determined the subject's reactions may theoretically have arisen either within or without the organism. We shall consider the latter first.

*A. Substitutes Derived from the External Environment.*—Under this heading, we shall consider two possibilities: (a) Were there three simultaneously present objective cues that may have served to *determine* the subject's reactions? and (b) were there three objective cues that varied from trial to trial with the position of the light which may have determined the reactions after the light was turned off? In any case there must be two or three cues each determining one of three reactions. We shall consider the possibilities in the above order.

(a) Simultaneously present objective cues.—Although every effort was made to secure uniformity in the visual appearance of the three light boxes, they differed at least in spatial position and in the nature of the part of the experimental room visible above the walls of the apparatus. In addition there were pos-

sibly olfactory differences in the boxes used by the animals due to the animal odor itself and (in the case of the rats and raccoons) to a food odor set up by the milk that was on the forefeet of these animals. Suppose now that these simultaneously present external factors be treated in their entirety for each box and be represented by the letters x, y and z. It *may* be assumed that the subjects learned to react not to the lights—a, b and c—alone, but to the complexes ax, by and cz. Then when the delays were begun, the reactions were made to x, y and z, either immediately or after a period of learning. Here we would have three cues governing as many reactions, and all three cues would be presented simultaneously at the moment of release whereupon the reaction would take place.

This hypothesis is not supported by the experimental facts. If the reactions during the delay were determined by these stimuli present at the moment of release, the animals should have learned to delay for almost any interval of time. It should be no more difficult to react to x, y and z, than to the lights, since the reactions during delays were just as "precipitate" and "headlong" as when the lights were present. Now it was demonstrated that all of the animals could react perfectly when held in the release 1 min. with the light on. Why then could they not delay a minute with the factors x, y and z which were also constantly present? Further, if the subjects succeeded in reacting to x, y and z for intervals of 5 or 6 secs., why should they be unable to reach 7 or 8 secs.? In other words, why, if they reached one stage of delay on this basis, should the subjects not go a little beyond and so up to a large delay? There is no answer to this, if one assumes, as we have done, that the cues are simultaneously present at the moment of response.

In the section on experimental results, tests were described where the entire sides of the problem box surrounding the entrances to the light boxes were covered with cardboards of widely separated grades of brightness. This device accentuated (from the experimenter's point of view) the constant differences between the hypothetical stimuli x, y and z, yet under these conditions neither rats, dogs nor raccoons showed improvement in their abilities to react. This series does not prove that no objective factors x, y and z were influential in initiating behavior; but, in conjunction with the immediately following theoretical discussion, it does make it very improbable that such fac-

tors should have exercised a determining influence on the reactions.

Let us grant for the moment that such objective factors were present, could they *alone* control successful reactions, i.e., could they be the *determining* factors in initiating correct delayed reactions? Since x, y and z are present *simultaneously* at every response, they *per se* cannot be the bases for differential responses. The subject must *select* one of the boxes. Hence the hypothesis would need to assume that the subjects are not reacting merely to x, but to x-where-the-light-has-just-been. Here we are forced face to face with the problem from which we started: What represents, or is a substitute for, the light? What is the element attaching *to* x that is equivalent to "where-the-light-has-just-been"? There can be no question but that x, y and z in some form constituted a part of the general stimulus, for the subject must apprehend the different spatial locations of the boxes in order to react to them. The objection may be offered that this contention as to the effectiveness of these objective stimuli is inconclusive, inasmuch as the animals may have been reacting entirely in terms of kinaesthesia, i.e., had learned the fixed order in which the three boxes were presented. This possibility was adequately eliminated by control tests whose results are discussed on p. 67. The point to be established here is that x, y and z could not have been the crucial substitutes for the lights. These must have been factors which were not all simultaneously present, each in its entirety, at every response, i.e., they must have alternated from trial to trial, depending therefore on the position of the light. The question now is: Were there any such factors in the objective environment?

(b) Alternating objective cues.—Controls were instituted: (1) To prove that during the learning series the light (an alternating factor) was the determining cue for the reactions; and (2), to prove that in the absence of the light no other external factor took its place as a determinant of the reactions.

1. The reagents did not derive any cues from the experimenter. Screens were so arranged that the operator was never visible to the dogs. Control tests were also made under these conditions with the rats, raccoons and children. The constant beating of a metronome covered up any noises due to the ex-

perimeter's breathing that the subjects might have utilized. The dogs and raccoons did not rely upon the *manner* of being released. This was proved by having different persons operate the release. This control was not used with the rats. However, it is extremely improbable that these animals depended upon cues from such a source. Moreover had they or any of the reagents done so, their intervals of delay should have been almost indefinitely great. It must be borne in mind that any cues derived from the experimenter, in order to afford extra aid in the reactions, must be present at the moment of release. This would make it possible for the animal to avail itself of the cue after a delay of any length. If the cues were given by the experimenter only at the beginning of the delay, the problem confronting the animal would not differ from that of delaying with reference to the light.

2. The reagents did not depend upon any after-glow of the lights. When the lights were left on for one minute—a period much greater than was ever used in the experiments proper—and were then turned off, there was no appreciable after-glow of the carbon filament that the experimenter could detect. (In any event, such an afterglow would not persist long enough to influence the longer delays of the reagents.) Hence any continued brightness of the boxes (considered from the reagent's point of view) after the current was switched off must have been due to the reagent's after-images. The possibility of using these for cues will be considered below.

3. The reagents did not depend upon variations in the temperature of the boxes in making their reactions. After the lights had been turned on for one minute in any box, the temperature of that box was never raised more than a degree centigrade. Indeed only occasionally could any change of temperature be detected. The "headlong" manner in which the animals reacted to the boxes, together with the fact that they oriented toward the light immediately upon its appearance indicate that they were not governed in their reactions by the slight variations in temperature. Yoakum<sup>47</sup> found that rats could discriminate differences of 16° C., but even then their behavior was the result of long special training. His squirrels

<sup>47</sup> Yoakum, C. S. Some Experiments Upon the Behavior of Squirrels. *Jour. Comp. Neur. and Psych.*, 1909, vol. 19, p. 565.



—and probably the rats also, although he does not say—were hesitant about entering the boxes even with a difference of 25° C. These data make it exceedingly improbable that the animals in the present tests were influenced by temperature.

4. The animals did not derive cues from the doors at the exits of the light boxes. Numerous control tests were made in which all of the doors—and they were of large wire mesh, and hence hard to see—were open. Under these conditions, the animals reacted as though only one door had been open, as was usually the case.

*B. Substitutes Derived from within the Subject's Body.*—The discussion of these intraorganic substitutes will be divided into two parts: (1) What were the internal cues used by the various reagents?; and (2), how did these internal cues operate in order to guide behavior?

(a) The type of internal cue used.—1. Did the subject anticipate the order of presentation of the lights, i.e., were the cues to the reactions the individual responses of an habitual series? The evidence is perfectly unambiguous in support of the fact that the reagents did not rely upon the presentation order of the boxes in making their reactions. The order for each group of animals (not children) followed a series of 30 presentations so arranged that each box recurred an equal number of times. A new series was given whenever one box was eliminated from the experiments. Control tests in which the regular order of presentation was varied were introduced practically once every two weeks with all the animals. Controls were also made in which the lights were not used.

2. Did the reagents guide their reactions by after-images of the light? Although the existence of after-images in animals has not been demonstrated, we shall, for the sake of the argument, disregard this fact and admit of their possibility. In order to lead to correct reactions in our experiment, these after-images must appear in the proper direction for each response. Their directional position is a function of the orientation of the head and eyes, and as a consequence the hypothesis can not explain those correct reactions resulting from faulty orientations. After-images can possess a possible function only in conjunction with the maintenance of a constant orientation, and the hypothesis would need to assume that these overt motor attitudes are but

subsidiary phenomena serving as a means for the effective functioning of the after-image processes. But after-images can hardly persist long enough to account for the maximum periods of delay attained by this method of solution. Such delays varied from 10 secs. for the rats to 25 secs. for the raccoons and 5 mins. for one dog. All of the conditions of the test were distinctly unfavorable for any persistence of possible after-images. The light was weak in intensity (3 c.p.). Its average duration of exposure was approximately but 5 secs. Any approximation to steady fixation either during or subsequent to the exposure of the light was the exception. The animals as a rule were continually on the move, nosing and clawing at the face of the release box both during the exposure and the period of delay. Steadiness of fixation after the perception of the stimulus is a very essential condition for the development of after-images. Movements of the head or eyes tends not only to prevent their appearance but also to destroy them when present. The significance of these conditions is more apparent by recalling the fact that any extended duration of after-images is an exceedingly rare phenomenon in the normal perceptual activities of humans. Fixation is too short and changeable for their development. Since we are forced to argue from analogy with human conditions, one must also distinguish between the possible presence of such processes and the ability to perceive them. The mind tends in the interest of clear vision to overlook and neglect such processes as it does in the case of entoptic phenomena. With many people the ability to see after-images involves a previous knowledge of their existence and some degree of training and practice in their observation. In other words, after-images as persistent objects of consciousness are a product of the laboratory, and the assumption of their effective existence as guides to conduct in the normal perceptual activity of an animal is exceedingly questionable. After-images exhibit the phenomenon of intermittence. No high percentage of correctness of response could result from such a cue, as its presence at the moment of response would be a chance coincidence. The theory of after-images, moreover, is entirely unnecessary, as we have the possibility that these motor attitudes of orientation may themselves serve as a sufficient guide to

conduct, and on this hypothesis the assumption of the effective presence of after-images is an explanatory luxury.

3. Motor attitudes of orientation as cues of response. The data of the preceding section conclusively prove that maintenance of orientation during delay was an essential condition for correct response with the rats and dogs, and that such motor attitudes exerted a strong influence upon the behavior of the raccoons. Either these attitudes serve as the substituted cues and control conduct directly, or they function indirectly as a means of support to some such cue as an after-image. The evidence unambiguously favors the first supposition. These orientation attitudes, like any sensory process, may be a stimulus to definite movements. This tendency of the animal to run in the direction of their orientation at the moment of release was natural and habitual. The tendency was present in full strength at the beginning of the experiment. The tests merely developed the maintenance of orientation for longer and longer periods. This fact indicates that the motor attitude functions directly upon subsequent conduct. If the attitudes were but a means of support to some other cue, one would expect that this relationship of means and end would need to be acquired gradually during the experiment. An alternative theory presents many theoretical and factual objections. Any such roundabout and forced type of explanation is entirely unnecessary when we know that motor attitudes are a natural guide to the direction of subsequent responses. The mechanism of such a cue may be entirely automatic and mechanical. It requires nothing more for its explanation than does any habit. A stimulus initiates a certain act whose completion is prevented by external means. This initial activity persists unchanged so far as its directional aspect is concerned until the raising of the release permits it to function in a normal and habitual manner.

4. Some unknown intra-organic cue non-observable by the experimenter. Our data prove conclusively that some such cue was utilized by the raccoons and the children. Our proof of this statement is based upon the method of exclusion and the nature of such a factor must necessarily be defined at present in negative terms. We have exhausted our ingenuity as to objective possibilities of explanation, and as a consequence

are forced to conclude in favor of an intra-organic factor. The possibility of a temporal series of habits was eliminated by control tests. Neither orientation nor any distinctive motor attitude could be detected in the children or in at least 25% of the responses of the raccoons. The after-image hypothesis is entirely inadequate when orientation is faulty. In those reactions of the raccoons resulting from wrong orientations, the percentage of correctness was so great as to eliminate the possibility of chance. The nature and mechanism of this factor will be discussed in subsequent sections.

5. The following table (XIII) summarizes the cues used by the different reagents. P.C. (possible cue) means that the reagents so listed may have used that cue at times. N.C. (necessary cue) means that the reagents so listed had to use that cue or fail in a significant number of their reactions. R stands for rats; D, for dogs; RA, for raccoons; and CH, for children. For convenience of reference a classification is made

TABLE XIII  
CUES USED BY THE RE-AGENTS

External	Internal							
	After-image		Orient. At.		Non-orient. At.		Idea	
	P.C.	N.C.	P.C.	N.C.	P.C.	N.C.	P.C.	N.C.
Never a determining cue	R D RA CH		RA	R D	RA CH		RA CH	

in this table of those reagents that may have guided their reaction by ideas. This phase of the table will not be clear until the final section on the Place of Ideas in the Grades of Animal Learning is read.

(b) The mechanism by which internal cues guide behavior.—As already indicated the mechanism of orientation attitudes presents no difficulties. The light stimulus arouses the proper act. This inhibited act persists unchanged so far as its essentials are concerned during the delay. At the moment of release the animal runs in the direction of its orientation. This tendency of responding in conformity with orientation is natural and

habitual with the animal. The whole process is explicable on the basis of habit. Maintenance of orientation is acquired gradually by the trial and error method as is any habit.

The case of the non-orientation cues presents more difficulty. During the preliminary learning tests there was established by the trial and error method an association between the lights and the three acts of securing food. Between the two terms of each of these three primary associations there was interpolated probably by the trial and error method an intermediary link. These three cues were associated with their respective lights on the one hand and their respective acts on the other. Each light will now awaken its corresponding cue and this cue will in turn initiate the act with which it has been associated. In order to insure correctness of response, the proper cue must be present at or immediately after the release. As the interval of delay between the light stimulus and the response is increased in length, we have three possibilities as to the behavior of the intermediary link or cue. (1) After being aroused by the light stimulus, the cue may persist, or be constantly maintained, during the interval of delay. All of the available evidence tends to disprove such a hypothesis. The raccoons were frequently distracted during the delays by various laboratory noises, such as the squealing of rats and the rattling of windows. Note was made of these occurrences and still correct responses were possible in spite of these distractions. The raccoons were exceedingly active during the delays, pawing and clawing and running all about the release box. I often distracted the animals by bending down over the release box and yelling at them at the top of my voice. A typical case occurred when Bob was making a delay of 15 secs.—with a very high percentage of correct responses. These distractions during delay lowered his percentage approximately eight points. His behavior indicated that this treatment actually diverted his "attention" from the problem at hand. The emotional character of such a disturbance makes the high percentage of correct behavior especially significant. I also continually distracted the children during the delays by engaging their attention with stories, drawing pictures, conversation, etc. In fact the attention and interest of the children were often engaged to the point of absorption by these devices with no effect upon the correctness of their reactions.

The constant maintenance of the cue under these conditions of distraction and length of delay is highly improbable. Speaking in conscious terms, it would require great concentration and mental ability even for a human adult to keep any cue constantly in "mind" during such conditions. (2) The cue might be some intrinsically intermittent process such as an after-image. Such substitutes, however, could not suffice to guide reactions under the conditions of the experiment. Their presence at the moment of release would be purely accidental and hence they can not account for the high percentage of correct responses obtained. (3) We are forced to adopt the third hypothesis that the cue disappears after being aroused by the light stimulus, and is rearoused in some manner at the moment of release. To explain the mechanism of this revival, we shall assume that all three of the intra-organic cues have become associated during the course of the experiment with some sensory factor connected with the releasing of the animal. Hence the release is a stimulus which tends to arouse all three intra-organic tendencies. This revival, however, must be selective and adaptive and this adaptiveness can be explained by two additional assumptions. The presence of each light stimulus at the beginning of delay excites its corresponding intra-organic factor, and this excitement subsides after the disappearance of the light. Although the release stimulus does *tend* to revive all three factors, yet it will arouse that one most *recently active*, viz., that excited by the light at the beginning of the delay. The assumption that the predisposition of a tendency to response depends upon its recency of functional activity is a recognized principle of human psychology.<sup>49</sup> With such a mechanism, it would seem that the problem of delay should present no serious difficulties. However, the time interval between tests, i.e., the differences between the recency of excitation of the three factors, is small in many cases. Learning to enter the box most recently *lighted* as opposed to the box most recently *entered* is also no easy problem to master.

As we have indicated, such a mechanism would apply only to the non-orientation cues used by the raccoons and children. The type of function here involved is *ideational* in character.

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<sup>49</sup> Ladd and Woodworth. *Elements of Physiological Psychology*. New York, 1911, p. 285.

By applying the term "ideas" to these cues, I mean that they are similar to the memory idea of human experience so far as *function* and *mechanism* are concerned. They are the residual effects of sensory stimuli which are retained and which may be subsequently reexcited. The revival, moreover, is selective and adaptive to the solution of a definite problem, and when aroused, they function successfully as a necessary substitute for a definite component of the objective stimulus aspect of the problem. The question as to the *content nature* of these cues, i.e., their sensory or imaginal character, is reserved for the succeeding section.

## 2. THE PLACE OF IDEAS IN THE GRADES OF ANIMAL LEARNING

A survey of animal reactions from those of the protozoa to those of the higher vertebrates leads one to the conclusion that the simplest behavior, from a genetic point of view, is the adjustment of a certain movement to a certain object or situation. This adjustment may be either native or acquired. In the former case, it is instinct; in the latter, the result of individual learning. (As such, it does not seem to be present in the protozoa.) The remarks in what is to follow are directed solely to the latter case. In the literature this is referred to as the stage of learning by experience on a sensori-motor level. Over against this genetically simple learning, may be placed a more complex form of behavior which involves a representative function. This ideational or representative process arises out of a genetically prior sensori-motor level of behavior. The field of its functioning is limited, moreover, to the representation either of some aspect of the object (or sensory) side or of some part of the movement and its consequences. In other words, the representative process must stand for either the sensory or the motor aspect presented in the genetically lower level of behavior. According to the law of parsimony, the only conclusive evidence in favor of the existence of such a representative element is the case where successful adaptations occur when that part of the sensori-motor process assumed to be represented is known to be absent at the moment of response. If the object or movement to be represented is present, why assume a representative or ideational process? The adjustment can be explained in terms of sensory stimulus and response.

This is the defect in all of the arguments and experiments which we have examined in the historical section above. Ideas *may* have been present, but since all of the behavior *can* be interpreted in terms of stimulus and response, the arguments are inconclusive.

From this point of view, there are from the standpoint of function two classes of ideas:—ideas of objects or those representing the stimulus aspect of the situation, and ideas of movement or those representing some aspect of the movement or its sensory consequences. Theoretically, the problem of ideas can be attacked from either point of view. Many discussions and experiments do approach the topic exclusively from the movement side, but practically such a procedure involves almost insuperable difficulties. Washburn,<sup>50</sup> e.g., makes the number of ideas of movement possessed by an animal more or less of a rough index of that animal's place in the scale of intelligence. But the presence of such ideas is as yet an assumption of very uncertain validity. Furthermore, an experimental technique that would isolate and control the movement factor would be extremely difficult, if not impossible to devise. When, e.g., an animal is brought along a path in a maze to a point where two possible reactions are presented, both responses are for the moment inhibited. But the two movements need not be represented, they may be actually there, although in an incipient form only, i.e., the conflict may be between the motor impulses themselves and the conflict may be resolved on this level without the influence of any factor representative of the effects of the movements. I doubt whether experimental technique can ever control this movement factor. Quite the reverse is true with respect to controlling the presence of the object, i.e., of the determining stimulus. This latter may be given or withheld at the investigator's pleasure. It is the merit of the experiments here set forth to have followed such a procedure. In the present case, there seems to be no room for doubt that the object reacted to was the light. Now if a representative function were involved in the behavior of the reagents, as seems to have been the case with the raccoons and children, it must, in part at least, have been representative of the lighted box, because all else—including the three possibilities of movement—

<sup>50</sup> Washburn, M. F. Op. cit., pp. 279-284.



was constant from trial to trial, whereas a selective response must needs have an alternating cue.

In the light of the evidence in the present monograph, let us grant the presence, in certain reagents, of a process representative of objects. The question now arises, Must this process be imaginal or may it be sensory? We may treat the latter possibility in two ways: (1) There may be a sensation arising from the reagents body—kinaesthetic, e.g.,—that stands for a certain reaction. Or (2), the substitute may simply consist of a differential meaning attached to the perception of the particular light box. In this case, when the reagent apprehended the box, he would simply recognize it as the one in which the light had been most recently. This would be perceptual recognition as the term is understood in human psychology.

With either of the above explanations, it must be remembered, the question that we are now raising is one concerning the content of the representative factor. There is no doubt in my mind that the *function* is an ideational one. Even should some critic claim that all of the present behavior is but perceptual recognition, the fundamental difference between the behavior of the class containing the rats and dogs and that of the class containing the raccoons and children will still challenge explanation. If the behavior of both the above classes of reagents is to be termed perceptual recognition, then two orders of this must be admitted—one on a level with habit, the other on a level with ideas.

One class of facts suffices to disprove the possibility of accounting for the behavior of the raccoons and children on the basis of perceptual recognition. These reagents did not stop and search for the proper box. They started their reactions immediately upon being released. The raccoons Jack and Bob, e.g., might be headed *away* from the proper box at the moment of release and yet whirl around in the proper direction and react immediately. Bob, in particular, might go half way to the wrong box and then turn suddenly and react successfully. When the reagent does not follow his orientation, one would expect him usually to look toward at least two boxes before reacting, if the behavior were on the basis of perceptual recognition. It is these reactions of children and raccoons that were not in accordance with orientation and yet that were "head-

long" and "precipitate" that lead me to regard perceptual recognition of the boxes as an inadequate explanation of the facts.

The objections just stated do not apply to the assumption that the representative process is an intra-organic sensation. A positive justification of an ideational function whose content is an internal sensation may now be elaborated. There is no doubt but that in human consciousness a sensation may carry a meaning that is woven into thought sequences. In reading, e.g., all of the actually discernible conscious content may be kinaesthetic sensations from the muscles of the throat or may be auditory sensations, if the reading be aloud. Whether one say that the sensation is the meaning or that sensation is there plus a meaning, the case for our purpose is unaltered. Each sensation and its meaning become incorporated in a train of thought. A slightly different situation is presented where it becomes necessary for the sensation to represent that which is not there and then stimulating the sense organs. The following illustration from Titchener is a case in point: "I had to carry across the room, from bookshelf to typewriter, four references—three volume numbers of a magazine, three dates, and four page numbers. The volumes and years I said aloud, and then consigned to the care of the preservative tendencies. Of the four page numbers, I held two by visual images, one by auditory, and one by kinaesthesia."<sup>51</sup> The case of the volumes and years and that of the page numbers remembered by means of kinaesthesia are significant. Each is an instance (so far as we can tell from the account) of memory without the revival of images. The sensory cues, present at the time when the data were written on the machine, elicited the proper material. It is a dangerous procedure to complete another investigator's introspections. I do not intend to do so here. I simply suggest the above as a possible supplement to Titchener's own brief statements. Whether it be right or wrong in his case, the experience in which a *single* sensory process represents an absent object is sufficiently frequent to give us the suggestion for which we are seeking. The suggestion may also be found elsewhere. That which has become known as "conscious attitude" in the

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<sup>51</sup> Titchener, E. B. *Experimental Psychology of the Thought Processes*. New York, 1909, p. 202.

literature of imageless thought is identical with the first phenomenon that we have had under discussion in this paragraph. So far as the attitude itself is concerned, it might well be designated *sensory thought* as may other experiences also, such as the reading aloud that was mentioned above. The memory cases also may go into the same category. The sensations, both in the memory instances and in those of the conscious attitudes, differ, if at all, only on their meaning side and not by the addition of any overt imagery. Already the reader may have surmised, and rightly, that the writer is introducing the theory that the representative function found in the raccoons and in the child, F, at least, who was most nearly comparable with them, is likewise *sensory thought* and essentially comparable with the cases above given.

Let us come to closer quarters with this theory. The proposed hypothesis is equivalent to making the so-called "imageless thought" genetically prior to "thoughts with images" and to placing the point of origin at least as low as the raccoon. As opposed to this, current discussion by advocates of imageless thinking would seem to assume the opposite, viz., that "thoughts with images" are prior.<sup>52</sup> It is to be noted that the assumption has no factual basis, but that it seems to be the result of prejudice or of temperamental leaning. My usage here takes for granted that imageless thought can be analyzed into either sensory or imaginal content which carries the meaning. It is the former case that I use as the type found as low as the raccoons. This thought is *imageless*, though not *sensationless*, in the strict sense. In the light of this, I can see no intrinsic objection to the above theory, while the following points are in its favor: (1) The evidence in support of the possession of sensations by animals is absolutely convincing to any save one who denies all consciousness to animals. On the other hand, the evidence for images is very meager and unsatisfactory. Indeed, it is even more so that has been thought if, as I believe to be true, the present theory of *sensory thought* will account for all of the controlled behavior that has been used to support a theory of images where that behavior is not open to a stimulus and response explanation. (2) Raccoons and young children

<sup>52</sup> Bühler is an illustration of this. Referred to by Jas. R. Angell, *Psych. Rev.*, vol. 18, p. 317.

are capable of reactions that seem explicable only on the assumption of the functional efficiency of a representative factor. If sensations *can* function in this manner, the law of parsimony forbids the assumption of images. (3) That sensations can function in this manner is indicated by the illustrations from human psychology noted above. It is a different matter, I know, to say that the sensations of animals also may function in this manner. But in order to explain the behavior of the raccoons, it is necessary to assume either the presence of images or the presence of sensations that function in this manner. The law of parsimony favors the latter type of process because sensations are genetically earlier than images.

Inasmuch as the rats and dogs, as well as the raccoons, used sensory processes in the solution of our problem, we are forced to recognize that sensations may be placed in two classes on the basis of function: (1) There are sensations that can initiate a correct reaction from at least three possible responses *only when they have not been displaced*, during the interval of delay, *by other sensations of the same modality*. Here would fall those sensory experiences of constant orientation possessed by the dogs and rats. (Those cases where these animals lost their orientation and then regained it were few enough to be rated as accidental.) (2) There are sensations that can initiate correct responses, under the above conditions, even though they have been displaced during the interval of delay by others of the same modality. Here would fall the sensory experiences of the raccoons and children. The evidence which indicates that no sensory process need be constantly maintained in order that these subjects may react correctly is unambiguous. It is this type of sensory process that I have denominated *sensory thought*.

On the basis of human *introspection*, there is another grade or kind of learning, viz., the stage of the functional efficiency of images or centrally aroused conscious processes. Why there should be both sensory and imaginal thought in human experience is very difficult to say. The most obvious suggestion would be that imaginal thought, since it is genetically later, could accomplish tasks which sensory thought could not. I shall hazard no guesses as to what such tasks might be. At the present stage of psychological theory, there seems to be no question but that the distinction of sensory and imaginal thought

itself is valid. Future work must attempt the formulation of the functional differences between them.

The result of the above theoretical considerations is a classification of the grades of animal learning different from those hitherto advanced. Washburn,<sup>53</sup> e.g., has presented three classes: "First, there is the condition where, so far as we can see, the animal does not learn by individual experience. \* \* \* In the next place, we have the grade where the animal learns by experience, without having the power to recall an image of its experience. \* \* \* Finally, \* \* \* we have the possibility of an image." I would modify this scheme by the insertion of a stage called *sensory thought* between the second and third stages. The table would then read: (1) The animal shows an absence of learning by experience. (2) The animal is able to profit by experience, but has no higher capacity than "trial and error" or the "stimulus and response" behavior would indicate. The rats and dogs of the present tests come here. (3) The animal can learn by the "trial and error" method. *Indeed, probably most of its reactions are on this basis*, being ruled by stimulus and response. But a new element now makes its appearance, viz., *sensory thought*, which is a representative function of strictly sensory content. The raccoons that I used are in this class. Of the children, F is the most likely to belong to this class. (4) The fourth grade reveals the presence both of "stimulus and response" behavior and of sensory thought, but added to these is the possibility of directing reactions by images. The older children of the present tests very probably belong here rather than in the third class.

## VII. SUMMARY AND CONCLUSIONS

The following is a statement of the results and conclusions that have been reached as a result of the foregoing experiments and analyses:

1. The rats (one excepted), dogs, raccoons and children made successful reactions in situations where the customary determining stimulus was absent at the moment of response. The stimulus might appear in any one of three boxes. These boxes were qualitatively alike, but situated in different directions

<sup>53</sup> Op. cit., p. 276.

from the release box. At every trial, three possibilities of reaction confronted the subject. A selection had to be made and that box chosen in which the stimulus had appeared most recently.

2. The conditions under which the maximal delay was tested and the results obtained are indicated as follows:

(a) Different classes of subjects were used. Table XIV gives the maximum and minimum delays that were obtained from the different classes.

TABLE XIV		
Subjects	Min. delay	Max. delay
Rats	either no learning or 3rd stage of delay	10 secs.
Dogs	2 secs.	5 mins.
Raccoons	3 secs.	25 secs.
Children	50 secs.	25 mins.

(b) Backgrounds of widely different grades of brightness did not affect the intervals of delay.

(c) The use of a large release which gave the animals the freedom of the interior of the box lengthened the intervals of delay in the case of some subjects.

(d) The use of two boxes as opposed to three lengthened the intervals of delay by increasing the accuracy of response.

(e) Neither punishment nor the particular number of trials per day appear to have affected the interval of delay.

3. An analysis of the possible cues that may have been used by the subjects in the solution of the present problem gave the following results: (a) Overt orienting attitudes were the probable cues for many reactions of the raccoons. These attitudes must be assumed as cues for the rats and dogs in order to explain their reactions. (b) Some intra-organic (non-orientation) factor not visible to the experimenter must be assumed in order to explain a significant number of the correct reactions of the raccoons and all of the successful reactions of the children. These cues fulfilled an ideational function. (c) All of the reagents were influenced by external stimuli that were constantly present from trial to trial, e.g., those given by the box itself. However, these could not be used as a basis for selective responses inasmuch as they were constant from trial to trial and hence could not furnish varying, or alternating, cues.

4. No animal that had used overt motor attitudes in solving the problem when the small release and similar backgrounds were used adopted another type of cue either when a large release or when backgrounds of different brightnesses were used.

5. The method used in the present tests for attacking the question of the functional presence of a representative factor in animal behavior is superior to that of imitation, use of tools and others that have been used in the past, because here it is possible to determine what stimulus controls the behavior. It is therefore possible to insure the absence of the stimulus at the moment of response.

6. The representative factor for which search has been instituted in this monograph stands primarily for "objects" and not movements. A technique that would make certain a control of the latter factor so as to insure its presence or absence at the will of the experimenter has not as yet been perfected.

7. From a consideration of the theoretical advantages to be derived from interpreting this representative factor as sensory rather than as imaginal, a decision was reached in favor of the former alternative for all reagents save possibly the older children, H, Hd, M and L. Illustrations were given from human consciousness where a sensation performed a memory function or served as a link in a train of thought. Such cases have been termed "conscious attitudes" or "imageless thought." This function, as considered in this paper, was designated *sensory thought*.

8. The theory was advanced that such a function as sensory thought represents the highest grade of behavior in raccoons and probably also in children of some two and one-half years of age. This theory is supported by the hardly-to-be-doubted presence of sensations in animal consciousness and by the assumption that these sensations can function as the illustrations indicate that such processes do in human behavior. Such a theory seems more in accordance with the law of parsimony than would a theory which made images perform the representative function found in the raccoons and the child F.

9. From this theory, it follows that subjects may be put into at least four classes on the basis of the highest type of learning present in their behavior: (a) Absence of learning; (b) trial and error; (c) sensory thought, and (d) imaginal thought.

## VIII. APPENDIX

A. *Detailed Records of Two Rats and Two Raccoons.*—The data given in the following two tables are self-explanatory. They give the course of the delayed reaction tests as these were presented to the animals.

TABLE XV

## RACCOONS

Bob			Jill		
Delays	Trials	%	Delays	Trials	%
1st stage	10	100	1st stage	15	100
2nd "	10	100	2nd "	75	97
3rd "	179	67	3rd "	75	97
Controls were not perfected until the last 100 trials. Of these 72% were correct. Of last 50 trials, 80% were correct.			1 sec.	150	77
3 sec.	20	80	3rd stage	150	92
3rd stage	5	100	1 sec.	150	84
1 sec.	7	28	2 "	75	50
3rd stage	4	50	1 "	30	40
1 sec.	10	30	3rd stage	120	87
learning	17	52	1 sec.	155	83
3rd stage	50	88	2 "	105	86
1 sec.	11	72	3 "	45	57
3 "	50	56	2 "	105	79
1 "	80	70	3 "	75	56
2 "	50	78	Backgrounds were used now:		
3 "	20	90	2 sec.	90	58
4 "	62	69	1 "	45	71
5 "	56	82	3rd stage	150	97
8 "	20	55	1 sec.	75	93
5 "	40	75	2 "	105	96
7 "	10	100	3 "	90	66
8 "	189	55	2 "	45	55
last	48	79	1 "	45	71
	25	76	3rd stage	60	96
10 "	29	68	1 sec.	45	97
12 "	30	73	2 "	60	98
15 "	1	100	(backgrounds off for 30	100)	
5 "	10	50	3 sec.	45	93
15 "	10	60	4 "	45	62
20 "	18	44	Learning with light on 5 sec.		
2 "	3	33		30	100
20 "	10	20	Learning with light on 1 min.		
2 "	3	33		25	80
5 "	9	55	Wire release now, 10 trials daily:		
10 "	13	46	Learning	80	95
5 "	4	50	2 sec.	50	98
7 "	10	33	4 "	50	60
A— 5 "	49	75	2 "	100	89
7 "	20	75	last	50	96
8 "	36	80		50	88
10 "	50	72	4 "	50	70
12 "	10	90	5 "	30	90
15 "	10	90	last	40	84
20 "	96	64		50	70
last	46	71	6 "	30	86
			Small release again:		
			6 sec.	30	56
			(dropped)		



TABLE XV—Continued  
RACCOONS

Bob			Bob		
Delays	Trials	%	Delays	Trials	%
Two boxes, a and c, from now on:			2nd stage	10	100
20 sec.	20	95	3rd "	50	100
25 "	20	90	1 sec.	30	100
30 "	10	90	2 "	40	97
35 "	70	64	3 "	20	100
25 "	10	40	4 "	50	80
B— 15 "	10	60	5 "	100	85
• 5 "	10	33	6 "	50	88
Boxes a and b now used to break			7 "	100	83
up position habit.			8 "	100	70
5 sec.	60	30	7 "	100	81
3rd stage	10	40	8 "	110	89
Learning	295	97	9 "	80	81
2nd stage	240	86	10 "	50	86
last	51	93	11 "	80	73
1 sec.	51	94	10 "	50	66
2 "	50	96	Boxes b and c from now on:		
3 "	51	60	10 sec.	40	100
2 "	50	66	12 "	30	100
1 "	10	40	15 "	50	96
3rd stage	51	86	20 "	30	90
Learning c alone			25 "	50	86
	21	14	30 "	71	80
Learning all three			35 "	101	70 or less
C—	120	80	4 "	30	96
last	100	100	5 "	30	100
Learning with light on 5 sec.,			8 "	10	100
boxes b and c			15 "	20	55
	30	86	8 "	50	86
last	20	100	12 "	90	84
Learning with light on 1 min.			20 "	80	70
	30	90	first	50	76
Wire release, 3 boxes:			(dropped)		
Learning	50	100			
2 sec.	50	48			
Two boxes, b and c:					
2 sec.	50	94			

TABLE XVI

RATS

Rat No. 4			Rat No. 16		
Delays	Trials	%	Delays	Trials	%
1st stage	5	100	1st stage	50	96
2nd "	10	80	2nd "	100	98
3rd "	30	50	3rd "	110	83
2nd "	5	100	3rd " last	50	76
3rd "	15	40	2nd "	100	99
2nd "	75	70	3rd "	50	100
3rd "	50	60	1 sec.	40	50
2nd "	75	85	3rd stage	20	60
3rd "	25	92	2nd stage	100	99
1 sec.	30	60	3rd " "	50	96
3rd stage	25	88	1 sec.	30	36
1 sec.	25	52	Different backgrounds now:		
Different backgrounds now:			3rd stage	20	30
3rd stage	25	60	2nd "	90	96
2nd "	60	80	3rd "	50	58
3rd "	50	80	2nd "	100	99
1 sec.	25	64	3rd "	50	48
1½ "	25	48	2nd "	70	100
1 sec.	35	68	3rd "	50	68
1½ "	50	62	Two boxes now, b and c.		
1 "	25	64	3rd stage	50	92
3rd stage	50	64	1 sec.	100	87
Old backgrounds now:			2 "	40	97
3rd stage	65	61	3 "	50	98
2nd "	75	94	5 "	50	90
3rd "	60	66	7 "	35	60
Two boxes now, a and c:			Series with wire release:		
3rd stage	25	100	Learning	80	80
1 sec.	20	75	2 sec.	70	78
(dead)			3 "	50	88
			4 "	40	92
			5 "	80	78
			6 "	60	76
			7 "	50	90
			9 "	40	82
			10 "	30	50
			(dropped)		

*B. Notes on Raccoons.*—Davis<sup>54</sup> and Cole<sup>55</sup> have given an excellent description of the habits of raccoons in captivity. In the main my observations substantiate theirs. A few differences, however, may be noted. Davis lays particular stress upon one habit which his raccoons formed, viz., that of covering their excrement. As well known this is contrary to their behavior in the natural state. My four animals were confined in one cage, 10 x 10 x 14 feet, the floor of which was covered an inch or more deep in shavings. Yet in all the long months during which the animals lived there, they never formed the habit of covering their faeces. These were always voided along the walls of the cage, and not once have I found evidence of an attempt to cover them. Moreover both Mr. DeVry of the Lincoln Park Zoölogical Garden, Chicago, and Dr. Hornaday of the New York Zoölogical Garden give an unqualified "no" in answer to the following question: Will the tame raccoon bury or cover his excrement, if given the opportunity?

No trouble was found in adapting the animals to a rather monotonous diet of bread and milk, varied occasionally with raw meat. But the amount of the rations to be given was more difficult to determine. Throughout the spring, summer and into the winter, the raccoon will eat voraciously. But by the middle of January, unless strict precautions be taken, the animals will be so fat that they will refuse to work and will sleep almost continually. My animals were kept on a back porch which was not artificially heated, and which was but slightly, if any, warmer than out of doors. Whether they would have gone into a genuine state of hibernation had I not cut down their rations in the fall, I cannot say. However, in view particularly of Betty's behavior, I am inclined to think that this would have happened. From about the middle of January until late in April, my notebook indicates that Betty lacked a motive for working. Tests were made during this period in order: (1) To prevent forgetting of the problem, and (2) to give the animal food in order that it might not become too weak from a long fast. The cases of the three other raccoons are not so extreme as this one,—yet all become less eager for food during the last

<sup>54</sup> Davis, H. B. *The Raccoon: A Study in Animal Intelligence.* *Amer. Jour. Psych.*, vol. 18, 1907.

<sup>55</sup> Cole, L. W. *Observations of the Senses and Instincts of the Raccoon.* *Jour. of Animal Behavior*, vol. 2, 1912.

two months of the winter. Davis found that all of his raccoons hibernated during the first winter when they were kept out doors. But during the second winter when they were kept within doors, although unsupplied with artificial heat, they did not hibernate. How he regulated the food supply at this period, and whether the animals became sluggish, he does not state. Further observations should be put on record before the conclusion is reached that the raccoon will change so fundamental an instinct upon so slight a change of habitat. Both Mr. DeVry and Dr. Hornaday inform me that their raccoons do not hibernate in the winter, although the living quarters are no warmer than outdoors. In addition, Dr. Hornaday replies as follows in answer to the question of how to prevent hibernation: "By constant feeding. Bears and raccoons hibernate because they cannot find food in the deep snow. Our bears never hibernate because they are constantly fed." I, too, fed my animals regularly, although sparingly, in the fall, yet indications of a desire to hibernate were observed. The subject of hibernation is very poorly understood at the present time, even among the biologists proper.

Both of the authorities above quoted inform me that raccoons reach maturity at three years of age. I note this fact because Cole's statement that "The year-old raccoons apparently are not quite full grown"<sup>56</sup> may be as misleading to some as it was to me.

Two other observations may be noted in passing: (1) The raccoon appears to have a very acute vision. I have seen several individuals chase flies that were crawling upon the floor of the experimental room whose illumination was extremely low. (2) The price of keeping tame raccoons is eternal vigilance. In the spring when the "wanderlust" strikes them, they will gnaw wood and tear wire,—anything to escape. And the usual reward for attempting to catch a loose raccoon is a severe bite.

<sup>56</sup> Op. cit., p. 213.



