THE CHICAGO EXPERIMENTS WITH RACCOONS

L. W. COLE

University of Colorado

At the University of Chicago, three of Professor Carr's graduate students, Dr W. S. Hunter,¹ and Messrs. F. M. Gregg and C. A. McPheeters² have been engaged in repeating experiments similar to mine on raccoons, with results which are most gratifying to me.

Hunter (p. 46 and beyond) found the behavior of the raccoons as different from that of his dogs and rats as I found it different from the behavior of cats. He was compelled, as a result of his experiments, to give up the mere sensori-motor explanation of the behavior of these animals, nor could he attribute it to the association of motor impulses with a whole situation. Motor attitudes could, he thought, account for the behavior of the rats and dogs. It would not serve for an explanation of the reactions of the raccoons. At the close of my experiments, I, too, was compelled to regard those explanations as inadequate. He found that children and raccoons could respond successfully to a stimulus after a much longer delay than could the rats and dogs. He found for the raccoons a maximum delay of twenty-five seconds. The longest delay that I used was at least six seconds, or possibly nine seconds, if we consider only positive reactions of the animals. He compares the behavior of the raccoon favorably with that of a two-and-a-half-year-old child. Moreover, he admits an idea as a "possible cue" used by the raccoons and the children, as against purely motor or sensory cues, used by the other animals tested, though he prefers to attribute the reactions of the raccoons and those of, at least, the youngest child to "imageless thought."

Now that my experiments have been confirmed so fully I must

¹ Hunter, Walter. S. The delayed reaction in animals and children. *Behavior Monographs*, vol. 1, no. 1, 1913.

² Gregg, F. M. and McPheeters, C. A. Behavior of raccoons to a temporal series of stimuli. *Jour. Animal Behavior*, 1913, 3, 241-259.

regard them as established. This seems to me to be an item of progress. The psychology of mammals must now cease to be a mere generalization of the psychology of cats. And two of my former students, Professor DeVoss and Miss Rose Ganson, have recently shown what I believe to be an excellent reason why cats may not be expected to behave the same as animals with less defective vision.

We have, then, been driven from the cover of accounting for all mammalian behavior by the sensori-motor hypothesis alone, and psychologists are free at last to try to learn how animals differ in their behavior, instead of denying all differences. This will help enormously, for it may enable us finally to discover a psychology of the higher animals which can explain as well as deny, which can be taken out of the laboratory and yet bear the light of day and the scrutiny of intelligent persons who observe animals. This we have not had. When you meet an observer of horses, who thinks his horse remembers its home, you do not convince him by denying his statements and the evidence he gives, or by calling him "naively anthropomorphic," or by telling him that he did not record the date of the occurrence, or by hurling at him the anathema of "anecdotal psychologist" with opinions "too trivial for serious analysis or notice". A science which can only deny everything and explain nothing is no science and will never receive nor deserve confidence. It certainly was legitimate in 1898 to start by denying the worth of anecdotes of animals for comparative psychology, but only if by denying them we should eventually find a way to explain them, or at least to explain observations of animal behavior which are made almost every day. Experimental animal psychology is now sixteen years old. Consequently it must soon cease to be a generalization of the behavior of cats and take some step which promises eventually to explain animal behavior. Otherwise it must confess bankruptcy and its inferiority to common sense, and remain a sort of science which cannot emerge from the laboratory and which cannot be believed by the psychologist himself the moment he emerges from it.

I am in no hurry for this science to make progress but I should like to see it *take a direction* which promises something. I do not think that a devoted effort to adhere to an objective nomenclature, or to hang the fate of progress on some word, as behavior,

or behaviorism, or forever to deny what many observers affirm, is taking a promising direction. It is true that the professors at the University of Pisa saw Galileo drop the weights, and saw them reach the ground at the same moment, and yet refused to believe the evidence of their senses. Animal psychology which merely denies has had an influence in university circles similar to the influence of Aristotle on the professors at Pisa, but it has gained no such influence upon intelligent observers elsewhere. Instead of denying all psychic traits to animals would it not be better to deny our competence to explain more than the merest trifle of animal behavior? I believe that Hunter's confirmation of my results should give a new stimulus to investigators to devise ingenious new experiments suited to find new facts. That avenue seems more hopeful than a denial that there are new facts to be found, and affirming that animal psychology must become a sort of "organic physics."

It is of interest to observe also that while current mammalian psychology cannot come out of the laboratory, common sense observations continually find their way into it. In this paper of Hunter's, for example, one animal is a "stranger" to another, and so pays close "attention" to the latter's movements. Preliminary experiments make his animals "acquainted" with the place and apparatus. The raccoons display a directness and "sureness" in their behavior which defies the mathematics of chance. Their "attention" was "distracted" by "yelling at them at the top of my voice" (P. 71). (A procedure likely to make them fierce beyond recall, and which, perhaps, explains the last statement of Dr. Hunter's paper. It is gratifying to learn that this method of distraction was used only infrequently.) Attention and association are everywhere ascribed to the animals and not the association so accurately described by Thorndike, but association pure and undefined. Surely these are greater and more gratuitous assumptions to make than that a horse remembers his stable, even when distant from it, or that a raccoon remembers the box from which it is difficult for him to escape.

I realize that these remarks will expose me to the charge of being as completely deceived as was Herr von Osten, but his is not my position. My view is that "imageless thought," if Hunter's hypothesis is deemed correct, or, at least sporadic, images if my own explanation is accepted as the simpler one,

are perhaps so rare in animal experience that the most refined experiments will be required to discover and identify them; experiments beside which Hunter's experiments, and mine, will pale into insignificance, because of their simplicity.

There is still another reason for hoping that the study of animal intelligence may sometime get beyond the stage of dispute and denial. Dispute and denial are poor material to occupy the time of college students. Long ago I had to give up that kind of teaching and occupy myself with the more solid information which we possess, of animal sense organs, because dialectic should be taught in philosophy and not in science. Note the extensive work of Kafka³, the first volume of which has just appeared.

Dr. Hunter's agreement with me does not end with the facts noted above. He is almost persuaded to credit my experiments in putting the animals through the act to be learned, because he has observed the same sort of behavior in rats. At least he admits my apparent credibility relative to my four raccoons. It would be unfair to him, however, not to state his qualifications. On page fourteen he says: "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" (Italics mine.)

The reader may reply, "You can surely get but cold comfort from this admission." It gives you the merest semblance or 'appearance' of credibility with regard to your report on your four animals alone. I at least have not charged you with having invented your records." True enough, but the admission means that Hunter's rats, if they have not made a breach, have at least made a weak place in the blank wall of opposition and denial. The latter is definitely given up. How wonderful is the rat at undermining!

The cold comfort comes from the facts that those experiments of mine have not been repeated at Chicago University. I fear, because their raccoons would not permit it (Hunter p. 86). Now should the Chicago laboratory secure a toothless raccoon

³ Kafka, Gustav. Einführung in die Tierpsychologie. Leipzig, 1914.

what may not become of the credibility, temporarily and with qualifications, accorded me? But what disposition, pray, will then be made of the behavior of Hunter's rats? With many misgivings, therefore, I await the report of experiments which may even now be in progress.

In instinctive behavior the Chicago raccoons confirmed my observations rather than those of Davis. Yet I am sure Davis's report is correct, despite the authorities quoted against him, for I saw occasional cases of what he observed regularly. We must not be too cocksure in these matters. Remember that Audubon never saw his pet raccoon wash its food in the water beside it (Davis p. 451) ⁴ Yet that behavior gives to the raccoon both the name "lotor," and the name "Waschbär" ⁵

Interpretations: The reader who is familiar with Dr. Hunter's thesis will recognize the agreements I have mentioned between the behavior of my raccoons and those of the Chicago laboratory. Our interpretations of this behavior are entirely different, of course, except that we were both forced to give up the sensorimotor explanation. Forced from that position, I thought the animal might have memory, or at least a few memory images carried in visual terms, hence a visual image. I still believe that this is the simplest, or as some prefer to say, the most "parsimonious hypothesis." Hunter prefers the assumption of "imageless thought" or "sensory thought" to account for the raccoons' behavior, and for that of at least the youngest child. This "imageless thought" must be, at least partly, *visual*, for he says (p. 74), "In the present case there seems to be no room for doubt that the object reacted to was the light." The reader must remember that this light had been turned off for twenty-five seconds before the animal was permitted to react to it, in the maximal delays with raccoons, hence the "representative functions," next mentioned. For he continues thus: "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

⁴ Davis, H. B. The raccoon. a study in animal intelligence. *Amer. Jour. of Psychology*, 1907, 18,

⁵ I have to thank Mrs. R. M. Yerkes for calling my attention to this splendidly appropriate German name for the raccoon, and its superiority to the American name, whose source is not certain.

of movement—was constant from trial to trial, whereas a selective response must needs have an alternating cue". (*Italics mine*).

I know of no way in which light can stimulate these animals except *visually*. And when the animals were permitted to react, it was by means of a function representative (at least partly) of the lighted box. One would think that the simplest escape from this dilemma would be by means of a visual image. But no, it is visual in source or cause, yet imageless in content. We have often been led to believe that sensation gives a rather fundamental content to thought. Perhaps we may now teach that Helen Keller, for example, has both the *content* of visual experience as well as a knowledge of its relations. Loeb⁶ has recently given evidence to show that a retinal image produces a brain image, which corresponds with the former point for point "Diese Tatsachen enthalten aber, wie mir scheint, auch den Nachweis, dass in Gehirn ein Bild der gesehenen Gegenstände entzehen muss" (p. 1016). By Hunter's hypothesis all of this image forming apparatus is rather useless, for no mental image arises in the raccoon, nor perhaps in the youngest child, under the conditions of the experiment.

Doubtless it will occur to the reader of Hunter's paper that this explanation of the raccoons' behavior, by means of imageless thought, was in no way suggested to him by his experiments and seems to be a rather foreign addition to his thesis, forced upon him by the milieu or suggested by current discussions of the topic in human psychology. In order to use the concept to account for the results of his experiments he must make the claim (p. 77) that imageless thought is genetically prior to thoughts with images, and he must dismiss the opposite teaching as having "no factual basis" but seeming to be "the result of prejudice or of temperamental leaning." Then the point of origin of imageless thought is placed "at least as low as the raccoon" (p. 77). All this seems a trifle complex to me but the actual advance made is, now that the old explanation has been given up, that the reader may choose what hypothesis he will under the law of parsimony.

Doubtless psychologists will be more interested in Hunter's immediate explanation than in his final one, which I have already

⁶ Loeb, J. Die Bedeutung der Anpassung der Fische an den Untergrund fuer die Auffassung des Mechanismus des Sehens. *Zeit. f. Physiol.*, 1911, 25, 1015-1017.

outlined. When the conditions of his experiment demanded that the animals go to an electric bulb, whose light had been extinguished some seconds before, in order to execute a successful reaction, the rats and the dogs oriented toward the light, either with the whole body, or at least faced in its direction. They kept this orientation during the period of delay in so many of their correct responses that this "motor attitude" evidently served to bridge the time gap between the disappearance of the light and the release of the animal. Consequently their "motor-attitude" accounts for the success of the rats and dogs. The raccoons and the children did not even face the light in so great a proportion of their successful responses that the "motor-attitudes" hypothesis breaks down completely, as an explanation of their behavior.

As a result of this outcome of the experiments, Hunter (p. 80) decides that, "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." (*Italics mine.*) And again (p. 72), "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. 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." He has already said that the effective component of the stimulus was the *light*. Unless he denies, then, a visual content to this "factor," it is a visual, imageless thought. But since he does deny it a representative content, though it has a representative function, he terms it "sensory thought," though the stimulus has been absent twenty-five seconds in the longest delays of the raccoons. This "sensory thought" then becomes the imageless thought of current discussion, by the genetic reversal of current opinion on that subject that I have mentioned above.

It is interesting to observe how very "similar to the memory idea" is this "intra-organic factor." It is a residual effect of a sensory stimulus. It may be retained and revived, is selective, etc. Elsewhere (p. 69), he describes this factor as "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, . . . the nature of such a factor must necessarily be defined at present in *negative* terms." When this statement was written it evidently had not occurred to Hunter to place this negative thing in the positive category of imageless thought. His experiments were completely described before reaching this point. Hence, it seems to me that imageless thought was an afterthought, as an explanation.

On the second page of the paper we find this significant statement "In the interpretative discussion at the close of the present monograph, we shall be confronted with the possibility that images or ideas *may* have guided the reactions of the subjects. In 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 stimulus (object) or movement represented." So images *may have been present*. Yet throughout his references to my paper Hunter complains that I did not reach a *proof* of the presence of images. When his experiments were completed, he seems to be in much the same position. Just how an experimenter can give *proof* that animals remember or think, even in imageless thought, I am quite unable to guess. I thought that my animals gave *evidence* of possessing visual memory. Hunter's experiments strengthen this opinion of mine very much.

Like Brehm and all subsequent observers of the raccoon, Hunter has noted the fly-catching activities of this animal. He consequently accords to the "Waschbar" the possession of acute vision. In this he agrees with my report.⁷

Errors. On page eighteen, in re-describing some of my experiments, Hunter says, "a block with a *steeple* was placed in a hole," etc. With absolute confidence I must assure the psychological public that I used no "steeple" in my apparatus.

⁷ Cole, Lawrence W. Observations of the senses and instincts of the raccoon. *Jour. of Animal Behavior*, 1912, 2, 302.

I have, very rarely, *heard* the word "steeple" used for "staple," but never before have I *seen* it so used in a scientific monograph.

Again, I am regarded as having been "misleading" (p. 86) in my statement that "the year-old raccoons apparently are not quite full grown," for Dr. Hornaday and Mr. DeVry say "that raccoons reach maturity at three years of age." But do Dr. Hornaday and Mr. DeVry mean, therefore, that the raccoon accomplishes but one third of his growth each year, as Hunter seems to interpret them? I cannot believe it. I kept my animals three years and I wish now to re-affirm the statement above. They grew but little after the first year. Work and confinement may have stunted them, though they were fed each day to satiety. In parks I have now seen many raccoons of about the same size which mine attained. They had been in confinement for a long time so they must have been full grown. I have also seen a number of much larger specimens.

Criticisms of my Work. The introduction to Dr. Hunter's thesis takes the form of a fearful arraignment of both my experiments and my arguments. To use his own phrase, most of the latter "can be dismissed summarily" (p. 16). They are in turn dismissed summarily in favor of the sensori-motor explanation, so his theory of raccoon behavior at the beginning of his paper differs entirely from that at its close. I suppose that I ought to make some reply to these criticisms, but I shall be as brief as possible and at that I shall select only the most important ones. It seems better to omit any answer at all to such remarks as, "To some it may seem too *trivial* either for serious analysis or notice" (p. 10), a criticism which I seem to share with Lloyd Morgan and others, save that I have *persisted* in their trivialities.

Criticism 1 "Hence assuming the facts that Thorndike and Cole assume 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." (P. 15.)

Reply. Yet he later found just such a factor functionally present in raccoons.

Criticism 2 "To argue that this means *image* of apple is certainly naive at least. Could the raccoon not sense the apple when his nose was within a foot of it?" (P. 18.)

Reply. Hardly probable, since the floor between him and the piece was carefully rubbed with another piece of the same apple, and his forepaws were still moist with the pieces of apple he had already eaten. But note Hunter's argument (p. 27) that smell was eliminated in his experiments because the rat was given only a bite, "so almost no food fell on the floor." Food was used with the raccoons in the same way.

Criticism 3. Varying means to the same end. My data under this head are just as inconclusive as that presented above. (P. 17.)

Reply. Curious then that Professor James thought this "the mark and criterion of the presence of mentality in a phenomenon." "We all use this test," says James, "to discriminate between an intelligent and a mechanical performance."

Criticism 4 "The criticisms on Cole's entire work reduce to these. (1) The facts are either inconclusive or irrelevant. And (2) there is no evidence of adequate controls." (P. 20.)

Reply (a) Why then are the same facts, namely, responses to an absent stimulus, so satisfyingly conclusive of imageless thought? This recalls the remark of Hodgson, "What you know least about, assert to be the explanation of everything else." (I quote from memory.) (b) "No controls." This is the repeated cry in these papers. It seems probable from the statements of the papers that their authors did not read my account and that they misunderstood the few pages they did read. I shall show this in detail in showing that Gregg and McPheeters (and their experiment was planned by Hunter) have entirely misunderstood what I did.

In concluding the discussion of Hunter's report alone the points of similarity between his experiments and mine may be enumerated. He extinguished lamps which were used as stimuli, while I put a series of objects in view of the animal, then out of view again, and he must discriminate, under these conditions, between absent stimuli. Hunter secured delay by caging the animal, while I secured it by not feeding the animal until every member of the series had been put in view and (except the last member) out of view again. Sometimes six objects were used by me (i. e., each of three cards was shown twice). Hunter found it inapplicable to use the third light in many cases.

The second paper, that of Gregg and McPheeters, had no other object than "to demonstrate the inadequacy of Cole's experiment." (P. 258.)

They reconstructed my "card-displayer," except that the levers were not screened from the view or touch of the animal and a system of strings and pulleys was added which the animal could also see.

Then they gave the two raccoons *two days training on the levers alone without any cards attached to them.* (P. 245.) One of the two animals, Jack, failed utterly to discriminate. "Further training *might* have developed discriminative reactions in his case but time did not permit a continuance of the tests." (P. 246.) Jill discriminated between the two series on some basis, but Jill also "soon acquired the habit of standing close to the levers and touching her nose to them as they appeared." (P. 247.) Here, the reader will doubtless say that *all* analogy with my experiment ends. I should agree to this so far as the method and apparatus are concerned but it seems easier to change those than to change the nature of the raccoon, for there is a startling agreement between the behavior of their one successful animal and my four.

Let us find this agreement. In the training series Gregg and McPheeters kept two constant factors. (1) A "normal" order of lever positions used, according to their respective distances from the animal. (2) They always presented the levers in series of three. Jill reacted to the order of lever positions chiefly, perhaps (p. 249), but she responded partly to the threes, for they say (p. 252), "Positive reactions of food getting may be stimulated successfully by any of the following groups, 1-2-3, 1-3-3, 1-2-2, 2-2-2, or 1-1-1. Likewise, inhibition, or negative responses may be stimulated by either group 3-3-3, or 2-2-2. The nature of the stimulus is relative to the character of the group with which it is *alternated*." One cannot help asking, why continue to alternate by threes only, unless they meant to teach the animal to respond to alternate threes? Why not alternate by sixes as I did? This was one of my "controls," which they have overlooked.

Jill reacted to the two constant factors. In my experiments only the color (and brightness) of the cards was kept constant. My four animals responded to that. In both the Chicago

experiments and my own the raccoons responded to *the constant factor*. What more could they do, pray? This seems to me to be an excellent example of the method of agreement.

But this was the behavior of Jill, the single raccoon which succeeded in discriminating in their experiments. One would suppose that no very weighty conclusions would be drawn from the behavior of the animal which failed. But he is said to have responded to the *sounds* of the levers. Their "usual sound." (P. 248.) (Why not make the levers noiseless?). This animal then responded to sound, perhaps partly to lever order and, I have no doubt, to any other element of the situation which was left constant, and which also enabled him to get food. "He seemed to watch the peep hole, although possibly he was merely listening for some sound upon which to base his reactions" (P. 246), so they set a metronome going to drown the noises made by movements of the experimenter!

I confess I can see nothing in these experiments except a rather determined effort to divert the animal's attention from the cards and to get him to respond to the levers. The following items seem to show this

1. Two days preliminary training on levers alone.
2. The board screen was reduced to "about five inches" in width (p. 244), thus showing apparently two thirds of the length of the levers, if Figures 2 and 4 (pp. 243 and 247) correctly represent the apparatus.
3. Putting the cards above the raccoon's line of vision, if Figure 4 is correct
4. Converting my visual experiment into a tactual one by letting the raccoon touch the levers.
5. Adding the cue of noises in operating the levers, as well as noises due to the experimenter's movements.
6. Each of the three strings attached to the levers (Figure 4, p. 247) must have changed from slack to taut *before* the lever appeared, thus further directing the animal's attention to the levers' positions. I am unable to find "controls" against the animals having reacted to the strings.
7. Feeding the raccoon for having reacted to the levers
8. The colored cards were much smaller than mine.
9. Finally only one of their animals succeeded in discriminating as compared with four of mine.

"The essentials of Cole's apparatus and method were duplicated in our experiment." (P 244.) Truly, with all these carefully arranged differences, I am quite unable to find that the "essentials" of the experiment were even *similar* to mine, but the reader may judge for himself. Any one who is familiar with my paper will remember that only a small part of the upper portion of the lever projected above the screen board. To be specific, my notes of Dec 6, 1905, state that the lever "when upright" extended "one inch above the upper edge of the front piece."

"Controls" By the charge that I did not employ "adequate controls" is meant chiefly that I did not guard against discrimination by position of cards and levers nor against discrimination by cues given by the experimenter. Let me call attention to two items which my critics have overlooked relative to the first precautions, and quote from notes of the experiments. On page 228, I say, "During one test red would be on the forward lever, one inch in front of the other, during the next test on the rear lever. The animal could not, therefore, react to the position of the cards." I did not re-state this precaution in the portion of my account on which the Chicago laboratory based its experiments, but one presumes that a critic reads completely the paper he criticises. To show that this precaution was kept up during the three-color work I will quote my "daily plan" for one animal for three consecutive days.

"April 23. Jack. Same as preceding. Blue middle, orange back, white front."

"April 24. Jack. Three colors Orange front, white middle, blue back."

"Jack. April 25. Three colors. Blue front, orange middle, white back."

It is evident that each card occupied every possible position in each three consecutive tests, and that no card occupied the same position for any two tests. Does this look as if I took no precautions against the animals reacting to the positions of the cards? I find no such precautions as this, to leave only the colors constant, in the work of Gregg and McPheeters, so it seems that the animal was fed for depending on another cue.

But the uninformed reader may ask, "But what of *lever* position?" At the beginning of each days work the levers

were "strung" on their supporting pivot in any order in which they were picked up. We did not remove them from the room in which the raccoons were kept and we generally found them scattered about the room. The levers were all alike so far as we could detect, until having split one, we replaced it with one having a "new" appearance. This should have brought a new type of result if the animals were responding to the appearance of the levers.

It is true that this change of card position is mentioned briefly at the outset of the experiments instead of within the portion on which the Chicago experiments were based. But was no further precaution taken which was described on the final pages of report? On page 259, Table 11, I record that for two hundred trials the threes were "shown twice." Since this has been unnoticed or misunderstood let me explain it. It means that I would show red, red, red, red, red, red, and the animal must stay down through it all. Then came white, orange, red, and only then would the animal climb up on the high step to be fed. Thus nine movements were made, and *all the levers were used*. Then followed white, orange, red, and the animal reacted positively and was fed. Thus he could hardly have been responding to alternate threes, or to lever position. Note also that there was an abrupt transition from showing the cards by threes to showing them by sixes. Yet the animal gradually learned to discriminate in this complicated experiment in which all factors were different, except the colors of the absent cards. I describe this showing the cards by sixes at the bottom of page 258, and refer to it as "while you raise three or even *six* colors, again on page 261." Perhaps this detailed account of the precautions taken to guard against discrimination by threes, and against discrimination by position will serve to convince the reader that the experiments were not so careless or hasty as my critics have supposed.

But it is further assumed that I mixed the experiments in which the experimenter manipulated the levers with those in which the animal was permitted to claw at them. The two types of experiment were separated by months. My paper states (p. 233) that no tendency to claw at the levers appeared for *six weeks* of the first type of experiment. After it did appear, clawing at the levers was not permitted until we had learned

what we could by the experimenter's operating them. Nor did the raccoons attempt to claw at the levers, if the experimenter manipulated them rapidly. In fact we developed the habit by moving the levers slowly. This confusion on the part of Hunter, Gregg and McPheeters appears to be due to my giving a logical, instead of a chronological account of my experiments.

The behavior of my raccoons was not, therefore due to touch. Consequently Hunter's experiments with lights is more similar to mine than that of Gregg and McPheeters, whose "card-displayer" had some points in common with mine.

As to cues from the experimenter, I always extended my hand as if to feed the animal, at the negative as well as at the positive series. My notes contain many instances, at first, of this response to the hand. These were of course counted against the animal, and finally he ceased to be influenced by the movement. Different experimenters operated the levers and, in one case, it was found that the animals were responding to unconscious movements of the operator. This is mentioned in describing the vision of the raccoon.⁸ This experience shows that if the mere presence of the experimenter, or his breathing, had been the cue to which the animals were responding the raccoons would have made far better records than they did, and the work of months would have been reduced to days. I should still prefer to have the experimenter present rather than to use the system of strings, which caused the noise, the peep hole, the opening for food, and to permit the noise of the experimenter's movements, which had to be overcome by a metronome, all of which were used by my critics.

Conclusions: It is noticeable that, so far as Gregg and McPheeters draw a conclusion, they ascribe the raccoon's behavior to "motor attitudes," "sensory attitudes" and, if images were present in our animal, they must have been kinaesthetic, i.e., imaginal attitudes." (P. 258.) Thus they give the explanation of the raccoon's behavior which Hunter found was entirely inadequate to account for it, but which, he believes, does account for the behavior of the dogs and rats. Perhaps, at the time their experiments were made, Hunter's results were still incomplete and it was assumed that the raccoon's behavior would

---

⁸ Cole, Lawrence W. Observations of the senses and instincts of the raccoon. *Jour. of Animal Behavior*, 1912, 2, 302.

be found in nowise different from that of the dogs and rats. At any rate, we now have three different hypotheses to account for the behavior of raccoons.

1. Attitudes, motor, sensory or imaginal. Gregg and McPheeters.

2. Not attitudes, but imageless thought. Hunter.

3. Visual memory, at least sporadic. Cole.

Truly, "Homines perfacile credunt id quod volunt."