HABIT FORMATION AND HIGHER MENTAL PROCESSES IN ANIMALS

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The present review covers primarily the year 1926, though earlier and later contributions will be referred to where the argument seems to demand it. The last review on our topic appeared in the Bulletin (**36**) a year ago and covered the years 1921–1925, inclusive.

The experimental literature for 1926 can be treated under two heads: I, experiments which measure with further precision the effect of relatively mechanical factors in learning; II, experiments and investigations which lead to a new nonmechanical envisagement of learning. Finally, under a third head, III, the reviewer will indulge in certain theoretical speculations.

I. Mechanical factors: Under this head will be included:
  A. Experiments measuring sub-mammalian learning.
  B. Experiments dealing with the reliability and validity of maze scores.
  C. Experiments seeking to measure the relative effects of different distributions of practice.
  D. Experiments measuring the effects upon learning of special physiological conditions, including cerebral and cerebellar destructions.

II. Nonmechanical factors: Under this head will be included:
  A. Experiments investigating the " Gestalting " of perceptual fields.
  B. Experiments leading to a new envisagement of maze learning.
  C. Experiments concerning " insight " and " higher mental processes."

III. Theory: Under this head the differences, if any, between trial and error learning and " insight " learning will be discussed.

I. EXPERIMENTS WHICH MEASURE WITH FURTHER PRECISION THE EFFECT OF RELATIVELY MECHANICAL FACTORS.

A. *Sub-mammalian learning.* Garth and Mitchell (8) taught a land snail (*Rumina decollata,* Linne) a simple T-maze made of glass. An electric light suspended at the entrance served as the motive; for the animals are negatively phototropic. If the snail entered the blind, it was punished by means of a heated wire. If it chose correctly, it found a dark box at the end of the true path in which it was allowed to remain for a short rest. Four to six trials were usually given in a day. Normal learning curves were obtained for both time and errors. It may be recalled in this connection that Thompson (33) was unable to teach a *water snail* a Y-maze. Is the water snail fundamentally inferior to the land snail, or was not the difference probably due to some difference in the techniques of the two experiments?

Schaller (31) taught the water beetle (*Dytiscus*) to find food at a particular place. A bit of meat was fastened upon a stick, the lower end of which projected into the water. The presence of such food and the route to it were first indicated to the beetle by trailing a brush moistened in sweetened water before him, thus finally bringing him out of the water and up the stick to the booty. After enough repetitions the animal learned to go to the stick and climb up it to the point where the meat was located, in response simply to a food signal. This food signal consisted merely in the dropping of a bit of sweetened water in front of the animal. In other words, the beetle, when he received this signal, went without further guidance to the stick, climbed up and found the food.

Schaller (31) also taught these beetles an avoidance response. He taught them, that is, not to bite at a wad of cotton which had been soaked in a salty meat solution, although they would continue to bite at one which had been soaked in a sweetened meat solution. Originally they had snapped with equal readiness at both. This discrimination avoidance was produced as a result of a training series in which the salty meat cotton was impregnated with quinine. After such a training series the beetles refused to bite at the salty cotton even though no longer impregnated with quinine.

Finally, Schaller (31) taught minnows (Ellritzen) to discriminate a food color from a nonfood color, and a food geometrical form from a nonfood geometrical form. Bits of edible meat and similar bits of meat made nonedible by impregnation with quinine were suspended upon the tips of needles about half a centimeter above the surface of the water. These needles were also mounted about one centimeter higher up with disks presenting signal colors (or signal forms). The fish readily learned to discriminate the edible food from the nonedible food on a basis of the colors or simple geometrical forms (rectangle, star, circle, triangle) mounted above the edible (nonedible) foods. These results confirm previous findings (See previous summary (36)) as to the goodness of fish vision and the possibility of teaching them discrimination habits.

B. *Reliability and validity of mazes. Methods of scoring.* Warden (41) has examined again the data of his previous study on the "Distribution of Practice in Animal Learning" (40) to discover how his results then obtained would be affected if he had scored in terms of other norms of mastery. He compares the outcomes, using two norms of accuracy: *i.e.,* (1) counting as errors all blind entrances however slight; and (2) counting as errors only blind entrances of more than ten centimeters; and pairing each of these with three different norms of fixity: A, a norm defining learning as complete when one perfect run (in terms of the given accuracy norm) has been obtained; B, a norm defining learning as complete only when four out of five such perfect runs have been obtained; and C, a norm defining learning as complete only when nine out of ten such perfect runs have been obtained. The same general conclusions as to the effect of the different distributions of practice upon the ease of learning showed up with each of the resultant six methods of scoring though there were some minor shiftings about, and though the degree of contrast between the results for the different distributions of practice became less marked as the more rigid norms were used.

The reviewer's own reaction to the whole matter of norms is, first, to feel that as far as *fixity* is concerned the problem can best be met, not by attempting to score in terms of *trials,* but by substituting instead the procedure of running all animals the same number of trials,—this number great enough so that at the end of the experiment nearly all the animals shall have finally achieved mastery. In this way the learning curves for individual animals or groups of animals can be compared throughout their *whole* lengths. And the differences which are sought for, between individuals or between groups of individuals, can be examined at all the different stages of these learning curves. For it may be that the beginnings and ends of the curves may have different stories to tell. And if so, any attempt to lump such differences into a single figure, such as differences between total trials, may be misleading. Secondly, the reviewer would feel that as regards *accuracy* (*i.e.*, what length of blind entrances to count as errors) further investigations directly centered on this problem are needed. Thus it may be noted that some work, done at California but not published, suggests that although partial errors (relatively slight blind entrances) correlate relatively highly with whole errors, they are nevertheless affected also by something like emotional factors which do not enter into whole errors (*i.e.*, complete blind entrances) to the same extent. But, if such be the case, then the decision as to whether to count slight blind entrances or not will depend upon what one is seeking to measure. Obviously, more work needs to be done to discover just what the interrelations between the different degrees of blind entrances are,—how nearly they do or do not measure the same thing and that the thing that one is or is not interested in investigating.

Reliability. Hunter and Randolph (**14**) present further evidence as to the unreliability of the maze. They used a simple T-maze with angora goats. The animals had to pass through the maze in going from their corral where they spent the night to the pasture where they spent the day (the day's ration of food and water was obtained in the pasture). Time records only were recorded. Three groups of three trials each were given. The second group of three trials was separated from the first by a rest interval of thirty days, during which the animals spent night and day in the pasture. The last group of three trials was separated from the second group by a similarly spent rest interval of 120 days. The correlations between the scores for each group of three trials correlated against the scores for each of the other two groups ranged from .065 to .316. In short, these data substantiate the earlier findings of Hunter and his students as to the unreliability of mazes.

At this point the reviewer must report, as will appear in forthcoming articles, that both Tolman and Nyswander (**37**), and Stone and Nyswander (**32**) have been able to obtain, using rats in mazes, reliability coefficients of *.90 and over.* And, very briefly, their results indicate that the trouble with previous mazes has been that the latter have been too short and too heterogeneously constructed.

Carr (**6**), in a criticism of Hunter, takes particular exception to the latter's conclusion that because mazes give low reliability coefficients, therefore the results from them are meaningless in the case even of group differences. As Carr points out, two issues are involved: (1) the use of the maze as a measure of individual differences; and (2) its use as a measure of group differences. Hunter is right that if mazes continue to give low reliability coefficients, they must be abandoned for measuring individual differences. It does not, however, follow, as Hunter asserts, that they must also be abandoned for measuring differences between group means. As Carr points out, the usual statistical formula for the significance of a difference between group means, *i.e.*, the difference between the means divided by the sigma of this difference, contains no reference to reliability coefficients. Carr suggests, therefore, that Hunter address himself to the statisticians.

Hunter (**13**), in reply to Carr, reiterates in a general fashion the importance of reliable instruments. And, finally, he quotes a letter from Kelley in which the latter suggests as a practical rule that if an investigator is to work successfully on individual differences, he must use an instrument with a reliability coefficient of .90 or better; whereas if he is to work on group differences, he needs only an instrument which gives a reliability of not less than .40. Kelley does not explain the theoretical justification for this difference in the two requirements.

Tryon (**38**), taking this controversy between Carr and Hunter as his starting point, seeks to derive an improved statistical formula for the significance of the difference between two means, which shall contain a reference to the reliability coefficients. And this corrected formula turns out to be such that it appears that as the reliability of one's instruments goes down, any difference between two group means actually obtained (assuming, of course, large numbers) seems to become more rather than less significant. This, at first sight paradoxical, conclusion has, it would seem, none the less a certain amount of common-sense justification. For all it means is that if a difference between the means of the two groups is *actually obtained, in spite of* the unreliability of the instruments, the chances are greater that there was a true difference behind and producing this obtained difference. For the unreliability of the instruments would, as such, tend to obscure any such true difference and to reduce the probability of an obtained difference being found.

Finally, whether or not this new formula of Tryon's proves able to withstand critical attack, it seems to the reviewer at any rate obvious that work with group differences does not require as reliable instruments as does work with individual differences (Kelley also indicated this in his letter quoted by Hunter). For if the groups worked with are large enough and if a real difference between their respective true means does exist, the latter will in time (*i.e.*, after the measurement of enough cases) be bound to manifest itself, even though each individual measurement be relatively unreliable. (For an amplification of this argument, see the article by Tolman and Nyswander (**37**) already referred to.) The real criticism, as the reviewer sees it, to be directed against most of the conclusions drawn from work with animal mazes in the past lies not in the unreliability of the mazes used but in the smallness of the numbers of animals that were run. The groups run have usually been so small in number that no statistical criteria of any sort could properly be applied to them.

C. *Different distributions of practice.* Williams (**44**) tested some seven dogs (five cocker spaniels and two Scotch terriers) in three successive discrimination experiments: *i.e.*, (1) the choice of the duller of two five-centimeter circles; (2) the choice of a flickering as against a steady light; (3) the choice of a steady as contrasted with an intermittent buzzer. After the given discrimination had been practiced to a given standard of perfection, a ninety-day rest interval intervened and a retention (relearning) test was given. Then the next type of discrimination and its retention were tested. Three different arrangements of practice were distributed among the three different problems and the seven different animals. These arrangements of practice were as follows: A, 5 trials 4 times a day, 4 hours apart; B, 10 trials twice a day, 10 hours apart; C, 20 trials once a day. (In the relearnings the repetitions were consecutive.) *As to original learning,* the greater the distribution of practice, the more repetitions were required for the original mastery. *As to retention,* it appeared that the greater the distribution of practice, and hence the greater the number of repetitions in the original learning, the greater the retention.

Williams also examined different distributions of practice in maze learning. His dogs learned eight successive mazes, and again three different arrangements of practice were distributed among these different problems and the different animals. Arrangement A consisted in 3 trials 4 times a day; arrangement B in 6 trials twice a day; and arrangement C in 12 trials once a day. No retention tests were given. Here the greater the distribution of practice, the sooner the original learning.

In evaluating both these sets of results, it must be noted that the number of animals was so small and the interacting effect of practice of one problem upon the next so uncontrolled that the findings can be taken as suggestive only. At their face value, however, they seem to indicate that distributed practice is more favorable for original learning in the case of maze experiments, but less favorable in the case of the discrimination experiments. This greater length of time spent in the original learning in the case of the discrimination experiments seems to be compensated for, however, by better retention. Whether or not this difference as regards the effects of distributed practice upon the original learning of discrimination problems and of maze problems suggests a fundamental difference between these two types of learning is a point wanting further investigation.

D. *Special physiological conditions.* Ball (3) investigated the influence of the oestrus cycle in white rats upon maze learning. The females were examined by the smear method. She ran an experimental group (16 animals), one trial each oestrum (that is, one trial approximately every four days), and a control group (16 animals) one trial also approximately every four days, but during dioestrum. No significant differences appeared between the two groups. She concludes that, at least when food is used as the incentive, the female sex cycle does not influence maze learning.

Anderson and Smith (2) contrasted the abilities of stunted rats and control rats of the same age and previous training to relearn a maze and to learn a new maze. Two methods of stunting were used: (1) a qualitative method in which the animals were fed the usual weight of food but in which gliadin was substituted for casein as the protein in a "normal" diet; (2) a quantitative method in which the complete normal diet was fed but in lesser amounts. Results: (1) the qualitatively stunted animals, while stunted, made uniformly better time and better error records than the controls; (2) the quantitatively stunted animals, while stunted, made better time records but somewhat worse error records than the controls. When realimentation was established, both groups again approximated the control group in both time and errors. The authors suggest two possible explanations: one, that stunting produces its general tendency to enhance learning primarily through increasing the drive; the other that it, as such, retards physiological age, and that it has already been demonstrated that younger animals learn somewhat faster than older ones. In this connection the reviewer would like to suggest that such findings as there may be on age differences are also, he believes, to be interpreted in terms of drive, that is, that the younger animals, as such, have a greater hunger and therefore a greater motive for running the maze than do the older animals.

Lashley (19) and Lashley and McCarthy (20) have during the year added two more contributions to the now extensive list of Lashley's investigations upon brain function in learning. The first of these constitutes the seventh in his series entitled "Studies of cerebral function in learning." It was summarized in our BULLETIN review of last year (36). The nature of the investigation it reports is indicated by the specific title, "The relation between cerebral mass, learning and retention." The procedure of the experiment consisted in destroying varying amounts of the occipital region in different rats and then training them in the visual discrimination habit. It was found that the ease of the original learning was unaffected by the amount of the occipital region destroyed before the beginning of the learning. On the other hand, when the occipital lesions were made after the habit had been acquired, a very high correlation was found between the amount of the destruction of the region and the resultant loss of the habit as measured by a retention test. This correlation between extent of lesion and loss of habit was .70 plus. There was no relationship found, however, between the specific part of the region destroyed and the loss of the habit. The general conclusion arrived at as a result of this and the preceding experiments in the series may be summed up in Lashley's own words: "No part of the cerebral cortex is better adapted for the formation of any particular habit than is any other. Any anatomically continuous cerebral area may serve the learning function, provided it presents a sufficient mass. This must mean that in a problem situation the effects of stimulation irradiate to all parts of the cortex. As the habit is established there comes into being a definite structural modification having topographical position and capable of destruction by brain injury. The learning process is independent of locus, whereas the mnemonic process or engram has a definite localization."

The second paper for the year, that of Lashley and McCarthy (20), investigates the survival of the maze habit after cerebellar injuries. Seven rats were taught a rectangular maze with eight *culs-de-sac* and later given a retention test. Then the cerebellum of each rat was more or less completely destroyed by cauterization through a median trephine opening just back of the parietooccipital suture. A second retention test was then given. There was practically no loss of the habit save in the case of one animal. In the case of this animal there proved, however, to have been a widespread infection which prevented any clear interpretation of this loss of the habit. It is noteworthy, however, that this animal relearned the maze with perfect success, although the postmortem showed that his cerebellum had been completely destroyed. In the case of two rats the cerebellar lesion was preceded by enucleation of the eyes. These animals also retained the habit successfully. This last proves, the authors think, that these animals did not need to fall back upon exteroceptive cues but were continuing to react on the basis either of kinesthetic stimuli or of some centrally organized mechanism. (They feel that the maze itself was constructed and controlled so as to offer no differentiating olfactory and tactile stimuli.) Their final conclusion is that the cerebellum plays no part in the learning or the retention of the maze habit.

One may here briefly summarize and evaluate all these contributions under this first heading of mechanical factors. The reviewer would schematize them as follows:

A. Further evidence that sub-mammals (fish and waterbeetles) are capable of acquiring both discrimination and food position habits.

B. Relatively unreliable mazes can be used to measure group differences provided the groups are large enough. Properly constructed difficult mazes will give high reliabilities even with rats. (At the date of the present writing, the articles in support of this latter finding have not yet appeared, but are about to appear.)

C. Further conflicting evidence on the relative advantages of distributed and of concentrated practice. Some suggestion that maze habits and sensory discrimination habits behave differently in this matter. Evidence that longer learning time induced by more distributed practice makes for better retention.

D. The female sex-cycle seems to have no effect upon maze learning.

The cerebellum is not involved in the maze habit.

And as regards the cerebrum: "No part of the cerebral cortex is better adapted [save for the closeness of its anatomical connections to the given incoming and outgoing paths] for the formation of any particular habit than is any other. . . . This must mean that in a problem situation the effects of stimulation irradiate to all parts of the cortex. As the habit is established there comes into being a definite structural modification having topographical position and capable of destruction by brain injury. The learning process is independent of locus, whereas the mnemonic process or engram has a definite localization."

So much for these already accepted, relatively mechanical factors. No particularly new envisagement as to the nature of learning has arisen out of them.[1]

II. Experiments and Investigations which Lead to New Nonmechanical Envisagements of Learning

A. *The "Gestalting" of the perceptual field.* Under this one head we shall group together three types of experiments as follows: (1) Experiments proving that animals (hens) respond to object qualities rather than to mere sensation qualities. (2) Experiments investigating "abstraction," *i.e.,* the tendency, after animals have been taught to discriminate two objects differing both as to color and as to form, later to discriminate in transfer experiments according to the previous form, or according to the previous color (hens, fish). (3) Experiments investigating the perceptual cues involved in "homing" (bees, ants, wasps, pigeons).

(1) *Object-qualities.* Gotz (9), under carefully controlled conditions, trained hens first to choose the larger of two grains of corn, when both grains were at approximately the same distance at the moment of choice. He then placed the larger grain at a sufficiently greater distance from the choice point so as to cast a retinal image only one-thirtieth of that cast by the smaller grain. The hens still chose the actually larger grain. This "Sehgrössenkonstanz" maintained itself even through variations of perspective foreshortening due to turning the grains at different angles relative to the vision of the oncoming bird.

(2) *"Abstraction" experiments.* Schaller (31), in addition, as reported above, to teaching his minnows (Ellritzen) to distinguish a

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[1] This does not hold, perhaps, for some of Lashley's work, particularly some of the experiments reported in the review a year ago. Those results indicated that the *simple* reflex arc concept of habit is no longer tenable. Thus a rat who had learned with only his right eye could then perform the habit without further practice equally well with only his left eye. And a monkey who had learned to open a box with his right arm, his left arm being at the time paralyzed due to cerebral lesions, could later without further practice, when this right arm had itself become paralyzed, perform the habit with his left arm, which in the meantime had recovered.

food color from a nonfood color and a food form from a nonfood form, also investigated the relative dominance of color or of form when the cues presented differed in both. Thus, after he had first taught the fish to discriminate two cues which differed both as to color and as to form, he then transferred the animals to a new pair of cues in which the connections between color and form were reversed. Presented with this new pair of cues, one fish responded according to the previous color contrast, the other fish according to the previous form contrast. It appeared, however, that if the two original cues differed outstandingly in color, then both fishes tended to respond to color in the second arrangement; while if the original cues had differed outstandingly as to form, both fish responded to form in the second arrangement.

Kroh and Scholl (18) made a similar investigation of the relative dominance of form and of color in hens. The birds were taught to pick from blue circles but not from red triangles. They were then presented with red circles and blue triangles, with the result that they then picked from the blue triangles, thus proving the dominance of color. A control test was instituted in which the birds were presented with circles and triangles both white. They picked with equal readiness from either. That is, no influence of form whatever held over from the original training. They were next, however, trained successfully to choose the white circles as against the white triangles; thus showing that the birds could discriminate according to form when necessary. The authors compared their results with those of previous investigators (*e.g.*, Révész, 30) and suggest that all such experiments should properly be designated investigations of "Teilinhaltlicher Beachtung" (discriminative response to partial contents) rather than investigations of "abstraction." The latter term should be reserved for cases involving higher conceptual processes. This recommendation seems sensible.

(3) *Perceptual cues in homing.* Rabaud presents certain new experiments (28) and an extremely important and thoroughgoing theoretical analysis based upon all the experimental data to date (29) concerning the perceptual cues to be assumed in the explanation of the homing instincts of insects (wasps, bees, ants) and of pigeons. A really adequate summary of the argument is possible in a short space. The author divides the process of homing into three parts: (a) the going in the correct direction to the immediate general locality of the nest; (b) the recognition of this locality; and (c) the

recognition of the nest itself. The general conclusion seems to be that the great majority of the evidence indicates as regards (a) that the animals in finding their way back to the general location of the nest use ordinary perceptual cues either of vision or of olfaction. primarily the former in the case of bees, wasps, pigeons, and primarily the latter in the case of ants. (Though it appears that if the usual cues fail, others can be resorted to.) As regards (b) and (c), the recognition of the general locality of the nest and the recognition of the nest itself, it appears that the two processes are intermingled. In other words, the nest does not seem to be singled out and recognized, as such, but rather it sinks into the other features immediately surrounding it. Thus if the nest be moved to one side during the absence of the animal, the latter upon returning is apparently guided by the general complex of stimuli coming from various adjacent environmental features and is led by them to try to find the opening of the nest at the old place. In other words, to talk in Gestalt terms. the " figure " which stands out for these animals seems to be not the nest, as such, standing out as it would for us upon the " ground " of the other surroundings, but rather the " figure " which stands out is apparently a complex uniting within itself both nest and surroundings. The nest is not an object independent of its immediate environment, but rather it and the immediate environment make a single object. Rabaud's article contains an important bibliography of seventy-one titles.

Summing up under the " Gestalting " of the perceptual field, it would appear: (1) that animals normally tend to react to object-qualities, *i.e.,* to qualities which maintain their constant size-, and shape-, and quality-values, rather than to momentary sense-qualities *per se;* (2) that when forced to discriminate between two objects differing in both color and form they tend to react to one of these, the one most striking under the given circumstances; and (3) that in finding and recognizing locations (as in homing) they are not rigidly bound to stimuli coming from any one sense mode.

Finally, it ought to be pointed out that this matter of the " Gestalting " of the perceptual field is an important item for the understanding of learning since the way a given animal is or is not able to "gestalt " his field will set the limits within which his learning, whether of the trial and error or the insight variety, can take place. The learning of animals is not, it would seem, a simple mechanical affair of unit stimuli and unit association bonds, but is rather a process arising out of and always statable in terms of meaning. Indeed, according to the Gestalt psychologists (see Koffka, **16**, **17**, in particular), learning is to be described primarily as merely a " re-Gestalting," often of a simple perceptual order, going on within the phenomenal field. (See also Hempelmann's recent and important book on animal psychology, **10**, p. 509, for an endorsement of this general Gestalt point of view.)

Let us turn, now, to the new experiments which, it seems to the reviewer, emphasize and reinforce the necessity of some sort of meaning concepts for the description of learning.

B. *New envisagements of maze learning.* Washburn (**42**) tested twenty white mice in a Watson circular maze and at the same time measured the strengths of their daily hunger drives. This latter she did by measuring the time spent at the end of each run actually eating (or, rather, drinking) in the food box. The animals had dried bread in their living cages continuously, but obtained their day's ration of liquid (*i.e.,* milk) in the food compartment only. They were allowed to drink as much as they wanted and the actual time spent in such drinking recorded. She also measured the actual speed of running by noting the time spent in actual movements, and also by tracing the total distances traveled. Her *first finding* is that in the case of some animals there was a high daily correlation between degree of hunger, as measured by length of time spent in lapping milk, and corresponding speed of running, while in the case of other animals there is little or no such correlation. She designates the group of animals for which there is such correlation as " hunger-driven " and the others as " activity-driven." Her *second finding* is that in the case of the hunger-driven animals there was also a high correlation between daily degree of hunger and goodness of error score on that day, while for the activity-driven animals such correlations were low. Her *third finding* is that as between individual mice there was no correlation between total learning ability and the degree to which the animal tends to be hunger-driven. In other words, in spite of the fact that one might expect hunger-driven animals to be, as such, better learners than activity-driven animals, such differences, if operative, were at any rate not sufficient to obscure the greater innate differences due to differences in " general ability " or " I.Q."

To the reviewer's mind, this experiment of Miss Washburn's, though only a beginning, is most important. It brings to a sharp focus, in a way that has not been done before, the fact that our animals run mazes for divergent motives. And may it not turn out that some of the differences as to methods and kinds of learning, which have been obtained, may eventually be traceable to such differences of motive? To suggest an example: it will be remembered that whereas Peterson (27) found that short blinds are eliminated first, White and Tolman (43) found that long blinds are eliminated first. May it not be that such a difference might be due to differences in the degrees of hunger under which the two sets of rats were run? It might be that activity-driven rats would *prefer* long blinds, "as something capable of much exploration," whereas hunger-driven rats would, rather, *dislike* long blinds "as causing undue delay in reaching the food." If so, then if Peterson's rats were less hungry than White and Tolman's, we should expect the contrast between results which was actually obtained. Such a possibility is not, perhaps, to be taken particularly seriously. It is merely thrown out by the reviewer as an illustration of the kinds of possibility which, in his mind, Miss Washburn's findings open up.

Alonzo (1) has added another investigation to the problem of the effect of guidance upon maze learning. His animals were taught a relatively simple right-left maze and guided through it for a certain number of trials by means of a leash attached to a collar about the neck of the animal. He compared ten different groups as follows: group 1, no guided trials; group 2, trials 1 and 2 guided; group 3, trials 1 to 4; group 4, trials 1 to 6; group 5, trials 1 to 8; group 6, trials 1 to 12; group 7, trials 1 to 16; group 8, trials 1 to 20; group 9, trials 3 to 6; and group 10, trials 5 to 8. In general, guidance was effective; that is, the animals which had had a certain number of guided trials usually required considerably less subsequent unguided trials than did those animals which had had no guided trials. (An exception appeared in the case of group 2, for which the first two trials were guided. These animals required more subsequent unguided trials than did those which had had no guided trials whatsoever. This result may have been a statistical fluke since there were only 16 rats in each group.) It appeared, secondly, however, that the guided trials, although teaching something, did not teach as much as a corresponding number of unguided trials. Thirdly, it appeared that guidance introduced after some initial nonguidance trials, *i.e.*, groups 9 and 10, produced the most effective arrangement of all. The animals in these latter two groups learned with a smaller number of total trials (guided and unguided) than did any other group, including the group with no guided trials at all. Finally, it may be noted that a few animals in nearly every group succeeded in mastering the maze during the period of guidance alone. That is, it is possible for a maze to be learned without any *actual* entrances into the blinds. This last seems to be the author's most significant conclusion.

He compares this last result with the previous ones of Koch (15), Ludgate (24), and Wang (39). Ludgate used a somewhat similar method with stylus mazes and human beings. During the guided trials she directed the subject's hand by taking hold of it. Her results differ from those of Alonzo in that her subjects were never able completely to learn the maze by guidance alone. This difference Alonzo atributes, and it seems to the reviewer rightly, to the fact that Ludgate's human subjects behaved in a different manner from that of the rats during guidance; the human subjects were relatively passive and did not, during guidance, note blinds or attempt to enter them. The rats, on the other hand, strained at the leash and made active attempts at going into the blinds. Hence Ludgate's subjects had no chance to learn about the blinds during guidance and to eliminate them, whereas Alonzo's subjects did have such a chance.

Koch performed an experiment on guidance, using both animals and humans. Instead, however, of a leash or similar control, the blind alleys were blocked during the period of guidance. Her results also differ from Alonzo's in that for her also none of the subjects, animal or human, were able completely to learn the maze during the guidance period alone. Alonzo points out that with Koch's subjects also there were no tendencies to make errors during the guided trials, and hence no opportunity offered for their correction and elimination.

Finally, Wang used a method of verbal guidance with human beings, in which the subjects were corrected verbally whenever they started to make an error. Wang's results agree with Alonzo's in that his subjects were on occasion able to master the maze completely during the guided trials.

Alonzo's conclusion is that in order to obtain complete and final blind elimination it is necessary that during the learning either actual entrances or attempts at entrances must be made. Otherwise such errors never have a chance to be completely corrected. Or as Carr (5) puts it: "A certain number of errors must be made and eliminated before the subject is ever able to run the maze correctly.

Correct modes of response are established in part by *learning what not to do.*[2] Undoubtedly the fixation of the correct response is partly responsible for the elimination of errors, but it is equally true that the establishment of the correct mode of response is an incidental result of the eliminative process." Or, to put it in his own words, the reviewer would conclude that learning, even in the case of a maze, must be said to be a process exhibiting the earmarks of insight. For the type of practice which most favors learning is the type which emphasizes the wrongness of the wrong as well as, and in contrast to, the rightness of the right.

Higginson (12) sets out to see if the rat, like the ape, is capable of "an extension of visual apprehension such as to initiate a sudden shift in the actual performance which is incapable of explanation by the usual conception of 'trial and error' and by the 'laws of frequency and recency.' Can the rat visually perceive to advantage a sudden change in its path which offers a major reduction in its usual course towards food?" He used a standard circular maze and at the entrance to alley 3 he put a snugly fitting door. During the first hundred trials the rat on coming down alley 2 finds this door closed, that is, he finds a smooth wall at his right, and he is required to run to the end of the *cul-de-sac* at his left. Only upon returning from this *cul-de-sac* does he then find the door into alley 3 open. In other words, during these hundred trials the rat develops a habit of running six feet out of his way down the *cul-de-sac* and back again. After these hundred trials the door is then opened and left open for all subsequent trials. Curiously enough, when the change was made only four of the nine rats ran into the blind and out again before turning into the newly opened door, and these four immediately shifted. And what is more significant, the remaining five "stopped suddenly and without interference ran the remainder of the maze correctly, thus dropping *at once*[3] six feet from the previous pathway and turning in a different manner." These results, as Higginson insists, are "inexplicable in the usual categories of 'kinaesthetic patterns' and 'frequency and recency' of problems." He concludes that, given the particular demand made upon his animals, "visual perception constitutes under the conditions an important functional resource for the organism." The reviewer is heartily in agreement with the probable correctness of this conclusion. It does, however,

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[2] Italics mine.

[3] Italics mine.

seem to him that there is a possible source of error in Higginson's experiment in that there seems (as has been found in the California laboratory) to be a general tendency for rats to enter *any newly opened pathway.* And in so far as this is true, the sudden switch from going down the useless *cul-de-sac* to leaving it out and going directly into the newly opened door may have been merely the expression of such a general tendency to enter new pathways, and not an "insightful" apprehension of the shorter distance as such.

Summing up these experiments of Washburn, Alonzo, and Higginson, they, it seems to the reviewer, agree in suggesting: (1) that maze learning takes place only under the spur of a motive or end; (2) that it is sensitive to changes in the nature of that end; and (3) that it must be described as of the nature of an improved knowledge of, insight(?) into, the position of that end.

We turn now, finally, to insight experiments proper.

C. *"Insight" and "higher mental processes."* Nelimann and Trendelenburg (25) repeated many of Köhler's "Umweg" experiments with monkeys, a *Rhesus* and a Pavian (*P. cynoceph*), both newly imported at the time the experiments began. They carried out some eighteen experiments. (Most of the successes reported, however, were obtained from the *Rhesus.*)

(i) Reaching experiments. These investigated the monkey's ability to estimate correctly the distance of food. Food was placed outside the cage, sometimes within reaching distance and sometimes beyond it. It was found that the monkey estimated very accurately (*i.e.,* to within two or three centimeters) whether or not he could reach the food with naked paw. If it was just out of reach, he did not attempt to grab for it.

(ii) String experiments. The monkey discriminated readily between a string attached to or touching the food and one not touching the food. Further, if the animal was allowed to observe the placing of the string, he could differentiate between the case where he saw the string merely laid on the food and the one in which he saw it actually tied to the food. In the former case he would not pull the string; in the latter he would.

(iii) Rake experiments. The results substantiated those of other observers that monkeys will not use a rake correctly unless it has already been properly placed with teeth behind the food.

(iv) Casket experiments. A box with a hinged upper lid was used. This lid on the outside showed no difference between the edge which lifted up and the edge which was hinged. If the food was placed in this box while the monkey was watching, and then the box was pushed to the edge of the cage with either side toward the animal, the latter never fumbled but immediately lifted the proper edge of the box, whether in placing it before the cage it had been turned through 90 degrees, 180 degrees, or 360 degrees from its original position. This memory of the box's position could not, however, be retained correctly for an interval of more than half a minute: after a longer time the monkey would forget which edge of the lid was the one toward him. In this connection it should be noted that recent experiments by Tinklepaugh (**34**) indicate that monkeys can delay successfully up to some twenty-four hours when the feature to be remembered is which one of two containers, relatively separated in space, the food was placed in; a striking difference, whatever the explanation may be, from the short delays obtained by Nelimann and Trendelenburg.

(v) The bolt experiment. The lid of the casket containing the food was fastened by means of bolts. These had to be pulled toward the monkey before the lid could be opened. The animal was unable to learn to manipulate these bolts except by trial and error.

(vi) Estimation of distance experiments. Fruit was hung from the top of the cage with varying lengths of suspending cord. The animals chose with great nicety the easier route for obtaining it. If it hung low enough, they reached it by a spring from the floor. If it hung higher, they made no attempt to spring from the floor, but immediately climbed to the top of the cage and reached down for it.

(vii) Swinging door experiment. The Rhesus used the top of a door to swing out on to reach suspended food immediately and without tuition.

(viii) Obstruction experiment. In the case where a box was placed against the bars of the cage in such a way as to prevent the reaching through the bars with the paw in order to obtain the food outside, the box was pushed out of the way immediately and without tuition. (A type of solution not achieved by Köhler's apes.)

(ix) Experiments involving angles and distances. When the further end of a string was tied to a point at some distance from the cage and fruit tied at a point on the string nearer the cage and then the string brought into the cage in an oblique direction, the animal readily and without trial and error moved the string hand over hand until it came perpendicularly to the cage, and thus brought the food within reaching distance. A similar solution was achieved when a stick was substituted for the string. In the case of the stick the pivot point of the stick was immediately adjacent to the cage and the fruit was placed at a point beyond the pivot. The stick was immediately turned into a position parallel with the cage and the fruit thus brought within reach.

(x) Pendulum experiment. When the fruit was swung on the end of a string pendulum, the monkey immediately took his stand at the proper position on the side of the cage for catching the fruit at the end of one of its swings.

(xi) Inverted flower pot experiment. Food was placed under one of two flower pots and then these were rotated about one another before the eyes of the monkey. After the rotation the monkey, who had been watching the process, immediately chose the right pot. He did not show " astonishment " in another experiment when upon lifting up the pot he found another one underneath, although he had not seen this latter one put in place. In other words, Buytendijk's (**4**) finding reported last year was not confirmed.

(xii) Turntable experiment. The food was placed upon the far side of a small turntable just outside the bars of the cage. The animal at once and without hesitation began rotating the turntable in such a way as to bring the food directly to the front of the cage.

(xiii) Mirror experiments. The monkeys when presented with a mirror sniffed at and touched their own images with much interest and responded in the usually reported fashion of peeking behind the mirror to hunt for the " other monkey."

(xiv) The hanging string experiment. When fruit was put in a high basket so that it then disappeared from the sight of the monkey, but with a string hanging from it, the latter was immediately pulled.

(xv) Tube and plunger experiment. The monkeys did not succeed in pushing food out of a hollow tube by means of a stick. Both glass tubes and opaque metal tubes were used and the stick was placed in position before the beginning of the experiment. In every case the monkey either neglected the stick or pulled it out instead of pushing it in.

(xvi) Slide ring experiment. After trying many other possibilities the monkey exhibited the solution of pulling a sliding ring to which food was attached over a slip wire to the side of the cage. He did this by means of a string dangling from the sliding ring.

(xvii) Swing rope experiment. The monkeys readily used a rope to swing on to reach distant hanging food.

(xviii) Box placing experiments. The experimenters never obtained complete success in this experiment. (The monkey died before the conclusion of the experiment.) Several approximations to success, however, did appear. The monkey spontaneously used the shoulder of an attendant to climb on and when later the attendant stood at some distance from the food, the animal pulled at his coat in a pleading fashion. The monkey also used spontaneously a loose blind to swing out on to reach the food. He also exhibited on numerous occasions a tendency to move the box a slight distance in the proper direction for getting it under the food, but invariably abandoned these attempts before the box had been moved a sufficient amount. The animal also learned readily to turn the box from lying supported at an angle to a vertical position so as to bring it into a position in which he could then stand upon it and reach the food.

In contrasting their results with those of Köhler on the apes, the authors conclude: that (1) the monkeys are far behind the apes in the use of tools; but that (2) they are apparently equal to the apes in their apprehension of spatial relationships and of spatial Umwege (*e.g.*, string, swinging rope, hindering box, etc.). And, finally, (3) they call attention to the fact that they have presented some new experiments not yet tried out with apes, to wit, the turntable, the slide ring, the hanging string.

Hertz (11) reports observations on a carrion crow (*Corvus corone*) and a jackdaw (*Colocus monedula*), both taken as young birds from the nest and raised in captivity. They were kept in adjacent flying cages which were large enough to be entered by the experimenter. The crow was generally the quicker learner and the more intelligent of the two. Both birds were highly accurate in visual discrimination and highly sensitive to any changes in their *visual* environment, whether near at hand or at a distance. They became very friendly with the experimenter. The crow showed an amusing discrimination between the experimenter's right hand which fed him, and cared for him in general, and the experimenter's left hand which was used to hold him or restrain him. The former was treated as a friend, the latter was pecked at and attacked whenever possible.

As regards insight and higher mental processes, the following is a somewhat telescoped summary of the experimenter's observations:

(1) The crow learned to throw hard food into the water to soften it. (This seemed to arise out of an original play tendency to throw all easily moveable objects into water.)

(2) The crow exhibited considerable skill in placing food into a crevice as a holder when he wanted to tear the food apart.

(3) The crow learned to push pine nuts through the wire to the experimenter for the latter to crack open. He would even go and find hidden nuts (oftentimes seeming to recall the exact spot where he had previously hidden them) in order then to hand them out to the experimenter.

(4) The crow was less and less ready in this experiment to hand out nuts to the experimenter the more they had, by painting or other artificial means, been made to differ in appearance from normal nuts. He in play often made collections, putting nuts with nuts, stones with stones, etc.

(5) When the experimenter wished to set the bird to hunting for hidden or buried nuts, she had merely to toss a nutshell into the cage (as a symbol?).

(6) The birds would hide food and playthings in corners, in holes, etc., and cover them up with all available light objects (*e.g.*, dry leaves, loose earth, etc.), and they would uncover them afterwards, going directly to the hidden caches. As much as five hours intervened on occasion between the hiding of an object and its later uncovering.

(7) They showed insight in their methods of removing various types of obstacles that on occasion covered the food. Thus, if these obstacles proved too heavy to be dislodged by the usual direct method of inserting the bill towards the food and pushing, the bird would at times take the roundabout route of going to the side *away from* the food and from there pulling.

(8) They exhibited a capacity for delayed reaction both in the uncovering of food which they themselves had buried and in the uncovering of food which they had seen the experimenter bury or cover. (No test of the lengths of time possible were made in this latter case.)

(9) They learned, at first through accident, how to open the doors of small nest cages used as puzzle boxes. After they had once learned this, they showed considerable insight into the manner by which the doors worked. Thus the crow removed a stone which blocked the door in an insightful fashion, and the jackdaw dis-

mounted and opened the door wider from the floor in order then to mount it again and perch in a more comfortable position.

(10) The crow learned to operate a bolt that was used to lock the doors and acquired considerable insight when the position of this bolt was changed and when the door was hinged on the opposite side.

(11) Both birds showed a high mastery of all sorts of spatial " Umwege."

(12) They readily solved the pendulum experiment in which the food was swung on the end of a string and the bird had to take up the appropriate position for catching it at the end of a swing.

(13) They hauled up food put in a pocket at the end of a dangling strip of cloth. They did not attempt, however, to thus haul up the food when it was tied to the end of a string. The strip of cloth was an easier problem because they could stand on it and thus hold the part of the cloth already hauled up. This would have been impossible in the case of the string. The jackdaw, strangely enough, showed, generally speaking, more ability and insight in the case of this one test than did the crow.

Let us now compare the results for birds and monkeys. The outstanding conclusion would seem to be that the two types of animal are on much the same level as to insight. Both are very expert in visual discrimination and in spatial Umwege. And, surprisingly enough, the hands of the monkey do not seem to put him into a very greatly favored position with respect to the bird. Only in the pulling of string and such beginnings of tool-using does the possession of hands seem to give him some advantage. In all matters of spatial Umwege and of the simple moving or carrying of objects the bird is as good as the monkey.

Finally, under this head of insight let us note that Duncker (7), Lindemann (21), Lipmann (22), Lipmann and Bogen (23), and Peisers (26) have sought to try Köhler's types of insight problems with human beings, for the most part children, normal or defective.[4]

Lipmann and Bogen, and Lindemann reiterate Köhler's distinction between ability to respond to the mere optics of the situation and the ability to respond to its fundamental " static " or physical principles. Lindemann finds that his feebleminded (imbecile) children were able, like the apes, to respond successfully to situations involving primarily

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[4] The dates of all these investigations, save Duncker's, are before 1926, but since they were not included in last year's review, I am reporting them now.

optics, but tended to fail in situations where more strictly physical, statical, principles were involved. He found, for example, in one of his defective children a similar response to that of Sultan. When the child was presented with the double stick, he pushed the one part out towards the goal with the other part, thus establishing an optically successful though physically nonsuccessful connection. Lipmann and Bogen found, on the other hand, that normal children of ten to fourteen years were capable of successful solutions in cases involving physical and statical, as well as optical, principles. Duncker found that the moment of insight was a moment of "penetration" in which the subject sees what is wrong with the results of some trial (or error) which he has already tried or thought of trying. Lipmann, and Lipmann and Bogen make a distinction between the behavioristic sort of intelligence involved in problems of these sorts (*i.e.*, a knowledge of "Naive Physik") and pure theoretical or "gnostic" intelligence.

In general, these authorities all agree in envisaging insightful problem-solving as something fundamentally different from trial and error problem-solving. Here, it seems to the reviewer, is an important issue, and the remaining section of the paper will be devoted to a discussion of it.

III. Theoretical Discussion (Insight vs. Trial and Error)

As an introduction we may note the new terminology suggested by Nelimann and Trendelenburg (25). They propose the terms "primary solution" and "secondary solution." A primary solution is one which arises when the specific problem has never been met with by the animal in his wild state, and when also it is now solved without the aid of overt "imitation," "trial and error," or "being put through." A secondary solution is, in contrast, one which does involve imitation, trial and error, or being put through. One further point about the primary solution must also be noted. Nelimann and Trendelenburg themselves do not stress this, but it seems to the reviewer obvious that in the case of a primary solution there must be supposed to have to occur, if not overt trial and error, some sort of internal happening then and there. Otherwise there would be no guarantee that the case was one of a SOLUTION at all and not merely one for which innate endowment or past habit had already prepared the appropriate response. To sum up, primary solutions do, then, set themselves off from secondary solutions chiefly in that whereas in the latter the required new adjustments come only after, and as a result of, overt behavior, in the former they seem to come without overt behavior but rather by means of some sort of internal happening then and there.

The reviewer has previously (35, 36) indicated that he believes that both trial and error learning and " insight " learning, so-called, are to be described as passings from one set of meanings (postulations as to the position of the goal) to a second improved set of meanings (postulations as to goal position). Both the end-states of learning are, in short, to be described as states of " insight." And this we would hold to be as true for trial and error learning as for " insight " learning.

Let us elaborate this hypothesis further. And let us begin with trial and error. We may take as our example Thorndike's kitten and its learning to pull the hanging loop of string in order to get out of the puzzle box. Before the learning, the kitten has, we would say, a postulation (insight) as to the position of the goal which finds expression in the specific character and direction of the array of strugglings, *i.e.*, the bitings, scratchings, squeezings, etc., which the animal is then and there peculiarly ready for. And after the learning, he has a new improved postulation (insight) as to the position of the food which expresses itself in the now acquired, especially strong propensity for pulling at anything like a hanging loop of string. The initial array of ready behaviors defines one postulation (insight); the acquired readiness for string-pulling defines a new, improved postulation (insight). The initial array could be shown experimentally to be contingent upon the general fact of the food being outside. And hence, we shall declare, it can be said to postulate this outsideness. And the new, especially strong readiness for the string-pulling could similarly be shown experimentally to be contingent upon the sequence: pulled string—opening a way out; and hence can be said to postulate that sequence. (It may be noted in passing that this final postulation [insight] in the case of the cat does not include, apparently, any reference to the exact nature of the mechanism, whether pulley, electrical connection, or what not, whereby this sequence is achieved. The cat's readiness to pull would not, in short, prove in any way experimentally contingent upon changes in that mechanism. The animal's final postulation would be merely that of the gross fact of the physical sequence.)

Turn, now, to " insight " learning. And let us take as our example the case of Köhler's ape and the occasion when he first used a stick to rake in the distant food. This case also would be described as the passage from one initial postulation of (insight into) the position of the food, to a second improved postulation of (insight into) that position. Before the solution, the ape's postulation would express itself in the general readiness for reaching, plus possibly some simultaneous tendency probably to suppress this readiness; this latter suppression being correlated with the excessive distance of the food. After the solution, his improved postulation (insight) would express itself by the newly acquired, peculiar readiness for the use of a stick or of any other similarly functioning long object. The initial set of reaching readinesses could, we believe, be shown experimentally to be contingent upon the actual position-relation of the food, as outside the cage and at a considerable distance, and hence to postulate this outsideness and this distance. And, similarly, the newly acquired readiness for the use of the stick, or of any stick-like object, which appears after learning, could, we believe, be shown experimentally to be contingent upon the further physical actualities (*i.e.*, the smallness and rollability of the food, etc.) whereby the distant food can, in fact, be raked in. And hence, we must say, this acquired stick readiness expresses this further insight into (postulation of) those further physical actualities.

In short, the learnings of both cat and ape are to be described as passings from one initial set of position postulations (insights) to a second improved set of position postulations (insights). But what, then, is the difference between the two cases? Let us return to the point we made above that primary or insight solutions are ones in which the new adjustments (new postulations of position) come *without* overt behavior, whereas secondary or trial and error solutions are ones in which this change comes only *through* overt behavior. And let us now apply this in more detail.

We will begin again with trial and error. The cat, it must be noted, learns only *after* she has actually pulled the loop and thus experienced its resultant success. That is, her learned readiness for loop pulling comes only after she has actually done it and experienced its good results, as contrasted with the bad results of the other trials and errors which she also tries as part of her initial array. In other words, we must assume that the learning arises in this type of case only when the relative values of the correct and wrong responses have actually been demonstrated through trying them out and obtaining actual experiences of their respective good and bad results.

Consider, however, the case of insight learning. When the experiment is a crucial one, when, that is, it is crucially definitive of a so-called " insight " or nontrial and error solution, the response which is finally chosen must *not* be one among the initial array of trials and errors. The ape must have never before, in this sort of a situation, used a stick. The virtue of this stick response must, then, in some way be discovered without, and before, actually trying it. The ape must " foresee " both its possibility and the goodness of its result. Herein, then, would seem to lie the peculiar essence of the primary or insight solution. In it, the new insight arises by " foresight " rather than as a mere by-product of acts after they have been performed.

But there is one point more. In trial and error learning the animal first performs an original array of trials and errors. He finds the result of one of these to be a situation such that (given his innate or acquired endowment) it leads him at once, when actually present, to an appropriate further response. Thus among the original array of the kitten's trials and errors is the pulling of the loop. But this results in the open door. And an open door is a type of situation such that (given the innate or acquired endowment of the cat) it leads at once to the appropriate further response of going out. (An open door in this kind of a problem is a prepotent stimulus situation for the cat.) Also, it is to be noted, that the other responses within the original array are such as to lead to situations for which no such further appropriate responses are in readiness. The unsuccessful bitings and squeezings result in situations which, as such, evoke merely neutral or negative responses. Learning, we shall declare, accrues, then, only in so far as these respective good and bad results become in some way, rapidly or gradually, clearly or dimly, *represented* by the organism to himself at the moments before the acts leading to them are released. For only by assuming such *representations* can we explain that the propensity towards the one act becomes reenforced while those towards the other acts become weakened.

Take, now, insight learning (or, as we had better perhaps call it from now on, " foresight " learning). By hypothesis the successful act in this case is not one among the array of original trials which are initially ready to go off. How, then, is this act finally hit upon? Again we must assume that the ends of acts can be represented at moments before those acts are performed. Thus we must assume that the ape is capable of in some way representing (after a certain amount of experience) the resulting spatial gap between its paw and the distant food which results when he actually reaches his paw out. And we shall also assume that he is able to represent the consequent extension of his arm which results when he picks up the stick. And, now, all we have to assume further is that he is capable of *combining these two representations,* in order to explain the appearance of the correct solution. We merely have to assume that at, say, some moment when he is looking at the stick he represents to himself not only the result of picking it up (*i.e.,* his lengthened arm) but also the result of reaching his paw out towards the food. For he will thus achieve the new composite representation of his arm lengthened by stick and this lengthened arm as reaching out towards and making contact with the food. But this new representation presents a situation which, if present, would lead then and there to an appropriate further response—that of raking the food in. (We are assuming, that is, that if the situation of stick-already-in-place were presented, the correct further response of raking it in would not have to be learned, but that it lies within those that an ape is, as such, ready to do;—just as, indeed, in the case of the cat, once the opened door is presented, the further correct response of going out is already within the cat's equipment and prepotently ready.) By dint, then, of some sort of mental play, the ape projects a new situation, *i.e.,* the combined result of a composite act, only the parts of which he has already done and these only separately. And since this represented new situation is one which, if actually presented, would immediately evoke an appropriate further response, he now does the new act leading to it. The new insight arises, in other words, out of an ability to achieve in representation the results of compound acts, only the component parts of which have previously been performed. Foresight learning involves free play among representations:—the ability to add them together (and also in some instances to subtract them) and thus to create new representations. Finally, it should be emphasized, however, that whereas we have thus far for the purposes of exposition stressed this peculiar play of representations (*i.e.,* "foresight") as distinctive of the ape, in reality we would suppose it also to function in some degree in the case of the cat. It would, in fact, seem to us probable that in so far as an animal can represent the ends of its acts at all (*i.e.,* in so far, indeed, as it can learn), it can also to some extent combine—add together or subtract—these representations one with another, and thus achieve new representations and the new acts which would lead to them. Indeed, we seem to have an actual illustration of this sort of thing in the case of Higginson's rats (12), who, it will be remembered, took, as soon as they had an opportunity, a new short cut which they had never, as such, previously performed. (This would be a case where the new representation was formed by substraction among the acts already performed.)

In conclusion, it may be well to tabulate the essential features of learning both of the trial and error and of the insight (or foresight) varieties, as these features have precipitated themselves out from the above discussion.

(1) All learning is to be said to arise out of an initial postulation of (insight into) the goal position, and to end in a new improved, or added to, postulation of (insight into) such goal position.

(2) All learning may be said to involve the representation of the ends of acts at moments before their actual occurrence.

(3) When these represented ends of acts are situations which when actually present lead at once (given the animal's innate and acquired endowment) to further appropriate responses, then the propensities towards the acts leading to those ends will become strengthened.

(4) When, on the other hand, these represented ends of acts are situations which when actually present lead only to negative or avoidance responses, then the propensities towards the acts leading to those ends will become weakened.

(5) The higher the animal, the fewer the number of experiences of an act which are probably necessary to achieve such representations of its end, and the clearer and more accurate such representations themselves probably are.

(6) The higher the animal, the more it would seem that these representations can be played with and manipulated; the more the animal can mentally add and subtract the acts to produce new representations; the more, in short, he can achieve "foresight," as opposed to mere trial and error solutions.

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