BEHAVIOR MONOGRAPHS

Edited by
JOHN B. WATSON
The Johns Hopkins University

Volume 3
1916-1919

Published at
Cambridge, Boston, Mass.
HENRY HOLT AND COMPANY
34 West 33d Street, New York
G. E. Stechert & Co., London, Paris and Leipzig, Foreign Agents

CONTENTS

12. The Mental Life of Monkeys and Apes: A Study of Ideational Behavior. By Robert M. Yerkes. Pp. iv+145. $1.50.

13. A Study of Perseverance Reactions in Primates and Rodents. By G. V. Hamilton. Pp. iv+65. $.75.

14. An Analysis of the Learning Process in the Snail, *Physa gyrina* Say. By Elizabeth Lockwood Thompson. Pp. iii+97. $1.25.

15. The Effect of Length of Blind Alleys on Maze Learning: An Experiment on Twenty-four White Rats. By Joseph Peterson. Pp. iii+53. $.75.

16. An Experimental Study of Transfer of Response in the White Rat. By Harry H. Wylie. Pp. iii+66. $1.00.

BEHAVIOR MONOGRAPHS

Volume 3, Number 1, 1916
Serial Number 12

Edited by JOHN B. WATSON
The Johns Hopkins University

The Mental Life of Monkeys and Apes: A Study of Ideational Behavior

ROBERT M. YERKES

Published
at Cambridge. Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

The Journal of Animal Behavior

An organ for the publication of studies concerning the instincts,
habits and intelligence of organisms

The Journal contains a Department of Notes in which appear brief accounts of especially interesting and valuable observations of behavior.

Published bi-monthly at Cambridge, Boston, Mass., by Henry Holt and Company, New York. Each volume contains at least 450 pages with plates and text-figures.

The subscription price is $3.00 per volume (foreign subscription $3.50) postpaid.

Manuscripts, subscriptions, and all business correspondence should be addressed to

THE JOURNAL OF ANIMAL BEHAVIOR
Emerson Hall, Cambridge, Massachusetts

Behavior Monographs

For the publication of studies in behavior and intelligence which are
too lengthy or too expensive for acceptance
by The Journal of Animal Behavior

Published at irregular intervals at Cambridge, Boston, Mass., in connection with the Journal of Animal Behavior, by Henry Holt and Company, New York. Each volume contains approximately 450 pages with plates and text-figures.

The subscription price is $3.00 per volume (foreign subscription $3.50) postpaid.

Monographs may be purchased separately at prices varying with the cost of manufacture.

Manuscripts and inquiries concerning terms of publication should be addressed to the Editor of the Behavior Monographs,

JOHN B. WATSON,
The Johns Hopkins University,
Baltimore, Md.

Subscription to The Journal of Animal Behavior and the Behavior Monographs should be sent to Emerson Hall, Cambridge, Massachusetts.

BEHAVIOR MONOGRAPHS

Volume 3, Number 1, 1916 Serial Number 12

Edited by JOHN B. WATSON
The Johns Hopkins University

The Mental Life of Monkeys and Apes: A Study of Ideational Behavior

ROBERT M. YERKES
Harvard University

WITH SIX PLATES AND FIVE TEXT FIGURES

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E STECHERT & CO., London, Paris and Leipzig, Foreign Agents

CONTENTS

I

INTERESTS, OPPORTUNITY AND MATERIALS

Two strong interests come to expression in this report: the one in the study of the adaptive or ideational behavior of the monkeys and the apes; and the other in adequate and permanent provision for the thorough study of all aspects of the lives of these animals. The values of these interests and of the tasks which they have led me to undertake are so widely recognized by biologists that I need not pause to justify or define them. I shall, instead, attempt to make a contribution of fact on the score of each interest.

While recognizing that the task of prospecting for an anthropoid or primate station may in its outcome prove incomparably more important for the biological and sociological sciences and for human welfare than my experimental study of ideational behavior, I give the latter first place in this report, reserving for the concluding section an account of the situation regarding our knowledge of the monkeys, apes, and other primates, and a description of a plan and program for the thorough-going and long continued study of these organisms in a permanent station or research institute.

In 1915, a long desired opportunity came to me to devote myself undividedly to tasks which I have designated above as " prospecting " for an anthropoid station and experimenting with monkeys and apes. First of all, the interruption of my academic duties by sabbatical leave gave me free time. But in addition to this freedom for research, I needed animals and equipment. These, too, happily, were most satisfactorily provided, as I shall now describe.

When in 1913, while already myself engaged in seeking the establishment of an anthropoid station, I heard of the founding of such an institution at Orotava, Tenerife, the Canary Islands, I immediately made inquiries of the founder of the station, Doctor Max Rothmann of Berlin, concerning his plans (Rothmann, 1912).[1] As a result of our correspondence, I was invited to visit and make use of the facilities of the Orotava station and to consider with its founder the possibility of coöperative work instead of the establishing of an American station. This invitation I gratefully accepted with the expectation of spending the greater part of the year 1915 on the island of Tenerife. But the outbreak of the war rendered my plan impracticable, while at the same time destroying all reasonable ground for hope of profitable coöperation with the Germans in the study of the anthropoids. In August, 1915, Doctor Rothmann died. Presumably, the station still exists at Orotava in the interests of certain psychological and physiological research. So far as I know, there are as yet no published reports of studies made at this station. It seems from every point of view desirable that American psychologists should, without regard to this initial attempt of the Germans to provide for anthropoid research, further the establishment of a well equipped American station for the study not only of the anthropoid apes but of all of the lower primates.

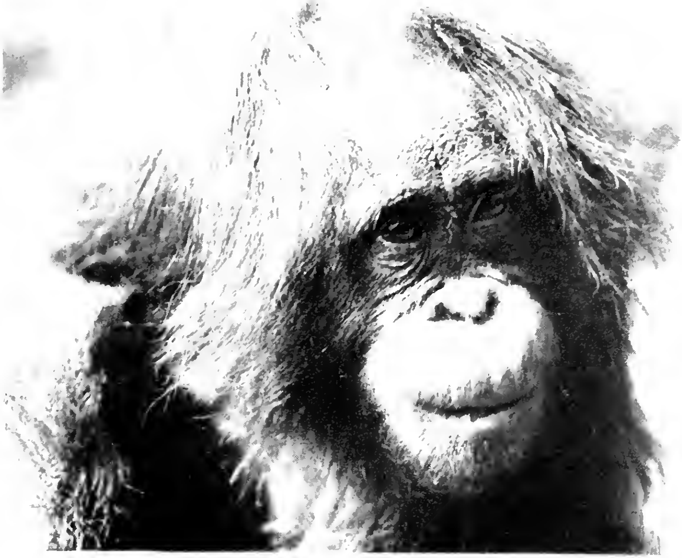
In the early months of the war while I was making every effort to obtain reliable information concerning conditions in the Canary Islands, I received an urgent invitation from my friend and former student, Doctor G. V. Hamilton, to make use of his collection of animals and laboratory at Montecito, California, during my leave of absence from Harvard. This invitation I most gladly accepted, and in February, 1915, I established myself in Santa Barbara, in convenient proximity to Doctor Hamilton's private laboratory where for more than six months I was able to work uninterruptedly under nearly ideal conditions.

Doctor Hamilton without reserve placed at my disposal his entire collection of animals, laboratory, and equipment, provided innumerable conveniences for my work, and in addition, bore the entire expense of my investigation. I cannot adequately thank him for his kindness nor make satisfactory acknowledgment here of his generous aid. Thanks to his sympathetic interest and to the courtesy of the McCormick family on whose estate the laboratory was located, my work was done under wholly delightful conditions, and with assistance from Ramon Jimenez and Frank Van Den Bergh, Jr., which was in-

---

[1] See bibliography at end of report.

1



2

FIGURE 1.—Orang utan, Julius, in grove of live oaks back of the laboratory.
FIGURE 2.— Portrait of same subject.

valuable. The former aided me most intelligently in the care of the animals and the construction of apparatus; and the latter, especially, was of very real service in connection with many of my experiments.

The collection of animals which Doctor Hamilton placed at my disposal consisted of ten monkeys and one orang utan. The monkeys represented either *Pithecus rhesus* Audebert (*Macacus rhesus*), *Pithecus irus* F. Cuvier (*Macacus cynomolgos*), or the hybrid of these two species (Elliot, 1913). There were two eunuchs, five males, and three females. All were thoroughly acclimated, having lived in Montecito either from birth or for several years. The orang utan was a young specimen of *Pongo pygmæus* Hoppius obtained from a San Francisco dealer in October, 1914 for my use. His age at that time, as judged by his size and the presence of milk teeth, was not more than five years. So far as I could discover, he was a perfectly normal, healthy, and active individual. On June 10, 1915, his weight was thirty-four pounds, his height thirty-two inches, and his chest girt twenty-three inches. On August 18 of the same year, the three measurements were thirty-six and one-half pounds, thirty-three inches, and twenty-five inches.

For the major portion of my experimental work, only three of the eleven animals were used. A growing male, *P. rhesus* monkey, known as Sobke; a mature male, *P. irus*, called Skirrl; and the young orang utan, which had been named Julius. Plates I and II present these three subjects of my experiments in characteristically interesting attitudes. In plate I, figure 1, Julius appears immediately behind the laboratory seated on a rock, against a background of live oaks. This figure gives one an excellent idea of the immediate environment of the laboratory. Figure 2 of the same plate is a portrait of Julius taken in the latter part of August. By reason of the heavy growth of hair, he appeared considerably older as well as larger at this time than when the photograph for figure 1 was taken. In plate II, figure 3, Julius is shown in the woods in the attitude of reaching for a banana, while in figure 4 of the same plate he is represented as walking upright in one of the cages.

Likenesses of Sobke are presented in figures 5 and 6 of plate II. In the latter of these figures he is shown stretching his mouth, apparently yawning but actually preparing for an attack

EXPLANATION OF PLATE II

FIGURE 3.—Orang utan, Julius, reaching for banana.
FIGURE 4.—Julius walking across his cage.
FIGURE 5.—*P. rhesus*, Sobke.
FIGURE 6.—Sobke stretching his jaws (yawn?) preparatory to a fight.
FIGURE 7.—*P. irus*, Skirrl.
FIGURE 8.—Skirrl using hammer and nail.
FIGURE 9.—Skirrl using a saw.

PLATE II

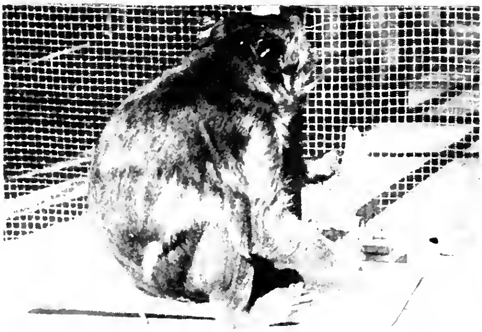

3


4


5


6


7




9

on another monkey behind the wire screen. Figure 7 of this plate indicates Skirrl in an interesting attitude of attention and with an obvious lack of self-consciousness. The same monkey is represented again in figures 8 and 9 of plate II, this time in the act of using hammer and saw.

All of the animals except the orang utan had been used more or less for experiments on behavior by Doctor Hamilton, but this prior work in no way interfered with my own investigation. Doctor Hamilton has accumulated a large mass of the most valuable and interesting observations on the behavior of monkeys, and he more thoroughly understands them than any other observer of whom I have knowledge. Much to my regret and embarrassment in connection with the present report, he has thus far published only a small portion of his data (Hamilton, 1911, 1914). In his most recent paper on "A study of sexual tendencies in monkeys and baboons," he has given important information concerning several of the monkeys which I have observed. For the convenience of readers who may make use of both his reports and mine, I am designating the animals by the names previously given them by Hamilton. The available and essential information concerning the individuals is presented below.

List of animals in collection

Skirrl. *Pithecus irus*. Adult male.
Sobke. *P. rhesus*. Young adult male.
Gertie. *P. irus-rhesus*. Female. Born November, 1910.
Maud. *P. rhesus*. Young adult female.
Jimmy II. *P. irus*. Adult male.
Scotty. *P. irus* (?). Adult male.
Tiny. *P. irus-rhesus*. Female. Born August, 1913.
Chatters. *P. irus*. Adult eunuch.
Daddy. *P. irus*. Adult eunuch.
Mutt. *P. irus*. Young adult male. Born August, 1911.
Julius. *Pongo pygmæus*. Male. Age, 4 years to 5 years.

When I arrived in Santa Barbara, Doctor Hamilton was about to remodel, or rather reconstruct, his animal cages and laboratory. This gave us opportunity to adapt both to the special needs of my experiments. The laboratory was finally

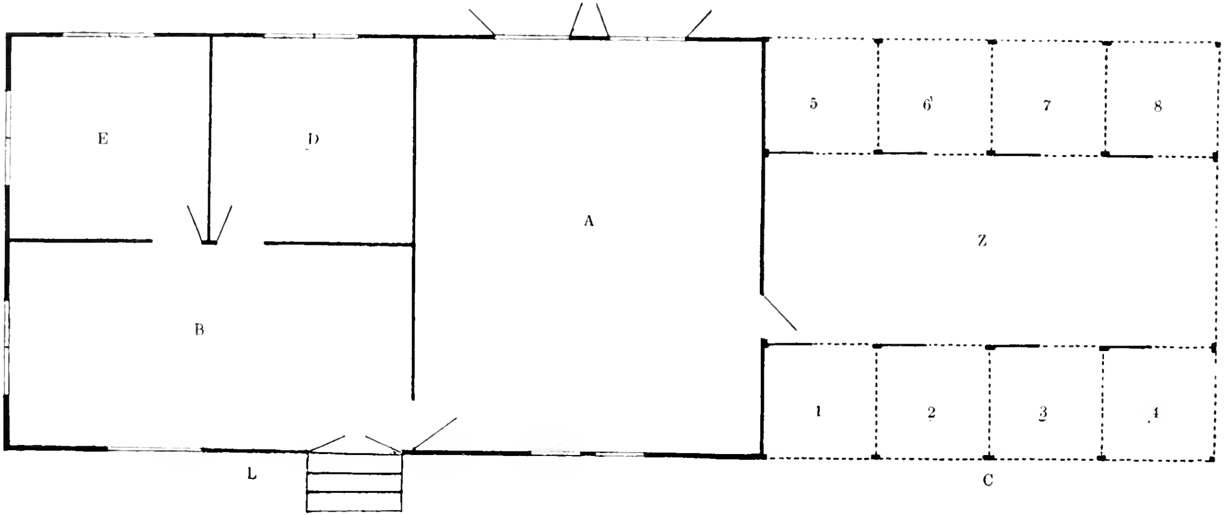
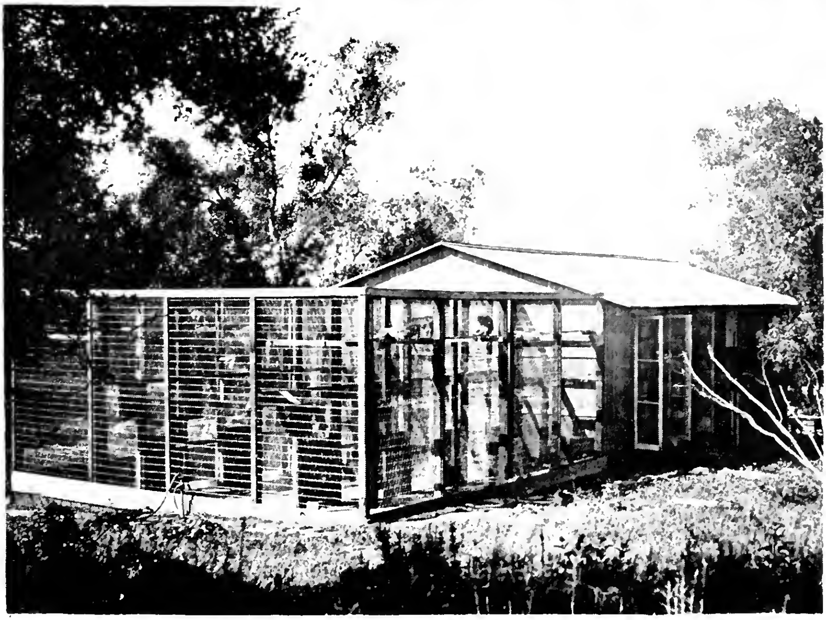


FIGURE 12.—Ground plan of Montecito laboratory and cages. Scale $\frac{1}{120}$

L, laboratory; C, cages; A, experiment room in which multiple-choice apparatus was installed; B, E, additional rooms for research; D, store room and shop; Z, large central cage communicating with the eight smaller cages 1-8.

10



11

FIGURE 10.—Montecito laboratory and cages from the front.
FIGURE 11.—Same from the rear.

located and built in a grove of live oaks. From the front it is well shown by figure 10 of plate III, and from the rear, by figure 11. Its location was in every way satisfactory for my work, and in addition, the spot proved a delightful one in which to spend one's time.

Figure 12 is a ground plan, drawn to scale, of the laboratory and the adjoining cages, showing the relations of the several rooms of the laboratory among themselves and to the nine cages. Although the construction was throughout simple, everything was convenient and so planned as to expedite my experimental work. The large room A, adjoining the cages, was used exclusively for an experimental study of ideational behavior by means of my recently devised multiple-choice method. Additional, and supplementary, experiments were conducted in the large cage Z. Room D served as a store-room and work-shop.

The laboratory was forty feet long, twenty-two feet wide, and ten feet to the plate. Each small cage was six, by six, by twelve feet deep, while the large compartment into which each of the smaller cages opened was twenty-four feet long, ten feet wide, and twelve feet deep.

II

OBSERVATIONAL PROBLEMS AND METHODS

My chief observational task in Montecito was the study of ideational behavior, or of such adaptive behavior in monkeys and apes as corresponds to the ideational behavior of man. It was my plan to determine, so far as possible in the time at my disposal, the existence or absence of ideas and the rôle which they play in the solution of problems by monkeys and apes. I had in mind the behavioristic form of the perennial questions: Do these animals think, do they reason, and if so, what is the nature of these processes as indicated by the characteristics of their adaptive behavior?

My work, although obviously preliminary and incomplete, differs from most of the previous studies of the complex behavior of the infrahuman primates in that I relied chiefly upon a specially devised method and applied it systematically over a period of several months. The work was intensive and quantitative instead of more or less incidental, casual, and qualitative as has usually been the case. Naturally, during the course of my special study of ideational behavior observations were made relative to various other aspects of the life of my subjects. Such, for example, are my notes on the use of the hands, the instincts, the emotions, and the natural aptitudes of individuals. It is, indeed, impossible to observe any of the primates without noting most interesting and illuminating activities. And although the major portion of my time was spent in hard and monotonous work with my experimental apparatus, I found time each day to get into intimate touch with the free activities of my subjects and to observe their social relations and varied expressions of individuality. As a result of my close acquaintance with this band of primates, I feel more keenly than ever before the necessity of taking into account, in connection with all experimental analyses of behavior, the temperamental characteristics, experience, and affective peculiarities of individuals.

The light which I have obtained on the general problem of ideation has come, first, through a method which I have rather inaptly named the multiple-choice method, and second, and more incidentally, through a variety of supplementary methods which are described in Section IV of this report. These supplementary methods are simple tests of ideation rather than systematic modes of research. They differ from my chief method, among other respects, in that they have been used by various investigators during the past ten or fifteen years. It was not my aim to repeat precisely the observations made by others, but instead to verify some of them, and more especially, to throw additional light on my main problem and to further the analysis of complex behavior.

What has been referred to as the multiple-choice method was devised by me three years ago as a means of obtaining strictly comparable objective data concerning the problem-solving ability of various types and conditions of animals. The method was first tried with human subjects in the Psychopathic Hospital, Boston, with a crude keyboard apparatus which, however, proved wholly satisfactory as a means of demonstrating its value. It has since been applied by means of mechanisms especially adapted to the structure and activities of the organisms, to the study of the behavior of the crow, pig, rat, and ring-dove (Yerkes, 1914; Coburn and Yerkes, 1915; Yerkes and Coburn, 1915). The method has also been applied with most gratifying results to the study of the characteristics of ideational behavior in human defectives,—children, and adults,—and in subjects afflicted with various forms of mental disease. It is at present being tried out as a practical test in connection with vocational guidance and various forms of institutional examination, such as psychopathic hospital and court examinations.

As no adequate description of the method has yet been published to which I can here refer, it will be necessary to present its salient characteristics along with a description of the special form of apparatus which was found suitable for use with monkeys and apes.

The method is so planned as to enable the observer to present to any type or condition of organism which he wishes to study any one or all of a series of problems ranging from the extremely simple to the complex and difficultly soluble. All of the problems, however, are completely soluble by an organism of excellent ideational ability. For the human subject, the solution of the easiest problem of all requires almost no effort, whereas even moderately difficult problems may require many repetitions of effort and hours or days of application to the task. In each case, the solution of the problem depends upon the perception of a certain constant relation among a series of objects to which the subject is required to attend and respond. Such relations are, for example, secondness from one end of the group, middleness, simple alternation of ends, or progressive movement by constant steps from one end of a group to the other.

It is possible to present such relational problems by means of relatively simple reaction-mechanisms. In their essential features, all of the several types of multiple-choice apparatus designed by the writer and used either by him or by his students and assistants are the same. They consist of a series of precisely similar reaction-devices, any one or all of which may be used in connection with a given observation. These reaction-mechanisms are so chosen as to be suited to the structure and action-system of the animal to be studied. For the human being the mechanism consists of a simple key and the total apparatus is a bank of keys, with such electrical connections as are necessary to enable the observer to obtain satisfactory records of the subject's behavior. Let us suppose the bank of keys, as was actually the case in my first form of apparatus, to consist of twelve separate reaction-mechanisms; and let us suppose, further, the constant relation (problem) on the basis of which the subject is required to react to be that of middleness. It is evident that in successive trials or experiments the keys must be presented to the subject in odd groups, the possibilities being groups of 3, 5, 7, 9, or 11. If for a particular observation the experimenter wishes to present the first three keys at the left end of the keyboard, he pushes back the remaining nine keys so that they cannot be operated and requires the subject to select from the group of three keys the one which on being pressed causes a signal to appear. It is of course the clearly understood task of the subject to learn to select the correct key in the group on first trial. This becomes possible only as the subject observes the relation of the key which produces the desired effect to the other keys in the group. On the completion of a subject's reaction to the group of three keys, a group of seven keys at the opposite end of the keyboard may, for example, be presented. Similarly, the subject is required to discover with the minimum number of trials the correct reaction-mechanism. Thus, time after time, the experimenter presents a different group of keys so that the subject in no two successive trials is making use of the same portion of the keyboard. It is therefore impossible for him to react to spatial relations in the ordinary sense and manner, and unless he can perceive and appropriately respond to the particular relation which constitutes the only constant characteristic of the correct reaction-mechanism for a particular problem, he cannot solve the problem, or at least cannot solve it ideationally and on the basis of a small number of observations or trials.

For the various infrahuman animals whose ideational behavior has been studied by means of this method, it has been found eminently satisfactory to use as reaction-mechanisms a series of similar boxes, each with an entrance and an exit door. An incentive to the selection of the right box in a particular test is supplied by food, a small quantity of which is placed in a covered receptacle beyond the exit door of each of the boxes, Each time an animal enters a wrong box, it is punished for its mistake by being confined in that box for a certain period, ranging from five seconds to as much as two minutes with various individuals or types of organism. This discourages random, hasty, or careless choices. When the right box is selected, the exit door is immediately raised, thus uncovering the food, which serves as a reward. After eating the food thus provided, the animal, according to training, returns to the starting point and eagerly awaits an opportunity to attempt once more to find the reward which it has learned to expect. With this form of the apparatus, the boxes among which choice may be made are indicated by the raising (opening) of the front door.

Since with various birds and mammals the box form of apparatus had proved most satisfactory, I planned the primate apparatus along similar lines, aiming simply to adapt it to the somewhat different motor equipment and destructive tendencies of the monkeys. I shall now briefly describe this apparatus as it was constructed and used in the Montecito laboratory.

The apparatus was built in room A (figure 12), this room

Explanation of Plate IV

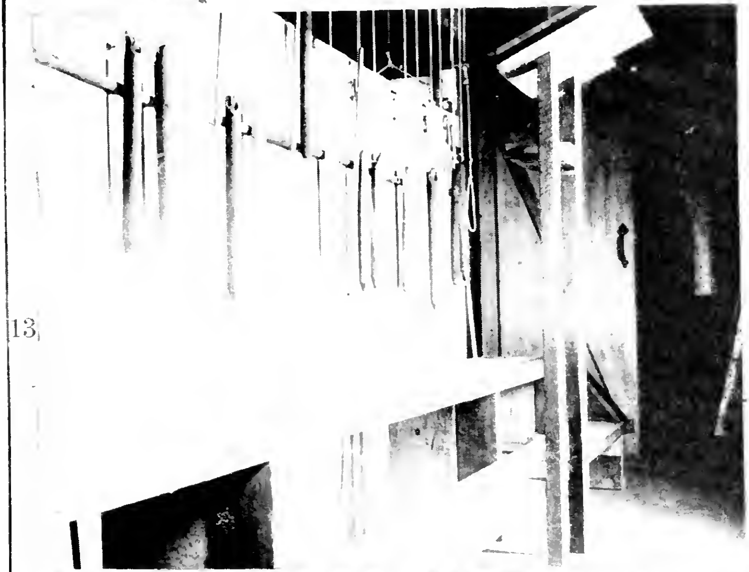
Figure 13.—Multiple-choice apparatus, showing observer's bench and writing stand.

Figure 14.—Apparatus as seen from observer's bench.

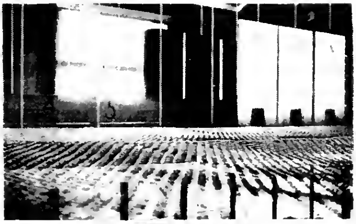
Figure 15.—Entrances to multiple-choice boxes as seen from the response-compartment.

Figure 16.—Apparatus as seen from the rear, showing exit doors, food receptacles, and covers for same.

PLATE IV



13



14

15

16

having been especially planned for it with respect to lighting as well as dimensions and approaches. It was unfortunately impossible to obtain photographs showing the whole of the apparatus, but it is hoped that the four partial views of plate IV may aid the reader who is unfamiliar with previously described similar devices to grasp readily the chief points of construction. In this plate, figure 13 shows the front of the complete apparatus, with the alleyway and door by way of which the experimenter could enter. The investigator's observation-bench and record-table also appear in this figure, together with weighted cords used to operate the various doors and the vertically placed levers by means of which each pair of doors could be locked. Figure 14 is the view presented to the observer as he stood on the bench or observation stand of figure 13 and looked over the entire apparatus. Three of the entrance doors are shown at the right of this figure as raised, whereas the remainder of the nine entrance doors of the apparatus are closed. Figure 15 is a view of the entrance doors from below the wire roof of the apparatus. Again, two of the doors are shown as raised, and three additional ones as closed. The rear of the apparatus appears in figure 16, in which some of the exit doors are closed and others open. In the latter case, the food receptacles appear, and on the lower part of the raised doors of the corresponding boxes may be seen metal covers for the food receptacles projecting at right angles to the doors, while on the lower edge of each door is an iron staple used to receive a sliding bar which could be operated from the observer's bench as a means of locking the doors after they had been closed. The space beyond the exit doors was used as an alleyway for the return of the animals to the starting point.

It will be necessary at various points in later descriptions to refer to these several figures. But further description of them will be more readily appreciated after a careful examination of the ground plan of the apparatus presented as figure 17. In accordance with the labelling of this figure, the experimenter enters the apparatus room through doorway 16, passes thence through doorways 17 and 10 to the large cage Z, from which he has direct access to the animals and can bring them into the apparatus. The multiple-choice mechanism proper, consisting of nine similar boxes (nine were used instead of twelve as a matter of convenience of construction, not because this smaller number is otherwise preferable) is labelled F. These boxes are numbered 1 to 9, beginning at the left. This numbering was adhered to in the recording of results throughout the investigation. The other important portions of the apparatus are the runway D, from which the subject at the experimenter's pleasure could be admitted through doorway 12 to the large response-chamber E; the alleyways G, H, and I, by way of which return to the starting point was possible; the observation bench C, with its approach step B; and the observer's writing table A.

In the construction of this large apparatus, it was necessary to make provision for the extremely destructive tendencies of monkeys and anthropoid apes,—hence the apparent cumbersomeness of certain portions. It was equally necessary to provide for the protection of the observer and the prevention of escape of the subjects by completely covering the apparatus and alleyways with a heavy wire netting.

Each of the eighteen doors of the multiple-choice boxes, and in addition doors 11, 12, and 15 of the runway D, were operated by the observer from his bench C by means of weighted window cords which were carried by pulleys appropriately placed above the apparatus. Each weight was so chosen as to be just sufficient to hold its door in position after the experimenter had raised it. For the convenience of the experimenter in the rapid operation of the twenty-one doors, the weights for the doors of runway D were painted gray, those for the entrance doors, white, and those for the exit doors, black.

In each entrance door, as is shown in figure 15 of plate IV, a window was cut so that the experimenter might watch the animal after it had entered a given box, and especially note when it left the box after having received its reward. This window was covered with wire netting. No such windows were necessary in the exit doors, but to them were attached heavy galvanized iron flanges which served to cover the food receptacles. One of these flanges is labelled o in figure 17. The food receptacles were provided by boring holes in a 2 by 4 inch timber securely nailed to the floor immediately outside of the exit doors. Into these holes aluminum cups fitted snugly, and the iron flanges, when the doors were closed, fitted so closely over the cups that it was impossible for the animals to obtain food from them.

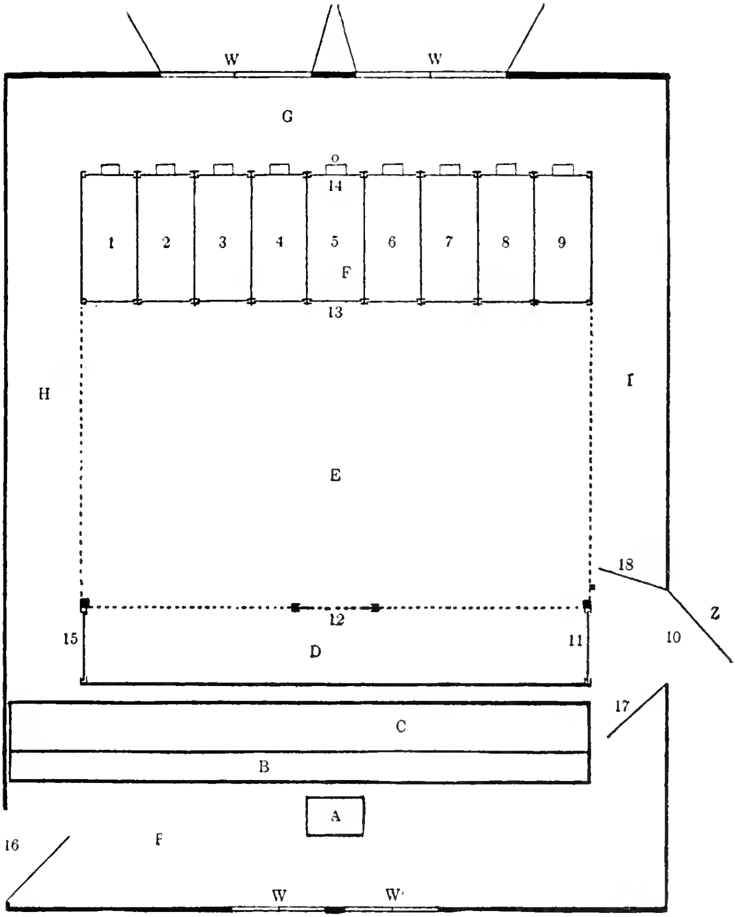


FIGURE 17.—Ground plan of multiple-choice apparatus in experiment room A.

Scale $\frac{1}{60}$

A, record stand; C, bench for observer; B, step as approach to C; D, alleyway leading to E, response-compartment; F, one of the nine (1-9) similar multiple-choice boxes; G, H, alleyways leading from boxes to starting point at D; I, alleyway used by experimenter as approach to rear of apparatus; W, W, windows; P, alleyway; Z, large cage; 16, entrance to room A; 17, entrance to apparatus and thence via 10 to cages; 18, entrance to alleyway I; 11, 15, entrances to D; 12, entrance to E; 13, entrance door of box 5; 14, exit door of box 5; o, cover for food receptacle.

As originally constructed, no provision was made in the apparatus for locking the entrance and exit doors of the several boxes when they were closed. But as two of the subjects after a time learned to open the doors from either outside or inside the boxes, it became necessary to introduce locking devices which could be operated by the experimenter from the observation bench. This was readily accomplished by cutting holes in the floor, which permitted an iron staple, screwed to the lower edge of each door, to project through the floor. Through these staples by means of a lever for each of the nine boxes, the observer was able to slide a wooden bar, placed beneath the floor of the room, thus locking or unlocking either the entrance door, the exit door, or both, in the case of any one of the nine boxes.

Since figure 17 is drawn to scale, it will be needless to give more than a few of the dimensions of the apparatus. Each of the boxes was 42 inches long, 18 inches wide, and 72 inches deep, inside measurements. The alleys D, I, and H were 24 inches, and G 30 inches wide, by 6 feet deep. The doors of the several boxes were 18 inches wide, by 5 feet high, while those in the alleyways were 24 inches wide by 6 feet high. The response-compartment E of figure 17 was 14 feet 4 inches, by 8 feet, by 6 feet in depth. In order that the apparatus might be used with adult human subjects conveniently, if such use should prove desirable, the depth throughout was made 6 feet, and it was therefore possible for the experimenter to walk about erect in it.

The experimental procedure was briefly as follows: A small quantity of food having been placed in each of the food cups and covered by the metal flanges on the exit doors, the experimenter raised door 11 of figure 17 and then opened door 10 and the door of the cage in which the desired subject was confined. After the latter, in search of food, had entered the runway D, the experimenter lowered door 11 to keep it in this runway, and immediately proceeded to set the reaction-mechanisms for an experiment (trial). Let us suppose that the first setting to be tried involved all of the nine boxes. Each of the entrance doors would therefore be raised. Let us further suppose that the right door is defined as the middle one of the group. With the apparatus properly set, the experimenter next raises door 12, thus admitting the animal to the response-compartment E. Any one of the nine boxes may now be entered by it. But if any except number 5, the middle member of the group, be entered, the entrance door is immediately lowered and both the exit and entrance doors locked in position so that the animal is forced to remain in the box for a stated period, say thirty seconds. At the expiration of this time the entrance door is raised and the animal allowed to retrace its steps and make another choice. When the middle box is chosen, the entrance door is lowered and the exit door immediately raised, thus uncovering the food, which the animal eats. As a rule, by my monkeys and ape the reward was eaten in the alleyway G instead of in the multiple-choice box. As soon as the food has been eaten, the exit door is lowered by the experimenter, and the animal returns by way of G and H to runway D, where it awaits its next trial.

As rewards, bananas and peanuts were found very satisfactory, and although occasionally other foods were supplied in small quantities, they were on the whole less constantly desired than the former.

Four problems which had previously been presented to other organisms were in precisely the same form presented to the three primates. These problems may be described, briefly, by definition of the right reaction mechanism, thus: problem 1, the first mechanism at the subject's left; problem 2, the second mechanism at the subject's right (that is, from the end of the series at the subject's right); problem 3, alternately, the first mechanism at the subject's left and the first at its right; problem 4, the middle mechanism of the group.

It was my intention to present these four problems, in order, to each of the three animals, proceeding with them as rapidly as they were solved. But as it happened, only one of the three subjects got as far as the fourth problem. When observations had to be discontinued, Sobke was well along with the last, or fourth problem; Skirrl was at work at the third problem; and Julius had failed to solve the second problem.

For each of the problems, a series of ten different settings of the doors was determined upon in advance. These settings differ from those employed in a similar investigation with the pig only in that the numbering of the doors is reversed. In the present apparatus, the boxes as viewed from the front (entrance) are numbered from the left to the right end, whereas those of the pig apparatus were numbered from the right end to the left end.

Below are presented for each of the several problems (1) the numbers of the settings presented in series; (2) the numbers of the doors open; (3) the number of doors open in each setting and for the series of ten settings; and (4) the number of the right door.

PROBLEM 1. First mechanism at left of group

Settings	Doors open	No. of doors open	No. of right door
1	1.2.3	3	1
2	8.9	2	8
3	3.4.5.6.7	5	3
4	7.8.9	3	7
5	2.3.4.5.6	5	2
6	6.7.8	3	6
7	5.6.7	3	5
8	4.5.6.7.8	5	4
9	7.8.9	3	7
10	1.2.3	3	1
		Total 35	

PROBLEM 2. Second mechanism from the right end of group

Settings	Doors open	No. of doors open	No. of right door
1	7.8.9	3	8
2	1.2.3.4	4	3
3	2.3.4.5.6.7	6	6
4	1.2.3.4.5.6	6	5
5	4.5.6.7.8	5	7
6	1.2.3	3	2
7	2.3.4.5	4	4
8	1.2.3.4.5.6.7.8.9	9	8
9	1.2.3.4	4	3
10	3.4.5.6.7.8	6	7
		Total 50	

PROBLEM 3. Alternately the first mechanism at the left and the first at the right end of the group

Settings	Doors open	No. of doors open	No. of right door
1	5.6.7	3	5
2	5.6.7	3	7
3	1.2.3.4.5.6	6	1
4	1.2.3.4.5.6	6	6
5	4.5.6.7.8	5	4
6	4.5.6.7.8	5	8
7	2.3.4.5	4	2
8	2.3.4.5	4	5
9	3.4.5.6.7.8.9	7	3
10	3.4.5.6.7.8.9	7	9
		Total 50	

PROBLEM 4. Middle mechanism of the group

Settings	Doors open	No. of doors open	No. of right door
1	2.3.4	3	3
2	5.6.7.8.9	5	7
3	1.2.3.4.5.6.7	7	4
4	7.8.9	3	8
5	4.5.6.7.8	5	6
6	1.2.3.4.5.6.7.8.9	9	5
7	1.2.3	3	2
8	2.3.4.5.6	5	4
9	3.4.5.6.7.8.9	7	6
10	6.7.8	3	7
		Total 50	

It was found desirable after a problem had been solved to present a new and radically different series of settings in order to determine to what extent the subject had learned to choose the correct door by memorizing each particular setting. These supplementary observations may be known as control experiments, and the settings as supplementary settings. In case of these, as for the original settings, the essential facts are presented in tabular arrangement.

Settings for Control Experiments

PROBLEM 1. First at left end

Settings	Doors open	No. of doors open	No. of right door
1	2.3.4	3	2
2	6.7.8.9	4	6
3	3.4.5	3	3
4	4.5.6.7.8.9	6	4
5	6.7.8.9	4	6
6	1.2.3.4.5	5	1
7	2.3.4.5.6.7.8	7	2
8	3.4.5.6.7.8	6	3
9	5.6.7	3	5
10	1.2.3.4.5.6.7.8.9	9	1

PROBLEM 2. Second from right end

Settings	Doors open	No. of doors open	No. of right door
1	5.6.7.8	4	7
2	2.3.4.5.6	5	5
3	1.2.3.4.5.6.7.8.9	9	8
4	5.6.7	3	6
5	1.2.3.4	4	3
6	4.5.6	3	5
7	2.3.4.5	4	4
8	1.2.3	3	2
9	1.2.3.4.5.6.7	7	6
10	2.3.4.5.6.7.8.9	8	8

PROBLEM 3. Alternate left and right ends

Settings	Doors open	No. of doors open	No. of right door
1	5.6	2	5
2	5.6	2	6
3	4.5.6.7.8.9	6	4
4	4.5.6.7.8.9	6	9
5	1.2.3.4.5	5	1
6	1.2.3.4.5	5	5
7	2.3.4.5.6.7	6	2
8	2.3.4.5.6.7	6	7
9	3.4.5.6.7.8	6	3
10	3.4.5.6.7.8	6	8

PROBLEM 4. Middle

Settings	Doors open	No. of doors open	No. of right door
1	4.5.6.7.8	5	6
2	1.2.3	3	2
3	1.2.3.4.5.6.7.8.9	9	5
4	2.3.4.5.6	5	4
5	6.7.8	3	7
6	3.4.5.6.7.8.9	7	6
7	7.8.9	3	8
8	1.2.3.4.5.6.7	7	4
9	2.3.4	3	3
10	3.4.5.6.7	5	5

It was my aim so far as possible to present to a given subject each day the ten settings under a given problem in order, without interruption. If for any reason the series of observations had to be interrupted, it was resumed at the same point subsequently. Occasionally it was found desirable or necessary to present only five of the series of ten settings in succession and then to interrupt observations for an interval of a few minutes or even several hours. But as a rule it was possible to present the series of ten settings. All things being considered, it proved more satisfactory to give only ten trials a day to each subject. Frequently twenty and rarely thirty trials were given on the same day. In such cases the series of settings was simply repeated. The only pause between trials was that necessary for resetting the entrance doors and replenishing the food which served as a reward for success.

III

RESULTS OF MULTIPLE-CHOICE EXPERIMENTS

1. Skirrl, *Pithecus irus*

Problem 1. *First at the Left End*

Systematic work with the multiple-choice apparatus and method described in the previous section was undertaken early in April with Skirrl, Sobke, and Julius. The results for each of them are now to be presented with such measure of detail as their importance seems to justify.

Skirrl had previously been used by Doctor Hamilton in an experimental study of reactive tendencies. He proved so remarkably inefficient in the work that Doctor Hamilton was led to characterize him as feeble-minded, and to recommend him to me for further study because of his mental peculiarities. With me he was from the first frank, aggressive, and inclined to be savage. It was soon possible for me to go into the large cage, Z, with him and allow him to take food from my hand. He was without fear of the experimental apparatus and it proved relatively easy to accustom him to the routine of the experiment. Throughout the work he was rather slow, inattentive, and erratic.

Beginning on April 7, I sought to acquaint him with the multiple-choice apparatus by allowing him to make trips through the several boxes, with the reward of food each time. Thus, for example, with the entrance and exit doors of box 7 raised, the monkey was allowed to pass into the reaction-compartment E and thence through box 7 to the food cup. As soon as he had finished eating, he was called back to D by the experimenter and, after a few seconds, allowed, similarly, to make a trip by way of one of the other boxes. By reason of this preliminary training he soon came to seek eagerly for the reward of food.

On April 10 the apparatus was painted white in order to increase the lightness and thus render it easier for the experimenter to observe the animal's movements, and when on April 12 Skirrl was again introduced to it for further preliminary training, he utterly refused to enter the boxes, giving every indication of extreme fear of the white floors and even of the sides of the boxes. Finally, the attempts to induce him to enter the boxes had to be given up, and he was returned to his cage unfed. The following day I was equally unsuccessful in either driving or tempting him with food into the apparatus. But on April 14 he was so hungry that he was finally lured in by the use of food. He cautiously approached the boxes and attempted to climb through on the sides instead of walking on the floor. It was perfectly evident that he had an instinctive or an acquired fear of the white surfaces. As the matter was of prime importance for the success of my work, I inquired of Doctor Hamilton, and of the men in charge of the cages, for any incident which might account for this peculiar behavior, and I learned that some three months earlier, while the animal cages were being whitewashed, Skirrl had jumped at one of the laborers who was applying a brush to the framework of one of the cages and had shaken some lime into his eyes. He was greatly frightened and enraged. Evidently he experienced extreme discomfort, if not acute pain, and there resulted an association with whiteness which was quite sufficient to cause him to avoid the freshly painted apparatus.

Having obtained an adequate explanation of this monkey's peculiar behavior, I proceeded with my efforts to induce him to work smoothly and rapidly, and on April 15, by covering the floor with sawdust, I so diminished the influence of the whiteness as to render the preliminary training fairly satisfactory. At the end of two more days everything was going so well that it seemed desirable to begin the regular experiment.

On the morning of April 19, Skirrl was introduced to the apparatus and given his first series of ten trials on problem 1. This problem demanded the selection of the first door at the left in any group of open doors. The procedure was as previously described in that the experimenter raised the entrance doors of a certain group of boxes, admitted the animal to the reaction-chamber, punished incorrect choices by confining the animal for thirty seconds, and rewarded correct choices by raising the exit door and thus permitting escape and the obtaining of food. The trials were given in rapid succession, and the total time required for this first series of ten trials was thirty-five minutes. Skirrl worked faithfully throughout this interval and exhibited no marked discouragement. When confined in a box he showed uneasiness and dissatisfaction by moving about constantly, shaking the doors, and trying to raise them in order to escape.

For the series of settings used in connection with problem 1, the reader is referred to page 18. In the first setting, the doors numbered 1, 2, and 3, were opened. As it happened, the animal when admitted to the reaction-chamber immediately chose box 1. Having received the reward of food, he was called back to D, and doors 8 and 9 having been raised in preparation for the next trial, he was again admitted to the reaction-chamber. This time he quickly chose box 9 and was confined therein for thirty seconds. On being released, he chose after an interval of four minutes, box 8, thus completing the trial.

As it is highly important, not only in connection with the present description of behavior, but also for subsequent comparison of the reactions of different types of organism in this experiment, to present the detailed records for each trial, tables have been constructed which offer in brief space the essential data for every trial in connection with a given problem.

Table 1 contains the results for Skirrl in problem 1. It is constructed as follows: the date of a series of trials appears in the first vertical column; the numbers (and number) of the trials for the series or date appear in column 2; the following ten columns present respectively the results of the trials for each of the ten settings. Each number, in these results, designates a box entered. At the extreme right of the table are three columns which indicate, first, the number of trials in which the right box was chosen first, column headed R; and second, the number of trials in which at least one incorrect choice occurred, column headed W. In the last column, the daily ratio of these first choices appears.

Taking the first line of table 1 below the explanatory headings, we note on April 19 ten trials, numbered 1 to 10, were given to Skirrl. In trial 1, with setting 1, he chose correctly the first time, and the record is therefore simply 1. In trial 2, setting 2, he incorrectly chose box 9, the first time. At his next oppor-

TABLE 1

Results for Skirrl, *P. irus*, in Problem 1

Date	No. of trials	S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	S. 10	R	W	Ratio of R to W
		1.2.3	8.9	**3**.4.5.6.7	**7**.8.9	**2**.3.4.5.6	**6**.7.8	**5**.6.7	**4**.5.6.7.8	**7**.8.9	**1**.2.3			
April 19	1- 10	1	9.8	6.7.3	9.7	6.2	7.8.6	6.7.7.7 6.5	4	7	2.3.3.1	3	7	1:2.33
20	11- 20	3.2.1	9.8	5.3	7	4.2	8.8.6	5	8.4	7	3.1	3	7	1:2.33
21	21- 30	3.1	8	3	8.7	6.2	6	5	6.4	9.7	1	5	5	1:1.00
22	31- 40	1	9.8	3	7	6.2	6	6.7.5	5.8.4	9.8.9.8.7	2.1	4	6	1:1.50
23	41- 50	2.3.1	8	5.7.3	7	4.2	6	5	7.8.4	7	3.1	5	5	1:1.00
24	51- 60	1	8	4.5.7.3	9.7	5.6.2	6	6.7.5	6.4	8.9.7	1	4	6	1:1.50
26	61- 70	1	8	6.7.4.7.3	7	4.5.6.2	6	5	8.4	7	3.2.3.1	6	4	1: .67
27	71- 80	3.1	8	3	9.7	4.6.2	7.6	6.5	5.8.4	7	1	4	6	1:1.50
28	81- 90	2.3.1	8	3	7	4.5.6.2	6	5	5.8.4	7	1	7	3	1: .43
29	91- 100	1	8	3	9.7	6.2	6	5	4	7	1	8	2	1: .25
30	101- 110	1	8	4.3	7	5.6.2	6	5	4	7	2.3.1	7	3	1: .43
May 1	111- 120	2.3.2.1	8	3	7	2	6	5	4	7	1	9	1	1: .11
3	121- 130	1	8	5.6.3	7	4.5 2	6	5	4	7	1	8	2	1: .25
4 and 5	131- 140	3.2.1	8[1]	3	7	2	6	5	4	7	1	9	1	1: .11
5	141- 150	1	8	4.3	7	2	6	5	4	7	1	9	1	1: .11
		2.3.4	**6**.7.8.9	**3**.4.5	**4**.5.6.7.8.9	**6**.7.8.9	**1**.2.3.4.5	**2**.3.4.5 6.7.8	**3**.4.5.6.7.8	**5**.6.7	**1**.2.3.4.5 6.7.8.9			
6	1- 10	2	6	3	4	6	3.2.1	6.2	5.6.7.8.3	5	6.1	6	4	1: .67

---

[1] End of series on May 4.

tunity, he chose box 8, which was the right one. The record therefore reads 9.8. In trial 3, setting 3, he chose incorrectly twice before finally selecting the right box. The record reads 6.7.3, and so on throughout the ten trials which constitute a series. The summary for this series indicates three right and seven wrong first choices, that is, three cases in which the right box was entered first. The ratio of right to wrong first choices is therefore 1 to 2.33. Since the total number of doors open in the ten settings is thirty-five, and since in each of the ten settings one door is describable as the right door, the probable ratio, apart from the effects of training, of right to wrong first choices is 1 to 2.50. It is evident, therefore, that Skirrl in his first series of trials closely approximated expectation in the number of mistakes.

By reading downward in any particular column of results, one obtains a description of the changes in the animal's reaction to a particular setting of the doors. Thus, for instance, in the case of setting 1, which was presented to the animal in trials numbered 1, 11, 21, and so on to 141, it is clear from the records that no definite improvement occurred. But oddly enough, in the case of setting 10, which presented the same group of open doors, almost all of the reactions are right in the lower half of the column. For setting 2, it is evident that mistakes soon disappeared.

Comparison of the data of table 1 indicates that the number of correct first choices is inversely proportional to the number of doors in use, while the number of choices made in a given trial is directly proportional to the number of doors in use.

During the first week of work on this problem, Skirrl improved markedly. His performance was somewhat irregular and unpredictable, but on the whole the experiment seemed fairly satisfactory. Cold, cloudy, or rainy days tended to diminish steadiness and to increase the number of mistakes. Similarly, absence of hunger was unfavorable to continuous effort to find the right box.

The period of confinement, as punishment for wrong choices, was increased from thirty seconds to sixty seconds on April 26. But there is no satisfactory evidence that this favored the solution of the problem. Work on May 4 was interrupted by a severe storm, the noise of which so distracted the monkey that he ceased to work. Consequently, observations were interrupted on the completion of trial 132, and on May 5, the series was begun with setting 3. On this date, eighteen trials were given in succession, and in only one of them did a mistake occur. Since the ten trials numbered 133 to 142 were correct, Skirrl was considered to have solved problem 1, and systematic training was discontinued.

On the following day, as a measure of the extent to which the animal had learned to select the first door at the left no matter what its position or the number of doors in the group presented, a control series was given in which the settings differed from the regular series of settings. These supplementary settings are presented at the bottom of table 1 together with the records of reaction in ten trials.

Since in only six of these ten control settings was the first choice correct, it is scarcely fair to insist that the animal was reacting on the basis of an ideational solution of the problem. Rather, it would seem that he had learned to react to particular settings. A careful study of all of the data of response, together with notes on the varied behavior of the animal during the experiments, justifies the statement that Skirrl's solution of problem 1 was incomplete and unreliable. It was highly dependent upon the particular situation, or even the particular door at the left end of the group, and slightly if at all dependent upon anything comparable to the human idea of first at the left of the group.

This particular series of observations has been described and discussed in some detail in order to make the chief points of method clear. It will be needless, hereafter, to refer explicitly to many of the characteristics of reaction or to the important points in the construction of tables which have been mentioned.

A graphic representation of Skirrl's learning process in problem 1 is presented in figure 18. The irregularities are most striking, and fairly indicate the erraticness of the animal. The curve is based upon the data in next to the last column of table 1, that is, the column presenting the errors or wrong first choices in each series of trials.

Unquestionably, the form of such a curve of learning should be considered in connection with the method or methods of selecting the right box employed by the animal during the course

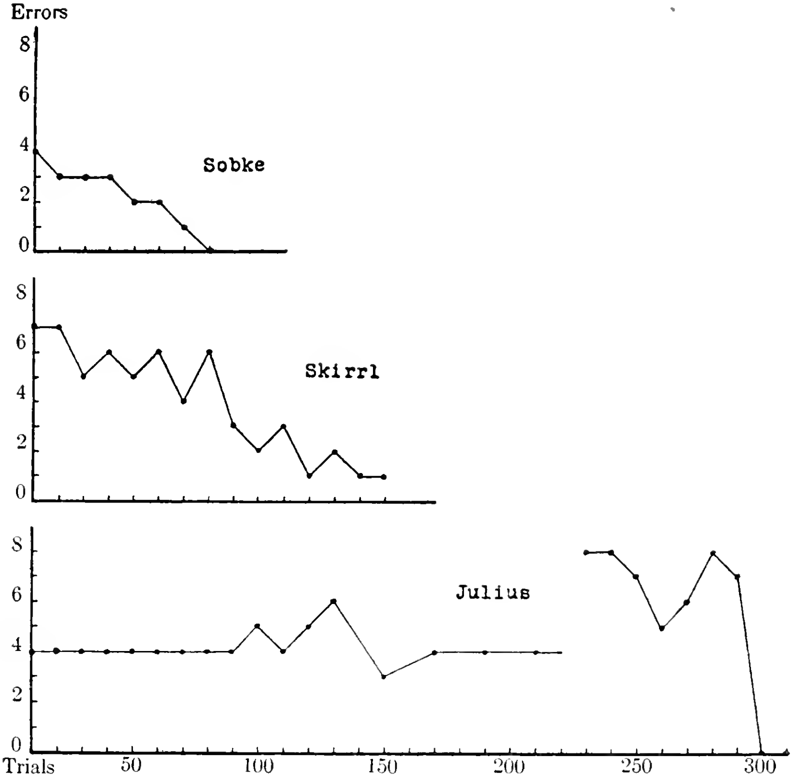


FIGURE 18.—Error curves of learning for the solution of problem 1 (first box at left end).

of experimentation. It appears from an analysis of the behavior of Skirrl in problem 1 that there developed a single definite and persistent method, namely, that of going to one box in the group, and in case it happened to be a wrong one, of choosing, on emergence from it, the next toward the right end of the group, and so on down the line. Having reached the extreme right end, the tendency was to follow the side of the reaction-chamber around to the opposite end and to enter the first box at the left end of the group, which was, of course, the right one. This method appears, with certain slight variations, in approximately ninety per cent of the trials which involved incorrect choices. Thus, in the case of trials 121 to 130, of which eight exhibit right first choices, the remaining two exhibit the method described above except that the final member at the right end of the group was in each case omitted.

On the whole, Skirrl's behavior in connection with this problem appears to indicate a low order of intelligence. He persisted in such stupid acts as that of turning, after emergence from the right box, toward the right and passing into the blind alley I, instead of toward the left, through G and H, to D. In contrast with the other animals, he spent much time before the closed doors of the boxes, instead of going directly to the open doors, some one of which marked the box in which the reward of food could be obtained. It is, moreover, obvious that his responses, as they appear in table 1, are extremely different from those of a human being who is capable of bringing the idea of first at the left end to bear upon the problem in question.

Problem 2. Second from the Right End

Following the series of control trials of problem 1 given to Skirrl on May 6, a period of four days was allowed during which the animal was merely fed in the boxes each day. This was done in order that he should partially lose the effects of his previous training to choose the first box at the left before being presented with the second problem, the second box from the right.

On May 11 regular experimentation was begun with problem 2. Naturally the situation presented unusual difficulties to the monkey because of his previously acquired habit, and on the first day it was possible to give only five trials, in all except the first of which Skirrl had to be aided by the experimenter to find the right box. He persistently, as appears in the first line of records of table 2, entered the first box at the left. The series was continued on May 13, but with very unsatisfactory results, since he apparently had been greatly discouraged by the unusual difficulties previously met. Only four trials could be given, and in these the showing made was very poor. It is noteworthy, however, that in trials 6, 7, and 8, May 13, there was no marked tendency to choose the first box at the left. Thus quickly had the force of the previous habit been broken.

For problem 2, the total number of open doors in the ten settings is fifty, as appears from the data on page 18, and as ten of these fifty open doors may be defined as right ones, the expected ratio of right to wrong first choices in the absence of previous training is 1 to 4. The actual ratio for the first series given in problem 2 is 1 to 8, while in the second series it is 0 to 10.

On the morning of May 13, work was interrupted in the ninth trial by what seemed at the moment a peculiarly unfortunate accident, but in the light of later developments, an incident most fruitful of valuable results.

Skirrl, in trial 9, directly entered box 1. Since this was not the right box, he was punished by being confined in it for ten seconds. While in the box he howled and when the entrance door was raised for him to retrace his steps, he came out with a rush, showing extreme excitement and either rage or fear, I could not be sure which. At intervals he uttered loud cries, which I am now able to identify as cries of alarm. Repeatedly he went to the open door of box 1 and peered in, or peered down through the hole in the floor which received the staple on the door. He refused to enter any one of the open boxes and continued, at intervals of every half minute or so, his cries. For thirty minutes I waited, hoping to be able to induce him to complete the series of trials, but in vain. Although it was obvious that he was eager to escape from the apparatus, he would not enter any of the boxes even when the exit doors were raised. Instead, he gnawed at the door (12 in fig. 17) to the alleyway D and attempted to force his way through, instead of taking the easy and clear route to the alleys, through one of the boxes. His behavior was most surprising and puzzling. Finally, I gave up the attempt to complete the series and returned him to his cage by way of the entrance door to the response-compartment E.

I then entered the apparatus to seek some explanation of the animal's behavior, and my search was rewarded by the finding of two sharp pointed nails which protruded for an inch or more in the middle of the floor of box 1. My assistant, who had been charged with the task of installing the locks for the several doors, had used nails instead of screws for attaching staples underneath the floor and had neglected to clinch the nails. Skirrl, in the dim light of the box, doubtless stepped upon one of the nails and inflicted a painful, although not serious, injury upon himself. It was impossible for him to see clearly the source of his injury. He was greatly frightened and expressed the emotion most vigorously. His behavior strongly suggested a superstitious dread of some unseen danger. It may be that the instinctive fear of snakes, so strong in monkeys, was partly responsible for his response.

The first result of this accident was that more than two weeks were lost, for it was impossible, during the next few days, to induce the animal to enter any of the multiple-choice boxes voluntarily. From May 14 to May 24, I labored daily to overcome his newly acquired fear. The usual procedure was to coax him through one box after another by standing at the exit door with some tempting morsel of food. After several days of this treatment, he again trusted himself to the boxes, although very circumspectly and only when both entrance and exit doors were raised. Not until May 24 was it possible to resume regular experimentation, and on that day it was found necessary to indicate the right box by raising the exit door slightly and then immediately lowering it. Trials in which this form of aid was given are indicated in table 2 by a star following the last choice.

Gradually, Skirrl regained his confidence in the apparatus and began to work more naturally. For a long time he would not stand punishment, and it was necessary for the experimenter to be very careful in locking the doors, since the sound of the bar sliding beneath the floor often frightened and caused him to quit work. Day after day the tendency to peer through the holes in the floor at the entrance to the boxes rendered it clear that the animal feared some danger from beneath the floor. This behavior was so persistent that much time was wasted in the experiments.

On the last day of May, punishment by confinement for ten seconds in wrong boxes was introduced, but since this tended to discourage the monkey, there was substituted for it on June 1 the punishment of forcing him to work his way out of each wrong box by raising the entrance door which had been closed behind him. This he could fairly readily do, and his stay in a box rarely measured more than ten seconds.

As a variation in the mode of procedure, confinement for thirty seconds was tried on June 5, but it worked unsatisfactorily and had to be abandoned. During this series, the animal was startled by the sound from one of the sliding bars under the floor, and in the sixth trial he refused to work.

As improvement was very slow, varied modes of rewarding and punishing the animal were tried in the hope of discovering a means of facilitating the work. Among the former are the use of banana, grapes, peanuts, and other eagerly sought foods in varying quantities, and in the latter are included periods of confinement ranging from ten seconds to sixty seconds. In the end, confinement of about thirty seconds, combined with a small quantity of food which was much to the monkey's taste, gave most favorable results.

All this time Skirrl's attention to the task in hand was seldom good. He was easily diverted and even when extremely hungry, often stopped work in the middle of an early trial, yawned repeatedly and finally sat down to wait for release from the apparatus.

The results obtained during the long continued trials with this animal in problem 2 are presented in table 2, which differs from the previously described table, first, in that several of the trials are followed by an asterisk to indicate that aid was given by the experimenter, and second, in that two additional columns, headed, respectively, R and W, are presented. These give the right and wrong first choices for each day, whereas the two columns preceding them give the same data for each series of ten trials. Similarly, the ratio of right to wrong choices is presented for each day in table 2, instead of for each series of ten trials as in table 1.

From the results of table 2, several peculiarly interesting facts appear. In the first place the influence of the habit of choosing the first box at the left disappears with surprising suddenness, and in the second place, there are remarkable contrasts in the results for different settings as they appear in their respective vertical columns. Thus, in the case of setting 1, after the first trial mistakes became relatively infrequent, whereas in setting 6, which involved the same number of doors, mistakes continued to be the rule until nearly a thousand trials had been given. The most likely explanation of this difference is that for some reason the animal avoided box 9.

The *reactive tendencies*, or better, the *methods of reaction* which manifested themselves during this long series of observations

TABLE 2

Results for Skirrl, *P. irus*, in Problem 2

Date	No. of trials	S. 1 7.**8**.9	S. 2 1.2.**3**.4	S. 3 2.3.4.5.**6**.7	S. 4 1.2.3.4.**5**.6	S. 5 4.5.6.**7**.8	S. 6 1.**2**.3	S. 7 2.3.**4**.5	S. 8 1.2.3.4.5 6.7.**8**.9	S. 9 1.2.**3**.4	S. 10 3.4.5.6.**7**.8	R	W	R	W	Ratio of R to W
May 11 & 13	1- 9	7.7.9.7.8	1.2.2.1.4.1 2.1.2.1.3	2.3.2.3.2.5 2.3.2.5.6	4.6.1.4.1.1 2.6.1.6.5	4.4.7	3.1.2	4	4.1.8	1		1	8	1	8	1: 8.00
24	11- 20	8*[1]	2.4.3*	4.5.6*	2.2.5*	5.6.6.7*	3.1.2	5.2.3.5.3.2 3.5.2.4*	4.6.8*	4.4.3*	5.5.6.7*	0	10	0	10	0:10.00
25	21- 30	8*	4.4.3*	5.6	6.6.2.3.4 6.6.5*	6.7	2	4	5.6.3.8	4.4.3	6.4.6.8.7	2	8	2	8	1: 4.00
26	31- 40	8	4.3	6	4.5	6.7	3.2	5.4	5.8	4.3	5.3.8.7	2	8	2	8	1: 4.00
27	41- 50	8	4.4.3	6	5	6.8.6.8.7	3.3.3.2	5.4	6.5.4.3 2.1.5.8	4.3	5.4.8.7	3	7	3	7	1: 2.33
28	51- 60	8	4.4.3	7.6	5	5.6.7	3.3.3.2	4	5.4.3 3.6.8	4.3	5.4.3.3.4.5 6.4.3.5.7	3	7	3	7	1: 2.33
29	61- 70	8	4.3	6	6.6.5	7	3.3.3.2	5.4	7.6.4.7.6.8	4.3	7	4	6	4	6	1: 1.50
31	71- 80	8	4.4.4.3	6	6.5	6.8.7	3.2	5.4	6.7.6.4.3 2.6.3.7.8	4.3	6.7	2	8	2	8	1: 4.00
June																
1	81- 90	8	4.3	6	5	6.8.6.5.4 6.5.6.5.8 5.4.6.4.7	3.1.3.2	5.4	8	4.3	7	5	5			
"	91- 100	9.7.8	4.2.4.3	7.5.6	5	6.8.7	3.3.1.2	5.3.4	8	4.3	6.8.7	2	8	7	13	1: 1.86
2	101- 110	8	4.3	6	5	7	3.2	5.4	7.8	4.3	6.8.6.5.7	4	6			
"	111- 120	8	4.3	7.3.5.7.6	6.2.3.6.4 3.6.2.5	7	3.2	5.2.3.5.3.2 3.5.2.3.4	9.6.4.7.8	4.1.2 4.2.3	6.8.7	2	8	6	14	1: 2.33
3	121- 130	8	4.4.3	6	5	6.7	3.2	5.3.2.3 5.2.5.4	8	4.2.3	6.8.6.3 5.4.5.8.8 6.3.8.7	4	6			
"	131- 140	8	4.3	5.7.3.2.6	4.5	5.7	1.3.2	5.3.4	6.7.8	4.2.1.3	7	2	8	6	14	1: 2 33
4	141- 150	8	4.3	7.6	6.5	7	2	5.3.2.3 5.5.4	6.8	4.1.3	5.6.7	3	7			

June																
4	151- 160	8	4.3	6	5	6.7	2	4	5.6.7.8	4.3	5.6.8.7	5	5	8	12	1: 1.50
5	161- 170	8	4.3	7.6	6.5	6.8.7	3.2	5.3.2.3.5.4	8	4.3	6.7	2	8			
“	171- 176	8	2.4.3	7.6	6.5*	8.7	3.2*					1	5	3	13	1: 4.33
7	177- 180							5.4	8	4.4.3	8.7	1	3			
“	181- 190	8	4.3	7.6	6.5	7	3.2	5.3.2.5.4	8	4.3	7	4	6	5	9	1: 1.80
8	191- 200	8	4.3	7.6	6.5	8.7	3.2	5.4	8	4.3	8.7	2	8			
“	201- 210	8	4.3	6	6.4.6.5	8.7	3.2	5.4	8	4.3	7	4	6	6	14	1: 2.33
9	211- 220	8	4.3	7.6	6.5	8.7	3.2	5.4	8	4.3	8.6.7	2	8			
“	221- 230	9.8	4.3	7.6	6.5	6.7	3.2	5.4	7.6.8	4.3	7	1	9	3	17	1: 5.67
10	231- 240	8	4.3	7.6	6.5	6.7	3.2	5.4	3.2.3.2.4.3 2.5.4.7.8	4.3	7	2	8	2	8	1: 4.00
11	241- 250	8	4.3	7.6	6.6.5	8.7	3.2	5.4	8	4.3	8.7	2	8	2	8	1: 4.00
12	251- 260	8	4.3	6	6.5	6.7	3.3.2	5.4	7.6.7.7 6.9.8*	3	3.7*	3	7	3	7	1: 2.33
14	261- 270	8	3	6	6.5	7	3.2	5.4	5.3.4.3 9.8*	3	3.3.3.3.4 4.6.4.7*	5	5	5	5	1: 1.00
15	271- 280	7.9.8	4.2.3	3.4.3.7.6	6.5	8.7	3.2	5.4	8	4.3	8.7	1	9			
“	281- 290	8	4.3	7.6	6.5	7	3.2	5.4	7.8	4.3	7	3	7	4	16	1: 4.00
16	291- 300	7.8	4.4.4 4.4.3	6	6.5	7	3.3.2	5.4	4.3.2.3 6.5.4.3 5.6.7.8	4.3	6.7	2	8			
“	301- 310	8	4.4.3	7.6	6.5	7	3.3.2	5.5.4	7.6.5.4.6 5.7.9.8	4.3	7	3	7	5	15	1: 3.00
17	311- 320	7.8	4.3	7.6	6.6.5	7	3.2	5.4	7.6.7.6.7.8	4.3	7	2	8			
“	321- 330	8	4.3	7.6	6.5	8.7	3.2	5.4	8	4.3	6.7	2	8	4	16	1: 4.00
18	331- 340	7.7.8	4.3	6	6.5	7	3.2	5.4	8	4.3	7	4	6			
“	341- 350	8	4.3	7.6	6.6.5	8.7	3.2	5.4	8	4.3	7	3	7	7	13	1: 1.86
19	351- 360	8	4.3	7.6	6.5	6.5.6.5.7	3.2	5.4	8	4.3	7	3	7			
“	361- 370	8	4.3	7.6	6.4.3.6.5	7	3.2	5.4	9.8	4.3	7	3	7	6	14	1: 2.33
21	371- 380	8	4.3	7.6	6.5	7	3.2	5.4	7.8	4.3	8.7	2	8			
“	381- 390	8	4.3	7.6	6.5	7	3.2	5.4	8	4.3	7	4	6	6	14	1: 2.33
22	391- 400	8	4.3	7.6	6.5	6.5.4.6.7	3.3.3.2	5.4	6.7.8	4.4.3	7	2	8			
“	401- 410	8	3	7.6	6.5	8.7	2	5.4	6.7.7.8	3	7	5	5	7	13	1: 1.86
23	411- 420	8	4.4.3	7.6	6.5	8.7	3.2	5.4	7.6.7.6 6.7.8	4.3	7	2	8	2	8	1: 4.00
24	421- 430	8	4.3	6	6.5	7	3.2	5.4	8	3	7	6	4			
“	431- 440	8	3	7.6	6.5	7	3.3.2	5.4	8	4.3	7	5	5	11	9	1: 0.82

---

[1] First choices correct by reason of aid from the experimenter are not counted as correct (R) in the summary.

* Aided by experimenter.

TABLE 2—*Continued*

Results for Skirrl, *P. irus*, in Problem 2

Date	No. of trials	S. 1 7.8.9	S. 2 1.2.**3**.4	S. 3 2.3.4.5.**6**.7	S. 4 1.2.3.4.**5**.6	S. 5 4.5.6.**7**.8	S. 6 1.**2**.3	S. 7 2.3.**4**.5	S. 8 1.2.3.4.5 6.7.**8**.9	S. 9 1.2.**3**.4	S. 10 3.4.5.6.**7**.8	R	W	R	W	Ratio of R to W
June 25	441- 450	7.8	4.4.3	7.6	6.5	6.5.7	3.3.2	5.5.4	7.8	4.3	7	1	9	1	9	1: 9.00
26	451- 460	7.8	4.3	7.6	6.5	7	3.2	5.4	8	4.3	7	3	7			
"	461- 470	8	4.3	7.6	6.5	7	3.2	5.4	8	4.3	8.7	3	7	6	14	1: 2.33
28	471- 480	8	4.4.3	7.6	6.5	7	3.2	5.4	9.8	3	8.7	3	7	3	7	1: 2.33
29	481- 490	8	4.3	7.7.6	6.5	7	3.2	5.4	8	4.3	8.7	3	7	3	7	1: 2.33
30	491- 500	7.9.8	4.3	7.6	6.5	7	3.2	5.4	8	4.3	7	3	7			
"	501- 510	8	4.3	7.6	6.5	8.7	3.2	5.4	8	4.4.3	8.7	2	8	5	15	1: 3.00
July 1	511- 520	8	4.3	7.6	6.5	7	3.2	5.4	8	4.3	7	4	6	4	6	1: 1.50
2	521- 530	8	4.3	7.6	6.5	7	3.2	5.4	7.6.5.6.5 6.5.6.8	4.4.3	7	3	7			
"	531- 540	8	4.3	7.6	6.5	7	3.2	5.4	8	4.3	7	4	6	7	13	1: 1.86
3	541- 550	7.8	4.4.3	6	6.5	7	3.2	5.4	8	4.3	5.5.7	3	7			
"	551- 560	7.8	4.3	6	6.5	7	3.2	5.4	8	3	7	5	5	8	12	1: 1.50
5	561- 570	7.7.8	4.3	6	6.5	6.7	3.3.2	5.4	8	4.3	7	3	7			
"	571- 580	8	4.3	6	6.5	7	3.2	5.4	7.8	4.3	7	4	6	7	13	1: 1.86
6	581- 590	7.8	4.3	7.6	6.6.5	6.5.4.6.5 5.4.5.4.4 6.5.6.5.8.7	2	3.4	6.5.4.3.7.8	3	7	3	7	3	7	1: 2.33
7	591- 600	8	4.3	6	6.5	7	3.2	5.4	7.8	4.3	8.7	3	7			
"	601- 610	7.8	4.3	7.6	6.5	8.7	3.2	5.4	8	4.3	8.7	1	9	4	16	1: 4.00
8	611- 620	8	4.3	7.6	6.5	8.7	3.2	5.4	8	4.3	8.7	2	8			
"	621- 630	8	4.3	7.6	6.5	8.7	3.2	5.4	9.8	4.3	8.7	1	9			
"	631- 640	8	4.4.3	7.7.6	6.5	8.7	3.2	4	8	4.3	8.7	3	7	6	24	1: 4.00
9	641- 650	7.8	4.3	7.6	6.5	6.7	3.2	3.2.5.3 2.5.4	7.6.5.4.8	3	8.7	1	9	1	9	1: 9.00
10	651- 660	7.8	4.3	6	6.5	7	3.2	5.4	6.5.4.3.7 6.5.4.7.6 5.4.8	4.3	7	3	7			

10	661- 670	8	3	7.6	5	7	3.2	5.4	8	3	8.7	6	4	9	11	1: 1.22
12	671- 680	7.8	3	6	6.5	7	3.2	5.4	6.5.4.7.8	4.3	8.7	3	7	3	7	1: 2.33
13	681- 690	8	3	7.6	6.5	6.5.4 6.5.7	3.2	4	6.7.8	3	6.5.4.5 6.5.8.7	4	6			
“	691- 700	8	3	6	5	7	3.2	5.4	8	3	7	8	2	12	8	1: 0.67
14	701- 710	8	3	7.6	6.5	6.5.4.5 4.6.8.7	2	3.5.4	8	3	7	6	4			
“	711- 720	8	3	6	5	7	2	5.4	6.5.4.8	3	6.5.7	7	3	13	7	1: 0.51
15	721- 730	7.8	3	6	6.5	7	3.2	5.4	8	4.3	7	5	5			
“	731- 740	8	3	7.6	6.5	7	3.2	5.4	8	3	7	6	4	11	9	1: 0.82
16	741- 750	7.8	3	6	6.5	7	3.2	4	8	3	7	7	3			
“	751- 760	7.8	3	7.6	6.5	7	2	4	7.8	4.3	7	5	5	12	8	1: 0.67
17	761- 770	8	4.3	6	5	8.7	3.2	5.4	8	4	7	6	4			
“	771- 780	8	2.2.3	7.6	6.5	7	3.2	5.4	7.8	4.3	7	3	7	9	11	1: 1.22
19	781- 790	8	3	7.6	5	7	3.2	3.4	7.6.5.8	3	7	6	4			
“	791- 800	7.8	3	6	5	7	2	5.4	8	3	6.5 6.7	7	3			
“	801- 810	8	2.3	6	5	6.5.7	2	5.4	8	3	7	7	3	20	10	1: 0.50
20	811- 820	7.8	3	7.6	5	7	3.3.2	5.4	8	2.2.3	7	5	5			
“	821- 830	8	3	6	6.5	7	3.2	4	8	2.3	8.7	6	4	11	9	1: 0.82
21	831- 840	8	3	5.4 5.6	5	7	2	4	6.7.8	3	8.7	7	3			
“	841- 850	8	3	7.6	5	7	3.2	3.2.4	8	3	7	7	3	14	6	1: 0.43
22	851- 860	8	4.3	6	5	7	3.2	3.5.4	8	3	8.7	6	4			
“	861- 870	7.8	4.3	7.6	6.5	8.7	3.2	4	8	3	8.7	3	7			
“	871- 880	8	4.3	7.6	6.5	8.7	3.2	5.4	8	4.3	8.7	2	8	11	19	1: 1.73
23	881- 890	8	3	7.6	6.5	8.7	3.2	5.4	8	3	8.7	4	6			
“	891- 900	8	3	7.6	6.5	7	3.3.2	5.4	8	4.3	7	5	5	9	11	1: 1.22
24	901- 910	8	4.3	7.6	6.5	7	3.2	5.4	8	3	8.7	4	6			
“	911- 920	8	3	7.6	5	7	3.2	5.4	8	3	7	7	3	11	9	1: 0.82
26	921- 930	7.8	3	7.6	5	7	3.2	5.4	8	2.2.3	8.7	4	6			
“	931- 940	8	3	7.6	6.5	8.7	3.2	5.5.4	8	4.3	8.7	3	7	7	13	1: 1.86
27	941- 950	8	3	6	6.5	7	3.2	5.4	8	3	7	7	3	7	3	1: 0.43
28	951- 960	8	3	7.6	6.5	5.4.7	2	5.5.4	8	3	7	6	4	6	4	1: 0.67
29	961- 970	8	3	7.6	5	8.7	2	4	8	3	7	8	2	8	2	1: 0.25
30	971- 980	8	3	4.3.2.6	5	6.5.4.6 6.5.7	2	5.5.4	8	3	7	7	3	7	3	1: 0·43
31	981- 990	8	3	6	6.5	8.7	2	4	8	3	7	8	2	8	2	1: 0.25
August 2	991-1000	8	3	7.6	5	7	2	2.3 5.3 2.3.3.4	7.6.8	3	7	7	3	7	3	1: 0.43

TABLE 2—*Concluded*

Results for Skirrl, *P. irus*, in Problem 2

Date	No. of trials	S. 1 7.8.9	S. 2 1.2.3.4	S. 3 2.3.4.5.6.7	S. 4 1.2.3.4.5.6	S. 5 4.5.6.7.8	S. 6 1.2.3	S. 7 2.3.4.5	S. 8 1.2.3.4.5 6.7.8.9	S. 9 1.2.3.4	S. 10 3.4.5.6.7.8	R	W	R	W	Ratio of R to W
August 3	1001-1010	8	3	7.6	5	7	2	4	7.6.5.6.7.8	3	5.4.3.4.3.7	7	3			
"	1011-1020	8	2.3	5.6	3.2.3.6.5	7	2	5.4	9.8	2.1.3	7	4	6	11	9	1: 0.82
4	1021-1030	7.8	3	5.4.3.7.6	6.5	6.5.6 5.6.7	3.2	5.4	8	2.2.4.2 4.2.3	8.7	2	8			
"	1031-1040	7.8	3	6	6.4.3.6.5	7	2	3.5.4	8	2.3	8.7	5	5	7	13	1: 1.86
5	1041-1050	8	3	6	2.3.2.6.5	8.7	2	4	8	2.2.4.3	8.8.6.8.4 6.5.8.7	6	4	6	4	1: 0.67
6	1051-1060	8	3	6	4.2.6.5	7	3.2	5.4	8	3	8.7	6	4	6	4	1: 0.67
7	1061-1070	8	3	5.4.3.6	4.5	6.5.6.5 4.8.7	2	4	8	3	7	7	3	7	3	1: 0.43
9	1071-1080	8	3	6	5	7	2	4	8	3	7	10	0	10	0	1: 0.00
		5.6.7.8	2.3.4.5.6	1.2.3.4.5 6.7.8.9	5.6.7	1.2.3.4	4.5.6	2.3.4.5	1.2.3	1.2.3.4 5.6.7	2.3.4.5 6.7.8.9					
10	1- 10	6.5.7	3.2.6.5	8	6	2.4.3	5	5.4	2	7.5.2.7.6	8	5	5	5	5	1: 1.00
11	11- 20	7	3.6.5	8	6	3	6.5	4	3.2	7.6	8	6	4	6	4	1: 0.67
12	21- 30	7	2.2.6.5	7.8	6	3	5	3.2.3.5.3 2.5.3.2 5.3.2.5 2.5.5.4	2	6	8	7	3	7	3	1: 0.43

---

* Aided by experimenter.

may be described as follows: (a) choice of the first box at the left; (b) random choice with tendency to choose first, a box near the middle of the group; (c) choice of first box at the right followed by the one next to it on the left; (d) direct choice of the right box.

The method of choosing the first box at the right end and then the one next to it developed in the case of all except two of the ten settings. The time of appearance is worth noting. In setting 1, it failed to appear; in setting 2, it developed early,—after about one hundred trials; in setting 3, after about one hundred and fifty trials; in setting 4, after about one hundred and fifty trials; in setting 5, after about one hundred and seventy trials; in setting 6, after about one hundred trials; in setting 7, after about fifty trials; in setting 8, it never developed; in setting 9, after about fifty trials; and in setting 10, it developed very late,—after about four hundred and seventy trials.

This method of reaction, although inadequate, proved remarkably persistent, and it is doubtful whether it had been wholly overcome at the conclusion of the experiment. In the case of the series of trials given on June 8, numbered 191 to 200, the method used was either that of the first at the right and then the next, or direct choice of the right box.

Throughout the trials with this problem, the end boxes, numbers 1 and 9, were avoided. This is at least partially explained by the fact that they never existed, and obviously never could appear, in problem 2, as right boxes. In trials 601 to 610, given on July 7, there occurred partial return to the formerly established method of choosing the first door at the right. This relapse was characteristic of what happened during the many days which intervened between the definite appearance of this habit and the final solution of the problem.

Especially in connection with such relapses, Skirrl showed extreme fatigue or ennui and often would refuse to work and simply sit before the open doors yawning. This happened even when he was extremely hungry and evidently eager enough for food.

From July 12 on the hunger motive was increased by feeding the monkey only in the apparatus and by so regulating the amount of food given in each trial that he should obtain barely enough to keep him in good physical condition. An increase in the number of correct choices promptly resulted, and continued until on July 14 the ratio of choices was 1 to .54. It appeared from these data that a relatively small number of choices, say not more than ten a day, the rewards in connection with which supplied the only food received by the animal, yielded most favorable results.

On July 16, the period of confinement in wrong boxes was increased to sixty seconds, and it was so continued for a number of days. But in the end, it became clear that the period of thirty seconds, combined with a liberal reward in the shape of desired food and a single series of ten trials per day, was most satisfactory. The detailed data of table 2 indicate that at this time Skirrl was making his choices by memory of the particular setting.

Skirrl, on July 17 was evidently hungry and eager to locate food, but seemingly unable to select the right box. In trial 5 (765th) of the series, he was punished by confinement in box 8. When the doors were unlocked in order that the entrance door might be raised to release him, the lock-bar, sliding under the floor, made a slight grating noise, and the instant the entrance door was opened, he jumped out excitedly. *He made no outcry, but as soon as he was out of the box, sat down, and taking up his right hind foot, examined it for a few seconds.* Having apparently assured himself that nothing serious had happened, he went on unconcernedly about his task. The presumption is that the sound of the lock-bar, associated as it was with his painful experience in box 1, revived the strongly affective experience of stepping on the nail. Psychologically described, the sound induced an imaginal complex equivalent to the earlier painful experience. The behavior seems to the writer a most important bit of evidence of imagery in the monkey.

Finally, on August 9, after ten hundred and seventy trials, Skirrl succeeded in choosing correctly in the ten trials of a series, and he was therefore considered to have solved the problem of the second door from the right end of the group.

On the following day, he was given a control series with the settings which are presented on page 19 and also at the bottom of table 2. In this series he chose correctly five times,—in other words, as often correctly as incorrectly. An analysis of the choices indicates, however, that two of the five correct choices were made in box 8, which, as it happened, had proved a peculiarly easy one for him throughout the training, since from the first he tended to avoid door 9. Consequently, it is only fair to conclude, from the results for this control series and for those given on August 11 and 12, that the animal chose not on the basis of anything remotely resembling a general idea of secondness from the right end, but instead on the basis of gradually acquired modes of reaction to the particular settings. This conclusion is strengthened by the fact that he had failed to learn to react appropriately and readily to most of the settings of the regular series.

The curve which represents the course of the learning process in this problem is presented in figure 19. For this and all other curves which involve more than a single series of observations a day, the method of construction was as follows: The first series for each day of training is indicated on the curve by a dot, while the second or third series on a given day, although space is allowed for them, are not so indicated. Consequently, the form of the curve is determined chiefly by the first series per day. The extreme irregularities of this curve are most interesting and puzzling, as are also the variations in the daily ratios of right to wrong first choices. Three times in the course of the training, this ratio rose to 1 to 9, or higher. The causes for such extreme variations are not easily enumerated, but a few of the most obvious contributory causes are variations in the weather, especially cloudiness or fogginess, which rendered the apparatus dark; variations in the degree of hunger or eagerness for food; differences in the activities of the animals in the cages outside of the laboratory (sometimes they were noisy and distracted the subject), and finally, differences in the physical fitness and attitude of the animal from day to day.

The more or less incidental behavior in connection with this experiment more strongly than the statistical results of the work on problem 2 indicate the existence of imagery. That ideas played a part in the solution of the problem is probable, but at best they functioned very ineffectively. The small number of methods used in the selection of the right box, and the slight variations from the chief method, that of choosing the first box at the right end and then the one next to it, apparently justify Doctor Hamilton's characterization of this monkey as defective.

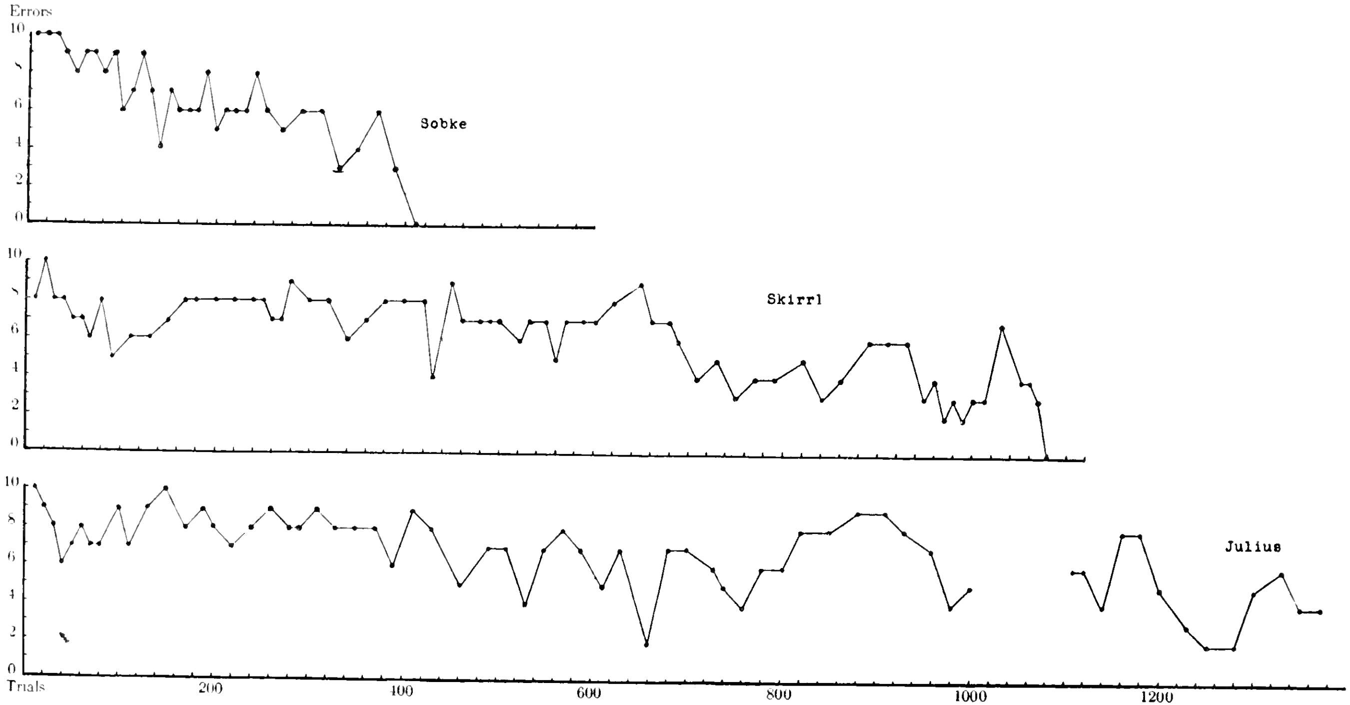


FIGURE 19.—Error curves of learning for the solution of problem 2 (second box from right end).

Problem 3. *Alternately First at Left and First at Right*

Following the control series given in connection with problem 1, an interval of rest lasting from August 12 to August 19 was allowed in order that Skirrl might in part at least lose the effects of his training and regain his customary interest in the apparatus by being allowed to obtain food easily instead of by dint of hard labor,—labor which was harder by far, apparently, than physical activity because it demanded of the animal certain mental processes which were either lacking or but imperfectly functional. The difficultness of the daily tasks appears to be reliably indicated by the tendency to yawn.

Systematic work on problem 3, which has been defined as alternately the first door at the left and the first door at the right of the group, was begun August 19, and for nine days a single series of ten trials per day was given. Work then had to cease because of the experimenter's return to Cambridge.

The results of the work on this problem demand but brief analysis and comment. The expected ratio of one right to four wrong choices per series appears (see table 3) for the first series of trials, and *this in spite of the fact that Skirrl had been trained for several weeks to choose the second door from the right end.* One would ordinarily have predicted a much larger number of incorrect choices. The right choices were due to the monkey's strong tendency to go first to the first door at the right and thence to the one next to it. Indeed in the series given on August 24, this method was followed without variation. In other words, in every one of the ten trials Skirrl entered first the box at the extreme right end of the group. This necessarily resulted in as many right as wrong first choices. Consequently, the ratio reads 1 to 1. But the method was not adhered to, and at no time either before or after that date did he succeed in equalling this achievement. There was, as a matter of fact, no steady improvement, and so far as one may judge from the records which were obtained, the course of events in the solution of this problem would have been similar to those in problem 2.

TABLE 3

Results for Skirrl, *P. irus*, in Problem 3

Date	No. of trials	S. 1 **5**.6.7	S. 2 5.6.**7**	S. 3 **1**.2.3.4.5.6	S. 4 1.2.3.4.5.**6**	S. 5 **4**.5.6.7.8	S. 6 4.5.6.7.8	S. 7 **2**.3.4.5	S. 8 2.3.4.**5**	S. 9 **3**.4.5.6 7.8.9	S. 10 3.4.5.6 7.8.**9**	R	W	R.	W	Ratio of R to W
August 19	1- 10	7.5	6.7	6.5.4.6 3.2.6.4 3.6.1*	5.3.6	7.8.7.6 8.6.4	8	4.3.5.4.5.2	5	9.8.7.6.4.3	8.7.6.5 4.5.8.7 6.5.9*	2	8	2	8	1:4.00
20	11- 20	7.6.5	6.5.6.5 7	5.4.6.5 4.6.5.3 2.5.1*	5.3.2.4.6	8.7.6.8 7.5.8.8 7.6.4*	8	5.4.3.2	4.5	8.7.6.5.4.3	8.7.6.8.3 7.6.4.3.9	1	9	1	9	1:9.00
21	21- 30	7.6.7.6.5	7	6.2.5.6 5.3.6.5 4.3.1*	2.5.5.6	8.6.5.4	8	5.3.5.4 3.5.3.5 4.3.2*	5	9.8.7.6 5.4.3	8.9	3	7	3	7	1:2.33
23	31- 40	7.6.5	6.5.7	6.4.3.2 5.6.2.1	3.2.6	8.7.6.4	8	5.4.5.3.2	5.2	8.7.3	9	2	8	2	8	1:4.00
24	41- 50	7.6.5	7	6.2.5 4.3.1	6	8.7.8.7 5.7.5.4	8	5.3.2	5	9.8.7.6 5.4.3	9	5	5	5	5	1:1.00
25	51- 60	7.6.5	6.5.7	5.2.1	6	8.5.4	8	2	2.5	9.8.7.4.3	8.7.3.6 8.7.5.3 8.7.9*	3	7	3	7	1:2.33
26	61- 70	7.6.5	6.5.7	1	2.1.6	8.7.6.4	8	2	3.2.5	9.8.7.5.3	3.6.8.3 4.7.9	3	7	3	7	1:2.33
27	71- 80	7.6.5	7	2.1	1.5.6	8.7.6.4	8	2	5	9.8.7.6.3	8.6.3.3 7.5.3.8 3.6.9*	4	6	4	6	1:1.50
28	81- 90	7.6.7.5	7	3.1	6	8.6.4	4.8	2	2.1.5	8.7.4.3	3.8.9	3	7	3	7	1:2.33

* Aided by experimenter.

2. Sobke, *Pithecus rhesus*

Problem 1. *First at the Left End*

Sobke was somewhat afraid of the experimenter when the investigation was undertaken, and instead of willingly coming out of his cage when the door was raised, he often had to be coaxed out and lured into the apparatus with food. Whereas Skirrl was frank and rather aggressive, Sobke was stealthy in his movements, furtive, and evidently suspicious of the experimenter as well as of the apparatus. He was perfectly safe to approach, but would not permit anyone to touch him. After a few days, he began to take food from the hands of the experimenter.

Preliminary work to acquaint this monkey with the routine of the experiment was begun on April 13. As in the case of Skirrl, he was lured into the apparatus and was taught the route through the boxes to the starting point by being allowed to obtain food once each day in each of the nine boxes. The procedure was simple. The entrance door and the exit door of a particular box were raised and the animal admitted to the reaction-compartment and permitted to pass through the box whose doors stood open, take its food, and return to the starting point. Sobke very quickly learned the route perfectly and came to work steadily and rapidly. After five days of preliminary work of this sort, he was so thoroughly accustomed to the apparatus that it was evidently desirable to begin with regular training experiments.

The first series of trials was given on April 19. Both punishment and reward were employed from the first. The punishment consisted of confinement for thirty seconds in each wrong box, and the reward of a small piece of banana, usually not more than a tenth of a medium sized banana for each correct choice. The total time for the first series of trials was fourteen minutes. This indicates that Sobke worked rapidly. My notes record that he worked quickly though shyly, wasted almost no time, made few errors of choice, and waited quietly during confinement in the boxes. In this, also, he differed radically from Skirrl who was restless and always tried to escape from confinement.

Throughout the work on problem 1, punishment and reward were kept constant. Everything progressed smoothly; there were no such irregularities of behavior as appeared in the case of Skirrl, and consequently the description of results is a relatively simple matter. Sobke invariably chose the end boxes. His performance was in every way superior to that of Skirrl.

As previously, the detailed results are presented in tabular form (table 4). From this table it appears that, whereas the expected ratio of right to wrong first choices for this problem is 1 to 2.5, the actual ratio for Sobke's first series was 1 to .67. This surprisingly good showing is unquestionably due to his marked tendency to choose the end box of a group; and this tendency, in turn, may in part be the result of the preliminary training, for during that only one box was open each time. But, if the preliminary training were responsible for Sobke's tendency, it should be noted that it had very different effect upon Skirrl, and, as will be seen later, upon Julius.

The results for the ten different settings of the doors for problem 1 as they appear in table 4 are of interest for a number of reasons. In the first place, the setting 1. 2. 3 appearing twice,—at the beginning of the series and again at the end—yielded markedly different results in the two positions. For whereas no mistakes were made in the case of setting 1, there were fifty per cent of incorrect first choices for setting 10. Again, satisfactory explanation is impossible. It is conceivable that fatigue or approaching satiety may have had something to do with the failures at the end of the series, but as a rule, as is indicated by settings 1, 2, and 6, if correct choices were made at the beginning, they continued throughout the day's work.

In this problem, Sobke's improvement was steady and fairly rapid, and in the eighth series, trials 71 to 80, only correct first choices appear. Consequently, seventy trials were required for the solution of the problem. This number is in marked contrast with Skirrl's one hundred and thirty-two trials.

Immediately following the first perfect series, Sobke was given two series of control tests on April 28. Conditions were unfavorable, since the day was stormy and the rain pattering on the sheet-iron roof made a great din. Nevertheless, he worked steadily and well up to the sixth trial, which was preceded by a slight delay because of the necessity of refilling some of the food boxes. After this interruption, wrong choices occurred in

TABLE 4

Results for Sobke, *P. rhesus*, in Problem 1

Date	No. of trials	S. 1	S. 2	S. 3	S. 4	S. 5	S. 6	S. 7	S. 8	S. 9	S. 10	R	W	R	W	Ratio of R to W
		1.2.3	8.9	3.4.5.6.7	7.8.9	2.3.4.5.6	6.7.8	5.6.7	4.5.6.7.8	7.8.9	1.2.3					
April																
19	1- 10	1	8	3	9.7	6.2	6	7.5	4	9.7	1	6	4	6	4	1:0.67
20	11- 20	1	8	3	7	2	6	7.5	8.4	9.9.7	1	7	3	7	3	1:0.43
21	21- 30	1	8	4.3	9.7	2	6	5	8.4	7	1	7	3	7	3	1:0.43
22	31- 40	1	8	3	7	6.2	6	6.5	4	7	3.1	7	3	7	3	1:0.43
23	41- 50	1	8	3	7	2	6	5	4	9.7	3.1	8	2	8	2	1:0.25
24	51- 60	1	8	3	9.7	2	6	5	4	7	2.1	8	2	8	2	1:0.25
26	61- 70	1	8	3	7	2	6	5	4	7	3.1	9	1	9	1	1:0.11
27	71- 80	1	8	3	7	2	6	5	4	7	1	10	0	10	0	1:0.00
		2.3.4	6.7.8.9	3.4.5	4.5.6.7.8.9	6.7.8.9	1.2.3.4.5	2.3.4 5.6.7.8	3.4.5.6.7.8	5.6.7	1.2.3.4.5 6.7.8.9					
28	1- 10	2	6	3	4	6	5.4.1	2	3	5	5.4.2.1	8	2			
"	11- 20	2	6	3	4	6	2.1	2	3	5	1	9	1	17	3	1:0.18

trial 6. And again after trial 9, there was brief interruption, followed by wrong choices in trial 10. The ratio of right to wrong choices for this first control series was therefore 1 to .25.

Six minutes after completion of the first control series, a second was given under slightly more favorable conditions, and in this only a single wrong choice occurred, in that box 2 was first chosen in trial 6 instead of box 1. From the results of these two control series, it is evident that Sobke's solution of problem 1 is reasonably adequate. He is easily diverted or disturbed in his work by any unusual circumstánces, but so long as everything goes smoothly, he chooses with ease and certainty. Whether it is fair to describe the behavior as involving an idea of the relation of the right box to the other members of the group would be difficult to decide. I hesitate to infer definite ideation from the available evidence, but I strongly suspect the presence of images and relatively ineffective or inadequate ideation.

It is perfectly evident that Sobke is much more intelligent than Skirrl. In practically every respect, he adapted himself more quickly to the experimental procedure and progressed more steadily toward the solution of the problem than did Skirrl. The contrast in the learning processes of the two monkeys could scarcely be better exhibited than by the curves of learning which are presented in figure 18. The first, that for Sobke, is surprisingly regular; the second, that for Skirrl, is quite as surprisingly irregular. These results correlate perfectly with the steadiness and predictability of the former's responses and the irregularity and erraticness of the latter's.

Problem 2. Second from the Right End

On the completion of problem 1 Sobke was in perfect condition, as to health and training, for experimental work. He had come to work quietly, fairly deliberately, and very steadily. His timidity had diminished and he would readily come to the experimenter for food, although still he was somewhat distrustful at times and became timid when anything unusual occurred in the apparatus.

As preparation for problem 2, a break in regular experimentation covering four days followed the control series of problem 1. On each of these four days the monkey was allowed to get food once from each of the nine boxes, both doors of a given box being open for the trial and all other doors closed. For this feeding experiment, the doors were opened in irregular order, and this order was changed from day to day.

Systematic work with problem 2 began on May 3, with punishment of thirty seconds for mistakes and a liberal reward of food for each success. Early in the series of trials it was discovered that Sobke was likely to become discouraged and waste a great deal of time unless certain aid were given by the experimenter. On this account, after the first two trials, the method was adopted of punishing the animal by confinement for the first ten mistakes in a trial, and of then, if need be, indicating the right box by slightly and momentarily raising the exit door. Every trial in which aid was thus given by the experimenter is indicated in table 5 by an asterisk following the last choice. In the first series of trials for this problem, aid had to be given in seven of the ten trials, and even so the series occupied seventy-one minutes. It is possible that had no aid been given, the work might have been continued successfully with a smaller number of trials than ten per day. But under the circumstances it seemed wiser to avoid the risk of discouraging and thus spoiling the animal for use in the experiment. It should be stated, also, that it proved impossible to adhere to the period of thirty seconds as punishment in this series. For the majority of the wrong choices confinement of not more than ten seconds was used.

For the second series, given on May 4, the conditions were unfavorable in that it was dark and rainy, and the noise of the rain on the roof frightened Sobke. He refused to work after the fourth trial, and the series had to be completed on the following day. The total time required for this series was seventy-eight minutes.

The work on May 6 was distinctly better, and the animal's behavior indicated, in a number of trials, definite recognition of the right door. He might, for example, make a number of incorrect choices, then pause for a few seconds to look steadily at the doors, and having apparently found some cue, run directly to the right box. No aid from the experimenter was needed in this series.

On the following day improvement continued and the animal's method of choosing became definite and fairly precise. He was deliberate, quiet, and extremely business-like. The time for the series was thirty-one minutes.

The period of punishment was increased on May 12 to thirty seconds. Previously, for the greater number of the trials, it had been ten to fifteen seconds. This increase apparently did not disturb the monkey, for he continued to work perfectly throughout the series, although making many mistakes in spite of deliberate choices and the refusal of certain boxes in each trial.

An interesting and significant incident occurred on May 13 when at the conclusion of trial 5, Doctor Hamilton came into the experiment room for a few minutes. Sobke immediately stopped working, and he could not be induced to make any choices until Doctor Hamilton had left the room. This well indicates his sensitiveness to his surroundings, and his inclination to timidity or nervousness even in the presence of conditions not in themselves startling.

Work was continued thus steadily until May 28 when, because of the failure of the animal to improve, it seemed wise to increase the period of confinement as punishment to sixty seconds. In the meantime, it had sometimes been evident that Sobke was near to the solution of his problem. He would often make correct choices in three or four trials in succession and then apparently lose his cue and fail utterly for a number of trials.

After June 1, in order to hasten the solution of the problem, two series per day were given. In some instances the second series was given almost immediately after the first, while in others an interval of an hour or more intervened. It was further found desirable to give Sobke all of his food in the apparatus. When the rewards obtained in the several trials did not satisfy his hunger, additional food was presented, on the completion of the series of experiments, in one or more of the food cups. On days marked by unwillingness or refusal to work, very little food was given. Thus, the eagerness of the monkey to locate the right box was increased and, as a matter of observation, his deliberateness and care in choice increased correspondingly. Sixty seconds punishment was found satisfactory, and it was therefore continued throughout the work on this problem.

It was evident, on June 9, from the behavior of the monkey as well as from the score, that the perfect solution of the problem was near at hand. This fact the experimenter recorded in his daily notes, and sure enough, on the following day Sobke chose correctly throughout the series of ten trials. The time for this series was only ten minutes. The choices were made deliberately and readily.

An analysis of the data of table 5 reveals five methods or reactive tendencies which appeared more or less definitely in the following order: (a) Choice of first box at the left, because of experience in problem 1. This tendency was very quickly suppressed by the requirements in connection with problem 2. Indeed one of the most significant differences which I have discovered between the behavior of the primates and that of other mammals is the time required for the suppression of such an acquired tendency. The monkey seems to learn almost immediately that it is not worth while to persist in a tendency which although previously profitable no longer yields satisfaction, whereas in the crow, pig, rat, and ring dove, the unprofitable mode of response tends to persist during a relatively large number of trials. (b) The tendency to choose, first, a box near the left end of the group, to go from that to the box at the extreme right end of the group, thence to the one next in order, which was, of course, the right box. This tendency appears fairly clearly from May 7th on. (c) The box at the extreme right was first chosen and then the one next to it. For example, in setting 2, box 4 would be chosen first, then box 3. Or, if this did not occur, the method previously described under (b) was likely to be employed, as for example, in setting 8, where such choices as 7.6.5.1.8 appear. (d) In certain series there appeared a marked preference for a particular box, usually box 3 (see results for May 24). This was doubtless due in a measure, if not wholly, to the fact that box 3 was the right box twice in each series of ten settings. But it should be added that the same is true of box 7, for which no preference was manifested at any time. (e) Direct choice of the right box.

The five reactive methods or tendencies enumerated above roughly appeared in the order named, but there were certain irregularities and the order as well as the time of appearance varied somewhat from setting to setting. In general, method c was the most frequently used prior to the development of method e, the direct choice of the right box.

TABLE 5

Results for Sobke, *P. rhesus*, in Problem 2

Date	No. of trials	S. 1 7.8.9	S. 2 1.2.**3**.4	S. 3 2.3.4.5.**6**.7	S. 4 1.2.3.4.**5**.6	S. 5 4.5.6.**7**.8	S. 6 1.**2**.3	S. 7 2.3.**4**.5	S. 8 1.2.3.4.5 6.7.**8**.9	S. 9 1.2.**3**.4	S. 10 3.4.5.6.**7**.8	R	W	R	W	Ratio of R to W
May 3	1- 10	7.7.9.7.7 9.7.9.7.9 7.9.9.7.8*	1.2.4.1 1.4.1.1 4.3*	3.2.7.2 7.7.7.5 2.7.6*	4.6.1.6 1.6.1.1 1.3.5*	5.8.6.7	3.1.3.3 3.3.1.1 1.1.2*	3.5.4	9.1.8	4.1.4.1 4.1.4.2 1.4.3*	3.8.3.3 3.8.6.3 8.3.7*	0	10	0	10	0:10.00
4 and 5	11- 20	7.9.7.9.9 9.7.7.9.8	1.4.1.3	4.7.2.7 2.7.3.2 7.2.6*	1.6.5	8.8.4.8 8.8.4.8 4.8.7*	3.3.1.1.3 3.3.1.2	5.5.5.5 5.5.5.3 5.5.4*	9.5.5.9 7.1.6.6 6.5.8*	4.4.4.4 4.4.4.3	8.8.6.8 8.7	0	10	0	10	0:10.00
6	21- 30	9.7.9.7.8	4.3	7.2.3.7.6	6.3.1.6 6.2.6.5	6.8.7	3.1.2	5.3.5.3 2.4	6.9.4.6.1 7.9.7.8	4.1.2.4 4.2.3	3.4.5.3 8.6.7	0	10	0	10	0:10.00
7	31- 40	7.9.7.8	1.4.3	2.7.6	3.5	4.8.7	3.1.2	3.2.3.5.4	8	4.3	5.8.3.4.8.7	1	9	1	9	1: 9.00
8	41- 50	7.9.7.8	4.2.4.1.4 4.2.2.4.3	6	5	6.5.4.8.7	3.1.3.1 3.1.3.2	5.3.5.4	7.4.2.1 9.8	4.2.4.1 4.3	5.3.8.7	2	8	2	8	1: 4.00
10	51- 60	7.7.8	3	7.3.2.7.6	6.4.6.5	6.4.8.5 4.8.7	3.1.3.2	5.3.5.3 2.5.3.5 2.5.4	5.9.8	4.2.4.3	6.5.4 3.8.7	1	9	1	9	1: 9.00
11	61- 70	7.9.7.8	1.4.3	4.3.2.7.6	6.5	5.4.8.6 5.4.8.7	3.2	5.4	6.5.2.3.1.8	3	6.5.3.8.7	1	9	1	9	1: 9.00
12	71- 80	7.9.8	3	7.3.2.7.6	6.5	5.4.8.7	1.2	5.4	8	4.3	5.4.3.8.7	2	8	2	8	1: 4.00
13	81- 90	7.8	4.3	3.7.6	6.5	8.7	1.3.2	2.5.2.5.4	7.6.5.9.8	3	8.7	1	9	1	9	1: 9.00
14	91- 100	7.8	3	6	5	6.5.4.7	1.3.2	5.4	9.8	1.2.1.2.2 1.4.2.2.1 4.1.4.3	7	4	6	4	6	1: 1.50
15	101- 110	7.8	3	7.6	5	5.4.5.7	3.2	5.4	8	2.1.3	8.8.7	3	7	3	7	1: 2.33
17	111- 120	7.8	4.3	7.6	2.1.2.1.2 1.3.2.6.5	8.7	3.2	5.4	7.6.5.1.8	3	8.7	1	9	1	9	1: 9.00
18	121- 130	8	4.3	7.6	6.5	7	3.2	5.4	7.6.5.2.8	3	8.7	3	7	3	7	1: 2.33
19	131- 140	8	3	3.2.6	5	7	3.2	5.3.2.5.3 2.5.2.5.3 5.5.2.5.5 3.2.5.5.3 2.5.5.4	7.6.9.8	3	7	6	4	6	4	1: 0.67

20	141- 150	7.8	3	3.2.7.6	6.5	5.4.7	3.2	3.2.5.4	6.5.4.3.8	3	7	3	7	3	7	1: 2.33
21	151- 160	7.8	3	5.7.5.3.7.6	5	7	3.2	5.3.5.2.5.4	·7.6.5.9.8	3	6.4.3.8.7	4	6	4	6	1: 1.50
22	161- 170	7.8	3	7.6	3.2.6.5	7	3.2	5.2.5.3 2.5.4	7.6.5.8	3	7	4	6	4	6	1: 1.50
24	171- 180	8	3	3.2.7.5 7.3.6	3.6.4 3.6.5	8.7	3.1.3 1.3.3 3.2	3.5.4	8	4.4.2.1 4.3	7	4	6	4	6	1: 1.50
25	181- 190	7.9.7.9.8	3	3.7.6	5	8.7	3.2	5.4	4.3.8	4.3	8.7	2	8	2	8	1: 4.00
26	191- 200	8	3	6	5	8.7	3.2	5.4	8	4.3	8.7	5	5	5	5	1: 1.00
27	201- 210	7.9.8	3	3.2.7.6	5	8.7	3.2	5.4	8	3	8.7	4	6	4	6	1: 1.50
28	211- 220	8	3	3.7.6	4.3.2.6.5	8.7	3.2	5.4	7.6.5.9.8	3	7	4	6	4	6	1: 1.50
29	221- 230	7.8	3	5.4.3.7.6	6.5	7	2	3.2.5.4	7.6.5.8	3	3.8.7	4	6	4	6	1: 1.50
31	231- 240	7.7.8	3	3.7.6	6.5	8.7	3.2	5.4	9.8	3	3.7	2	8	2	8	1: 4.00
June																
1	241- 250	8	3	5.4.7.6	3.2.6.5	8.7	3.2	5.4	8	3	8.7	4	6			
“	251- 260	7.9.8	3	3.7.6	5	6.5.4 8.7	3.2	4	5.4.3.8	4.2.1.4 2.4.4.3	7	4	6	8	12	1: 1.50
2	261- 270	7.8	3	6	3.2.6.5	7	3.2	3.4	8	3	6.5.3.8.7	5	5			
“	271- 280	7.8	3	3.7.6	3.6.5	7	3.2	5.4	7.6.5.4.3.8	3	7	4	6	9	11	1: 1.22
3	281- 290	7.8	3	7.6	3.6.5	8.7	3.2	4	7.8	3	7	4	6			
“	291- 300	9.8	3	3.6	4.3.6.5	8.7	2	3.5.4	7.6.5.9.8	3	8.7	3	7	7	13	1: 1.86
4	301- 310	8	3	7.6	3.4.3.6.5	8.7	2	3.2.5.4	7.6.5.8	3	3.8.7	4	6			
“	311- 320	8	3	5.4.7.6	3.2.6.5	7	2	3.2.5.4	8	3	7	7	3	11	9	1: 0.82
5	321- 330	8	3	6	4.6.5	7	2	3.5.4	8	4.3	7	7	3			
“	331- 340	8	3	7.4.7.6	3.2.4.6.5	7	3.2	3.5.4	8	3	7	6	4	13	7	1: 0.54
7	341- 350	8	3	7.6	5	8.7	1.3.1.2	3.5.4	8	3	7	6	4			
“	351- 360	8	3	7.6	5	8.7	2	3.5.4	8	3	7	7	3	13	7	1: 0.54
8	361- 370	7.8	3	4.7.6	3.5	8.7	2	3.4	9.8	3	7	4	6			
“	371- 380	8	3	7.6	3.4.4.3.5	8.7	3.2	3.4	8	3	7	5	5	9	11	1: 1.22
9	381- 390	8	3	6	4.2.1.5	7	2	3.4	8	3	8.7	7	3			
“	391- 400	8	3	6	5	7	2	3.4	8	3	7	9	1	16	4	1: 0.25
10	401- 410	8	3	6	5	7	2	4	8	3	7	10	0	10	0	1: 0.00
		5.6.7.8	**2.3.4.5.6**	**1.2.3.4.5 6.7.8.9**	**5.6.7**	**1.2.3.4**	**4.5.6**	**2.3.4.5**	**1.2.3**	**1.2.3.4 5.6.7**	**2.3.4.5 6.7.8.9**					
11	1- 10	6.7	3.5	8	6	3	5	3.4	3.2	7.7.2.6	8	5	5	5	5	1: 1.00
12	11- 20	7	3.6.6.2.5	8	6	3	4.5	4	2	7.6	8	7	3	7	3	1: 0.43

---

* Aided by experimenter.

Examination of table 5 indicates that some of the settings proved very easy for Sobke; others, extremely difficult. Consequently, the number of methods which were tried and rejected for a given setting varies from two to five. Setting 2 proved a fairly simple one, and after the inhibition of the tendency to choose the first box at the left, the only definite tendency to appear was that of choosing the first box at the right, and then the one next to it. After one hundred and thirty trials, this method suddenly gave place to direct choice of the right box, and during the following twenty-eight series, no error occurred for this setting. Setting 4, on the contrary, proved extremely difficult, and a variety of methods is more or less definitely indicated by the records.

It is needless to lengthen the description by analyzing the data for each setting, since the reader by carefully scanning the columns of data in table 5 may observe for himself the various tendencies and their mutual relations.

Sobke's curve of learning (figure 19) in problem 2, is extremely irregular, as was that of Skirrl. Similar irregularities appear in the daily ratios of right to wrong first choices presented in the last column of table 5. Most of these irregularities were due, I have discovered, to unfavorable external conditions. Thus, dark rainy days and disturbing noises outside the laboratory were obviously conditions of poor work.

On the day following the final and correct series for problem 2, a control series was given. In this Sobke seemed greatly surprised by the new situations which presented themselves. Repeatedly he exhibited impulses to enter the box which would have been the correct one in the regular series of settings. He frequently inhibited such impulses and chose correctly, but at other times he reacted quickly and made mistakes. It was evident from his behavior that he was not guided by anything like a definite idea of the relation of the right box to the other members of the group.

In a second control series given on the following day, June 12, confusion appeared, but less markedly. For the first setting, a correct choice was made with deliberation. For the second setting, box 3 was immediately chosen, as should have been the case in the regular series of settings. Sobke seemed confused when he emerged from this box and had difficulty in locating the right one. Then followed direct correct choices for settings 3, 4, and 5. For setting 6, there is recorded a deliberately made wrong choice, and so on throughout the series, the choices being characterized by deliberateness and definite search for the right box. Uncertainty was plainly indicated, and in this the behavior of the animal differed markedly from that in the concluding series of the regular experiment.

It seems safe to conclude from the results of these control series that Sobke has no free idea of the relation of secondness from the right and is chiefly dependent upon memory of the particular settings for cues which lead to correct choice.

Problem 3. *Alternately First at Left and First at Right*

For four successive days after the last control series in connection with problem 2, Sobke was merely fed in the apparatus according to previous description (p. 43). He exhibited a wonderfully keen appetite and was well fed during this interval between problems.

The method of experimentation chosen for problem 3 in the light of previous experience was that of confining the monkey for a short time, ten to fifteen seconds, in the wrong box, in each of the first ten mistakes for a given trial, and of then aiding him to find the right box by the slight and momentary raising of the exit door. Aid proved necessary in a few of the trials during the first four days. Then he worked independently. As work progressed it was found possible and also desirable to increase the period of confinement, and in the end, sixty seconds proved satisfactory. It was also thought desirable to increase the number of trials per day from a single series during the early days to two or even three series from June 29 on. Often three series could be given in succession without difficulty. During the early trials on this problem Sobke worked remarkably well, but later his willingness diminished, evidently because of his failure readily to solve the problem, and it became extremely difficult to coax him into the apparatus. On days when he entered only reluctantly and as it seemed against his will, he was likely to be nervous, erratic, and often slow in making his choices, but above all he tended to waste time by not returning to the starting point, preferring rather to loiter in the alleyways or run back and forth.

TABLE 6

Results for Sobke, *P. rhesus*, in Problem 3

Date	No. of trials	S. 1 **5**.6.7	S. 2 5.6.**7**	S. 3 **1**.2.3.4.5.6	S. 4 1.2.3.4.5.**6**	S. 5 **4**.5.6.7.8	S. 6 4.5.6.7.8	S. 7 **2**.3.4.5	S. 8 2.3.4.**5**	S. 9 **3**.4.5.6 7.8.9	S. 10 3.4.5.6 7.8.**9**	R	W	R	W	Ratio of R to W
June 17	1- 10	6.6.7.6 6.5	5.7	4.4.3.5.4 5.4.2.1	3.1.2.1 4.4.2.1 5.4.6*	7.8.7.8 8.8.7.8 7.7.4*	8	2	3.2.5	9.8.7.9 8.7.6.9 8.7.3*	3.8.9	2	8	2	8	1:4.00
18	11- 20	6.6.6.5	5.7	3.4.2.5 4.5.6.4 2.3.1*	3.1.4.3.6	8.7.7.5 8.8.7.8 7.8.4*	8	3.2	3.4.2.4 4.5	9.8.8.7.3	8.6.9	1	9	1	9	1:9.00
19	21- 30	6.5	7	3.6.5.4 2.5.2.6 3.5.1*	3.6	8.8.7.7 7.8.8.8 8.4	8	4.3.2	3.2.4.2 2.5	9.8.7.9.7 6.5.4.3	9	3	7	3	7	1:2.33
21	31- 40	6.5	5.6.5.7	3.5.4.6 4.3.5.1	2.5.3.6	8.8.8.8 7.8.3.8 8.7.4*	8	4.2	2.5	9.9.8.9 9.5.9.9 9.9.3*	9	2	8	2	8	1:4.00
22	41- 50	7.6.5	6.5.5.7	1	2.1.3.6	8.8.8.8 7.8.4	8	2	2.3.2.2.5	9.8.9.8.7 6.5.9.3	9	4	6	4	6	1:1.50
23	51- 60	5	6.5.7	1	1.2.1.1 3.2.6	8.5.4	8	5.4.2	2.4.2.5	9.8.4.9 7.3	9	4	6	4	6	1:1.50
24	61- 70	7.6.5	7	2.3.1	2.1.5.4.2.6	8.7.8.4	8	4.5.4.3.2	2.2.4.5	9.7.6.8.3	9	3	7	3	7	1:2.33
25	71- 80	6.5	5.7	3.1	6	8.5.4	8	2	3.2.5	9.8.7.3	3.9	3	7	3	7	1:2.33
26	81- 90	7.7.6.5	6.5.7	3.1	1.4.1.1 5.1.6	8.4	8	5.4.2	2.5	9.8.9.6.3	9	2	8	2	8	1:4.00
28	91-100	7.6.5	7	1	1.2.4.6	8.4	8	2	3.2.2.2.5	9.8.8.7.9.3	9	5	5	5	5	1:1.00
29	101-110	7.6.5	5.7	1	1.1.5.3 2.1.6	8.4	8	2	4.2.5	9.9.8.9.4 9.7.6.3	9	4	6			
"	111-120	6.5	6.5.7	1	2.1.1.3.6	8.5.4	8	2	3.2.5	9.8.7.6.4.3	9	4	6	8	12	1:1.50
30	121-130	5	6.5.6.5.5.7	1	3.1.6	8.7.6.8.5.4	8	2	4.2.4.2.5	9.3	9	5	5			
"	131-140	5	7	1	2.3.6	8.5.6.4	8	2	5	9.8.3	3.9	6	4	11	9	1:0.82
July 1	141-150	5	7	1	1.6	8.7.4	8	2	3.2.5	9.8.6.9.3	9	6	4			
"	151-160	5	7	1	2.5.3.6	8.4	8	2	2.5	9.3	8.8.7.5.4.9	5	5	11	9	1:0.82
2	161-170	6.5	7	1	2.6	8.4	8	2	3.5	9.3	9	5	5	5	5	1:1.00
3	171-180	6.5	7	1	1.5.6	8.4	8	2	3.5	9.3	9	5	5			
"	181-190	5	7	1	4.6	8.6.4	8	2	5	9.8.4.8.5.3	9	7	3	12	8	1:0.67

5	191-200	6.5	5.7	1	6	8.4	8	2	5	9.5.3	9	6	4			
"	201-210	5	7	6.1	2.6	8.6.4	8	5.3.5.4.3.2	4.3.5	9.7.3	9	4	6	10	10	1:1.00
6	211-220	5	5.7	1	2.6	8.6.4	8	2	4.3.5	9.3	5.4.8.3 8.5.4.9	4	6			
"	221-230	5	7	1	5.3.6	8.6.8.4	8	4.2	3.2.5	9.3	9	5	5	9	11	1:1.22
7	231-240	5	7	1	2.6	8.4	7.4.8	2	4.3.5	8.3	9	5	5			
"	241-250	5	7	2.6.3.5.1	6	8.4	8	2	5	9.3	9	7	3			
"	251-260	5	7	2.6.2.6.4 6.3.2.6 5.4.3.1.	6	8.4	8	2	5	9.5.3	9	7	3	19	11	1:0.58
8	261-270	7.5	7	1	6	8.4	8	5.2	5	8.3	8.5.4.9	5	5			
"	271-280	5	7	2.6.4.6.1	6	8.5.8.4	7.4.8	5.5.3.5.2	5	9.3	9	5	5	10	10	1:1.00
9	281-290	5	5.7	1	6	8.4	8	2	2.5	8.3	7.3.8.6.8 4.7.3.9	5	5			
"	291-300	5	7	1	6	8.4	8	2	5	9.3	9	8	2			
"	301-310	5	7	1	6	8.4	8	5.2	5	9.5.3	9	7	3	20	10	1:0.50
10	311-320	5	7	1	6	8.4	7.4.8	2	5	9.3	9	7	3			
"	321-330	5	7	1	5.2.6	8.7.4	8	2	5	9.3	9	7	3	14	6	1:0.43
12	331-340	5	7	1	6	8.4	8	2	5	8.3	9	8	2			
"	341-350	5	7	1	6	8.4	8	5.2	5	9.3	9	7	3	15	5	1:0.33
13	351-360	5	5.7	1	6	8.4	8	5.2	5	3	9	7	3			
"	361-370	5	7	6.1	6	4	8	5.2	3.2.5	9.3	9	6	4	13	7	1:0.54
14	371-380	5	7	1	6	8.4	8	2	5	3	7.4.3.9	8	2			
"	381-390	5	7	1	6	8.4	8	2	5	9.3	4.7.3.9	7	3	15	5	1:0.33
15	391-400	5	5.5.7	1	3.6	8.4	8	3.2	5	8.3	9	5	5			
"	401-410	5	7	1	6	8.4	8	2	5	7.3	6.5.3.9	7	3	12	8	1:0.67
16	411-420	5	7	1	6	8.4	8	2	5	3	9	9	1			
"	421-430	5	7	1	6	4	8	2	5	3	4.7.4.9	9	1			
"	431-440	5	7	1	6	8.4	8	2	5	3	5.7.3.9	8	2	26	4	1:0.15
17	441-450	5	7	1	6	4	4.8	2	5	4.3	3.5.9	7	3			
"	451-460	5	7	1	3.6	4	5.4.8	2	5	9.3	9	7	3			
"	461-470	5	7	1	6	4	8	2	5	3	9	10	0	24	6	1:0.25
19	471-480	5	7	1	6	8.4	8	2	5	3	9	9	1	9	1	1:0.11
		5.6	**5.6**	**4**.5.6.7.8.9	4.5.6.7.8.**9**	**1**.2.3.4.5	1.2.3.4.**5**	**2**.3.4.5.6.7	2.3.4.5.6.**7**	**3**.4.5.6.7.8	3.4.5.6.7.8					
19	1- 10	5	6	4	9	1	5	3.7.2	7	3	8	9	1	9	1	1:0.11

* Aided by experimenter.

The data of table 6 indicate for this problem only three pronounced reactive tendencies: (a) As the initial tendency, the choice of the second box from the right end. This proved surprisingly weak, in view of the animal's long training on problem 2, and it disappeared quickly. (b) Choice of the end boxes, and (c) direct choice of the right box.

For this, as for the other problems, extreme differences in method and in time and degree of success appear for the different settings. Thus, while settings 1, 2, 3, 6, 7, and 10 proved to be easy, settings 4, 5, 8, and 9 were evidently more difficult.

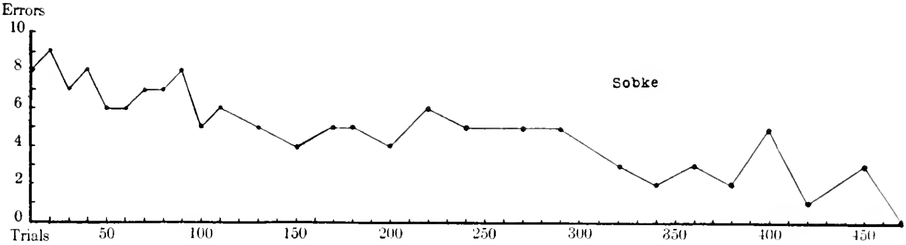


FIGURE 20.—Error curve of learning for the solution of problem 3 (alternately the first box at the left end and the first at the right end) by Sobke.

From the first this problem promised to be much easier for Sobke than problem 2, and although the actual number of trials necessary for the solution is greater by sixty for problem 3 than for problem 2, comparison of the data of the tables justifies the statement that the third problem was both easier and more nearly adequately solved than the second. This is not surprising when the nature of the two problems is considered, for whereas problem 2 requires choice by perception of the relationship of secondness from the right end of the group, problem 3 requires, instead, the choice of the end member of the group each time, with the additional variation of alternation of ends. Now as it happens, the end member is easily selected by the monkey, and it appears further that alternation was relatively easy for Sobke to acquire. Consequently, the combination of end and alternation proved easier than the choice of the second from the right end of the group.

The above statements are supported by comparison of the curves of learning. The curve for problem 2, figure 19, is extremely irregular; that for problem 3, figure 20, much more regular. Similarly, the daily ratios of right to wrong choices as exhibited in tables 5 and 6 indicate smaller variations for the third problem than for the second.

Sobke made ten correct first choices in the third series for July 17, but he was working very uncertainly and it seemed rather a matter of good luck than good management that he succeeded in presenting this perfect series. For this reason and also because it did not seem feasible to have Sunday intervene between the final and perfect regular series and the control series, an additional regular series was given on July 19, in which, as the table indicates, a single mistake occurred, in trial 5. The monkey was working perfectly. The series of trials required only ten minutes, and it was evident that carelessness and eagerness to obtain food were chiefly responsible for the mistake.

The control series given on July 19 immediately after the series just described resulted similarly in one failure and nine successes. The choices were made easily and with certainty, and the only mistake, that of setting 7, was apparently due to carelessness.

This excellent showing for the control series wholly justifies the comparison of problems 2 and 3 as to difficultness, made above. Whereas in both problems 1 and 2 the control trials caused confusion, in the case of problem 3, they did not essentially alter the behavior of the animal. The fact seems to be that for this problem the particular setting is of relatively little importance; while turning alternately to the extreme left and the extreme right is of prime importance. That Sobke had the idea of alternation or of the end box, there seems no more reason for insisting than that he had the idea of secondness from the right end in problem 2. It is possible, even probable, that these ideas existed rather vaguely in his consciousness, but there is obviously no necessity for insisting that the solution of the problems depended upon them.

Problem 4. *Middle*

As the available time for the continuation of the experiment was limited, it was decided to proceed with work on problem 4 immediately upon the completion of problem 3, and on July 20, the problem of the middle door was presented to Sobke. Since it was anticipated that this sudden change would confuse and discourage him greatly, the only form of punishment administered was the momentary lowering of the entrance door of the wrong box. As in the previous problem, he was aided after ten successive wrong choices. As might have been anticipated, he persistently entered the end boxes of the groups, and this in some instances probably would have been kept up for many minutes had not the experimenter lured him into the right box by slightly raising the exit door. In the first series, he had to be aided in five of the ten trials. The total time for the series was forty-five minutes, the total number of choices, eighty-eight. In the second series, he was aided in four of the trials. The total time required was seventy-two minutes, and the total number of choices was seventy-six.

Throughout the first series, Sobke worked hard, but with evidently increasing dissatisfaction. He clung persistently to his acquired tendency to choose the end boxes, and after each trial he returned less willingly to the starting point.

Up to this time his attitude toward the experimenter had been perfectly friendly, if not wholly trustful. But when on July 21 he was brought into the apparatus for the second series, he exhibited a wholly new form of behavior, for instead of attending diligently to the open doors and devoting his energies to trying to find the right box, he instead, after gazing at them for a few seconds, turned toward the experimenter and jumped for him savagely, throwing himself against the wire netting with great force. This was repeated a number of times during the first two or three trials, and it occurred less frequently later in the series. Since nothing unusual had happened outside of the experiment room, the suggested explanation of this sudden change in attitude and behavior is that the monkey resented and blamed on the experimenter the difficulty which he was having in obtaining food.

From this time on until the end of my work, Sobke was always savage and both in and out of the apparatus he was constantly on the watch for an opportunity to spring upon me. Previously, it had been possible for me to coax him into the apparatus by offering him food and to return him to his cage by walking after him. But on and after the twenty-first of

July, it was impossible for me to approach him without extreme risk of being bitten.

Doctor Hamilton when told of this behavior, reported that several times monkeys have shown resentment toward him when they were having trouble in the experiment. I therefore feel fairly confident that I have not misinterpreted Sobke's behavior.

When on July 22 I gave Sobke an opportunity to enter the apparatus, he refused, and it was impossible to lure him in with food. Two hours later, having waited meantime for his breakfast, he entered readily and worked steadily and persistently through his third series of trials, but in no one of these trials did he choose correctly. Neither on this day nor the following did he exhibit resentment while at work. He apparently had regained his affective poise and was able to attend as formerly to the task of locating his rewards.

During these first three series, although the ratio of right to wrong choices stood 0 to 10, there occurred a marked reduction in the number of trials in which aid was necessary as well as in the total number of choices, and on July 23 correct reactions began to appear. Improvement during the next hundred trials was steady and fairly rapid, and on July 31, a record of seven right to three wrong trials was obtained. This was surprising to the experimenter, as well as gratifying, since he was eager to have the animal complete this problem before work should have to be discontinued.

Everything went smoothly until August 2, when my assistant, who had been left in charge of the experimental work for a week, attempted to increase the number of trials per day to two series. Sobke apparently was not quite ready for this increase in the amount of his day's labor and refused to work at the end of the first series. In this series he did less well than on the previous day. The following day, August 3, unfortunately and contrary to the wishes of the experimenter, the laboratory was painted and there was necessarily considerable disturbance because of the presence of the workmen, and in addition, the pervasive odor of fresh paint. Sobke chose still less successfully on this date, and on August 4, he refused to work after the eighth trial. It is true that during these bad days the total number of choices steadily diminished while the successes, also, diminished, or at best, failed to increase. When on August 9, I returned to the

TABLE 7

Results for Sobke, *P. rhesus*, in Problem 4

Date	No. of trials	S. 1 2.**3**.4	S. 2 5.6.**7**.8.9	S. 3 1.2.3.**4** 5.6.7	S. 4 7.**8**.9	S. 5 4.5.**6**.7.8	S. 6 1.2.3.4.**5** 6.7.8.9	S. 7 1.**2**.3	S. 8 2.3.**4**.5.6	S. 9 3.4.5.**6** 7.8.9	S. 10 6.**7**.8	R	W	R	W	Ratio of R to W
July 20	1- 10	2.4.2.4.3	5.9.5.5 5.9.5.6 5.8.7*	1.7.1.3 1.7.1.7 1.7.4*	9.7.9.7 7.9.7.9 7.7.8*	8.4.8.4 4.6	1.9.3.1 9.2.9.3 9.1.5*	3.1.3.2	6.3.2.6 3.6.3.2 6.3.4*	9.3.4.3 3.9.3.6	8.6.6.8.6 8.6.8.6.7	0	10	0	10	0:10.00
21	11- 20	2.3	5.6.5.5 9.5.5.7	1.2.7.1 7.3.7.4	7.9.7.7 7.9.7.7 9.7.8*	4.8.5.5 8.4.8.4 5.4.6*	1.4.3.2 8.9.1.9 1.9.5*	3.1.2	6.2.3.2.6 3.5.2.4	3.9.3.8 4.3.3.5 3.4.6*	6.7	0	10	0	10	0:10.00
22	21- 30	2.3	5.6.5.6.7	1.7.4	7.9.7 7.7.8	4.7.4.6	1.4.6.3 2.7.5	3.1.2	5.2.6.4	3.7.4.8 4.3.5.8 3.7.6*	6.6.7	0	10	0	10	0:10.00
23	31- 40	2.4.3	5.6.8.7	1.7.4	7.7.7.7 7.7.8	4.7.6	2.7.5	3.1.2	6.4	5.4.7.4 7.5.4.3 7.3.6	7	1	9	1	9	1: 9.00
24	41- 50	3	5.6.7	1.6.4	7.7.7.8	4.7.6	2.7.6.4 2.7.3.8 9.6.5*	3.2	6.5.4	5.3.8.6	6.7	1	9	1	9	1: 9.00
26	51- 60	4.3	6.5.7	2.7.4	7.8	6	6.4.2.7 4.8.6.5	3.2	6.5.3.5.2 6.5.6.4	7.6	7	2	8	2	8	1: 4.00
27	61- 70	3	6.5.7	2.5.4	7.7.8	5.7.6	4.7.3.7 4.6.1.4 7.3.5	2	6.5.4	5.7.6	6.7	2	8	2	8	1: 4.00
28	71- 80	3	7	6.5.4	7.8	5.4.7.6	2.7.6.5	2	5.5.4	7.6	7	4	6	4	6	1: 1.50
29	81- 90	3	6.5.7	2.4	7.7.8	5.4.6	2.7.7.6 2.7.6.5	2	4	5.4.7.6	7	4	6	4	6	1: 1.50
30	91-100	3	7	2.6.5.4	7.8	6	5	2	5.4	5 4.6	7	6	4	6	4	1: 0.67
31	101-110	3	7	2.4	7.8	6	5	2	4	7.6	7	7	3	7	3	1: 0.43
August 2	111-120	3	7	6.5.7.6.4	7.8	6	4.2.7.6.5	2	6.5.4	7.6	7	5	5	5	5	1: 1.00
3	121-130	3	6.5.7	7.6.5.4	7.8	7.6	5	2	6.5.4	7.6	7	4	6	4	6	1: 1.50

4	131-140	3	7	6.5.7.6.4	7.8	6	2.7.6.4 8.7.6.5	2	6.4	3.5.4.6	7	5	5	5	5	1: 1 00
5	141-150	2.4.4.4 2.4.4.2 4.3*	6.5.7	2.7.6.7 5.4	8	7.6	2.8.7.6 8.6.5	3.2	6.4	7.6	8.7	1	9	1	9	1: 9.00
6	151-160	2.4.4.3	7	2.6.5.4	7.8	7.6	7.6.5	2	6.4	6	7	4	6			
"	161-170	4.4.2.4 4.2.4.2 4.4.3	7	6.5.4	7.8	7.6	7.6.5	3.2	5.4	7.6	7	2	8	6	14	1: 2.33
7	171-180	4.3	7	6.5.4	8	7.6	5	2	6.5.4	7.6	7	5	5			
"	181-190	4.2.4.4 2.4.3	7	7.6.5.4	7.8	6	6.5	2	6.5.4	7.6	7	4	6	9	11	1: 1.22
9	191-200	3	7	5.4	8	8.7.6	6.5	2	6.5.4	7.6	8.7	4	6	4	6	1: 1.50
10	201-210	3	7	2.5.4	7.8	7.6	2.8.7.6 8.7.6.5	2	6.5.4	7.6	7	4	6	4	6	1: 1.50
11	211-220	3	7	6.5.4	7.8	6	7.6.4.3 2.7.6.5	2	6.5.4	7.6	7	5	5	5	5	1: 1.00
12	221-230	3	7	2.4	7.8	6	7.6.5	2	6.5.4	8.7.6	7	5	5	5	5	1: 1.00
19	231-240	3	7	2.4	7.8	6	5	2	6.4	8.7.6	7	6	4	6	4	1: 0.67
20	241-250	3	7	5.4	8	7.6	2.4.1.2.7 8.7.6.5	3.2	6.4	7.6	7	4	6	4	6	1: 1.50
21	251-260	3	7	6.5.4	7.8	7.4.5.4 8.7.6	6.4.3.2 7.6.5	2	6.5.4	8.7.6	7	4	6	4	6	1: 1.50
23	261-270	3	7	6.5.4	7.8	6	6.5	2	6.5.4	7.6	7	5	5	5	5	1: 1.00
24	271-280	3	7	6.4	7.8	7.6	2.5	2	4	7.4.3.8.7.6	7	5	5	5	5	1: 1.00
25	281-290	3	7	2.5.4	8	7.6	5	2	6.4	7.6	7	6	4	6	4	1: 0.67
26	291-300	3	7	6.5.4	8	7.6	7.6.5	2	6.5.4	7.6	7	5	5	5	5	1: 1.00
27	301-310	3	7	2.6.5.4	8	7.6	5	2	6 5.4	7.5.4.9.8.6	7	6	4	6	4	1: 0.67
28	311-320	3	7	2.5.4	8	6	9.8.7.5	2	3.4	8.6	7	6	4	6	4	1: 0.67

* Aided by experimenter.

laboratory to take charge, I found that Sobke was no longer trying to solve the problem as when I had gone away. His attitude had changed in that he had become indifferent, careless, and obviously discouraged with his task.

I immediately set about reinstating the former attitude by lessening the number of trials and the punishment, and by increasing the value of the reward, but my best efforts, continuing up to August 28, failed markedly to improve the condition. The number of correct choices did somewhat increase, but at no time did the animal attain the degree of success which he had achieved as early as July 31 in the eleventh series of trials.

During the last two weeks of experimentation, all possible efforts were put forth to discover the best combination of rewards and punishments. Punishment was varied from 0 to confinement of sixty seconds, and many kinds of food in different amounts were tried as rewards, but in spite of everything Sobke failed to improve markedly. From time to time, notably on August 12 and 21, he exhibited peculiarly strong resentment toward me and repeatedly attempted to attack me.

The outcome of my experiments with problem 4 is peculiarly interesting in that it indicates the importance of a favorable attitude toward the work and the extreme risk from disturbing or discouraging conditions. It seems not improbable that had the work progressed without change in experimenter, or method of procedure, and above all without the disturbance of the painting, Sobke might have solved problem 4 within a few days. This is by no means certain, however, for in problems 2 and 3 the ratio of right to wrong choices instead of increasing steadily increased very irregularly.

The detailed results for this problem are given in table 7. Reactive tendencies which appear are: (a) persistent choice of the end boxes followed, subsequently, by (b) the tendency to locate the middle box directly. This proved fairly easy when the number of boxes involved was only three as in settings 1, 4, 7, and 10. Setting 4 was most difficult of all, because box 9 was avoided or ignored. When the number of open boxes was as great as five, as in settings 2 and 8, the task was obviously more difficult, but whereas success in setting 2 appeared early, in setting 8 it failed to appear during the course of experimentation. For the settings 3, 6, and 9, involving either seven or nine open boxes, the direct choice of the middle box was next to impossible, and Sobke tended to choose, first of all, a particular box toward one end of the series, for example, box 2, in setting 3, and box 7 in setting 9. To the experimenter, as he watched the animal's behavior, it looked as though effort each time were being made to locate the middle member of the group. This appeared relatively easy for groups of three boxes, extremely difficult for as many as five boxes, and almost impossible for seven or nine.

3. Julius, *Pongo pygmæus*

Problem 1. *First at the Left End*

The orang utan, Julius, was gentle, docile, and friendly with the experimenter throughout the period of investigation. He at no time showed inclination to bite and could be handled safely. As contrasted with Skirrl and even with Sobke, he adapted himself to the multiple-choice apparatus very promptly, and only slight effort on the part of the observer was necessary to prepare him, by preliminary trials, for the regular experiments. But in order to facilitate work, he was familiarized with the apparatus by means of regular route training and feeding in the several boxes from April 5 to April 9.

On April 10 the apparatus was painted white as has been stated previously, and on the following Monday, April 12, Julius when again introduced to it gave no indications of fear, uneasiness, or dislike, but worked as formerly, making his round trips quickly and eagerly entering any box which happened to be open, in order to obtain the reward of food.

The regular experimentation was undertaken on April 13, and the results of the first series of trials with Julius are sharply contrasted with those obtained with the monkeys in that fewer choices were necessary. Instead of the expected ratio of right to wrong first choices, 1 to 2.5, the orang utan gave a ratio of 1 to 1. An additional markedly different result from that obtained with the monkeys is indicated below in the total time required for a series of trials. As examples, the data for the first, second, fifth, and tenth series are presented.

TIME FOR SERIES OF TRIALS

	1*st series*	2*nd series*	5*th series*	10*th series*
Skirrl..............	35 min.	20 min.	14 min.	10 min.
Sobke.............	14 "	17 "	10 "	9 " (8*th series*)
Julius.............	12 "	11 "	14 "	9 "

It is also noteworthy that Julius in the presence of visitors or under other unusual conditions worked steadily and well, whereas the monkeys, and especially Sobke, tended to be distracted and often refused to work at all.

Almost from the beginning of his work on problem 1, Julius began to develop the tendency to enter immediately the open door nearest the starting point. In case the group of open doors lay to the right of the middle of the apparatus, this method naturally yielded success; whereas if the group included doors to the left of the middle, it resulted in failure. Obviously it was a most unsatisfactory method, and although it enabled him to make more right than wrong first choices, it prevented him from increasing the number of right choices, and as table 1 indicates, it maintained the ratio of 1 right to .67 wrong first choices for eight successive days.

On April 23 a break occurred in which the number of correct choices was reduced from six to five. Julius worked very rapidly and with almost no hesitation in choosing. My notes record " he seems to miss the point wholly. It is doubtful whether the punishment is sufficiently severe." At this time he was being punished by thirty seconds confinement in each wrong box, the interval having been held fairly steadily from the first series of experiments. On April 26 it was increased to sixty seconds, in an effort to break him of the habit of choosing the " nearest " door. But he became extremely restless under the longer confinement and tried his best to raise the entrance and exit doors. Since there was at this time no mechanism for locking them when closed, it was difficult for the experimenter to prevent him from escaping by way of the entrance door or from raising the exit door sufficiently to obtain the food. Indeed, the longer confinement worked so unsatisfactorily that on the following day I substituted for it the punishment of forcing him to raise the entrance door of the wrong box in order to escape for a new choice. He was rewarded with food in the alleyway H, beside door 15 (figure 17), only when he chose correctly on first attempt.

This method discouraged him extremely and proved wasteful of time. Consequently, in a second series on the same date return was made to the former method, and he was rewarded with food whenever he found the right box. But on April 28, the two methods were again employed, the first in the initial series and the second in a final series of trials. The animal's persistent attempts to raise the doors gave the experimenter so much trouble that on April 29 barbed wire was nailed over the windows of the entrance doors with the hope that it might prevent him from working at them. But he quickly learned to place his fingers between the barbs and raise the doors as effectively as ever.

On April 30 the reward of food was given only when the first choice was that of the right box and in that event it was placed in the alleyway H as stated above.

As it seemed absolutely essential to break the unprofitable habit of choosing the nearest door, on May 3 a new series of settings was presented, in which only the doors to the left of the middle of the row of nine boxes were used as right doors. That is, in this new series, doors 1 to 4 occur as right doors; 5 to 9 do not. As punishment for wrong choices on this date, Julius was confined in the wrong box from one to five minutes. It was difficult to keep him in, but by means of cords which had been attached to the doors, this was successfully accomplished. Yet another and slightly different series of settings was employed on May 4, and this, proving satisfactory, was continued in use until the end of the experiment, with punishment ranging from sixty to one hundred and twenty seconds for each mistake.

Naturally the modification of settings introduced May 3 greatly increased the proportion of wrong first choices. Indeed, as appears in table 8, the ratio of right to wrong immediately changed from 1:.67 to 1:4.00. Between May 3 and May 10, no steady and consistent improvement in method or in the number of correct first choices occurred, and on the last named date, Julius chose correctly only three times in his ten trials. At this time there was, as my notes record, no satisfactory indication of progress, and the status of the experiment seemed extremely unsatisfactory in as much as in spite of the experimenter's best efforts to break up the habit of choosing the nearest door, the orang utan still persisted, to a considerable extent, in the use of this method. The only encouraging feature of the results was an evident tendency to choose somewhat nearer the left end of a group than previously.

A series of correct first choices was obtained on May 11,

TABLE 8

Results for Orang utan in Problem 1

Date	No. of trials	S. 1 1.2.3	S. 2 8.9	S. 3 3.4.5.6.7	S. 4 7.8.9	S. 5 2.3.4.5.6	S. 6 6.7.8	S. 7 5.6.7	S. 8 4.5.6.7.8	S. 9 7.8.9	S. 10 1.2.3	R	W	R	W	Ratio of R to W
April 13	1- 10	3.1	8	4.3	7	4.2	7.6	5	4	7	3.1	5	5	5	5	1:1.00
14	11- 20	3.2.1	8	4.3	7	4.4.2	6	5	4	7	3.1	6	4	6	4	1:0.67
15	21- 30	3.2.1	8	4.3	7	4.5.5.2	6	5	4	7	3.1	6	4	6	4	1:0.67
16	31- 40	3.1	8	4.3	7	4.2	6	5	4	7	3.2.1	6	4	6	4	1:0.67
17	41- 50	3.2.1	8	4.3	7	4.2	6	5	4	7	3.1	6	4	6	4	1:0.67
19	51- 60	3.1	8	4.3	7	4.2	6	5	4	7	3.1	6	4	6	4	1:0.67
20	61- 70	2.1	8	4.3	7	5.3.2	6	5	4	7	3.1	6	4	6	4	1:0.67
21	71- 80	3.1	8	4.3	7	5.4.3.2	6	5	4	7	3.2.1	6	4	6	4	1:0.67
22	81- 90	3.1	8	5.3	7	6.3.2	6	5	4	7	3.2.1	6	4	6	4	1:0.67
23	91-100	3.2.1	8	5.3	7	4.3.2	6	5	5.4	7	3.2.1	5	5	5	5	1:1.00
24	101-110	3.2.1	8	4.3	7	4.3.2	6	5	4	7	3.1	6	4	6	4	1:0.67
26	111-120	3.1	8	4.3	7	4.3.2	6	5	5.4	7	3.1	5	5	5	5	1:1.00
27	121-130	3.2.1	8	4.3	7	4.3.2	6	6.5	5.8.6.4	7	3.3.3.1	4	6			
"	131-140	3.1	8	4.3	7	4.3.2	6	5	4	7	3.2.1	6	4	10	10	1:1.00
28	141-150	3.2.1	8	3	7	5.4 2	6	5	4	7	3.1	7	3			
"	151-160	3.1	8	3	7	3.2	6	5	4	7	3.2.1	7	3	14	6	1:0.43
29	161-170	3.1	8	4.3	7	4.3.2	6	5	4	7	3.2.1	6	4			
"	171-180	3.2.1	8	4.3	7	4.2	6	5	4	7	3.2.1	6	4	12	8	1:0.67
30	181-190	3.1	8	4.3	7	4.5.6.4 5.6.4.2	6	5	4	7	3.1	6	4			
"	191-200	3.1	8	4.5.6.7.3	7	4.5.3.2	6	5	4	7	3.2.1	6	4	12	8	1:0.67
May 1	201-210	3.1	8	4.3	7	3.2	6	5	4	7	3.1	6	4			
"	211-220	3.2.1	8	4.3	7	4.2	6	5	4	7	3.1	6	4	12	8	1:0.67

		1.2.3	**3.4.5.6.7**	**2.3.4.5** 6.7.8	**1.2.3**	**3.4.5.6.7**	**4.5.6.7.8.9**	**2.3.4.5**	**1.2.3**	**4.5.6.7.8.9**	**2.3.4.5** 6.7.8.9					
3	221-230	3.1	4.3	4.2	3.1	4.3	4	4.3.2	3.1	4	4.3.2	2	8	2	8	1:4.00
		1.2.3	**3.4.5.6.7**	**2.3.4.5** 6.7.8	**4.5.6.7** 8.9	**2.3.4.5**	**3.4.5.6**	**1.2.3**	**4.5.6.7.8**	**2.3.4.5** 6.7.8.9	**1.2.3**					
4	231-240	3 2.1	4.3	4 2	4	4 3.2	4 3	3.2.1	4	3.2	3.2.1	2	8	2	8	1:4.00
5	241-250	2.1	3	3 2	4	3.2	4.3	3.2.1	4	3.2	3.2.1	3	7	3	7	1:2.33
6	251-260	2 1	3	2	4	3.2	3	2.1	4	3 2	2 1	5	5	5	5	1:1.00
7	261-276	2 1	3	3 2	4	3.2	3	2.1	1	4.2	2.1	4	6	4	6	1:1.50
8	271-280	2 1	4 3	4 3 2	4	3 2	4 3	3.1	4	3.2	2 1	2	8	2	8	1:4.00
10	281-290	1	4 3	4 2	4	3.2	4.3	2.1	4	3.2	2.1	3	7	3	7	1:2.33
11	291-300	1	3	2	4	2	3	1	4	2	1	10	0	10	0	1:0.00
		1.2.3	**8.9**	**3.4.5.6.7**	**7.8.9**	**2.3.4.5.6**	**6.7.8**	**5.6.7**	**4.5.6.7.8**	**7.8.9**	**1.2.3**					
12	301-310	1	8	3	7	2	6	5	4	7	1	10	0	10	0	1:0.00

greatly to the surprise of the experimenter, for no indication had previously appeared of this approaching solution of the problem. It seemed possible, however, that the successes were accidental, and it was anticipated that in a control series Julius would again make mistakes. But on the following day, May 12, the presentation of the original series of ten settings, which, of course, differed radically from the settings used from May 4 to May 11 was responded to promptly, readily, and without a single mistake.

Julius had solved his problem suddenly and, in all probability, ideationally.

Only three reactive tendencies or methods appeared during Julius's work on this problem: (a) choice of the open door nearest to the starting point (sometimes the adjacent boxes were entered); (b) a tendency to avoid the " nearest " door and select instead one further toward the left end of the group; (c) direct choice of the first door on the left.

The curve of learning plotted from the daily wrong choices and presented in figure 18, had it been obtained with a human subject, would undoubtedly be described as an ideational, and possibly even as a rational curve; for its sudden drop from near the maximum to the base line strongly suggests, if it does not actually prove, insight.

Never before has a curve of learning like this been obtained from an infrahuman animal. I feel wholly justified in concluding from the evidences at hand, which have been presented as adequately as is possible without going into minutely detailed description, that the orang utan solved this simple problem ideationally. As a matter of fact, for the solution he required about four times the number of trials which Sobke required and twice as many as were necessary for Skirrl. Were we to measure the intelligence of these three animals by the number of trials needed in problem 1, Sobke clearly would rank first, Skirrl second, and Julius last of all. But other facts clearly indicate that Julius is far superior to the monkeys in intelligence. We therefore must conclude that *where very different methods of learning appear, the number of trials is not a safe criterion of intelligence.* The importance of this conclusion for comparative and genetic psychology needs no emphasis.

Problem 2. Second from the Right End

Julius was given four days' rest before being presented with problem 2. He was occasionally fed in the apparatus, but regular continuation of training was not necessary to keep him in good form. During this rest interval, locks were attached to the doors of the apparatus so that the experimenter by moving a lever directly in front of him could fasten either one or both of the doors of a given box by a single movement. On May 13 Julius was given opportunity to obtain food from each of the boxes in turn, and trial of the locks was made in order to familiarize him with the new situation. He very quickly discovered that the doors could not be raised when closed, and after two days of preliminary work, he practically abandoned his formerly persistent efforts to open them. The locks worked satisfactorily from a mechanical point of view as well as from that of the adaptation of the animal to the modified situation.

Problem 2 was regularly presented for the first time on May 17, on which day a single series was given. The period of punishment adopted was twenty seconds, and for each successful choice a small piece of banana was given as a reward. After the first trial in this series, in which Julius repeatedly entered the first box at the left, that is box 7, there was but slight tendency to reënter the first box at the left of the group. Instead, Julius developed the method of moving box by box toward the right end of the group. The choices were made promptly, and their systematic character enabled the animal to obtain his reward fairly quickly, in spite of the large number of mistakes.

In the second series, the orang utan developed the interesting trick of quickly dodging out of the wrong box before the experimenter could lower the door behind him. This he did only after having been punished for many wrong choices to the point of discouragement. The trick was easily broken up by the sudden lowering of the entrance door as soon as he had passed under it.

There appeared on May 21 an unfavorable physical condition which manifested itself, first of all through the eyes which appeared dull and bloodshot. On the following day they were inflamed and the lids nearly closed. Julius refused to eat, and experimentation was impossible. Until June 2 careful treatment and regulation of diet was necessary. He passed through what at the time seemed a rather startling condition, but rapidly regained his usual good health, and on June 3, although somewhat weak and listless, he again worked fairly steadily.

Since it was now possible to lock the doors and confine the animal for any desired period, on June 5 the interval of punishment was made sixty seconds, and a liberal quantity of banana, beet, or carrot was offered as reward. No increase in the number of successful choices appeared, and Julius showed discouragement. Sawdust had been strewn on the floor, and in the intervals between trials as well as during confinement in wrong boxes, he took to playing with the sawdust. He would take it up in one hand and pour it from hand to hand until all had slipped through his fingers, then he would scrape together another handful and go through the same process. Often he became so intent on this form of amusement that even when the exit door was raised, he would not immediately go to get the food.

The reactive tendencies which appeared in the work on problem 2 will now be presented in order, since I shall have to refer to them repeatedly, and the list will be more useful to the reader at this point than at the conclusion of the presentation of daily results. The following is not an exhaustive list but includes only the most important and conspicuous tendencies or methods together with the dates on which they were most apparent.

(a) May 17, choice of first box at left of group or near it, then the next in order, and so on, until the second from the right was reached. This method with irregularities and certain definite skipping was used at various times, sometimes over periods of several days, during the course of the work.

(b) June 3, preference for number 3 and number 4 developed immediately after the orang utan's illness and when he was working rather listlessly.

On June 9 and 10, the original tendency (a) reappeared and persisted for a number of series.

(c) June 14, a tendency to choose the box at or near the right end of a group, and then the one next to it. In connection with this tendency, which of course required only two choices in any given trial, interest in playing with the sawdust on the floor developed.

Again on June 21, the animal returned to the use of tendency (a).

(d) June 29, movement to box at right end of group, hesitation before it, and turning through a complete circle so that the second box from the right was faced. This, the correct box, was often promptly entered. This method, if persisted in, would obviously have yielded solution of the problem.

(e) July 5, approach to and pretense to enter the box next to the right end (right one), and then choice of some other box. This *feint* is peculiarly interesting, and its origin and persistence are difficult to account for.

(f) In connection with the tendency to pretend that he was going to enter the second box from the right end, Julius developed also the tendency to turn around in front of the box at the right end, starting sometimes to back into it, and then to enter, instead, the box second from the end.

(g) July 6 and 7, a fairly definite tendency to take the one next in order or, instead, to go directly to the right box.

(h) July 10, direct first choices without approach to other boxes appeared for the first time on this date.

For this problem, it proved impossible to establish and maintain uniform conditions of experimentation. Instead, because of the failure of the animal to improve and the tendency to discouragement, both punishment and reward had to be altered from time to time, and other and more radical changes were occasionally made in the experimental procedure. Below for the sake of condensed and consecutive presentation, the most important conditions from day to day are arranged in tabular form:

CONDITIONS OF EXPERIMENT FROM DAY TO DAY FOR PROBLEM 2

Date	Punishment	Reward
May 17	20 sec. confinement (Aid after 10 trials)	Food in right box for each trial
“ 18 to 21	30 sec. confinement	Food (banana) in right box for each trial
“ 22 to June 2	Illness, no experiments	
June 3	15 sec. confinement	Food (banana) in right box for each trial
“ 4	30 “ “	Food (banana) in right box for each trial
“ 5-10	60 “ “	Beet, carrot and loquat, in addition to banana
“ 11	10 to 30 sec. confinement	Beet, carrot and loquat, in addition to banana
“ 12 to 15	60 sec. confinement	Beet, carrot and loquat, in addition to banana
“ 16	60 “ “	Banana and sweet corn—former preferred

CONDITIONS OF EXPERIMENT FROM DAY TO DAY FOR PROBLEM 2—*Continued*

Date	Punishment	Reward
June 17 (1st series).	60 sec. confinement..........	Food (banana, as in early series)
“ 17 (2nd series).	No confinement in wrong box; but instead, return to starting point by way of alleys	Food only for correct first choices
“ 18 to 22......	No confinement in wrong box; but instead, return to starting point by way of alleys	Food only for correct first choices
“ 22 (2nd series).	No punishment; allowed to enter boxes until right one was found	Food for each trial
“ 23...........	Return to starting point. After five wrong choices of a given box the animal was held for 60 secs. in one of the boxes and was then released by way of the exit door and rewarded when the right one was chosen	
“ 23 (2nd series).	No punishment..............	Reward for each trial
“ 24 (1st series).	Return to starting point......	Food only for correct first choices
“ 24 (2nd series).	No punishment..............	Reward for each trial
“ 25-30.........	Same as on 24th............	
July 1 (1st series).	No punishment..............	“ “ “ “
“ 1 (2nd series).	Return to starting point......	Reward only for correct first choices
“ 2-8..........	Same as on July 1...........	
“ 8 (2nd series).	No punishment..............	Reward for each trial
“ 8 (3rd series).	Return to starting point......	Reward only for correct first choices
“ 9-10.........	Same as for July 8 (3rd series)	
“ 10 (2nd series).	Momentary confinement in wrong boxes	Reward for each correct choice
“ 12...........	Return to starting point......	Reward for correct first choice
“ 12 (2nd series).	30 sec. confinement..........	Reward for each correct choice
“ 12 (3rd series).	5 “ “	“ “ “ “ “
“ 13...........	30 “ “	“ “ “ “ “
“ 14-17.........	Return to starting point......	Reward for correct first choices
“ 17 (2nd series).	60 sec. confinement..........	Reward for each correct choice
“ 19...........	30 “ “	“ “ “ “ “
“ 20-26.........	10 “ “	“ “ “ “ “
“ 27-30.........	Right box indicated by slight raising of exit door momentarily. No punishment	Reward in each right box
“ 30 (2nd series).	Return to starting point......	Reward for correct first choices
“ 31...........	“ “ “ “	“ “ “ “ “
“ 31 (2nd series) to Aug. 10..	10 to 60 sec. confinement.....	Reward for each correct choice
Aug. 10 (2nd series).	Threatened with whip........	“ “ “ “ “
“ 11 (1st series).	“ “ “	“ “ “ “ “
“ 11 (2nd series).	10 sec. confinement..........	“ “ “ “ “
“ 12...........	Threatened with whip........	“ “ “ “ “
“ 12 (2nd series).	10 sec. confinement..........	“ “ “ “ “
“ 19...........	10 “ “	“ “ “ “ “
“ 19 (2nd series).	Threatened with whip........	“ “ “ “ “

With the above reactive tendencies and modifications of method in mind we may continue our description of results. On June 9 there developed a tendency to increase the magnitude of the original error by choosing nearer the left end of the groups. This is odd, since one would naturally suppose that an animal as intelligent as the orang utan would tend to avoid the general region in which success was never obtained and to focus attention on the right, as contrasted with the wrong end of each group. *It obviously contradicts the law of the gradual elimination of useless activities.* In other words, it is wholly at variance with the principle of trial and error exhibited by many infrahuman organisms. Julius, although making many mistakes, worked diligently and, for the most part, fairly rapidly. The day's work proved most important because of the change in method and also because of the appearance of hesitation, the rejection of certain boxes, and the definite choice of others. My notes record "this is a most important day for Julius in problem 2;" but subsequent results do not clearly justify this prophecy.

The method of choosing the first box at the left and then of moving down the line until the right one was reached was so consistently followed that during a number of days it was possible for me to predict almost every choice. Indeed, to satisfy my curiosity in this matter during a number of series I guessed in advance the box which would be chosen. The percentages of correct guesses ranged from ninety to one hundred. June 10, for example, yielded two series for which the ratio of right to wrong first choices was 0 to 10, and in which the method described above was used consistently throughout.

It was inevitable that punishment by confinement and the discouragement resulting therefrom should interfere with the regularity of work and make it extremely difficult to obtain strictly comparable results from series to series and from day to day. The data for this problem, as presented in table 9, have values quite different from those for the monkeys, chiefly because of the more variable conditions of observation.

It was occasionally noted that the disintegration of a definite method and the disappearance of the tendency on which it depended occurred rather suddenly. Frequently it happened that having used an inadequate method fairly persistently on a given day, the animal would on the following day exhibit a

TABLE 9

Results for Orang utan in Problem 2

Date	No. of trials	S. 1 7.8.9	S. 2 1.2.**3**.4	S. 3 2.3.4.5.**6**.7	S. 4 1.2.3.4.**5**.6	S. 5 4.5.6.**7**.8	S. 6 1.**2**.3	S. 7 2.3.**4**.5	S. 8 1.2.3.4.5 6.7.8.9	S. 9 1.2.**3**.4	S. 10 3.4.5.6.**7**.8	R	W	R	W	Ratio of R to W
May 17	1- 10	7.7.7.7 7.7.7.7 7.7.8	1.1.3	2.4.6	2.3.4.5	4.5.7	3.2	2.3.4	2.3.4.5 6.7.8	1.2.3	3.4.5.6.7	0	10	0	10	0:10.00
18	11- 20	7.8	1.3	3.4.5.6	2.4.5	4.5.6.7	2	2.3.4	2.3.4.5 6.7.8	1.2.3	4.5.6.8 8.8.8.3 4.5.6.7	1	9	1	9	1: 9.00
19	21- 30	7.8	1.3	2.4.7.7.2 5.7.7.2 3.2.4.6	5	4.6.8.4.7	1.2	2.3.4	5.8	3	5.3.4.5 6.8.4.5 3.5.6.7	2	8	2	8	1: 4.00
20	31- 40	7.9.7.7 9.7.8	3	4.5.6	4.5	5.6.7	2	4	5.6.7.8	3	5.6.7	4	6	4	6	1: 1.50
21	41- 50	7.8	3	3.4.5.6	4.5	4.5.6.7	2	3.4	4.5.6.8	3	4.5.6.7	3	7	3	7	1: 2.33
June 3	51- 60	7.8	3	4.5.6	3.4.5	4.5.6.7	3.1.2	3.4	3.7.9.7 9.7.6.8	3	4.5.6.7	2	8	2	8	1: 4.00
4	61- 70	7.8	3	4.5.6	4.5	4.5.6.7	3.3.1.2	4	4.5.6.7.8	3	4.5.6.7	3	7	3	7	1: 2.33
5	71- 80	7.9.7.8	3	3.4.5.6	3.6.3.4.5	4.7	3.1.2	4	3.4.5.6.7.8	3	4.5.6.7	3	7			
"	81- 90	7.8	3	3.4.5.6	3.4.5	4.5.6.7	2	3.4	3.4.5.6.7.8	2.3	4.5.6.7	2	8	5	15	1: 3.00
7	91- 100	7.8	3	4.5.6	4.5	4.5.6.7	1.2	3.4	3.4.5.6 7.8	2.3	3.4.5.6 8.5.6.7	1	9	1	9	1: 9.00
8	101- 110	7.8	3	4.5.6	4.5	4.5.6.7	2	3.4	4.5.6.7.8	3	4.5.6.7	3	7			
"	111- 120	7.8	3	4.5.6	3.4.5	5.6.7	2	5.5.2.3.4	4.5.6.7.8	4.4.1.2.3	4.5.6.7	2	8	5	15	1: 3.00
9	121- 130	7.8	2.3	4.5.6	4.5	4.5.6.7	2	3.4	2.3.4.5 6.7.8	1.2.3	3.4.5.6.7	1	9			
"	131- 140	7.8	1.2.3	2.3.4.5.6	5	4.5.6.7	1.2	2.3.4	6.7.8	2.3	3.4.5.6.7	1	9	2	18	1: 9.00
10	141- 150	7.8	1.2.3	2.3.4.5.6	1.2.3.4.5	4.5.6.7	1.2	2.3.4	1.2.3.4 5.6.7.8	1.2.3	3.4.5.6.7	0	10			
"	151- 160	7.8	1.2.3	2.3.4.5.6	1.2.3.4.5	4.5.6.7	1.2	2.3.4	1.2.3.4 5.6.7.8	1.2.3	3.4.5.6.7	0	10	0	10	0:10.00

11	161- 170	8	4.1.2.4.1 4.1.4.1.3	2.3.4.5.6	5	6.7	3.1.3.1 3.1.2	5.2.3.4	2.6.7.8	4.1.2.3	8 5.6.7	2	8			
"	171- 180	9.7.8	4.3	5.6	4.5	7	3 1.2	5.4	8	4.2.3	7	3	7	5	15	1: 3.00
12	181- 190	7.9.7.9.8	1.2.3	3.4.5.6	6.4.5	5.6.7	2	5.3.4	7.8	4.1.2.3	4.5.6.7	1	9	1	9	1: 9.00
14	191- 200	9.8	3	7.6	6.5	8.7	3.2	5.4	6.7.8	4.2.3	7	2	8			
"	201- 210	8	2.3	7.6	6.5	8.7	3.1.2	5.4	8	4.3	7	3	7	5	15	1: 3.00
15	211- 220	7.9.7.9 7.9.8	4.2.3	6	5	8.7	3.2	5.4	9.8	4.3	7	3	7			
"	221- 230	9.8	4.3	7.6	6.5	5.6.7	1.2	2.3.4	3.4.5.6.7.8	4.3	7	1	9	4	16	1: 4.00
16	231- 240	7.9.8	3	7.6	6.5	8.7	2	5.4	5.6.7.8	4.1.4.3	8.7	2	8			
"	241- 250	9.8	4.3	7.6	6.5	7	3.2	5.4	6.7.8	4.3	6.7	1	9	3	17	1: 5.67
17	251- 260	9.8	4.3	7.6	6.5		3.2	5.4	6.5.6.7.8	3	5.6.7	1	9			
"	261- 270	9.7.7.7 7.7.7.8	4.4.4.4.3	6	5	6.5.4.6 5.4.5.7 6.6.6.5.6 5.5.5.6.6 6.6 6.6.6 6.5.4.5 6.5.5.4 4.5.6.7	3.3.3.3 3.3.2	4	4.5.4.5.4 5.6.7.8	3	5.5.5.5 5.5.8.7	4	6	5	15	1: 3.00
18	271- 280	7.7.7.7 7.7.8	4.4.4.4.3	5.5.5.5.6	5	5.5.5.6 5.5.6	3.3.3.3 3.3.2	4	4.6.5.6 6.7.4.8	4.4.3	5.6.4.7	2	8	2	8	1: 4.00
19	281- 290	7.7.7.9 9.9.9.7 7.7.8	4.4.4 4.4.3	5.7.7 7.4.6	5	5.7	3.3.3.3 3.3.2	4	5.8	4.4.4.4 4.4.3	6.5.6.5 5.5.7	2	8			
"	291- 300	7.7.7.7.8	4.4.4.4.3	5.5.6	5	5.6.6.5.6 6.6.6.7	3.3.2	5.5.5 5 3.4	5.6.8	4.4.3	6.6 6.7	1	9	3	17	1: 5.67
21	301-310	7.7.8	4.4.3	5.6	5	6.6.6.5 5.6.5.7	3.3.3.3 3.2	5.5.5.2.4	5.6 6 6.7 3.4.3.5.5 1.1.2.1.7 2.3.2.2.8	4.2.2.4 4.3	5.3.7	1	9			
"	311- 320	7.7.7.7 7.7.8	1.1.1.2.3	5.5.4.2 2.5.6	1.1.6.5	4.6.7	1.3.2	2.2.2.2 2.2.3.4	1.1.2.3 6.6.6.8	2.2.2.4 2.3	6.3.3 8 4.8.7	0	10	1	19	1:19.00
22	321- 330	7.8	2.4.2.1.4 4.4.4.3	6	5	6.6.8.7	3.2	5.5.3.3.4	3.3.7.7.8	4.1.4.2.3	6.7	2	8			
"	331- 340	7.8	3	6	5	6.7	3.2	5.4	6 7.8	3	5.6.7	4	6	6	14	1: 2.33
23	341- 350	7.8	4.2.4.3	6	5	6.7	3.3.3.1 3.3.3.2	5.5.4	7.5.4.3 6.7.7.8	4.4.4.3	6.6 3.6 8.6.8.7	2	8			
"	351- 360	7.8	4.4.3	6	6.5	6.7	3.3.2	5.4	6.7.6.5 7.6.8	4.3	5.6.7	1	9	3	17	1: 5.67

TABLE 9—*Continued*

Results for Orang utan in Problem 2

Date	No. of trials	S. 1 7.**8**.9	S. 2 1.2.**3**.4	S. 3 2.3.4.5.**6**.7	S. 4 1.2.3.4.**5**.6	S. 5 4.5.6.**7**.8	S. 6 1.**2**.2	S. 7 2.3.**4**.5	S. 8 1.2.3.4.5 6.7.**8**.9	S. 9 1.2.**3**.4	S. 10 3.4.5.6.**7**.8	R	W	R	W	Ratio of R to W
June 24	361- 370	7.8	4.4.2.3	7.7.6	6.6.4.5	7	3.3.3.3 3.3.2	5.4	8	4.4.3	5.6.7	2	8			
"	371- 380	8	4.3	7.6	5	7	3.2	5.4	7.6.8	4.3	7	4	6	6	14	1: 2.33
25	381- 390	8	4.4.3	7.7.7.6	6.6.6.6 6.4.6.5	8.6.7	3.1.2	5.3.5.4	8	3	7	4	6			
"	391- 400	8	4.3	6	5	6.5.8.7	3.2	5.4	8	3	8.7	5	5	9	11	1: 1.22
26	401- 410	9.9.9.8	3	7.7.7.7.3 3.7.7.6	6.6.6.6 6.6.5	8.8.8.7	3.2	5.5.5.5 5.5.4	7.8	4.4.4.4 4.4.3	6.6.8.8.7	1	9			
"	411- 420	8	4.3	7.6	6.5	7	3.2	5.4	8	4.3	8.7	3	7	4	16	1: 4.00
28	421- 430	8	4.4.4.4 4.4.3	7.6	6.6.3.6 6.6.6.5	7	3.3.3.3 3.3.2	5.5.5.4	9.7.7.5.8	4.4.4.3	8.7	2	8			
"	431- 440	8	4.3	6	6.5	7	3.2	5.4	7 8	4.3	7	4	6			
"	441- 450	8	4.3	7.6	6.5	8.7	3.2	5.4	7.6.5.4 3.2.1.5 7.9.8	4.3	7	2	8	8	22	1: 2.75
29	451- 460	8	3	6	6.6.6.5	8.6.7	3.3.3.3 3.3.2	5.4	8	4.3	7	5	5			
"	461- 470	8	4.3	6	5	8.7	3.2	3.2.3.4	7.8	4.3	7	4	6			
"	471- 480	8	4.4.3	7.7.6	6.6.5	7	2	5.5.5.5.5.4	7.8	4.4.3	7	4	6	13	17	1: 1.31
30	481- 490	8	4.4.4.4 4.4.3	7.7.6	6.6.6.5	8.6.6.5.7	3.1.3.3 3.3.3.2	5.5.5.4	8	4.4.4.3	7	3	7			
"	491- 500	8	4.3	7.7.7.6	6.6.5	8.8.7	3.3.3.3.2	5.4	9.9.7.4 9.6.8	3	8.8.7	2	8	5	15	1: 3.00
July 1	501- 510	9.7.9.8	4.3	6	6.5	8.6.7	3.3.2	5.4	8	4.3	7	3	7			
"	511- 520	9.7.7.7 7.9.8	4.3	7.6	6.4.5	7	3.2	4	8	4.4.4.3	8.6.5.6.7	3	7	6	14	1: 2.33
2	521- 530	9.8	3	7.5.7.6	6.4.5	8.7	2	4	8	3	7	6	4			
"	531- 540	9.9.7.8	3	7.4.6	5	6.6.7	3.3.2	3.4	7.3.5.4.8	4.3	8.8.6.5 4.5.7	2	8	8	12	1: 1.50

3	511- 550	8	1.3	6	6.6.6.6 6.6.5	6.8.7	3.3.3.2	5.5.4	9.6.9.6.8	4.3	7	3	7			
“	551- 560	9.9.7.8	4.3	6	5	8.6.5.7	3.3.3.3 3.2	5.4	7.6.5.7 9.7.9.8	3	7	4	6	7	13	1: 1.86
5	561- 570	8	4.3	7.6	6.5	8.8.8.8 8.8.7	3.3.3.3 3.3.3.2	5.5.5.5.4	8	4.4.4.3	8.8.8.8.7	2	8			
“	571- 580	9.8	4.1.1.3	6	6.5	8.8.7	3.3.3.3.3 3.3.3.2	5.5.5.5.5 5.5.5.4	7.6.8	4.4.3	7	2	8	4	16	1: 4.00
6	581- 590	9.8	4.3	7.7.6	6.6.5	7	2	5.4	7.8	4.3	7	3	7			
“	591- 600	8	3	6	4.5	6.6.6.7	2	5.3.5.4	8	4.4.3	8.8.8.7	5	5	8	12	1: 1.50
7	601- 610	8	1.3	6	5	6.5.7	2	5.4	5.6.6.6.7.8	4.3	7	5	5			
“	611- 620	9.7.7.7.9 7.7.7.8	3	6	5	6.5.6.5.7	3.2	5.4	7.9.7.9.8	4.4.4.4 4.4.4.3	8.7	3	7	8	12	1: 1.50
8	621- 630	9.9.9.7.9 9.9.8	4.3	6	6.6.6.5	8.8.8.8 5.6.5.7	3.2	5.4	7.6.8	3	7	3	7			
“	631- 640	9.8	3	6	6.5	7	3.2	4	7.6.5.7 6.5.1.6 9.7.9.8	3	7	6	4			
“	641- 650	8	3	6	5	7	2	4	7.9.6.7.9.8	3	8.6.8.7	8	2	17	13	1: 0.76
9	651- 660	8	3	6	5	7	2	1	7.6.7.6.8	3	8.6.8.8.4.7	8	2			
“	661- 670	9.9.8	3	7.6	5	7	3.2	5.4	7.6.7.6.8	4.3	5.6.8	3	7	11	9	1: 0.82
10	671- 680	9.8	3	5.4.5.6	5	6.4.8.6.7	3.2	3.5.3.2.3 5.3.5.2.4	7.6.5.6 5.8	3	5.4.3.7	3	7			
“	681- 690	8	4.3	6	5	7	2	5.4	8	3	6.8.7	7	3	10	10	1: 1.00
12	691- 700	7.8	3	5.6	5	7	3.2	3.1	7.7.7.7.8	4.2.2.3	8.7	3	7			
“	701- 710	9.8	4.3	7.6	6.5	7	3.2	5.4	7.6.5.4.3 2.1.4.8	3	8.7	2	8			
“	711- 720	8	4.3	6	6.5	7	3.2	5.1	7.6.5.4.3 2.5.9.8	3	6.5.4.3.6 5.4.3.8.7	4	6	9	21	1: 2.33
13	721- 730	7.8	4.3	6	4.3.6.5	4.6.5.4.7	2	4	6.5.1.3 2.1.8	3	5.4.3.6 8.7	4	6	4	6	1: 1.50
14	731- 740	8	3	5.5.6	5	5.6.5.5 8.8.7	3.2	4	4.7.5.4 7.7.4.6 5.6.8	3	4.6.7	5	5			
“	741- 750	8	3	5.6	5	6.5.6.6 6.8.4.7	2	4	5.4.5.5.4 5.6.4.4.4 7.6.8	3	4.4.4.5.5 5.4.8.8.4 5.5.6.4.7	6	4	11	9	1: 0.82
15	751- 760	7.7.9.7.8	3	6	5	6.6.7	2	4	6.6.8	3	6.6.6.8 5.6.7	6	4			

TABLE 9—*Continued*

Results for Orang utan in Problem 2

Date	No. of trials	S. 1 7.8.9	S. 2 1.2.3.4	S. 3 2.3.4.5.6.7	S. 4 1.2.3.4.5.6	S. 5 4.5.6.7.8	S. 6 1.2.3	S. 7 2.3.4.5	S. 8 1.2.3.4.5 6.7.8.9	S. 9 1.2.3.4	S. 10 3.4.5.6.7.8	R	W	R	W	Ratio of R to W
July 15	761- 770	8	3	5.6	5	6.6.6.6.7	2	3.4	5.4.7.8	3	5.4.6.6.5 5.3.5.8.5 5.5.7	5	5	11	9	1: 0.82
16	771- 780	7.7.7.8	3	6	4.5	5.7	2	3.4	4.6.7.6.9 7.7.5.5 6.6.6.8	3	5.6.8.5 3.7	4	6	7	13	1: 1.86
"	781- 790	7.8	3	4.5.4.3 2.6	4.6.4.4 1.4.3.5	4.4.6.8 5.7	2	3.4	6.6.7.2 3.4.4.4 9.9.8	3	6.7	3	7			
17	791- 800	7.8	2.3	6	4.4.4.3.6 2.4.2.5	4.8.4.6.7	2	4	5.7.6.6.1 7.4.4.8	3	5.7	4	6	6	14	1: 2.33
"	801- 810	7.8	2.4.4.4.2 4.2.4.1.4 2.4.3	5.7.6	5	5.7	2	3.5.5.4	6.8	2.4.4.2 4.2.4.3	6.8.7	2	8			
19	811- 820	7.9.7.8	2.4.2.4.3	5.7.6	3.6.4.6.5	7	2	3.5.4	6.8	4.4.3	6.8.7	2	8	8	22	1: 2.75
"	821- 830	7.8	2.4.4.3	6	4.6.6.6.5	6.8.7	2	3.5.5.5.4	6.8	2.4.3	6.5.7	2	8			
"	831- 840	7.9.8	2.4.4.2.4.3	7.6	5	7	2	4	7.6.4.6.8	2.4.3	6.8.7	4	6			
20	841- 850	7.8	3	5.7.6	4.6.5	6.8.7	2	3.5.4	3.5.8	2.4.3	6.8.6.8.7	2	8	9	21	1: 2.33
"	851- 860	8	2.4.3	6	3.6.5	5.7	2	2.5.4	4.7.8	2.4.3	4.4.6.7	3	7			
"	861- 870	7.8	2.4.3	6	5	7	2	2.4	6.8	2.4.3	4.7	4	6			
21	871- 880	7.9.8	2.4.3	5.7.6	4.6.4.6 3.2.4.5	4.6.7	2	3.5.3.5.4	5.7.8	2.4.3	5.7	1	9	5	25	1: 5.00
"	881- 890	7.8	2.4.2.3	3.5.7.6	3.4.6.4.6.5	4.6.5.6.7	2	4	6.8	3	4.6.5.7	3	7			
"	891- 900	7.8	2.4.3	5.7.6	4.5	5.7	3.3.3.2	4	4.6.5.7.6 4.2.4.8	2.4.3	5.7	1	9			
22	901- 910	7.8	2.4.3	5.7.6	4.6.5	6.8.7	2	2.5.4	5.6.8	2.3	6.7	1	9	2	18	1: 9.00
"	911- 920	7.8	2.3	5.6	4.5	5.7	2	3.4	4.6.8	2.3	5.6.7	1	9			
23	921- 930	7.8	2.3	3.2.4.4 5.6	5	4.6.5.6.7	2	3.2.4	5.4.6.8	2.3	4.5.4.3.5 6.5.6.7	2	8			

23	931- 940	7.9.7.8	3	5.4.6	5	5.7	2	4	6.7.8	3	6.8.7	5	5		
"	941- 950	7.8	3	5.6	4.6.5	7	2	3.5.4	5.6.8	2.3	6.7	3	7	10	20	1: 2.00
24	951- 960	7.8	2.3	2.7.7.4 5.7.4.6	4.6.5	5.7	2	4	4.6.8	3	6.8.7	3	7			
"	961- 970	7.9.7.8	3	6	5	6.8.6.8 6.7	2	5.5.3.4	7.8	3	7	6	4	9	11	1: 1.22
26	971- 980	7.9.7.8	3	6	4.2.6.4 6.4.6.4 6.1.5	7	2	4	6.8	3	6.8.7	6	4			
"	981- 990	7.9.7.8	3	6	5	7	2	4	7.6.8	3	6.8.7	7	3			
"	991-1000	7.8	3	6	5	6.8.5.7	2	4	6.8	4.2.3	6.5.7	5	5	18	12	1: 0.67
In trials 1001 to 1100 the right door was indicated by being raised before the choice was made.															
30	1101-1110	8	4.3	7.6	6.6.5	7	2	5.4	7.7.7.7.7 7.6.6.7.8	3	6.6.7	4	6	4	6	1: 1.50
31	1111-1120	7.7.8	3	7.6	5	6.7	3.3.2	4	7.7.6.7 7.6.6.7 6.6.8*	4.4.4.3	7	4	6			
"	1121-1130	8	4.3	6	6.5	8 7	3.2	5.4	7.8	3	7	4	6	8	12	1 :1.50
August 2	1131-1140	8	4.4.3	6	5	7	3.3.3.2	4	7.8	4.3	7	6	4			
"	1141-1150	7.8	4.4.3	7.6	6.5	7	3.2	5.4	8	4.3	7	3	7	9	11	1: 1.22
3	1151-1160	7.9.8	4.3	7.6	5	6.7	2	5.4	7.6.7.8	4.3	6.7	2	8			
"	1161-1170	8	4.3	6	5	7	2	5.4	7.8	4.3	7	6	4	8	12	1: 1.50
4	1171-1180	8	4.3	6	6.5	6.7	3.2	5.4	7.8	4.3	6.5.6.7	2	8			
"	1181-1190	8	3	6	5	7	3.3.2	4	7.8	3	7	8	2	10	10	1: 1.00
5	1191-1200	7.8	4.4.3	6	5	6.7	3.3.3.2	4	6.7.8	3	7	5	5			
"	1201-1210	8	4.3	6	6.5	7	3.2	5.4	7.8	3	7	5	5			
"	1211-1220	7.8	3	7.6.5	5	7	2	4	7.7.6.7.8	3	7	7	3	17	13	1: 0.76
6	1221-1230	8	3	6	5	6.5.6.6 5.6.5.7	2	4	5.6.5.7 9.7.8	3	6.8.6.7	7	3			
"	1231-1240	7.8	3	6	4.5	7	2	4	6.7.8	3	6.7	6	4	13	7	1: 0.54
7	1241-1250	7.8	3	6	5	7	2	4	8	3	8.7	8	2			
"	1251-1260	8	3	6	6.5	7	3.2	5.4	8	3	7	7	3			
"	1261-1270	8	3	6	5	7	3.2	5.3.4	8	3	7	8	2	23	7	1: 0.30
9	1271-1280	8	3	6	5	7	2	5.4	9.7.8	3	7	8	2			
"	1281-1290	8	3	6	5	7	3.2	4	7.8	3	7	8	2	16	4	1: 0.25
10	1291-1300	7.8	3	7.6	5	6.7	3.2	4	6.7.8	3	7	5	5			

TABLE 9—*Concluded*

Results for Orang utan in Problem 2

Date	No. of trials	S. 1 7.8.9	S. 2 1.2.**3**.4	S. 3 2.3.4.5.**6**.7	S. 4 1.2.3.4.**5**.6	S. 5 4.5.6.**7**.8	S. 6 1.**2**.3	S. 7 2.3.**4**.5	S. 8 1.2.3.4.5 6.7.**8**.9	S. 9 1.2.**3**.4	S. 10 3.4.5.6.**7**.8	R	W	R	W	Ratio of R to W
August 10	1301-1310	7.8	4.3	5.6	4.5	6.7	2	4	6.8	3	6.8.7	3	7	14	16	1: 1.14
"	1311-1320	7.8	3	6	5	5.7	2	4	5.7.8	3	4.6.8.7	6	4			
11	1321-1330	7.8	3	6	5	6.7	3.2	4	4.6.7.8	2.3	5.7	4	6	10	10	1: 1.00
"	1331-1340	7.8	3	6	4.5	6.7	2	4	6.7.8	3	7	6	4			
12	1341-1350	9.8	3	6	5	6.7	2	4	6.7.8	3	6.7	6	4	12	8	1: 0.67
"	1351-1360	7.8	3	6	5	6.7	2	4	7.8	3	6.7	6	4			
19	1361-1370	7.8	3	6	5	6.7	2	4	6.7.8	3	6.7	6	4	12	8	1: 0.67
"	1371-1380	9.8	3	6	5	7	3.2	4	7.9.8	3	6.8.7	6	4			

* Aided by experimenter.

wholly different method. Even over night a new method might develop. In the monkeys, although there was occasionally something comparable with this, it was by no means so evident.

After two hundred and fifty trials on problem 2 had been given Julius, it seemed desirable to introduce a radical change in method in order to stimulate him to maximal effort. It was therefore decided to force him to make a round trip through the apparatus in connection with each choice, and to let this forced labor serve, in the place of confinement, as punishment for mistakes. This new method yielded peculiar and characteristic results. They differ from those previously obtained largely because of the orang utan's remarkably strong tendency to reënter the box through which he had just passed. This occurred so persistently, as may be seen in table 9 (June 17, second series, June 18, etc.), that a further modification of method was introduced in that after the same wrong box had been entered five times in succession, the experimenter on the next choice of the box confined the animal for a stated interval, say sixty seconds, in it, and then allowed it to escape by way of the exit door and choose repeatedly until it finally located the right box. Were it not for this particular feature of the method, the number of choices recorded after June 17 would unquestionably be very much greater than the table indicates.

The new method proved a severe test of the orang utan's patience and perseverance, for he had to work much harder than formerly for his reward, and often became much fatigued before completing the regular series of ten trials. Early in the use of this method, he developed the habit of rolling around from exit door to starting point by a series of somersaults. When especially discouraged he would often bump his head against the floor so hard that I could hear the dull thud. As has been noted, I found it desirable to vary the procedure repeatedly. It proved especially interesting to give one series per day with the round trip as punishment and another series with confinement as punishment.

Day after day, as the experiment progressed, slight or great fluctuations of the ratios of right to wrong choices appeared, but without consistent improvement. There was, to be sure, as the last column of table 9 shows, a radical improvement during the first six hundred and fifty trials, for the number of right choices per series increased from 0 to 8. But, as the observations were continued from day to day, it became more and more evident that the animal was merely passing from tendency to tendency—method to method—mixing tendencies, and occasionally developing new ones, without approach to the solution of the problem. This fact would have led me to discontinue the work much earlier than I actually did had it not been for the peculiarity of the results obtained with problem 1. It seemed not improbable that at any time Julius might succeed in perfectly solving this problem over night precisely as he had solved the first problem.

A curiously interesting bit of behavior appeared for the first time on June 29. Julius had gone to the first box at the right end of the group, and instead of entering, he had wheeled around toward his right, and turning a complete circle, faced the right box, which he promptly entered. Subsequently, the tendency developed and the method was used with increasing frequency. On June 30, it appeared in the first series, four times, in the second series, six times; on July 1, in the first series, three times, and in the second series, four times; on July 2, in the first series, five times, and in the second series, nine times. It was indeed only by accident that the animal failed to fulfill the technical requirement for perfect solution of the problem in this series. Yet, had he done so, his subsequent trials would doubtless have revealed the lack of any other idea than that of turning completely around before entering a box.

This odd bit of behavior proved peculiarly interesting and significant in that the tendency to turn became dissociated from the position (in front of the first box at the right end of the group) in connection with which it originally developed. After a few days, Julius would enter the reaction-chamber and instead of proceeding directly to the right end of the group, would stop suddenly wherever he happened to be, turn toward his right in a complete circle, and hasten into the box nearest to him which, as often as not, proved to be the wrong one. Thus the idea of turning completely about, which had it continued its association with the idea of facing the first box at the right, would have yielded success, instead became useless because of its dissociation.

That the orang utan is capable of using free ideas seems clear enough in the light of this behavior. That he proved incapable of getting the idea of second from the right end is as clearly shown by the detailed results of table 9,—the fruits of weeks of experimenting.

Certain other interesting tricks developed in Julius's behavior. Thus, on July 5, there appeared the tendency to move as though about to enter the right box (feint), then to stop suddenly and promptly enter another box, which was, of course, a wrong one. The reason for the development of this tendency could not be discovered, but in connection with it, there appeared another tendency which possibly can be explained. Julius took to backing into the chosen box so that he could face the experimenter. He would then, after a period of hesitation, come out and promptly enter one of the other boxes. This tendency was apparently due to the fact that during one or two series the experimenter growled at the orang utan every time he made a mistake. The growl startled him and caused him to look around. He evidently felt the need of keeping his eyes on the experimenter,—hence the backing into the open box. The tendency disappeared shortly after the experimenter gave up the use of the growl as a method of punishing the animal for what were suspected to be careless choices.

Curiously enough, it was not until July 10 that direct choice of the right box was made at all frequently. Previously, selection of it had been made almost invariably after approach to other boxes. But in the second series for July 10 there was an extraordinary improvement in method. This developed in the presence of two visitors, and it is therefore all the more surprising. The choices were made not only directly, but with decision and evident certainty that was quite at variance with the previous behavior of the animal.

All the while through variation of methods, I was seeking to discover the best means of holding the orang utan to his maximum effort and care in attempting to select the right box. One day it would seem as though forcing him to make round trips with rewards only for correct first choices proved most satisfactory, and the next it might seem equally clear that punishment by confinement for thirty seconds or sixty seconds, with reward for correct choice in every trial, yielded better results. In the end I had to admit that no best method had been demonstrated and that I had failed to develop conditions which served to compel the animal's attention to the problem and to lead him to work without discouragement. There were, it is true, days on which it seemed practically certain that the problem would be solved, but as it turned out, Julius never succeeded in choosing correctly throughout a series of ten trials.

As a last resort, in order to make perfectly sure that the orang utan was doing his best, I decided to introduce corporal punishment in a mild form. For this purpose, I placed my assistant in charge of the apparatus and the series of trials, and stationed myself in one corner of the reaction-chamber with a whip in my hand. Whenever Julius entered a wrong box, I approached him with the whip and struck at him, being careful not to injure him and rarely striking him at all, for the threat was more effective than a blow. He was extremely afraid of the whip and would begin to whine and attempt to get out of the way as soon as he saw it.

This method was introduced on August 10, but no improvement resulted, and in the end there was no reason to consider it more satisfactory than the other procedures. I am now wholly convinced that Julius did his best to choose correctly in the majority of the numerous series which were given him in connection with problem 2.

From trials 1001 to 1100, a radical departure from the previous methods was introduced in that the right box was indicated to the animal by the slight and momentary raising of its exit door. Of course no records of the choices for this group of one hundred trials appear in table 9, for the simple reason that the animal inevitably and immediately entered the right box. It was thought that this method might serve to break up the previously developed tendencies toward inadequate forms of response and so encourage the animal that he would later solve the problem when given opportunity to select the right box without aid from the experimenter. But as a matter of fact, while the ratio of right to wrong first choices was 1 to .67 in the series preceding this change of method, it was 1 to 1.50 in the first series following its use. There is no satisfactory evidence that Julius profited by this experience, though as a matter of fact he did succeed in making his best daily record, eight right to two wrong choices, on August 4, after 1190 trials.

The curve of learning for this problem has been plotted and is presented in figure 19. It is of course incomplete and it is offered only to indicate the extreme irregularity in performance.

Problem 1*a*. *First at the Right End*

It was decided on August 19 that the further continuation of the work of Julius on problem 2 was not worth while. He had become much discouraged, and although willing to work for food, gave no indications whatever of improvement and seemed to have exhausted his methods. It seemed wise instead of giving up work with him in the multiple-choice method to return to a form of problem 1. We may designate it as problem 1a. The right box is definable as the first at the right end of the series instead of the first at the left end as in the original problem 1. It was thought possible that Julius might quickly solve this problem by a process similar to that used for problem 1.

Work was begun on problem 1a, August 20, and for six successive days two series of trials per day were given, the settings for which as well as the resulting choices are given in table 10. Most notable in these results is the large number of cases in which Julius chose first the second box from the right end of the series, or in other words that box which had been the right one in problem 2. Contrary to expectation, he showed no inclination to abandon this tendency to choose the second from the right end, and the ratio of right to wrong choices changed in the direction opposite from expectation, beginning with 1 to 4 and ending on the sixth day with 0 to 20.

It was obviously useless to continue the experiment further since Julius had given up his attempts to locate the right box in the first choice and was apparently satisfied to discover it by a process of trial and error. He had, it would seem, satisfied himself that the problem was insoluble. These results obtained in problem 1a constitute a most interesting comment on the effects of problem 2 on the orang utan. Behavior similar to that which he developed well might have been obtained from a child of three to four years placed in a like situation and forced to strive, day after day, to solve a problem beyond its ideational capacity.

In many respects the most interesting and to the experimenter the most surprising result of this long series of observations with Julius was the lack of consistent improvement. It seemed

TABLE 10

Results for Orang utan in Problem 1a

Date	No. of trials	S. 1 5.6	S. 2 1.2.3.4	S. 3 6.7.8.9	S. 4 2.3.4.5	S. 5 3.4.5.6.7	S. 6 1.2.3	S. 7 5.6.7.8	S. 8 1.2	S. 9 2.3.4.5.6	S. 10 1.2.3.4 5.6.7	R	W	R	W	Ratio of R to W
August 20	1- 10	6	3.4	6.7.8.9	4.5	6.7	3	7.8	2	5.5.6	6.7	3	7			
"	11- 20	5.6	3.4	7.8.7.8 8.7.8.9	4.5	6.7	2.3	7.6.7.7 6.7.7.8	2	5.6	6.7	1	9	4	16	1: 4.00
21	21- 30	5.6	3.4	7.8.7.6 8.7.9	4.5	6.7	2.3	7.8	2	5.6	5.7	1	9			
"	31- 40	5.6	3.4	7.7.6.8.9	4.5	6.7	3	6.7.8	2	6	6.7	3	7	4	16	1: 4.00
23	41- 50	5.6	3.4	7.8.9	4.5	6.7	2.3	6.7.8	2	5.6	5.6.7	1	9			
"	51- 60	5.6	3.4	7.8.9	4.5	6.7	2.3	6.8	2	5.6	6.7	1	9	2	18	1: 9.00
24	61- 70	5.6	3.4	6.8.9	4.5	5.7	2.3	6.7.8	1.2	5.6	6.7	0	10			
"	71- 80	5.6	3.4	6.7.8.9	4.5	5.7	2.3	5.7.8	2	5.6	6.7	1	9	1	19	1:19.00
25	81- 90	5.6	3.4	6 7.8.9	5	5.6.7	2.3	7.8	1.2	5.6	4.5.6.7	1	9			
"	91-100	5.6	3.4	6.7.8.9	3.4 5	6.6.7	2.3	6.7.8	1.2	5.6	6.7	0	10	1	19	1:19.00
26	101-110	5.6	3.4	6.7.8.8 6.7.6.9	3.5	5.6.7	2.3	5.6.7.6.7.8	1.2	5.6	6.7	0	10			
"	111-120	5.6	2.3.4	6.7.8.8 6.7.8.7 6.7.9*	3.4.5	5.6.7	2.3	7.8	1.2	5.6	4.5.6.7	0	10	0	20	0:20.00

* Aided by experimenter.

almost incredible that he should continue, day after day, to make incorrect choices in a particular setting while choosing correctly in some other setting which from the standpoint of the experimenter was not more difficult.

The evidence suggests that in this young orang utan ideational learning tended to replace the simpler mode of problem solution by trial and error. Seemingly incapable of solving his problems by the lower grade process, he strove persistently, and often vainly, to gain insight. He used ideasin effectively. Animals far lower in intelligence (e. g., the pig), surpass him in ability to solve these relational problems because they use the method of elimination by trial consistently and effectively. Julius, in these experiments, made a poor showing because his substitute for trial and error is only slightly developed. Would he have succeeded better with the same problems if mentally mature?

There are many important features of the results which, for lack of space, have not been indicated or discussed. They can be developed from later comparative studies of the data, for in the tables appear all of the essential facts of response apart from those mentioned in the text.

IV

RESULTS OF SUPPLEMENTARY TESTS OF IDEATIONAL BEHAVIOR

1. Julius, *Pongo pygmæus*

Box Stacking Experiment

In addition to the multiple-choice experiments which have been described in detail in the previous section, it was possible to conduct certain less systematic tests of ideational behavior in the monkeys and the orang utan. From the technical standpoint these tests were relatively unsatisfactory because only inexactly describable. But their results are in many respects more interesting, if not also more important, in the light which they throw on ideation than are those previously presented.

First, in order of time, comes a test which may be designated as the box stacking experiment. The method will now be described in connection with an account of the behavior of Julius as contrasted with that of a child of three years and four months of age.

In the large central cage labelled Z, figure 12, which was twenty-four feet long, ten feet wide, and ten to twelve feet deep, the following situation was arranged. From the center of the wire covering of the cage, a banana was suspended on a string so that it was approximately six feet from the floor, five feet from either side of the cage, and twelve feet from either end. From all approaches it was far beyond the reach of Julius, since it was impossible for him to climb along the wire roof and thus reach the string. Two boxes were placed on the floor of the cage several feet from the point directly under the banana. The one of these boxes was heavy and irregular in shape, as is shown in figures 21, 23 and 24 of plate V. Its greatest height was twenty-one inches; its least height, eighteen inches; its other dimensions, twelve and sixteen inches respectively. The smaller and lighter box measured twenty-two by twelve by ten inches. According to the experimenter's calculations, the only way in which Julius could obtain the banana was by placing the smaller box upon the larger and then climbing upon them.

At 10 a. m. on March 5, Julius was admitted to the large cage, and the banana was pointed out to him by the experimenter. He immediately set about trying to get it, and worked diligently during the whole of the period of observation, which, because of the unfinished condition of some of the cages, was limited to slightly over ten minutes. Within this period he made upward of a dozen fairly well directed attempts to obtain the food. Chief among them were three attempts to reach the banana from different positions on the left wall of the cage (as the experimenter faced the laboratory); two attempts to reach it from different positions on the right wall; two from the large box in positions nearly under the banana; two from the large box with the aid of the experimenter's hand; and one from the distant end of the cage(?). There occurred, also, less definite and easily describable efforts to get at the reward.

On account of the unfinished condition of the cages, the experimenter had to remain in the large cage with Julius during the test. This interfered with the experiment because the animal tended both to try to escape and to get the experimenter to help him with his task. Particularly interesting is the latter sort of behavior. After the orang utan had made two or three futile attempts to obtain the food he came to the experimenter, who was standing in one corner of the cage, took him by the hand, and led him to a point directly under the banana. He then looked up toward the banana, grasped the experimenter's arm, raised it, and then tried to pull himself up. He was not allowed to get the food by climbing up on the experimenter. A few minutes later, he again led the experimenter toward the banana, but receiving discouragement in this activity, he proceeded to devote himself to other methods.

Apart from the distractions which have been mentioned above, Julius's attention to the food was surprisingly constant. Whatever his position with respect to it, he seemed not for an instant to lose his motive, and to whatever part of the cage he went and whatever he did during the interval of observation was evidently guided by the strong desire to obtain the banana. Frequently he would look directly at it for a few seconds and

EXPLANATION OF PLATE V

Orang utan, Julius, obtaining banana by piling boxes or by using pole

FIGURE 21.—Julius in act of setting larger box on end.

FIGURE 22.—Placing smaller box on larger.

FIGURE 23.—Balancing on larger box preparatory to reaching for banana.

FIGURE 24.—Balancing and reaching to the utmost.

FIGURE 25.—Standing on three boxes (after stacking them) and reaching for reward.

FIGURE 26.—Lifting smaller box up toward banana.

FIGURE 27.—The act of stacking the boxes.

FIGURE 28.—Sequel to figure 27.

FIGURE 29.—Box and pole experiment. Pushing the second pole into the box.

FIGURE 30.—Pushing pole into box.

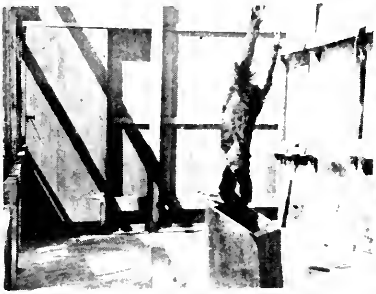
FIGURE 31.—Enjoying the reward of success.

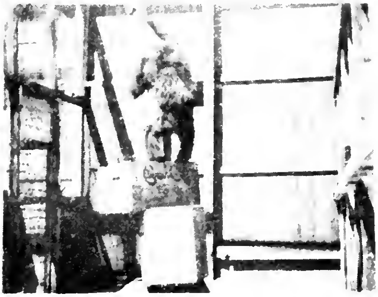
PLATE V


21


22


23


24


25


26


27


28


29


30


31

then try some new method of reaching it. His gaze was deliberate and in the handling of the boxes he accurately gauged distances. Several times he succeeded in placing the larger box almost directly under the banana, and repeatedly he located that portion of the side wall from which he could most nearly reach the coveted prize.

From my notes I quote the following comment on the results of the initial experiment: "Despite all that has been written concerning the intelligent behavior of the orang utan, I was amazed by Julius's behavior this morning, for it was far more deliberate and apparently reflective as well as more persistently directed toward the goal than I had anticipated. I had looked for sporadic attempts to obtain the banana, with speedy discouragement and such fluctuations of attention as would be exhibited by a child of two to four years. But in less than ten minutes Julius made at least ten obvious and well directed attempts to reach the food. There were also wanderings, efforts to obtain aid from the experimenter, and varied attempts to escape from the cage."

Before proceeding further with the description of the behavior of Julius in the box stacking test, I shall describe for contrast the behavior of a boy three years four months of age when confronted with a situation practically identical with that which the ape was given an opportunity to meet. For the child, the banana was suspended, as previously described, from the roof of the cage. The same two boxes were placed on the floor at considerable distances from the banana, and in addition, a light stick, about six feet long, and a piece of board, the latter by accident, were on the floor. The child was asked to get the banana for Julius, and he eagerly and confidently volunteered to do so.

His behavior may best be described by enumeration of the several attempts made. They include (1) placing the larger box nearly under the banana and reaching from it. (2) Standing of the larger box on end with resulting failure because the child could not stand on the sloping edges of the top of the box. (3) The larger box was turned on its side and the lighter box drawn up opposite it and stood on end. The child then mounted the larger box and from it stepped to the top of the smaller. But the boxes had not been placed beneath the banana, and when the child reached for it, he found himself several feet away from his prize. (4) The boxes were moved to a position nearly under the banana and another futile attempt was made to reach it without placing the smaller box on top of the larger one, the only position from which the child could readily obtain it. (5) The piece of board was placed on top of the larger box and from this height the child again reached upward. (6) The six-foot stick was taken up and an attempt was made to strike the banana and thus dislodge it, but it was too securely fastened to be obtained thus. (7) Attention shifted to other things, and the child played for a time with the board. Reminded of the banana by the experimenter, he again tried method (3). (8) He again used the stick on the banana. (9) The effort to knock the prize to the floor having failed, he became discouraged and said that he must go home. (10) When told that Julius was very hungry and wanted the banana, he repeated efforts similar to those described in (3) and (6).

Up to this time the observations had covered a period of twenty minutes. The child was now taken from the cage and allowed to play about for fifteen minutes. Asked then whether he would go back and try to get the banana, he replied, "No, 'cause I don't want to get it," thus indicating his discouragement with the situation. When taken into the cage, he, nevertheless, made the additional attempts indicated below: (11) Use of one of the boxes. (12) He remarked, "Now I know, I'll get it," and after so saying, repeated (3). (13) Failing, he turned to me and said, "I could get it if I was on your head," but he did not, as Julius had done, lead me to the proper place and try to reach the banana by climbing up or by urging me to lift him. (14) Later, he played in the boxes, apparently forgetful of his task. Finally he remarked: "I'll get the banana," but he made no attempt to do so, and instead, watched the monkeys intently. Thereafter, he showed no further interest in the solution of the problem, and the experiment, after a total period of fifty-five minutes, was discontinued.

Comparison of the behavior of the ape with that of the child indicates a greater variety of ideas for the latter. Julius gauged his distances much more accurately than the child, attended more steadily, and worked more persistently to obtain the reward, but he did not so nearly approach the idea of stacking the boxes as did the child, for the latter, in placing the board on one of the boxes, exhibited in ineffective form the idea which should have yielded the solution of the problem.

The child was given no further opportunity to work at the problem, whereas Julius, as I shall now describe, continued his efforts on subsequent days under somewhat different conditions.

On Wednesday, March 10, the banana was suspended as formerly, and three boxes, all of them small and light enough to be readily handled by the ape, were placed in distant parts of the cage. The six-foot stick which had been present in the test with the child, but not in the first test with Julius, was also placed in the cage.

Julius was allowed to work for about an hour. As formerly, he was sufficiently hungry to be eager to get the food and evidently tried all of the possible ways which occurred to him. Chief among these were (1) the use of the various boxes separately or in pairs in very varied positions but never with one upon another,—the only way in which the banana could be reached; (2) climbing to various points on the sides of the cage, with infrequent attempts to reach the banana. Usually his eyes saved him the vain effort.

Unlike the child, Julius paid little attention to the six-foot stick. Two or three times he took it up and seemingly reached for the banana, but in no case did he try persistently to strike it and knock it from the string. It is but fair, however, to remark that such an act is very difficult for the young orang utan, as compared with the child, because of the weakness of the legs and the awkwardness of striking from a sitting posture.

As previously, the steadiness of attention and the persistence of effort toward the end in view were most surprising. At one time Julius walked to the end of the cage and there happened to see one of the monkeys eating. He watched intently a few seconds and then hastened back to the banana as if his task had been suggested to him by the sight of the feeding animal.

Most interesting and significant in this behavior was the suddenness with which he would turn to a new method. It often looked precisely as though a new idea had come to him, and he was all eagerness to try it out.

On March 11, Julius was given another opportunity to obtain the banana by the use of the three boxes. Although he used them together he made no effort to place one upon another. Certain of his methods are shown in plate V, especially by figures 21, 23 and 24.

This experiment was continued on April 2 under yet different conditions, for this time only two boxes were placed in the cage, the one of them the heavy, irregularly shaped box and the other the smaller, lighter one originally used. On the end of the heavier box had been nailed a two by two inch wooden block in order to increase the difficulty in using this box alone. As previously, Julius made varied attempts to obtain the banana, but on the whole his interest and attention seemed somewhat weaker than previously and there were indications of discouragement because of repeated failures.

He handled the boxes conspicuously well, and it seemed at times that he would certainly succeed in placing the one upon the other and in reaching the food.

After one series of attempts from the sides of the cage and from the large box, he deliberately turned away from the box and neatly executed a somersault on the floor of the cage, as much as to say, "I am disgusted with the whole situation." Again, later on the same day, after falling from the top of the larger box, which tilted over very easily, he rolled himself into a ball, and childlike, played with his feet. An additional evidence of his changed affective attitude toward his task, especially in connection with definite failures, appeared in his rough handling and biting of the boxes. When most impatient, he worked very roughly.

Julius was allowed to work for the reward from thirty to ninety minutes, or, as a rule, until he had become completely discouraged on April 3, 5, 6, 7, 8, 9 and 13. His behavior was interesting and significant, but nothing new appeared except that his willingness to work gradually disappeared, and on April 13, although previously hungry, he made only a single attempt to obtain the banana and then paid no further attention to it.

The prolonged and varied efforts to obtain the banana were due in a measure at least to three accidental successes. Thus on April 2, 3 and again on the 5th, by fortunate combinations of circumstances, he succeeded in getting the banana, contrary to the intention of the experimenter.

Although active at first on April 6, he soon wearied of his task and quit work. The same was true on April 7, and again on the 8th and 9th. On these days, although hungry, he did not care to enter the large cage and worked only a few minutes each day, seldom making more than two or three half-hearted attempts to obtain the banana. His attitude toward the task had changed completely, in that hopelessness had taken the place of eager expectancy. By the 13th of April he had so nearly given up voluntary efforts to solve the problem that it seemed worth while to test his ability to get the idea by watching the experimenter. For this purpose the following test of imitation was made.

On the morning of April 14, having placed a banana in the usual position, I took Julius into the large cage, dragged the two boxes to the proper position beneath the banana, placed the smaller one upon the larger one and then climbed up on them to show the ape that I could reach the banana. I then stepped down and gave him a chance to climb on the boxes. He did so immediately and obtained the food.

Another piece of banana was supplied, the boxes were placed in distant corners of the cage, and fifteen minutes were allowed Julius so to place them that he could obtain his reward. He gave no indications of having profited by my demonstration, but worked with the boxes singly, usually with the larger one.

On April 16, with the banana in position and the two boxes also in the cage, Julius was admitted and allowed to work for five minutes, but again without success. I then placed the boxes properly for him and he immediately climbed up and got the banana. While he was eating, the boxes were carried to distant corners of the cage and another banana placed in position. Now thirty minutes were allowed him for unaided work on the problem. As formerly, the larger box was used repeatedly and attempts to reach from the side of the cage appeared, but there was no tendency to try to use the two boxes together. He worked fairly persistently, however, and showed clearly the stimulating and encouraging effect of aid from the experimenter.

Once more, on April 17, Julius was taken into the cage and allowed to watch me place the boxes in proper position. He then climbed up and obtained the desired food. After the bait had been renewed and the boxes displaced, he immediately tried to use the larger one, then he reached for the small one as though to use both together. But the impulse died out and he turned again to the larger box as usual, standing it on end, and persistently trying to balance himself on it. Nothing else of special interest happened during the interval of unaided effort.

Similarly, I placed the boxes for the ape on April 19, allowed him to get the banana and then gave him opportunity to try for himself after the boxes had been displaced. This time he immediately reached for the smaller box and moved it about a little, thus indicating a new association. He next turned to the larger box and worked with it persistently. Later, he once more worked with the smaller box in an unusual manner. He repeatedly stood on it, but made no attempt to lift it or to place it on the larger box. Clearly the usually neglected smaller box had become associated with the satisfaction of obtaining the banana. The same method was carried out on April 20. As I placed the boxes in position beneath the banana, Julius watched with unusual intentness, and when it came his turn to try to obtain the food by the use of the boxes, he began at once to work with the smaller box, but as on April 19, he soon abandoned it and turned to the other. While I was making note of this particular feature of his behavior, he suddenly seized the smaller box by two corners with his hands and by one edge with his teeth, and after a few attempts placed it on top of the larger box, climbed up, and obtained the banana.

Because of bad weather on April 21, the next test was made on April 22, with everything as usual. Unaided, the ape was given an opportunity to obtain the coveted reward, while I stood ready to obtain records of his behavior with my camera. He wasted no time, but piled the smaller box on top of the larger one immediately, and obtained his reward. As soon as opportunity was offered, he repeated the performance. The same thing happened on April 23 and several succeeding dates.

Julius had got the idea, and the only further improvement possible was in skill in manipulating the boxes.

One of the curious performances which appeared during the imitative period is pictured in figure 26, plate V, where the ape is seen lifting the smaller box into the air. This he did three or four times one day, raising it toward the banana each time as though he expected thus to obtain the reward. As he did not go up with the box (according to his expectation?), he abandoned this method, and looking about, discovered the larger box in a distant corner. Thereupon, he promptly pulled the boxes to their proper position beneath the banana, stacked them, and obtained his food.

After considerable skill had been acquired in the placing of the boxes, the one upon the other, the height of the banana above the floor was increased so that three boxes were necessary. Figure 25 of plate V shows him standing on three boxes and reaching upward, and figures 22, 27 and 28 show various modes of handling the boxes and of reaching from them. He was not at all particular as to the stability of his perch, and often mounted the boxes when it seemed to the experimenter inevitable that they should topple over and precipitate him to the floor. Only once, however, during the several days of experimentation did he thus fall.

Obviously important is the evident change in the animal's attention on April 20. He watched with a keenness of interest which betokened a dawning idea. Before he had succeeded in stacking the boxes, I had written in my note-book, " He seemed much interested today, in my placing of the boxes." Interesting, and important also, is the ease and efficiency with which he met the situation time after time, after this first success. " Trial and error " had no obvious part in the development of the really essential features of the behavior. The ape had the idea and upon it depended for guidance.

Except for the fact that Julius was immature, probably under five years of age, it is likely that he would have stacked the boxes spontaneously instead of by suggestion from the experimenter or imitatively.

No unprejudiced psychologist would be likely to interpret the activities of the orang utan in the box-stacking experiment as other than imaginal or ideational. He went directly, and in the most business-like way from point to point, from method to method, trying in turn and more or less persistently or repeatedly, almost all of the possible ways of obtaining the coveted food. The fact that he did not happen upon the only certain road to success is surprising indeed in view of the many ineffective methods which he used. It seemed almost as though he avoided the easy method.

It is especially important, in connection with these results, to point out the risk of misinterpretation of observations on the anthropoid apes. If they can imitate human activities as readily and effectively as Julius did in this particular experiment, we can never be sure of the spontaneity of their ideational behavior unless we definitely know that they have had no opportunity to see human beings perform similar acts.

Of all the methods of eliciting ideational or allied forms of behavior used in my study of the monkeys and ape, none yielded such illuminating results as the box stacking test, and although from the technical standpoint, it has many shortcomings, as a means to qualitative results it has proved invaluable.

Other Methods of Obtaining the Reward

Some weeks later, I tried to discover how Julius would obtain the much desired banana when the boxes were absent. I placed in the large cage a stick about six feet long and an old broom. When admitted, he looked about for the boxes, but not seeing them, picked up the broom and placing it with the splints down, beneath the banana, he tried to climb it, but as it fell over with him, he abandoned this after a few trials, went to his cage, and picking up some old bags which he used at night as covers, he dragged them out and placed them on the floor beneath the banana. He next put the broom upon them and tried to climb up. This general type of behavior persisted for several minutes, everything within reach being used as were the bags, as a means of raising him in the desired direction. Finally, he placed his feet on the broom where the handle joins the splints, seized the handle near the top with his hands, drew himself up as far as possible, and then launched himself in the air and tried to seize the banana. On the third attempt he succeeded.

Later, he was given a plain stick about five feet long. Figure 32 of plate VI shows him using this to obtain the banana in the manner described above. He would grasp it with one or both feet, usually one, ten to fifteen inches from the floor of the cage, meanwhile holding with his hands near the top of the stick. He would then, with all his strength, draw himself up suddenly and jump toward the banana. Often he came down rather hard on the cement floor, much to his disgust.

Yet another method of obtaining the reward developed a day or two later. A light red-wood stick about five feet long and an inch in its other dimensions was the only object in the cage which could possibly be of use in obtaining the banana. The aim of the experimenter was to discover whether Julius would use this as a club.

Previously, in connection with the use of the boxes, he had taken up the same stick two or three times and reached for the banana with it, but in no case had he struck at it or clearly tried to knock it from the string, as did the child most readily and naturally. When provided with this same stick, and it alone, as a means of obtaining the food, he hit upon the following interesting method. Placing one end of the stick between a wooden brace and the wire side of the cage, he climbed up to a level with the banana as is shown in figure 33 of plate VI. Then holding with one hand and one foot to a timber of the cage and to the stick with his other foot, he swung outward as far as possible and reached the banana with his free hand. Having once succeeded by the method, he used it whenever given an opportunity. It was impossible for him to make the reach without the use of the small stick, while with it he succeeded fairly easily and regularly.

Box and Pole Experiment

Following the box stacking test, Julius was given an opportunity to exhibit ideation in another type of experiment. This may be designated the box and pole test. The conditions are describable thus. A strong wooden box eighty-four inches long, by four inches wide, by four inches deep, with open ends, was built with one side hinged. Hasps and padlocks enabled the experimenter to lock this "lid" after food had been placed in the center of the box. This box could be placed in the center of the large cage and there fastened by means of cross bars. It is well shown in position in figure 29, plate V. Two poles each eight feet long and approximately one and a half inches in their other dimensions were the only additional materials in the experiment.

On May 1, Julius was allowed to see the experimenter place a half banana in this box, close the lid, lock it in position, and securely fasten the box by means of the cross bars. He was then given opportunity to try to get the banana. The two poles lay on opposite sides of the box and near the edges of the cage. Doctor Hamilton and the writer were in the cage watching. Julius looked into the box through one end, and seeing the banana, reached for it. He could not obtain it in this way, so he began to bite at the box and to pull at it with all his strength. During the fifteen minutes allowed him, he worked at the box in a great variety of ways, fooling with the locks which had been attached to the hasps as well as with the cross bars and continually reaching in at the one or the other end. He was somewhat distracted by the presence of the two observers and attended rather unsatisfactorily to the task in hand. Not once did he touch the poles, and it is doubtful whether he even noticed them. He was not very hungry at this time, and after a few minutes active work he virtually gave up trying to get the food.

Two days later, on May 3, the box was once more placed in position, this time with a half banana in the middle and a small piece of banana near each open end. The two poles lay on the floor of the cage, each several feet distant from the box. Julius was eager for food. When released he went immediately to the box, reached in and obtained a piece of banana from the end nearer the laboratory. He then looked in and saw the piece near the middle of the box. His next move was to pick up the eight foot pole and push it into the box, but before pushing it all the way through, he stopped and began to pull at the box in various ways. Shortly he returned to the pole and twice thrust it in as far as he could reach. The first time, after thrusting it all the way through, he pulled it out and examined the end as though expecting the banana to come out with it. After a third attempt he looked into the box, presumably seeing the banana, then turned a backward somersault, came to the end of the cage, and looked at me. Had it been at all possible, he would have taken me by the hand and led me to the box as a helper. After a few seconds, he returned to the pole, pried the lid of the box with it, then gnawed at the pole. For about five minutes he worked fairly rapidly and steadily, using the poles, pulling, gnawing, and walking about.

His next move was to go to the opposite end of the box, look in, take the piece of banana which was near the opening, then pick up the second pole, which had not previously been noticed, and after a number of attempts, push it into and through

PLATE VI


32


33

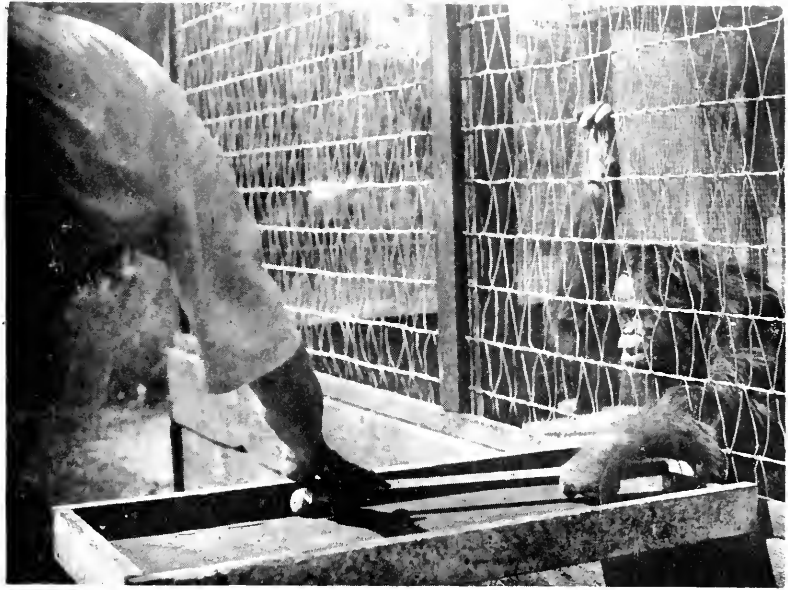

34

FIGURE 32.—Julius obtaining banana by using pole to climb up on and spring from.
FIGURE 33.—Using pole to swing out on so that banana could be grasped.
FIGURE 34.—Using stick to draw carrot within reach.

the box, looking after it and then pulling it out and looking into the box. Having done this he again came to my end of the cage, and from there returned to try once more with the pole which he had first used. He pushed this pole all the way through, then walked to the other end of the box, looked in and reaching in, obtained the banana which had been pushed far enough along to be within his grasp. Figures 29, 30 and 31 of plate V show stages of this process.

Julius had worked twenty-four minutes with relatively little lost time before succeeding. He had shown almost from the start the idea of using the pole as an instrument, and his sole difficulty was in making the pole serve the desired purpose.

The experiment was rendered still more crucial on May 5 by the placing of the two poles upright in opposite corners of the large cage. For a few minutes after he entered the cage, Julius did not see them, and his time was spent pulling and gnawing at the box. Then he discovered one of the poles, seized it, and pushed it into the box. He tried four times, then went and got the other pole and pushed it into the opposite end of the box. Twice he did this, then he returned to the original pole, bringing the second one with him. He pushed it in beside the first, and as it happened, shoved the banana out of the opposite end of the box. But he did not see this, and only after several seconds when he happened to walk to that end of the box did he discover the banana. The total time until success was fifteen minutes.

Subsequently the ape became very expert in using the pole to obtain the banana, and often only a minute or two sufficed for success. It was not possible for him to direct the stick very accurately, for when he was in such a position that he could look through the box, he could not work the stick itself. It was, therefore, always a matter of chance whether he obtained the banana immediately or only after a number of trials.

Although it is possible that the use of the poles in this experiment was due to observation of human activities, it seems probable in the light of what we know of the natural behavior of the anthropoid apes that Julius would have solved this problem independently of human influence. It was the expectation of the experimenter that the pole would be used to push the banana through the box, but as a matter of fact the ape used it, first of all, to pull the food toward him, thus indicating a natural tendency which is important in connection with the statements just made. Subsequently he learned that the banana must be pushed through and obtained at the farther end of the box. I am not prepared to accept the solution of this problem as satisfactory evidence of ideation, but I do know that few observers could have watched the behavior of the orang utan without being convinced that he was acting ideationally.

Draw-in Experiment

An interesting contrast with the box and pole test is furnished by what may be called the draw-in experiment. This was planned as a simple test of Julius's ability to use a stick to draw things into his cage from beyond the wire side. A board was placed, as is shown in figure 34 of plate VI, with sides to hold a banana, carrot, or some other bit of food, in position. In the actual test either a carrot or a banana was placed about two feet from the wire netting and a stick two feet long was then put into the cage with the ape.

When this situation was first presented to Julius, he looked at the banana, reached for it, and failing, picked up a bag from the floor of the cage and tried to push it through the wire mesh toward the banana. He also used a bit of wire in the same way, but was unable thus to get the food. As soon as a stick was placed in his cage, he grasped it and used it in a very definite, although unskillful, way to pull the banana toward him. He was extremely eager and impatient, but nevertheless persistent in his efforts, and within five minutes from the beginning of the first trial, he had succeeded in getting two pieces of banana, using always his left hand to manipulate the stick. This test was repeated a number of times with similar results. He had from the first the ability to use a stick in this way, and the only difficulty with the test as a means of obtaining evidence of ideational behavior is that the possibility of imitation of man cannot be certainly excluded.

Lock and Key Test

By my assistant it was reported on May 5 that the orang utan had been seen to place a splinter of wood in a padlock which was used on the cages and to work with it persistently.

It looked very much like imitation of the human act of using the key, and I therefore planned a test to ascertain whether Julius could readily and skillfully use a key or could learn quickly to do so by watching me.

The first test was made on May 15 with a heavy box whose hinged lid was held securely in position by means of a hasp and a padlock. The key, which was not more than an inch in length, was fastened to a six inch piece of wire so that Julius could not readily lose it. With the animal opposite me, I placed a piece of banana in the box, then closed the lid and snapped the padlock. I next handed Julius the key. He immediately laid it on the floor opposite him and began biting the box, rolling it around, and occasionally biting also at the lock and pulling at it. During these activities he had pulled the box toward his cage. Now he suddenly looked up to the position where the banana had been suspended in the box experiment. Evidently the box had suggested to him the banana. For thirty minutes he struggled with the box almost continuously, chewing persistently at the hinges, the hasp, or the lock. Then he took the key in his teeth and tried to push it into one of the hinges, then into the crack beneath the lid of the box.

Subsequently I allowed him to see me use the key repeatedly, and as a result, he came to use it himself now and then on the edge of the box, but he never succeeded in placing it in the lock, and the outcome of the experiment was total failure on the part of the animal to unfasten the lock of his own initiative or to learn to use the key by watching me do so. I did not make any special attempt to teach him to use the key, but merely gave him opportunity to imitate, and it is by no means impossible that he would have succeeded had the key been larger and had the situation required less accurately coördinated movements. However, it is fair to say that the evidence of the idea of using the key in the lock was unconvincing. My assistant's observation was, perhaps, misleading in so far as it suggested that idea. It may and probably was purely by accident that the animal used the splinter on the padlock.

2. Skirrl, *Pithecus irus*

Box Stacking Experiment

The monkey Skirrl was tested by means of the box stacking experiment much as Julius had been. On August 23, with a carrot suspended six feet from the floor of the large cage and three boxes in distant corners, the animal was admitted and his behavior noted.

The boxes, which were made of light, thin material, ranged in size from one six inches in its several dimensions to one twenty inches long, thirteen inches wide, and eleven inches deep. Only by using at least two of these boxes was it possible for the animal to reach the carrot.

Immediately on admission to the cage, Skirrl began to gnaw at the boxes, trying with all his might to tear them to pieces. After some thirty minutes of such effort, interrupted by wanderings about the cage and attempts to get at the other monkeys, he suddenly went to the largest box of all, set it up on end almost directly under the carrot, mounted it, and looked up at the food. It was still beyond his reach and he made no effort to get it, but instead, he reached from his perch on the big box for the next smaller box, which was approximately sixteen inches, by fourteen, by twelve. This he succeeded in pulling toward him, at the same time raising it slightly from the floor, but his efforts caused the large box to topple over and he quit work. The experiment was discontinued after a few minutes, the total period of observation having been thirty-five minutes.

Skirrl handled the boxes with ease and with evident pleasure and interest. He also noticed the carrot at various times during the interval, but his attention was fixed on it only for short periods.

The test was continued on August 24 when, instead of a carrot, a half banana was used as bait. It was placed only five feet from the floor, and three boxes were as formerly placed in distant corners of the cage. When admitted, Skirrl looked at the banana, then pulled one of the boxes toward it, but instead of mounting, he went to the smallest box and began to gnaw it. Shortly, he mounted the middle sized box and looked up toward the banana, but the box was not directly under the bait, and in any event, it would have been impossible for him to reach it. He next went to the largest box, gnawed it vigorously, turned it over several times, and then abandoned it for the middle sized box, from which by skillful use of his teeth and hands, he quickly tore off one side.

By this time, apparently without very definitely directed effort on the part of the monkey, all three of the boxes were in the center of the cage and almost directly beneath the banana. Skirrl climbed up on the largest box and made efforts to pull the middle sized one up on to it, the while looking at the banana every few seconds. He did not succeed in getting the boxes properly placed, and after a time began moving them about restlessly.

His behavior plainly indicated that hunger was not his chief motive. He was more interested in playing with things or in working with them than in eating, and the satisfaction of tearing a box to pieces seemed even greater than that of food. It is especially noteworthy that when Skirrl attempts to dismember a box, instead of starting at random, he searches carefully for a favorable starting point, a place where a board is slightly loosened or where a slight crack or hole enables him to insert his hand or use his teeth effectively. Many times during this experiment he was observed to examine the boxes on all sides in search of some weak point. If no such weak point were found, he shortly left the box; but if he did find a favorable spot, he usually succeeded, before he gave up the attempt, in doing considerable damage to the box.

Following the behavior described above, Skirrl returned to the middle sized box, placed it on end under the banana, mounted, and looked upward at the bait, but as it was a few inches beyond his reach, he made no attempt to get it, but instead, after a few seconds, went to the smallest box, and finding a weak point, began to tear it to pieces.

Later he rolled what was left of the smallest box close to the other two boxes, nearly under the banana, and the remainder of his time was spent gnawing at the boxes and playing with pieces which he had succeeded in tearing from them. During the remainder of the thirty minute interval of observation, no further attention was given the bait.

Again, on August 25, the test was tried, but this time with boxes whose edges had been bound with tin so that it was impossible for the monkey to destroy them. He spent several minutes searching for a starting point on the middle sized box, but finding none, he dragged it under the banana, looked up, mounted the box, but, as previously, did not reach for the bait because it was beyond his reach. He then played with the boxes for several minutes. Finally he worked the two smaller boxes to a position directly under the banana, put the middle sized one on end, mounted it, and looked at the bait, but again abandoned the attempt without reaching.

During the thirty minutes of observation he made no definite effort to place one box upon another. Three times he mounted one or another of the boxes when it was under the banana or nearly so, but in no case was it possible for him to reach the bait.

From the above description of this monkey's behavior, it seems fairly certain that with sufficient opportunity, under strong hunger, he would ultimately succeed in obtaining the bait by the use of two or more boxes. For his somewhat abortive and never long continued efforts to drag two boxes together or to place the one upon the other clearly enough indicate a tendency which would ultimately yield success. The possibility of imitation is not excluded, for Skirrl had opportunities to see Julius and the experimenter handle the boxes.

Because of the other work which seemed more important at the time, this experiment was not continued further. The results obtained suggest the desirability of testing thoroughly the ability of monkeys to use objects as only the anthropoid apes and man have heretofore been thought capable of using them.

Box and Pole Experiment

Skirrl was first tested with the box and pole experiment on August 12. As in the case of Julius, a half banana was placed in the middle of the long box and the attention of the monkey was attracted to the bait by small pieces of carrot placed near each open end. Two poles were placed near the box on the floor of the cage. When admitted to the cage Skirrl went almost directly to the ends of the box, took the pieces of carrot which were in sight, but apparently failed to perceive the bait in the middle of the box. For a while he played with the locks on the box, shoved it about, and amused himself with it, showing no interest in obtaining the food. Later he looked through the box and saw the banana. He then dragged the box about, apparently trying to get it into his cage, but he gave no attention to the poles nor did he make any evident effort to obtain the banana which was easily visible in the center of the box. The period of observation was only twelve minutes.

On August 24 this experiment was repeated with an important modification of the apparatus in that the wooden lid of the long box had been replaced by a wire cover through which the animal could see the bait. Two poles were as formerly on the floor of the cage, not far from the box. Skirrl almost immediately noticed the banana and tried to get it by gnawing at the box. He did not once reach in at the ends of the box, but he did handle the poles, throwing them about and pounding with them. There was not the slightest attempt to use them in obtaining the bait.

This experiment was later repeated three times at intervals of a number of days, but in no case did Skirrl show any tendency to use the poles as means of obtaining the food.

Draw-in Experiment

This also was arranged in the same manner as for Julius, and on each of five days Skirrl was allowed at least thirty minutes to work for the bait. Either a banana or a carrot was each day placed on the board well beyond his reach, and one or two, usually two, small sticks were put into his cage. Not once during the several periods of observation did Skirrl make any attempt to use a stick or any other object as a means of drawing the food to him. Instead, he reached persistently with his arm, pulled and gnawed at the wires which were in his way, and occasionally picked up and gnawed or pounded with the sticks in the cage. His attention every now and then would come back to the food, but it tended to fluctuate rather rapidly, and in the regular period of observation, thirty minutes, it is unlikely that he attended to the bait itself for as much as five minutes. In this respect as well as many others, Skirrl's behavior contrasts sharply with that of the orang utan.

The results of this experiment indicate the lack in the monkey of any tendency or ability, apart from training, to use objects as means of obtaining food. Ways of using objects as tools which apparently are perfectly natural to the anthropoid apes and to man are rarely employed by the lower primates.

Hammer and Nail Test

One day I happened to observe Skirrl playing with a staple in his cage. He had found it on the floor where it had fallen and was intently prodding himself with the sharp points, apparently enjoying the unusual sensations which he got from sticking the staple into the skin in various portions of his body, and especially into the prepuce.

A few days later I saw him playing in similar fashion with a nail which he had found, and still later he was seen to be using a stick to pound the nail with. This suggested to me the hammer and nail test.

A heavy spike was driven into an old hammer to serve as an indestructible handle. This hammer, along with a number of large wire nails and a piece of redwood board, was then placed in the monkey's cage. Skirrl immediately took up the hammer, grasping the middle of the handle with his left hand, and with his right hand taking up a nail. He then sat down on the board, examined the nail, placed the pointed end on the board, and with well directed strokes by the use of the head of the hammer drove the nail into the board for the distance of at least an inch. He then tried to pull it out, but was forced to knock it several times with the hammer before he could do so.

This performance, during the next few minutes, was repeated several times with variations. Often the side of the hammer was used instead of the head, and occasionally, as is shown in figure 8 of plate II, he seized the hammer well up toward the juncture of the same with the spike. This figure does justice to the performance. At the moment the picture was taken, Skirrl's attention had been attracted by a monkey in an adjoining cage, and he had momentarily looked up from his task, the while holding nail and hammer perfectly still.

This test was repeated on various days, and almost uniformly Skirrl showed intense interest in hammer and nails and used them more or less persistently in the manner described. Occasionally, apparently for the sake of variety, he would put the blunt end of the nail on the board and hammer on the point. Again, he would try persistently to drive the nail into the cement floor, and once by accident, when hammer and nails were left in his cage over night, he succeeded in making several holes in the bottom of his sheet iron water pan. There was no doubting the keen satisfaction which the animal took in this form of activity.

It is impossible to say that the behavior was not imitative of man, for Skirrl, along with all of the other monkeys, had had abundant opportunity to see carpenters working. But this much can be said against the idea of imitation,—no one of the other animals, not excepting the orang utan, showed any interest whatever in hammer and nails. Occasionally they would be played with momentarily or pushed about, but Sobke, Jimmie, Gertie, Julius, although given several opportunities to exhibit any ability which they might have to drive nails, made not the least attempt to do so. Evidently we must either conclude that Skirrl had a peculiarly strong imitative tendency in this direction, or instead, a pronounced disposition or instinct for the use of objects as tools. It would seem fair to speak of it as an instinct for mechanical activity.

Under this same heading may be described Skirrl's reactions to such objects as a handsaw, a padlock, and a water faucet. The saw was given to him in order to test his ability to use it in human fashion, for if he could so expertly imitate the carpenter driving nails, it seems likely that he might also imitate the use of the saw.

As a matter of fact, he showed no tendency to use the saw as we do. Instead, he persistently played with it in various ways, at first using it as a sort of plane to scrape with, later often rubbing the teeth over a board so that they cut fairly well, but never as effectively as in the hands of a man. After two or three days' practice with the saw, Skirrl hit upon a method which is, as I understand, used by man in certain countries, namely, that of placing the saw with the teeth up, holding it rigid, and then rubbing the object which is to be sawed over it. This Skirrl succeeded in doing very skillfully, for he would sit down on the floor of the cage, grip with both feet the handle of the saw, with the teeth directed upward, then holding either end in his hands, he would repeatedly rub a stick over the teeth. In this way, of course, he could make the saw cut fairly well. But still more to his liking was the use of a spike instead of a stick as an object to rub over the teeth, for with this he was able to make a noise that would have satisfied even a small boy.

Further light is shed on the force of the tendency to imitate man by the saw test. After Skirrl had been given an opportunity to show what he could do with the tool spontaneously, I demonstrated to him the approved human way of sawing. Often he would watch my performance intently as though fascinated by the sound and motion, but when given the tool he invariably followed his own methods. Although I repeated this test of imitation several times on three different days, the results were wholly negative.

Other Activities

One day as Skirrl was being returned to his own cage by way of the larger cage, he picked up an unfastened padlock and carried it into the cage with him. For more than an hour he amused himself almost without interruption by playing with this lock. The things which he did with it during that time would require pages to describe. His interest in it was very similar to that which he had exhibited in hammer and nails, saw, and indeed any objects which he could play with. The lock was pounded in various ways, bitten, poked with nails, hooked into the wires of the cage, used to pull on, pounded with a stick, used to hammer on the floor of the cage with, and in fine, manipulated in quite as great a variety of ways as a human being could have discovered. Finally it was hooked to the side of the cage and snapped shut, and as Skirrl was unable to dislodge it from this position, he shortly gave up playing with it.

At the end of the large cage and just outside the wire netting was a faucet to which a hose was usually attached. The valve could be opened by turning a wheel-shaped hand piece. Both Skirrl and Julius learned to turn this wheel in order to get water to play with, but usually the former's strength was not sufficient to turn on the water. The latter could do it readily. The indications are that both animals profited by seeing human beings turn on the water. This unquestionably attracted their attention to the faucet, and probably by playing with it they accidentally happened upon the proper movement. At any rate, Skirrl's behavior was significant in this connection, for he would pick up the hose to see if water were flowing, and if it were not, he would throw it down, go directly to the faucet, and try to turn the wheel. The association of the wheel with the desired flow of water was therefore definitely established. Shall we describe the act as ideational? It seems the natural thing to do.

3. Sobke, *Pithecus rhesus*

Box Stacking Experiment

For this test, in the case of Sobke, three light boxes made of redwood about one-third of an inch thick were used. The smallest, box 1, was six inches in each direction, the next larger, box 2, was twelve inches, and the third, box 3, eighteen inches. As in the case of the other animals, bait, either banana or carrot, was suspended from the middle of the roof of the large cage at such distance from the floor as to be reached by the animal only by the use of the boxes.

The first observations on Sobke were made on June 14. The three boxes had been placed in the form of a pyramid directly under the banana, which hung about eighteen inches above the uppermost box. Sobke's attention while in his cage had been attracted to the bait by seeing me fastening it in position, but when admitted to the large cage, he simply glanced at it and then wandered about the cage, picking up bits of food and struggling to get at the other monkeys. This he did for about five minutes. He then went to the boxes, placed his hands on top of the bottom one, but did not climb up on it. A few minutes later he returned to the box again, climbed up, and readily reached the food, which he ate while resting on boxes 1 and 2.

I now replaced the bait and gave the monkey a second chance to obtain it. Almost immediately he climbed up as far as the second box, but although he could reach the banana only from the uppermost box, he deliberately shoved it off to the ground and sat down upon box 2. As he was unable to obtain the banana from this, he soon began to gnaw and pull at it, and as he was succeeding all too well in his efforts to tear the box to pieces, he had to be returned to his cage.

The most important features of his behavior were, first, his stealthy and indirect manner, and second, his failure to use other means of obtaining the bait than that supplied by the observer. Instead of looking straight at the experimenter, or at the object which he wished to obtain, he apparently looked and attended elsewhere. For this reason it was often difficult to decide whether or not he had noticed the bait or the boxes. Finally I was led to conclude that he usually knew exactly what was going on and had in his furtive way noted all of the essential features of the situation, and that his manner was extremely indicative of his mental attitude of limited trust. Both Julius and Skirrl went to the opposite extreme in the matter of directness, or as we should say in human relations, frankness. They would look the experimenter directly in the eye, and they usually gazed intently at anything, such for example as the bait, that interested them. Sobke, even when very hungry, instead of going directly toward the bait, and trying to obtain it, usually did various other things as though pretending that he had no interest in food.

On the following day, June 15, the three boxes were again placed nearly under the banana, but this time the two smaller boxes, numbers 1 and 2, were pushed to the extreme end of the lower box and so far from the bait that it could not be reached from box 1. It was necessary then for the animal to push boxes 1 and 2 along on box 3 until they were nearer the bait.

Sobke, when admitted to the cage, evidently noticed the banana, but as formerly, he made no immediate effort to obtain it. After wandering in search of food and quarreling with the other monkeys for several minutes, he went to the boxes, pushed the topmost one, number 1, off on to the floor, and then carried it into his cage where he quickly tore one side off. He next returned to the large cage, climbed up on box 2, and he was able, by jumping, to reach and obtain the banana.

As Sobke was very good at jumping, his new method rendered the box stacking experiment of uncertain value, since it was next to impossible so to arrange the spatial relations of bait and boxes that he should be neither discouraged by too great a distance nor encouraged to jump by too small a distance. Evidently it would be more satisfactory to simplify the conditions by trying to discover, first of all, whether he would use a single box as a means of reaching the reward.

In pursuance of this idea, I suspended a piece of bread five feet from the floor of the cage, and a few feet to one side of it, I placed a box from which it could be reached, or at least easily seized by jumping. Sobke shortly walked to a point beneath the bait and leaping into the air, seized it.

I then replaced the bait, raising it to a height of five feet ten inches from the floor of the cage. When I had retired, Sobke placed himself in the proper position beneath, looked up at it, but went away without jumping for it. During the remaining ten minutes of observation, he paid no further attention to the bait, having satisfied himself evidently that it was beyond his reach.

My use of this test was concluded on June 16 when once more I suspended a piece of bread six feet from the floor and placed a few feet to one side the eighteen inch box, number 3, from which had the monkey pushed it to a point directly under the bread, he could have obtained the food easily.

Sobke noticed the food promptly, and from time to time as he wandered about, he glanced at it out of the corner of his eye, but not once did he sit down and look at it steadily and directly as Julius and Skirrl might have done.

In the first twenty minutes of observation the monkey made no attempt either to use the box or to reach the food by jumping. I then placed the box directly under the bait, and scarcely had I withdrawn from the cage before Sobke climbed up on it and looked toward the food. He could not reach it without jumping, and he made no effort to get it. I had left a second box in the cage,—one which I had been using as a seat. Sobke now went to this box, placed his hands on it, looked toward the bait, and then went to a distant part of the cage. No further indications were obtained during the remainder of the period of observation of interest in the boxes as possible means of obtaining the desired food.

It is of course obvious that this experiment was not long enough continued to justify the conclusion that either Sobke or Skirrl could not use the boxes or even learn to place one box upon another in order to obtain the bait. The experiment, like several others which are being described briefly, was used to supplement the multiple-choice experiment, and the experimenter's chief interest was to discover the number and variety of methods which would be used by the animal in the first few presentations of a situation. It is practically certain that both of these monkeys would have succeeded ultimately in solving the problem of obtaining the food had they been left in the cage with a number of boxes, for Skirrl very early indicated interest in moving the boxes about, and Sobke showed a tendency in that direction which perhaps was inhibited partially by his distrust of the experimenter.

Draw-in Experiment

For Sobke, as for Julius and Skirrl, the draw-in test was made by putting food on a shelf outside the cage, beyond the reach of the animal, and placing in the cage with the animal one or two sticks long enough to be used for drawing in the bait.

Sobke was first given this test on July 24. He tried persistently to reach the banana with his hand, seized the box which supported the bait, shook it, picked up one or other of the sticks, and chewed at it repeatedly, but not once did he make any move to use a stick to draw the food toward him.

This experiment was repeated on July 27, 29, 30 and 31, a period of thirty minutes being allowed on each day for observation. At no time did Sobke show any inclination to use either a stick or any other object as a means of reaching the bait. Instead, he confined himself strictly to the use of hands and teeth.

This test makes it fairly certain that Sobke had no natural tendency to use objects as tools. In so far as he attended to things about the cage or laboratory, it seemed to be rather to play with them in a general way than to use them ideationally or otherwise for definite purposes.

The definitely negative result of the draw-in experiment rendered needless prolonged observation with the box and pole test, whose results are now to be presented.

Box and Pole Experiment

The eighty-four inch box, previously used for a similar test with Julius, was presented to Sobke on August 24, the wooden cover having been replaced by a wire one so that the monkey could readily see the bait in the middle of the box. Sobke, when admitted to the large cage, went directly to the box and at once discovered the banana which was midway between the ends. He evidently desired it. Shortly, he went to one end of the box and looked in. This he repeated later. He also shook the box and tried to pull it about and tear it with his teeth, but to the two poles lying nearby on the floor of the cage he gave not the slightest attention during a thirty minute period of observation.

The experiment was not repeated because of more important work.

Other Activities

In more respects than I have taken time to enumerate in the above descriptions of behavior, the relations of Sobke to objects differed from those of Skirrl, and still more from those of Julius. Hammer, nails, saw, stones, sticks, locks, and various other objects received relatively little attention from Sobke unless they happened to come in his way; then they were usually pushed aside with but scant notice. Rarely he would carry something to the shelf of his cage with him, but as a rule only to lay it down and attend to something else. Skirrl, on the contrary, attended persistently to anything new in the shape of a movable object. He was extremely partial to objects which could be manipulated by him in various ways, and especially to anything with which he could make a noise. His interest in hammer and nails, saw, locks, etc., seemed never to wane. I have seen him play for an hour almost uninterruptedly with a hammer and a nail, or even with a big spike which he could use to pry about his cage. In the absence of anything more interesting, even a staple or a small nail might receive his undivided attention for minutes at a time. How important is the species difference in this connection, I have no means to judge, but if we may not consider these different modes of behavior characteristic of *P. rhesus* as contrasted with *P. irus*, we must conclude that remarkable individual differences exist among monkeys, for whereas Skirrl is by nature a mechanical genius, Sobke has apparently no such disposition. I can imagine no more fascinating task than the careful analytical study of the temperaments of these two animals. Skirrl's behavior has importantly modified my conception of genius.

V

MISCELLANEOUS OBSERVATIONS

1. *Right- and left-handedness*

Several years ago Doctor Hamilton reported to me observations which he had made on preference for the right or left paw in dogs. He has not, I believe, published an account of his work. Subsequently, Franz observed a similar preference in monkeys which, according to his report, exhibit marked tendency to be right-handed, left-handed, or ambidextrous.

My own observations, although they are wholly incidental to my other work, seem worthy of description at this point.

I noted, first of all, that the orang utan Julius tended to use his left hand. He by no means limited himself to this, but in difficult situations he almost invariably reached for food or manipulated objects in connection with food getting with the left hand. Figures 23 and 24 of plate V, show him reaching for a banana with the left hand. Likewise, figure 34 exhibits the use of the left hand in the draw-in experiment.

So marked was Julius's preference for his left hand that I became interested in observing similar phenomena in the monkeys.

Skirrl, when driving nails, held the hammer with his left hand and the nail with his right hand. The fact that he never was observed to reverse the use of the hands is surprising, for other observations indicate that he preferred the right hand for certain acts.

Stimulated by the obvious left-handedness, in certain connections, of Julius and Skirrl, I tested the preference of several of the monkeys in the following simple way. Standing outside the cage I would hold out a peanut to a hungry animal, keeping it so far from the cage that the monkey could barely reach it with its fingers. I noted the hand which was used to grasp the food. Next I varied the procedure by placing the peanut on a board in order to make sure that I was not definitely directing the animal's attention.

With Sobke the following results were obtained. In forty trials given on two different days, he reached for and obtained the food each time with his left hand. Only by holding the bait well toward the right side of his body was it possible to induce him to use the right hand. So far as may be judged from these observations and from others in connection with the experiments, this animal is definitely left-handed.

With Skirrl the results are strikingly different. As stated above, he used the hammer consistently with his left hand, but in twenty attempts to obtain food by reaching, he used his right hand seventeen times and his left only three times. It was quite as difficult to induce him to use his left hand for this purpose as it was to induce Sobke to use his right. We must therefore conclude that Skirrl is right-handed in connection with certain movements and left-handed in others.

The monkey named Gertie in the reaching experiment consistently used her left hand, never once using the right.

Jimmie, so far as it was possible to make tests with him, also used his left hand, but it should be said that the results are unsatisfactory because he was at the time extremely pugnacious and paid attention to the experimenter rather than to the food.

Scotty, in the first series of ten trials, used his right hand eight times, his left twice. In the second series, given the following day, he used the right hand three times and the left seven times. From this we should have to infer that he is ambidextrous.

A female *rhesus* monkey which had been brought to the laboratory only a few days previously showed a preference for the right hand by the use of it fourteen times to six.

In connection with these data which are, I should repeat, too scanty to be of any considerable value, I wish to describe my own experience. Although naturally left-handed, I am by training right-handed to the extent of having been able to use my hands in writing and in various other activities equally well at the age of twelve. I am at present ambidextrous in that there are many things which I do with equal readiness and skill with either hand. Delicate, exact, and finely coördinated movements, such as those of writing and using surgical instruments, I perform always with my left hand, while grosser movements involving the whole hand or arm, I am rather likely to perform with my right hand.

It seems not improbable in the light of my own experience that we shall find some specialization among the lower animals with respect to preference for right and left hand or arm. I should not be at all surprised to discover that it is the rule for animals to possess or to develop readily definite preference for one hand in connection with a given act of skill and to have quite as definite a preference for the other hand in connection with a radically different kind of act.

2. *Instinct and emotion*

Of the many presumably instinctive modes of behavior which were observed, only those which have to do with social relations seem especially worth reporting. From among them I shall select for description a few which have already been referred to in connection with the experimental observations.

Maternal Instinct

Aspects of the maternal instinct I had opportunity to observe in Gertie, who on February 27 gave birth to a male infant, I present below the substance of a previously published note on her behavior (Yerkes, 1915).

"On February 27 one of the monkeys of our collection gave birth, in the cages at Montecito, to a male infant. The mother is a *Macacus cynomolgus rhesus* (*P. irus rhesus*) who has been described by Hamilton (1914, p. 298) as 'Monkey 9, Gertie, *M. cynomolgus rhesus* (*P. irus rhesus*). Age, 3 years 2 months. (She is now, May 1, 1915, 4 years and 6 months.) Daughter of monkeys 3 and 10. First pregnancy began September, 1913.' The result of this pregnancy was, I am informed, a still-birth.

"The second pregnancy, which shall now especially concern us, resulted likewise in a still-birth. Parturition occurred Saturday night, and the writer first observed the behavior of the mother the following Monday morning. In the meantime the laboratory attendant had obtained the data upon which I base the above statements.

"At the time of parturition Gertie was in a 6 by 6 by 12 foot out-door cage containing a small shelter box, with an exceptionally quiet and gentle male (not the father of the infant) who is designated in Hamilton's paper as Monkey 28, Scotty.

" My notes record the following exceptionally interesting and genetically important behavior. On March 1, when I approached her cage, Gertie was sitting on the floor with the infant held in one hand while she fingered its eyelids and eyes with the other. Scotty sat close beside her watching intently. When disturbed by me the mother carried her infant to a shelf at the top of the cage. Repeatedly attempts were made to remove the dead baby, but they were futile because Gertie either held it in her hands or sat close beside it ready to seize it at the slightest disturbance.

" Especially noteworthy on this, the second day after the birth of the infant, are the male's, as well as the female's, keen interest in the body and their frequent examinations of the eyes, as if in attempts to open them. Often, also, the mother searched the body for fleas.

" Observations were made from day to day, and each day opportunity was sought to remove the body without seriously frightening or exciting the female. No such opportunity came, and during the second week the corpse so far decomposed that, with constant handling and licking by the adults, it rapidly wore away. By the third week there remained only the shriveled skin covering a few fragments of bone, and the open skull from the cavity of which the brain had been removed. This the mother never lost sight of: even when eating she either held it in one hand or foot, or laid it beside her within easy reach.

" Gradually this remnant became still further reduced until on March 31 there existed only a strip of dry skin about four inches long with a tail-like appendage of nearly the same length.

" The male, Scotty, on this date was removed to another cage. Gertie made a great fuss, jumping about excitedly and uttering plaintive cries when she discovered that her mate was gone. Whenever I approached her cage she scurried into the shelter box and stayed there while I was near. This behavior I never before had observed. It continued for two days. On April 2, it was noted that she had lost her recently acquired shyness and she no longer made any attempts to avoid me. As usual, on this date, she was carrying the remnant about with her.

" The following day, April 3, Gertie was lured from her cage to a large adjoining compartment for certain experimental observations. After she had been returned to her own cage the remnant was noticed on the floor of the large cage. I picked it up. Gertie evidently noticed my act, for although at a distance of at least ten feet from me, she made a sharp outcry and sprang to the side of the cage nearest me. I held the piece of skin (it looked more like a bit of rat skin than the remains of a monkey) out to her and she immediately seized it and rushed with it to the shelf at the top of the cage.

" Two days later the remnant was missing, and careful search failed to discover it in the cage. It is probable that Gertie had carelessly left it lying on the floor whence it was washed out when the cages were cleaned. On this date Gertie seemed quieter than for weeks previously.

" Thus it appears that during a period of five weeks the instinct to protect her offspring impelled this monkey to carry its gradually vanishing remains about with her and to watch over them so assiduously that it was utterly impossible to take them from her except by force.

"After reading this note in manuscript, Doctor Hamilton informed me that Gertie had behaved toward her first still-birth as toward her second. And, further, that Grace, a baboon, also carried a still-birth about for weeks.

" I am now heartily glad that my early efforts to remove the corpse were futile, for this record of the persistence of maternal behavior seems to me of very unusual interest to the genetic psychologist."

Fear

In connection with the multiple-choice experiments Skirrl exhibited what seemed to be instinctive fear as a result of his unfortunate experience with nails in the floor of box 1. He seemingly referred his misadventure to some unseen enemy under the floor, and this in spite of the fact that he was given abundant opportunity to examine the floor of the box, but not until after the dangerous nails had been clinched. His long continued avoidance of the experiment boxes and his still more persistent hesitancy in entering them, coupled with his almost ludicrous efforts to see beneath the floor through the holes cut for the staples on the doors, gave me the impression of superstitious fear of the unseen. As I watched and recorded his behavior day after day during the period of most pronounced fear, I could not avoid the thought that the instinctive fear of snakes had something to do with his peculiar actions, and although I have never studied either the natural or the acquired responses of monkeys to snakes, I suspect that lacking such instinctive equipment, Skirrl would have behaved differently as a result of the pricks which he received from the nails.

It is needless to redescribe his acquired fear of whiteness as it manifested itself in the freshly painted apparatus.

Accompanying these instructive modes of response and their emotions are suggestions of peculiarly interesting problems as well as of modes of attacking them. As a matter of fact, Skirrl's fear-reactions did much to alter my conception of the constitution of his mind. I should not have been surprised by the features of behavior exhibited, but I was by no means prepared for their persistence, and for the highly emotional attitude toward the particular situation. Only an organism of complexly constituted nervous system and fairly highly developed affective life could be expected to respond as did this monkey. As has been suggested above, I find the appeal to instinct, modified by experience, a natural mode of accounting for the unexpected features of Skirrl's behavior.

Sympathy

The instinctive playfulness of the young monkey Tiny contrasted most strikingly with the more serious, if not more sedate, modes of behavior of the older individuals.

During the greater part of my period of observation Tiny was cage-mate of Scotty, the most calm and apparently lazy of all the monkeys. Tiny delighted in teasing Scotty, and her varied modes of mildly tormenting him and of stirring him to pursuit or to retaliation were as interesting as they were amusing. Her most common trick was to steal up behind him and pull the hair of his back, or seize his tail with her hands or teeth. Often when he was asleep she would suddenly run to him, give a sudden jerk at a handful of hairs, and leap away. He was surprisingly patient, and I never saw him treat her roughly in retaliation.

Another of Tiny's favorite forms of amusement was that of trying to stir up the other monkeys to attacks on one another. She very cleverly did this by pretending that she herself was being attacked. The instant the older animals began to show hostility toward one another she would leap out of the way and watch the disturbance with evident satisfaction. It was this mode of behavior in the little animal which ultimately provided opportunity for the observations which I wish now to report as indicative of sympathetic, possibly I may say altruistic, emotions.

Tiny was confined with Scotty in a cage adjoining the one in which Jimmie and Gertie were being kept. The cages were separated by wire netting of half-inch mesh.

One morning as I was watching the behavior of the animals in the several cages, I noticed Tiny dressing with her teeth a wounded finger. It had evidently been bitten by one of the other animals, in all probability either by Jimmie or Gertie. Tiny was trimming away the loose bits of skin very neatly and cleansing the wound. After working at this task for a few minutes, she quickly climbed up to the shelf near the top of her cage, and by rushing to the partition wire between the two cages, she lured Gertie to an attempted attack on her. Gertie sprang up to the partition, placed her hands on it, with the fingers projecting through the meshes, and attempted to seize Tiny's fingers with her teeth. But the latter was too quick for her, and withdrawing her hands, like a flash seized in her teeth the middle finger of Gertie's left hand. She then bit it severely and with all her might, at the same time pulling and twisting violently, often placing the entire weight of her body on the finger. Her sharp teeth cut to the bone, and it was impossible for the larger and stronger monkey to tear away. For several seconds this continued, then Gertie succeeded in escaping, whereupon she at once retreated to the opposite end of her shelf and proceeded to attend to her injured finger. She cried, wrung her hands, and from time to time placed the finger in her mouth as though in an effort to relieve the pain. By this time Jimmie's attention had been attracted by the disturbance and he rushed up to the shelf, and facing Gertie, watched her intently for a few seconds. The look of puzzled concern on his face was most amusing. Apparently he felt dimly that something in which he should have intelligent interest was going on, but was unable wholly to understand the situation. After watching Gertie for a time and trying to discover what she was doing, which was rendered difficult by her tendency to turn away from him, in order to shield her injured finger, he rushed over to the wire partition and made strenuous efforts to seize Tiny with his hands and teeth. But although she continued close to the partition and often crowded against it with face and hands flattened on the wires, he was not able to get hold of her, and after a few vain attempts he returned to his mate, and again with evident solicitousness and the most troubled expression, watched her wringing her hands and chewing or sucking at her injured finger. Shortly he again returned to the partition and renewed his attempts to seize the young monkey. Thus he went back and forth from one place of interest to the other several times, but being unable to achieve anything at either point, he finally gave up and returned to his breakfast on the floor of the cage.

I report this incident fully because the behavior of Jimmie was in marked contrast with the usual behavior of the monkeys. Selfishness seemed everywhere dominant, while clear indications of sympathetic emotions were rare indeed. The above is undoubtedly the best evidence of anything altruistic that I obtained.

It is possible that Tiny's action was retaliatory, but although it is practically certain that either Gertie or Jimmie inflicted the wound on her finger, I of course cannot be sure that the spirit of revenge stirred her to punish Gertie so severely. Jimmie's part in the whole affair is, however, perfectly intelligible from our human point of view, and there seems no reason to doubt that he did experience something like a feeling of sympathy with his mate, coupled with a feeling of resentment or anger against Tiny.

VI

HISTORICAL AND CRITICAL DISCUSSION OF IDEATIONAL BEHAVIOR IN MONKEYS AND APES

It is my purpose in this section to indicate the relations of my work on monkeys and apes to that of other investigators. Although throughout the report I have used freely the psychological terms idea and ideation, it has been my aim to describe the behavior of my animals rather than to interpret it or speculate concerning its accompaniments. Certain acts I have designated as ideational simply because they seemed to exhibit the essential features of what we call ideational behavior in man. Further study may, and probably will, modify my opinion concerning this matter. It is of prime importance to analyze ideational behavior so that it may be accurately described and satisfactorily defined in terms of its distinguishing characteristics. I had hoped to be able to present a tentative analysis in this report, but the results of my efforts are so unsatisfactory that I do not feel justified in publishing them.

The terms idea and ideation have been used to designate contents of consciousness which are primarily representative. Nowhere have I attempted to indicate different types or grades of ideational behavior and nowhere have I found it necessary to emphasize differences between image and idea. In general, the acts which I have called ideational have been highly adaptive, and the learning processes in connection with which they have appeared have differed strikingly from those of the selective sort in their abruptness of appearance.

Extremely interesting and valuable definitions of ideation and discussions of the characteristics of different sorts of ideas in the light of original observations on monkeys have been presented by Thorndike (1901, pp. 1, 2; 1911, p. 174); Kinnaman (1902, p. 200); and Hobhouse (1915, p. 270). As these authors have contributed importantly to our knowledge of the behavior of monkeys, their discussions of the meaning of terms are especially valuable. Serviceable definitions are to be found, also, in

Romanes (1900), Morgan (1906), Washburn (1908), and Holmes (1911).

Evidences of Ideation in Monkeys

Aside from anecdotal and traveller's notes on the behavior of monkeys and apes we have only a scanty literature. In fact, the really excellent articles on the behavior and mental life of these animals may be counted on one's fingers; and not more than half of these are experimental studies. I shall, in this brief historical sketch, neglect entirely the anecdotal literature, since my own work is primarily experimental, and since its results should naturally be compared with those of other experimenters.

Thorndike (1901), the American pioneer in the application of the experimental method to the study of mind in animals, published the first notable paper on the psychology of monkeys. His results force the conclusion that "free ideas" seldom appear in the monkey mind and have a relatively small part in behavior. That the species of Cebus which he observed exhibits various forms of ideation he is willing to admit. But he insists that it is of surprisingly little importance in comparison with what the general behavior of monkeys as known in captivity and as described by the anecdotal writers have led us to expect. It is important to note, however, that Thorndike's observations were limited to Cebus monkeys which, as contrasted with various old world types, are now considered of relatively low intelligence.

In many respects the most thoroughgoing and workmanlike experimental study of monkeys is that of Kinnaman (1902), who has reported on the study of various forms of response in *P. rhesus.* He presents valuable data concerning the learning processes, sensory discrimination, reaction to number, and to tests of imitation. His results indicate a higher level of intelligence than that discovered by Thorndike, but this is almost certainly due to difference in the species observed. Kinnaman goes so far as to say "We have found evidence, also, of general notions and reasoning, both of low order" (p. 211).

The contribution of Hobhouse (1915) to our knowledge of the mental life of monkeys, although in a measure experimental, is based upon relatively few and unsystematic observations as contrasted with those of Thorndike and Kinnaman. It appears, however, that Hobhouse's experiments were admirably planned to test the ideational capacity of his subjects, and one can not find a more stimulating discussion of ideation than that contained in his "Mind in Evolution." The results of his tests made with a *P. rhesus* monkey are similar to those of Kinnaman, for almost all of them indicate the presence and importance of ideas.

Watson (1908) in tests of the imitative ability of *P. rhesus* saw relatively little evidence of other than extremely simple forms of ideation. But in contrast with his results, those obtained by Haggerty (1909), in a much more extended investigation in which several species of monkey were used, obtained more numerous and convincing evidences of ideation in imitative behavior. Although this author wholly avoids the use of psychological terms, seeking to limit himself to a strictly objective presentation of results, it is clear from an unpublished manuscript (thesis for the Doctorate of Philosophy, deposited in the Library of Harvard University) that he would attribute to monkeys simple forms of ideational experience.

Witmer (1910) reports, in confirmation of Haggerty's results, intelligently imitative behavior in *P. irus*.

The work of Shepherd (1910) agrees closely, so far as evidences of ideation are concerned, with that of Thorndike. He obviously strives for conservatism in his statements concerning the adaptive intelligence of his monkeys, all of which belonged to the species *P. rhesus*. At one point he definitely states that they exhibit ideas of a low order, or something which corresponds to them. Satisfactory evidences of reasoning he failed to obtain.

Franz's (1907, 1911) studies of monkeys, unlike those mentioned above, have for their chief motive not the accurate description of various features of behavior but instead knowledge of the functions of various portions of the brain. His results, therefore, although extremely interesting and of obvious value to the comparative psychologist, throw no special light upon the problem of ideation.

The investigation by Hamilton (1911) of reactive tendencies in *P. rhesus* and *irus* yielded preëminently important data concerning complex behavior. For the ingenious quadruple-choice method devised by this observer showed that mature monkeys exhibit fairly adequate types of response. As Hamilton's interest centered in behavior, he did not discuss ideation, but this does not prevent the comparison of his data with those of the present report, and the agreement of his findings with my own is obvious.

My work contrasts sharply with that briefly mentioned above in that I applied systematically and over a period of several months an experimental method suited to reveal problem solving ability. Previously, the so-called problem or puzzle-box method had been used as a means of testing for the presence of ideas. For this I substituted the multiple-choice method. One of the chief advantages of this new method is the possibility of obtaining curves of learning for the solution or attempted solution of relational problems of varying difficultness. I am confident that these curves of learning will prove far more valuable than such data as are yielded by the puzzle-box method.

The Pithecus monkeys, which I studied intensively, yielded relatively abundant evidences of ideation, but with Thorndike I must agree that of "free ideas" there is scanty evidence; or rather, I should prefer to say, that although ideas seem to be in play frequently, they are rather concrete and definitely attached than "free." Neither in my sustained multiple-choice experiments nor from my supplementary tests did I obtain convincing indications of reasoning. What Hobhouse has called articulate ideas, I believe to appear infrequently in these animals. But on the whole, I believe that the general conclusions of previous experimental observers have done no injustice to the ideational ability of monkeys. It is clearly important, however, that we always should take into account the species of animal observed, for unquestionably there are extreme differences in mental development among the monkeys.

As I view my results in the light of their relations to earlier work, I am strongly impressed with the importance of the use of improved methods for the study of complex behavior. The delayed reaction method of Hunter, the quadruple-choice method of Hamilton, and my multiple-choice method offer new and promising approaches to forms of activity which thus far have been only superficially observed.

The ability exhibited by Skirrl to try a method out and then to abandon it suddenly is characteristic of animals high in intelligence. Most of the problems which I presented to my animals would be rated as difficult by psychologists, for as a rule they involved definite relations and demanded on the part of the subject both perception of a particular relation and the ability to remember or re-present it on occasion.

I was greatly surprised by the slow progress of the monkeys toward the solution of these problems. It had been my supposition that they would solve them more quickly than any lower type of mammal, but as a matter of fact they succeeded less well than did pigs. Their behavior throughout the work proved that of far greater significance for the experimenter than the solution of a problem is definite knowledge of the modes of behavior exhibited from moment to moment, or day to day. This is true especially of those incidental or accidental modes of response which so frequently appeared in connection with my work that I came to look upon them, the surprises of each day, as my chief means of insight.

Evidences of Ideation in Apes

Reliable literature of any sort concerning the behavior and mental life of the anthropoid apes is difficult to find, and still more rare are reports concerning experimental studies of these animals. There are, it is true, a few articles descriptive of tests of mental ability, but even these are scarcely deserving of being classed as satisfactory experimental studies of the psychology of the ape. I have the satisfaction of being able to present in the present report the first systematic experimental study of any feature of the behavior of an anthropoid ape.

Among the most interesting and valuable of the descriptions which may be classed among accounts of tests of mental ability is Hobhouse's (1915) study of the chimpanzee. The subject was an untrained animal, so far as stated, of somewhat unsatisfactory condition because of timidity. Nevertheless, Hobhouse was able to obtain from him numerous and interesting responses to novel situations, some of which may be safely accepted as evidences of ideation of a fairly high order.

Similar in method and result to the work of Hobhouse is that of Haggerty (unpublished thesis for the Doctorate of Philosophy, deposited in the Library of Harvard University). Haggerty's tests of the ability of young orang utans and chimpanzees to solve simple problems and to use tools in various ways yielded results which contrast most strikingly with those obtained in his experimental study of the imitative tendency in monkeys. His observations, had he committed himself to anything approaching interpretation, doubtless would have led him to conclusions concerning the ideational life of these animals very similar to those of Hobhouse.

Koehler, working in the Canary Islands, has, according to information which I have received from him by letter, made certain experiments with orang utans and chimpanzees similar to those of Hobhouse and Haggerty. His results I am unable to report as I have scanty information concerning them. They are, presumably, as yet unpublished.

In his laboratory at Montecito, California, Hamilton has from time to time kept anthropoid apes, but without special effort to investigate their ideational behavior. He has most interesting and valuable data concerning certain habits and instincts, all as yet unpublished.

To a congress of psychologists Pfungst (1912) briefly reported on work with anthropoid apes in certain of the German zoölogical gardens. His preliminary paper does not enable one to make definite statements concerning either his methods or such results as he may have obtained concerning ideational behavior. So far as I know, he has not as yet published further concerning his investigation.

Möbius (1867) has described interesting observations concerning the mental life of the chimpanzee. But this, like all of the work previously mentioned, is rather in the nature of casual testing than thoroughgoing, systematic, and analytic study.

In addition to the above reports, there are a few concerning the behavior of apes which have been especially trained for purposes of exhibition. Most interesting of these is that of Witmer (1909), who studied in exhibitions and in his own laboratory the behavior of the chimpanzee Peter. The varied forms of intelligently adaptive behavior exhibited by this ape convinced Witmer of ideational experience and even of an approach to reasoning. In his brief report he expresses especial interest in the possibility of educating this "genius among apes" to the use of language.

A chimpanzee named Consul was observed several years ago by Hirschlaff (1905), and his tricks were interestingly described from the pedagogical standpoint.

Similar in character is Shepherd's (1915) brief description of the stage behavior of Peter and Consul, both chimpanzees. It is impossible to determine from the account whether these animals are the same as were observed by both Witmer and Hirschlaff. As no reference is made in Shepherd's paper to other descriptions of the behavior of these animals and as he adds nothing to what had already been presented, the reader obtains no additional light on ideation.

I have mentioned only samples of the articles on trained anthropoids. All are necessarily descriptions of the behavior of individuals who had been trained not for psychological purposes but for the vaudeville stage, and although such observations unquestionably have certain value for comparative psychology, it is well known that unless an observer knows the history of an act, he is not able to evaluate it in terms of intelligence and is especially prone to overestimate its value as evidence of ideation.

There remain studies of the apes, dealing primarily with behavior and mental characteristics, which are slightly if at all experimental and deserve to be ranked as naturalistic accounts. Such is, for example, the book of Sokolowski (1908), in which attention is given to the characteristics of young as well as fairly mature specimens of the gorilla, chimpanzee and orang utan.

The various publications of Garner (1892, 1896, 1900) deal especially with the language habits of monkeys and apes, but observations bearing on ideation are reported.

Wallace (1869) describes certain features of the behavior of an infant orang utan whose mother he shot in Borneo. He also reports observations concerning the behavior of adult orang utans, many specimens of which were shot by him during his travels.

Early in the last century, Cuvier (1810) interested himself in studies of the intellectual characteristics of the orang utan, and his data, taken with those of Wallace, Sokolowski, and others similarly interested in the natural history of mind, give one a valuable glimpse of the life of the anthropoid ape.

Finally, the data brought together by Brehm (1864, 1875, 1888) in his famous Tierleben; by Darwin (1859, 1871) in "The

Origin of Species," and other works, by Romanes (1900), especially in his books on mental evolution, by C. Lloyd Morgan (1906) in his several works on comparative psychology, and by Holmes (1911) in his discussion of the evolution of intelligence, contribute not unimportantly to our all too meagre knowledge of the mental life of the anthropoid apes.

My own results, viewed in the light of what one may learn from the literature, stand out as unique because of the method of research. Never before, so far as I have been able to learn, has any ape been subjected to observation under systematically controlled conditions for so long a period as six months. Moreover, my multiple-choice method has the merit of having yielded the first curve of learning for an anthropoid ape. This fact is especially interesting when one considers the nature of the particular curve. For so far as one may say by comparing it with the curves for various learning processes exhibited by other mammals, it is indicative of ideation of a high order, and possibly of reasoning. I do not wish to exaggerate the importance of my results, for as contrasted with what might be obtained by further study, and with what must be obtained if we are adequately to describe the mind of the orang utan, they are meager indeed.

Especially noteworthy, as evidences of ideation, in the results yielded by the multiple-choice method are (1) the use by the orang utan of several different methods in connection with each problem; (2) the suddenness of transition from method to method; (3) the final and perfect solution of problem 1 without diminution of the initial errors; (4) the dissociation of the act of turning in a circle from that of standing in front of a particular box.

To these features of behavior others of minor importance might be added. But as they have been sufficiently emphasized in the foregoing detailed descriptions, I need only repeat my conclusion, from the summation of evidence, that this young orang utan exhibited numerous free ideas and simple thought processes in connection with the multiple-choice experiment. His ultimate failure to solve the second problem is peculiarly interesting, although in the light of other features of his behavior by no means indicative of inferior intelligence.

The various supplementary experimental tests which I employed are in no wise importantly distinguished from those used by other observers. The box stacking experiment has, according to my private information, been used by Koehler. It is obviously important that such tests be applied in the same manner to individuals not only of the different genera of anthropoid apes, but of different ages, sex, and condition of training.

The box stacking experiment, although it yielded complete success only as a result of suggestion on my part, proved far more interesting during its progress than any other portion of my work. In connection with it, the orang utan exhibited surprisingly diverse and numerous efforts to meet the demands of the situation. It is fair to characterize him as inventive, for of the several possible ways of obtaining the banana which were evident to the experimenter, the ape voluntarily used all but two or three, and one of these he subsequently used on the basis of imitation.

Had Julius been physically and mentally mature, my results would undoubtedly have been much more impressively indicative of ideas, but even as matters stand, the survey of my experimental records and supplementary notes force me to conclude that as contrasted with the monkeys and other mammals, the orang utan is capable of expressing free ideas in considerable number and of using them in ways highly indicative of thought processes, possibly even of the rational order. But contrasted with that of man the ideational life of the orang utan seems poverty stricken. Certainly in this respect Julius was not above the level of the normal three-year-old child.

In common with other observers, I have had the experience of being profoundly impressed by the versatility of the ape, and however much I might desire to disprove the presence of free ideas and simple reasoning processes in the orang utan, I should feel bound to accept many of the results of my tests as evidences of such experience.

I have attempted to indicate briefly the historical setting of my investigation. I propose, now, in the concluding section, to look forward from this initial research and to indicate as well as I may in a few words the possibilities of results important for mankind from the thorough study of the monkeys and anthropoid apes.

VII

PROVISION FOR THE STUDY OF THE PRIMATES, AND ESPECIALLY THE MONKEYS AND ANTHROPOID APES[1]

I should neglect an important duty as well as waste an opportunity if in this report I did not call attention to the status of our knowledge concerning the monkeys and apes and present the urgent need of adequate provision for the comparative study of all of the primates.

Although for centuries students of nature have been keenly interested in the various primates, the information which has been accumulated is fragmentary and wholly inadequate for generally recognized scientific and practical needs. There is a voluminous literature on many aspects of the organization and lives of the monkeys and apes, but when one searches in it for reasonably connected and complete descriptions of the organisms from any biological angle, one is certain to meet disappointment.

Concerning their external characteristics we know much; and our classifications, if not satisfactory to all, are at least eminently useful. But when one turns to the morphological sciences of anatomy, histology, embryology, and pathology, one discovers great gaps, where knowledge might reasonably be expected. Even gross anatomy has much to gain from the careful, systematic examination of these organisms. With still greater force this statement applies to the studies of finer structural relations. Little is known concerning the embryological development and life history of certain of the primates, and almost nothing concerning their pathological anatomy.

Clearly less satisfactory than our knowledge of structure is the status of information concerning those functional processes which are the special concern of physiology and pathology. Certain important experimental studies have been made on the nervous system, but rarely indeed have physiologists dealt systematically with the functions of other systems of organs. There

---

[1] Much of the material of this section was published originally in *Science* (Yerkes, 1916).

Sub-divisions of the Order Primates

Order	*Sub-orders*	*Families*
Primates	a. Prosimii (Lemurs and Aye-Ayes)	
	b. Anthropoidea	i. Hapalidae (Marmosets)
		ii. Cebidae (Howling Monkeys, Tee Tees, Squirrel Monkeys, Spider Monkeys, and Capuchin Monkeys)
		iii. Cercopithecidae (Baboons and Macaques)
		iv. Simiidae (Gibbons, Orangs, Chimpanzees, and Gorillas)
		v. Hominidae (Man)

are almost no satisfactory physiological descriptions of the monkeys, anthropoid apes, or lower primates.

When we turn to the science of genetics we meet a similar condition, for the literature reveals only scattered bits of information concerning heredity in the primates. No important experimental studies along genetic lines have been made with them, and such general observations from nature as are on record are of extremely uncertain value. Were one to insist that we know nothing certainly concerning the relation of heredity in other primates than man, the statement could not well be disputed.

Occasionally in recent years students of human diseases have employed monkeys or apes for experimental tests, but aside from the isolated results thus obtained, extremely little is known concerning the diseases peculiar to the various types of infrahuman primates or the significant relations of their diseases to those of man.

Next in order of extent to our morphological knowledge of these organisms is that of their behavior, mental life, and social relations. But certainly no one who is conversant with the behavioristic, psychological and sociological literature could do otherwise than emphasize its incompleteness and inadequacy. For our knowledge of behavior has come mostly from naturalistic observation, scarcely at all from experimentation; our knowledge of social relations is obviously meager and of uncertain value; and finally, our knowledge of mind is barely more than a collection of carelessly drawn inferences.

This picture of the status of scientific work on the primates, although not overdrawn, will doubtless surprise many readers, and even the biologist may find himself wondering why we are so ignorant concerning the lives of the organisms most nearly akin to us, and naturally of deepest interest to us. The reasons are not far to seek. Most scientific investigators are forced by circumstances to work with organisms which are readily obtained and easily kept. The primates have neither of these advantages, for many, if not most of them, are expensive to get and either difficult or expensive to keep in good condition. Clearly, then, our ignorance is due not to lack of appreciation of the scientific value of primate research but instead to its difficultness and costliness.

Strangely enough, the practical importance of knowledge of the primates has seldom been dwelt upon even by those biologists who are especially interested in it. It is, therefore, appropriate to emphasize the strictly human value of the work for which I am seeking provision.

During the past few years it has been abundantly and convincingly demonstrated that knowledge of other organisms may aid directly in the solution of many of the problems of experimental medicine, of physiology, genetics, psychology, sociology, and economics. In the light of these results, it is obviously desirable that all studies of infrahuman organisms, but especially those of the various primates, should be made to contribute to the solution of our human problems.

To me it seems that thoroughgoing knowledge of the lives of the infrahuman primates would inevitably make for human betterment. Through the science of genetics, as advanced by experimental studies of the monkeys and anthropoid apes, practical eugenic procedures should be more safely based and our ability to predict organic phenomena greatly increased. Similarly, intensive knowledge of the diseases of the other primates in their relations to human diseases should contribute importantly to human welfare. And finally, our careful studies of the fundamental instincts, forms of habit formation, and social relations in the monkeys and apes should lead to radical improvements in our educational methods as well as in other forms of social service.

Along theoretical lines, no less than practical, systematic research with the primates should rapidly justify itself, for upon its results must rest the most significant historical or genetic biological descriptions. It is beyond doubt that genetic psychology can best be advanced to-day by such work, and what is obviously true of this science is not less true of all the biological sciences which take account of the developmental or genetic relations of their events.

In view of the probable values of increasingly complete accounts of primate life, it seems far from extravagant to insist that the securing of adequate provision for systematic and long continued research is the most important task for our generation of biologists and the one which we shall be least excusable for neglecting. Indeed, when one stops to reflect concerning the situation, it seems almost incredible that the task has not been accomplished.

Some ten years ago Professor John B. Watson (1906) entered a plea for the founding of a station for the experimental study of behavior. He made no special mention of work with the monkeys and apes, but it is clear from the problems which he enumerates that he would consider them most important subjects for observation. Professor Watson's plea has apparently been forgotten by American biologists, and it seems not inappropriate to revive it at this time. For surely we have advanced sufficiently along material and scientific lines during the last ten years to render possible the realization of his hope.

To my knowledge, only one definite attempt has thus far been made to gain special provision for the study of the primates. Somewhere about the year 1912 there was established on Tenerife, one of the Canary Islands, a modest station for the study of the anthropoid apes. I have already referred to it briefly on page 1. The plan and purpose of this station, which is of German origin, have been presented briefly by Rothmann (1912). From personal communications I know that a single investigator has been in residence at the station since its founding and that psychological and physiological results of value have been obtained, but no published reports have come to my attention.

When I first heard of the existence of the German anthropoid station I naturally thought of the possibility of coöperative work, but the events of the past two years have rendered the chances of coöperation so remote that it now seems wholly desirable and indeed imperative to seek the establishment of an American station, which, unlike the German station, shall provide adequately not only for the study of the anthropoid apes but for that of all of the lower primates. It should be the function of such a station or research institute (1) to bring together and correlate all the information at present available; (2) to fill in existing gaps observationally and thus complete and perfect our knowledge of these organisms; (3) to seek to bring all available information to bear upon the problems of human life.

Hitherto the unsatisfactoriness of progress has been due to the lack of a definite plan and program. Every investigator has gone his own way, doing what little his personal means and opportunity rendered possible. The time has at last come when concerted action seems feasible as well as eminently desirable. I am therefore offering a plan and program which, if wisely developed, should lead ultimately to fairly complete and practically invaluable knowledge of the lives of all of the primates.

There should be provided in a suitable locality a station or research institute which should offer adequate facilities (1) for the maintenance of various types of primate in normal, healthy condition; (2) for the successful breeding and rearing of the animals, generation after generation; (3) for systematic and continuous observation under reasonably natural conditions; (4) for experimental investigations from every significant biological point of view; (5) for profitable coöperation with existing biological institutes or departments of research throughout the world.

The station should be located in a region whose climate is highly favorable to the life of many of the lower primates as well as to that of man. Such a location is by no means easy to find. Because of my intense interest in the subject, I have, during the past five years, prospected in various parts of the world for a satisfactory site. I shall now attempt to indicate the chief requirements and also the foremost advantages and disadvantages of several regions which have been considered.

It is first of all requisite that the climate be such as to agree with the organisms to be studied and such, also, as to render their breeding normal and dependable. Second in importance is its satisfactoriness for the life and scientific productiveness of the observer. While certain tropical localities would meet the first requirement perfectly, they would prove extremely unsatisfactory for research activity. It therefore seems essential to find a region whose climate shall reasonably meet the needs of the experimenter while adequately meeting those of the animals to be studied.

A further factor which has important bearing upon the productiveness of the observer is the degree of isolation from civilization and from other scientific work. No scientist can long work effectively, even in a reasonably healthy and stimulating climate, if entirely cut off from similar interests and activities. It is therefore desirable, if at all possible, to discover a location in the midst of civilization and with reasonably good opportunities for scientific associations.

With these several desiderata before us, I shall call attention to a number of possible sites for a station, several of which I have visited. Southern California, and especially the portion of the State between Santa Barbara and San Diego, promises fairly well. It is definitely known that certain, if not all, species of monkey will breed there fairly satisfactorily, and although it has not yet been demonstrated, there is no reason to suppose that in certain regions the anthropoid apes might not also be kept in perfect health and successfully bred. The main advantages of this general region are (a) a climate which promises to be reasonably satisfactory for many if not all of the primates; (b) admirable climatic conditions for investigators; (c) wholly satisfactory scientific and cultural environment for the staff of a station. The most significant disadvantages are (a) a temperature, which is at times a trifle too low for the comfort of certain of the monkeys and apes. It is by no means certain, however, that they would not usually adapt themselves to it. (b) The necessity of importing all of the animals and of having to rely upon successful acclimatization. Of course it is to be assumed that importation would be necessary only at the outset of such work, since the animals later should replenish themselves within the confines of the station.

Florida offers possibilities somewhat similar to those of southern California, but as I have not had opportunity to examine the conditions myself, I can say only that in view of such information as is available the advantage seems to be greatly in favor of the latter.

Cuba, Jamaica, Porto Rico, and for that matter, several of the West Indies, offer possible sites for a successful station. I have reasonably intimate personal knowledge only of the conditions in Jamaica. The major advantages in the West Indies are (a) suitable climatic conditions and food supply for the animals; and (b) reasonably satisfactory climatic conditions for the staff. These are, however, more than counterbalanced in my opinion by the following serious disadvantages: (a) the relative isolation of the investigators from their fellow scientists; (b) the necessity of importing all of the animals originally used; (c) the risk of destruction of the station by storms.

It is definitely known that anthropoid apes as well as monkeys can be successfully kept, bred, and reared in the West Indies. During the past year, on the estate of Doña Rosalía Abreu, near Havana, Cuba, a chimpanzee was born in captivity. A valuable account of this important event and of the young ape has been published by Doctor Louis Montané (1915). It therefore seems practically certain that regions could be found readily on Jamaica, Porto Rico, or smaller islands, which would be eminently satisfactory for the breeding of apes.

There are obvious reasons why an American station for the study of the primates should be located on territory controlled by the United States Government, and if a tropical location proves necessary, it would probably be difficult to find more satisfactory regions, aside from the inconveniences and risk of importation and the relative isolation of the investigators, than are available on Porto Rico.

I have not seriously considered the possibility of locating an American station on the continent of Africa, for although two of the most interesting and important of the anthropoid apes, the gorilla and the chimpanzee, are African forms, while many species of monkey are either found there or could readily be imported, it has seemed to me that the islands of the West and East Indies and the portions of the United States referred to above are much to be preferred over anything available in Africa.

In the East, Borneo, the Philippine Islands, and Hawaii are well worth considering. Borneo is the home of the gibbon and of at least one species of orang utan, and in addition to these important assets, it presents the advantages of (a) a wholly suitable climate and food supply for monkeys and apes; and (b) climatic conditions for investigators which, I am informed by scientific friends, are nearly ideal. For investigators the most serious disadvantage here, as in all other parts of the East, would be the isolation from other scientific work and workers.

The possibilities of Central America I considered several years ago when it seemed to me possible that work might profitably be done with monkeys and apes on the Canal Zone. The advantages are (a) a climate which promises fairly well for the animals; and (b) reasonable accessibility from the United States. The disadvantages are (a) a far from ideal climate for long continued scientific work; and (b) an environment which from the cultural and scientific point of view leaves much to be desired.

Were a permanent psycho-biological station for the study of the primates to be established in southern California, it would, even though wholly satisfactory conditions for the breeding, rearing, and studying of the animals were maintained, furnish more or less inadequate opportunity for the observation of the animals under free, natural conditions. It would therefore be necessary to supplement the work of such a station by field work in Borneo, Sumatra, Africa, India, South America, and such other regions as the species of organism under consideration happen to inhabit.

Considering equally the needs of the experimenter and the demands of the animals, it seems to me reasonable to conclude that southern California should be definitely proved unsuitable before a more distant site were selected. For the information which I have been able to accumulate convinces me that it would in all probability be possible successfully to breed and keep the primates there, and it is perfectly clear that in such event the output of a station would be enormously greater because of the more favorable conditions for research than in any tropical region or in a more isolated location.

Assuming that satisfactory provision in the shape of a scientific establishment for the study of the primates in their relations to man were available, the following program might be followed: (1) Systematic and continuous studies of important forms of individual behavior, of social relations, and of mind;

(2) experimental studies of physiological processes, normal and pathological, and especially of the diseases of the lower primates, in their relations to those of man; (3) studies of heredity, embryology, and life history; (4) research in comparative anatomy, including gross anatomy, histology, neurology, and pathological anatomy.

Each of these several kinds of research should be in progress almost continuously in order that no materials or opportunities for observation be needlessly wasted. Because of the nature of the work, it would be necessary to provide, first of all, for those functional studies which demand healthy and normally active organisms, whose life history is intimately and completely known. This is true of all studies in behavior, whether physiological, psychological, or sociological. Simultaneously with behavioristic observations and often upon the same individuals, genetic experiments might be conducted. This would be extremely desirable because of the relatively long periods between generations. After the usefulness of an animal in behavioristic or genetic inquiries had been exhausted, it might be made to render still further service to science in various experimental physiological, or medical inquiries. And finally, the same individual might ultimately be used for various forms of anatomical research. Thus, it is clear that the scientific usefulness of a lemur, a monkey, or an ape might be maintained at a high level throughout and even beyond the period of its life history.

The program thus briefly sketched would provide either directly or indirectly for work on every aspect of primate life. Especially important would be the intimacy of interest and coöperation among investigators, for the comparative method should be applied consistently and to the limit of its value. The results of various kinds of observation should be correlated so that there should ultimately emerge a unitary and practically valuable account of primate life, to replace the patchwork of information which we now possess.

Because of the costliness of maintaining and breeding the monkeys and apes, it is especially desirable that the several kinds of research mentioned above should be conducted. Indeed, it would seem inexcusably wasteful to attempt to maintain a primate or anthropoid station for psychological observations alone, or for any other narrowly limited biological inquiry.

Furthermore, the station should be permanent, since for many kinds of work it would be essential to have intimate knowledge of the life history and descent of an individual. With the lower primates, a generation might be obtained in from two to five years; with the higher, not more frequently, probably, than from ten to fifteen years. It therefore seems not improbable that the value of the work done in such a station would continue to increase for many years and would not reach its maximum short of fifty or even one hundred years.

A staff of several highly trained and experienced biologists would be needed. The following organization is suggested as desirable, although, as indicated below, not necessarily essential in the beginning: (1) An expert especially interested in the problems of behavior, psychology, and sociology, with keen appreciation of practical as well as of theoretical problems; (2) an assistant trained especially in comparative physiology; (3) an expert in genetics and experimental zoölogy; (4) an assistant with training and interests in comparative anatomy, histology, and embryology; (5) an expert in experimental medicine, who could conduct and direct studies of the diseases of man as well as of the lower primates and of measures for their control; (6) an assistant trained especially in pathology and neurology.

To this scientific staff of six highly trained individuals there should be added a business manager, a clerical force of three individuals, a skilled mechanician, a carpenter, and at least four laborers.

The annual expenditures of an institute with such a working staff, would in southern California, approximate fifty thousand dollars. It would therefore be necessary that it have an endowment of approximately one million dollars.

In the absence of this foundation it would, of course, be possible to make a reasonably satisfactory beginning on the work which has been outlined in the following less expensive manner. A working plant might be established, on ground rented or purchased at a low figure, for about ten thousand dollars; the salary of a director, assistants, a clerical helper, and combined mechanic and laborer might be estimated at the same figure; the cost of animals and of maintenance of the plant would approximate five thousand dollars. Thus, we should obtain as an estimate of the expenditures for the first year twenty-five thousand dollars. Without expansion, the work might be conducted during the second year for fifteen thousand dollars, and subsequently it might be curtailed or expanded, resources permitting, according as results achieved and in prospect justified.

An institute established on such a modest basis as this still might render largely important scientific service through its own research and through organized coöperation with other existing research establishments. Thus, for example, supposing that behavioristic, psychological, sociological, and genetic inquiries were conducted in the institute itself, animals might be supplied on a mutually satisfactory basis to institutes for experimental medicine, for physiological research, and for anatomical studies. Under such conditions, it is conceivable that extremely economical and good use might be made of all the available primate materials. But it is not improbable that even coöperative research would prove on the whole more profitable, except possibly in the case of morphological work, if investigators could conduct their studies in the institute itself rather than in distant laboratories. In any event, the idea of coöperation should be prominent in connection with the organization of a research station for the study of the primates. For thus, evidently, scientific achievement in connection with these important types of animal might be vastly increased over what would be possible in a single relatively small institution with a limited and necessarily specialized staff of workers.

Despite the fact that biologists generally recognize the importance of the work under consideration and are eager to have it done, it is perfectly certain that we shall accomplish nothing unless we devote ourselves confidently, determinedly and unitedly, with faith, vision, and enthusiasm, to the realization of a definite plan. Our vision is clear,—if we are to gather and place at the service of mankind adequate comparative knowledge of the life of the primates and if we are to make this possible harvest of scientific results count for human betterment, we must bend all our efforts to the establishment of a station or institute for research.

VIII

BIBLIOGRAPHY

Brehm, A. Tierleben.
1888.

Coburn, C. A. and Yerkes, R. M. A study of the behavior of the crow, *Corvus*
1915. *Americanus* Aud. by the multiple-choice method. *Journal of Animal Behavior,* **5**, 75-114.

Cuvier, Frederic. Description d'un orang-outang, et observations sur ses facultées
1810. intellectuelles. *Annales du Museum d'Histoire naturelle,* **16**, 46-65.

Darwin, C. Origin of species.
1859.
1871. The descent of man, and selection in relation to sex.

Elliot, D. G. A review of the primates. New York.
1913.

Franz, S. I. On the functions of the cerebrum: the frontal lobes. *Archives of*
1907. *Psychology,* no. 2, 1-64.
1911. On the functions of the cerebrum: the occipital lobes. *Psychological Monographs,* **13**,. 1-118.
1913. Observations on the preferential use of the right and left hands by monkeys. *Journal of Animal Behavior,* **3**, 140-144.

Garner, R. L. The speech of monkeys. London.
1892.
1896. Gorillas and chimpanzees. London.
1900. Apes and monkeys; their life and language. Boston and London.

Haggerty, M. E. Imitation in monkeys. *Journal of Comparative Neurology and*
1909. *Psychology,* **19**, 337-455.

Hamilton, G. V. A study of trial and error reactions in mammals. *Journal of*
1911. *Animal Behavior,* **1**, 33-66.
1914. A study of sexual tendencies in monkeys and baboons. *Journal of Animal Behavior,* **4**, 295-318.

Hirschlaff, L. Der Schimpanzee Konsul. *Zeit. f. päd. Psy.,* **7**, **1**.
1905.

Hobhouse, L. T. Mind in evolution. London.
1915.

Holmes, S. J. Evolution of intelligence. New York.
1911.

Hrdlicka, A. Anatomical observations on a collection of orang skulls from west-
1907. ern Borneo; with a bibliography. *Proceedings of the United States National Museum,* **31**, 539-568.

Kinnaman, A. J. Mental life of two *Macacus rhesus* monkeys in captivity. *Amer-*
1902. *ican Journal of Psychology,* **13**, 98-148, 173-218.

Lashley, K. S. and Watson, J. B. Notes on the development of a young monkey.
1913. *Journal of Animal Behavior,* **3**, 114-139.

Möbius, K. Zur psychologie des Schimpanse. *Zool. Garten.,* **8**, 279-280.
1867.

Montané, L. Un chimpancé Cubano. Havana, *El Siglo XX.*
1915.

Morgan, C. L. Introduction to comparative psychology. London.
1906.

Pfungst, O. Zur Psychologie der Affen. *Ber. über d. V. Kongress f. exp. Psychol.,*
1912. 200-205.

ROMANES, G. J. Mental evolution in animals. New York.
1900.
ROTHMANN, M. Ueber die errichtung einer Station zur psychologischen und hirn-
1912. physiologischen Erforschung der Menschenaffen. *Berliner klin. Wochenschr.*, Nr. 42.
SHEPHERD, W. T. Some mental processes of the rhesus monkey. *Psychological*
1910. *Monographs*, **12**, 1-61.
1915. Some observations on the intelligence of the chimpanzee. *Journal of Animal Behavior*, **5**, 391-396.
SOKOLOWSKY, A. Beobachtungen über die Psyche der Menschenaffen. Frankfurt
1908. a. M.
THORNDIKE, E. L. The mental life of the monkeys. *Psychological Monograph*,
1901. **3**, 1-57.
1911. Animal intelligence. New York.
WALLACE, A. R. The Malay Archipelago. London.
1869.
WASHBURN, M. F. The animal mind. New York.
1908.
WATSON, J. B. The need of an experimental station for the study of certain prob-
1906. lems in animal behavior. *Psychological Bulletin*, **3**, 149-156.
1908. Imitation in monkeys. *Psychological Bulletin*, **5**, 169-178.
1909. Some experiments bearing upon color vision in monkeys. *Journal of Comparative Neurology and Psychology*, **19**, 1-28.
WITMER, L. A monkey with a mind. *Psychological Clinic*, **3**, 179-205.
1909.
1910. Intelligent imitation and curiosity in a monkey. *Psychological Clinic*, **3**, 225-227.
YERKES, R. M. The study of human behavior. *Science*, **39**, 625-633.
1914.
1915. Maternal instinct in a monkey. *Journal of Animal Behavior*, **5**, 403-405.
1916. Provision for the study of the monkeys and apes. *Science*, **43**, 231-234.
YERKES, R. M. and COBURN, C. A. A study of the behavior of the pig *Sus scrofa*
1915. by the multiple-choice method *Journal of Animal Behavior*, **5**, 185-225.

Behavior Monographs

VOLUME 1

No. 1 The development of certain instincts and habits in chicks. By Frederick S. Breed. Pp. iv + 78, $1.00, postpaid.

No. 2 Methods of studying vision in animals. By Robert M. Yerkes and John B. Watson. Pp. iv + 90, $1.25, postpaid.

No. 3 An experimental study on the death-feigning of Belostoma (–Zaitha Aucct.) flumineum Say and Nepa apiculata Uhler. By Henry H. P. Severin and Harry C. Severin. Pp. iii + 47, $.65, postpaid.

No. 4 The biology of Physa. By Jean Dawson. Pp. iii + 120, $1.50, postpaid.

No. 5 The function of the vibrissae in the behavior of the white rat. By Stella Burnham Vincent. Pp. iv + 81, $1.15, postpaid.

VOLUME 2

No. 6 The delayed reaction in animals and children. By Walter S. Hunter. Pp. v + 86, $1.15.

No. 7 The Canada porcupine: a study of the learning process. By Leroy Walter Sackett. Pp. iii + 84, $1.15.

No. 8 Audition and habit formation in the dog. By Harry Miles Johnson. Pp. iv + 78, $1.00.

No. 9 Habit formation in a strain of albino rats of less than normal brain weight. By Gardner Cheney Basset. Pp. iv + 46, $.65.

No. 10 Distribution of effort in learning in the white rat. By John Linck Ulrich. Pp. iii + 51, $.65.

No. 11 The effect of age on habit formation in the albino rat. By Helen B. Hubbert. Pp. v + 55, $.65.

VOLUME 3

No. 12 The mental life of monkeys and apes: a study of ideational behavior. By Robert M. Yerkes. Pp. iv + 145, $1.50.

Directory of American Psychological Periodicals

American Journal of Psychology—Worcester, Mass.: Florence Chandler.
Subscription $5. 600 pages annually. Edited by G. Stanley Hall.
Quarterly. General and experimental psychology. Founded 1887.

Pedagogical Seminary—Worcester, Mass.: Florence Chandler.
Subscription $5. 575 pages annually. Edited by G. Stanley Hall.
Quarterly. Pedagogy and educational psychology. Founded 1891.

Psychological Review—Princeton, N. J.: Psychological Review Company.
Subscription (with Psychological Bulletin) $5. 480 pages annually.
Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin—Princeton, N. J.: Psychological Review Company.
Subscription $2.75. 480 pages annually. Psychological literature.
Monthly. Founded 1904. Edited by Shepherd I. Franz.

Psychological Monographs—Princeton, N. J.: Psychological Review Company.
Subscription $4. 500 pages per vol. Founded 1895. Ed. by James R. Angell.
Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.: Psychological Review Company.
Subscription $1. 200 pages. Founded 1895. Edited by Madison Bentley.
An annual bibliography of psychological literature.

Journal of Philosophy, Psychology and Scientific Methods—New York:
Science Press. Bi-weekly. 728 pages per volume. Founded 1904.
Subscription $3. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Substation 84, New York: Archives of Psychology.
Subscription $5. 600 pp. ann. Founded 1906. Edited by R. S. Woodworth.
Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology—Boston: Richard G. Badger.
Subscription $4. 480 pages annually. Edited by Morton Prince.
Bi-monthly. Founded 1906. Entire field of abnormal psychology.

Psychological Clinic—Philadelphia: Psychological Clinic Press.
Subscription $1.50. 280 pages annually. Edited by Lightner Witmer.
Monthly (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Training School Bulletin—Vineland, N. J.: The Training School.
Subscription $1. 160 pages annually. Ed. by E. R. Johnstone. Founded 1904.
Monthly (10 numbers). Psychology and training of defectives.

Journal of Religious Psychology—Worcester, Mass.: Louis N. Wilson.
Subscription $3. 480 pages annually. Founded 1904. Edited by G. Stanley Hall.
Published without fixed dates. 4 numbers constitute a volume.

Journal of Race Development—Worcester, Mass.: Louis N. Wilson.
Subscription $2. 460 pages annually. Founded 1910.
Quarterly. Edited by George H. Blakeslee and G. Stanley Hall.

Journal of Educational Psychology—Baltimore: Warwick & York.
Subscription $2.50. 600 pages annually. Founded 1910.
Monthly (10 numbers). Managing Editor, J. Carleton Bell.
(Educational Psychology Monographs. Edited by Guy M. Whipple.
Published separately at varying prices. Same publishers.)

Journal of Animal Behavior—Cambridge, Mass.: Emerson Hall.
Subscription $3, Foreign $3.50. 450 pages annually. Founded 1911.
Bi-monthly. Robert M. Yerkes, Managing Editor.

The Behavior Monographs—Cambridge, Mass.: Emerson Hall.
Subscription $3. 450 pages per volume. Edited by John B. Watson.
Published without fixed dates, each number a single research.

Psychoanalytic Review—New York: 64 West 56th Street.
Subscription $5. 500 pages annually. Psychoanalysis.
Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J.: Psychological Review
Company. 480 pages annually. Experimental. Founded 1916.
Subscription $3. Bi-monthly. Edited by John B. Watson.

THE BRANDOW PRINTING COMPANY
ALBANY, N. Y

BEHAVIOR MONOGRAPHS

Volume 3, Number 2, 1916 Serial Number 13

Edited by JOHN B. WATSON
The Johns Hopkins University

A Study of Perseverance Reactions in Primates and Rodents

G. V. HAMILTON

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

The Journal of Animal Behavior

An organ for the publication of studies concerning the instincts, habits and intelligence of organisms

The Journal contains a Department of Notes in which appear brief accounts of especially interesting and valuable observations of behavior.

Published bi-monthly at Cambridge, Boston, Mass., by Henry Holt and Company, New York. Each volume contains at least 450 pages with plates and text-figures.

The subscription price is $5.00 per volume (foreign subscription $5.50) postpaid.

Manuscripts, subscriptions, and all business correspondence should be addressed to

THE JOURNAL OF ANIMAL BEHAVIOR
Emerson Hall, Cambridge, Massachusetts

Behavior Monographs

For the publication of studies in behavior and intelligence which are too lengthy or too expensive for acceptance by The Journal of Animal Behavior

Published at irregular intervals at Cambridge, Boston, Mass., in connection with the Journal of Animal Behavior, by Henry Holt and Company, New York. Each volume contains approximately 450 pages with plates and text-figures.

The subscription price is $3.00 per volume (foreign subscription $3.50) postpaid.

Monographs may be purchased separately at prices varying with the cost of manufacture.

Manuscripts and inquiries concerning terms of publication should be addressed to the Editor of the Behavior Monographs,

JOHN B. WATSON,
The Johns Hopkins University,
Baltimore, Md.

Subscription to The Journal of Animal Behavior and the Behavior Monographs should be sent to Emerson Hall, Cambridge, Massachusetts.

BEHAVIOR MONOGRAPHS

Volume 3, Number 2, 1916 Serial Number 13

Edited by JOHN B. WATSON
The Johns Hopkins University

A Study of Perseverance Reactions in Primates and Rodents

G. V. HAMILTON
Montecito, California

TWO FIGURES

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E STECHERT & CO., London, Paris and Leipzig, Foreign Agents

CONTENTS

A STUDY OF PERSEVERANCE REACTIONS IN PRIMATES AND RODENTS

G. V. HAMILTON
Montecito, California

INTRODUCTION

The present study is a continuation of a previously published one (1), which was also devoted to an investigation of perseverance (trial and error) reactions. The method that was employed in both studies requires the subject to find his way out of an enclosure from which there are four apparently possible exit places. During a series of 100 trials each of the four exit places affords escape 25 times, and during any given trial only one of these places can be used for this purpose. The right exit place to try for escape from the enclosure varies from trial to trial, i.e., it is useless, during any given trial, to seek exit from the enclosure by way of the place that afforded escape during the immediately preceding trial. While the experimenter's order of varying the right place to try for exit contains this one negatively directive feature, it contains no specifically directive positive feature that has thus far been discovered by any of the subjects. The appearance of the apparatus, as it is seen from within by the subject, affords no clue whatsoever as to whether a given exit place is a right or a wrong one to try until actual trial is made. It follows, therefore, that this method operates against the formation of a specifically adaptive habit, i.e., a habit which would involve no unproductive quests of an avenue of escape from the apparatus.

The relation of this method to Yerkes' (3) multiple choice method requires more explicit definition than it has yet been given. Yerkes' method, which is of later development than mine, embodies some of its secondary features but has a quite different intention. Both methods exclude all specifically directive *sensory* stimuli, and require the subject to vary the place through which he escapes from the apparatus from trial to trial.

But Yerkes' method, unlike mine, supplies a specifically directive feature which is deliberately introduced with the intention of enabling the subject to form a definite, specifically adaptive habit. The construction of his apparatus is such that now one, now another group of apparently possible exit places (or their equivalents) can be exposed to the subject, with trial to trial variations of the position and constitution of such groups. For example, in one series of trials by the Yerkes' method any three, five, seven, nine or eleven of twelve exit places may be exposed to the subject, who will invariably find that the middle exit place of any three, five, seven, nine or eleven that may be exposed is the right one to try. In other series it is the place at the extreme left or at the extreme right of whatever group the experimenter may expose that should be sought for escape.

Watson's comparison of the two methods (5) implies a similarity of intention which does not exist. While it is true that Yerkes' method, like most methods that are employed for the study of mammalian behavior, affords information as to various types of searching reaction, it does so only incidentally, and since it encourages habit formation rather than prolonged variability of response its intention is practically opposite to that of my method.

Watson's (5) reference to my method as a multiple stimuli one, is in my opinion, somewhat misleading. One would not refer to ordinary maze experiments as conforming to the multiple stimuli type, since they employ confinement as the primary and unvarying stimulus to reaction. Even where this stimulus is reinforced by the introduction of features which render confinement particularly apt to lead to prompt reaction (e.g., electrical stimulation within the enclosure) it remains, in a sense, a single, general stimulus. The descriptions of my method and apparatus that are given in subsequent pages will show that unless the term "multiple stimuli method" may properly be used to designate maze experiments, my method cannot be regarded as a multiple stimuli one.

The relation of my work to a general behavioristic program cannot be satisfactorily defined without reference to the standpoint from which I have proceeded to a study of behavior.

A mammalian's initial adjustments to a situation which is not characteristically encountered by the members of its species are apt to consist of, (a) various non-adaptive activities, and (b) a more or less accidental activity which terminates reaction by withdrawing the subject from the stimuli supplied by the situation. If the situation be frequently encountered the subject tends more or less gradually to abandon the non-adaptive activities and to develop a habit of manifesting only the adaptive one whenever the situation is encountered anew. It is in this familiar circumstance that the behaviorist finds many of his problems. Unfortunately, the non-adaptive activities, which are accounted "errors," are usually given no more attention than is involved in recording their number, duration, distribution and mode (gradualness or abruptness) of disappearance. The experimenter's interest is apt to center in what is significant for estimating the reactive value of the stimuli involved in terms of the subject's sensory equipment or of its capacity to learn. That behaviorists have displayed so little interest in the qualitative aspect of "errors" (non-adaptive activities) is in part due, I believe, to the influence of pedagogical traditions which direct attention toward the positive determinants of learning.

My interest in the qualitative aspect of non-adaptive activities is largely due to the fact that I have approached the study of behavior from the standpoint of the clinical psychopathologist. In spite of the fact that I have never been able to assimilate the doctrines of the Vienna and Zurich schools of psychoanalysis to my own experience and conceptions without many heretical reservations and qualifications, I am indebted to these doctrines for certain behavioristic concepts which, it seems to me, underlie them. If I do not misconceive Freud's and Jung's writings they assume that every reaction, no matter how inappropriate it may be as an attempt at adjustment to the particular situation that elicits it, is the expression of an innate tendency which enters as a functional unit into the composition of the organism's total reactive equipment. Although a given reaction may prove to be inimical to the welfare of both the individual and his race, the tendency of which it is an expression possesses in itself conservative value either for the individual or for his race, or for both.

The innate tendencies of an organism are disclosed in the first instance, of course, by his reactions and are identified through studies of them. But reactions do not always stand out as clean-cut entities: response often consists of manifold activities which lack a common objective reference. The task of identifying relatively separate types of reaction is, therefore, a difficult one, and is not apt to appeal to the investigator who is impatient to have done with roughly exploratory methods and to apply only highly exact ones. At this point it is to be remembered that in a technological field of comparative psychology—psychoanalytic psychopathology—a rough and admittedly often unnecessarily crude exploration is currently revealing material for behavioristic investigation, the existence of which was barely if at all suspected by certain psychopathologists who were and still are committed to the exclusive use of highly exact methods. I can think of no better justification of a method that is exploratory in intention and that seeks to prepare a way for the identification of reactive tendencies than is contained in Holt's (2) statement that Freud has given us "the first key which psychology has ever had which fitted, and moreover I believe that it is the only one that psychology will ever need."

Apparatus for Rodents.—Figure 1 gives a view of the apparatus as a whole. Its essential feature is a semicircular enclosure, which is built on a table top. The walls of this enclosure are interrupted by five apertures. The one that divides the straight part of the wall into two equal parts is shown as blocked by a box in fig. 1, and can best be seen in fig. 2. This aperture is for entrance. The curved part of the wall is broken by four apertures, each of which opens into its own exit way or alley.

Figure 2 gives the plan of the entire table top, and is therefore a floor plan of the semicircular enclosure, the entrance and exit alleys, the bases of the wall and the parts of the table top that lie outside the wall. The shaded parts of the figure marked with W's represent the bases of the enclosure walls, which are also the walls of the alleys. The bases or bottoms of these box-like walls are of 1-inch redwood, as are also their tops, which are of the same dimensions. They afford attachment for vertical sides of sheet tin. The horizontal dimensions of the walls are given in fig. 2. The sides have a uniform elevation of 15 cm. The curved wall forms a half circle, and has a 36 cm. radius. Reference to fig. 2 will show that the exit alleys are equidistant from the midpoint of the inner boundary line of the exit alley, hence the subject, on first entering the apparatus, is subjected to none of the influences that might be contained in inequalities of distance between his starting point and the four possible places for exit.

The floor of the semicircular space and that of the entrance and exit alleys consist of alternate 3 mm. strips of iron and 6 mm. strips of paraffine. The first, third, fifth, etc., iron strips are connected for continuous electrical conduction by a copper wire, which is soldered to one end of each strip. The opposite ends of the second, fourth, sixth, etc., iron strips are similarly connected by a second copper wire. The paraffine strips insulate adjacent iron strips from one another. The two copper wires are carried to the poles of an induction coil, the position of which on the table is shown in fig. 1. This coil is in turn connected with the dry cells shown in fig. 1, and is equipped with a rheostat. One of the connecting wires between the coil and the dry cells is permanently attached, whilst the other is unattached at its battery end, thus obviating the need of a switch. I find that it is convenient and practically noiseless to close the circuit between batteries and coil by merely touching the loose end of the wire to the appropriate pole. It is obvious that when a subject is within the enclosure or its tributary alleys he will receive a shock whenever the experimenter closes the battery-coil circuit.

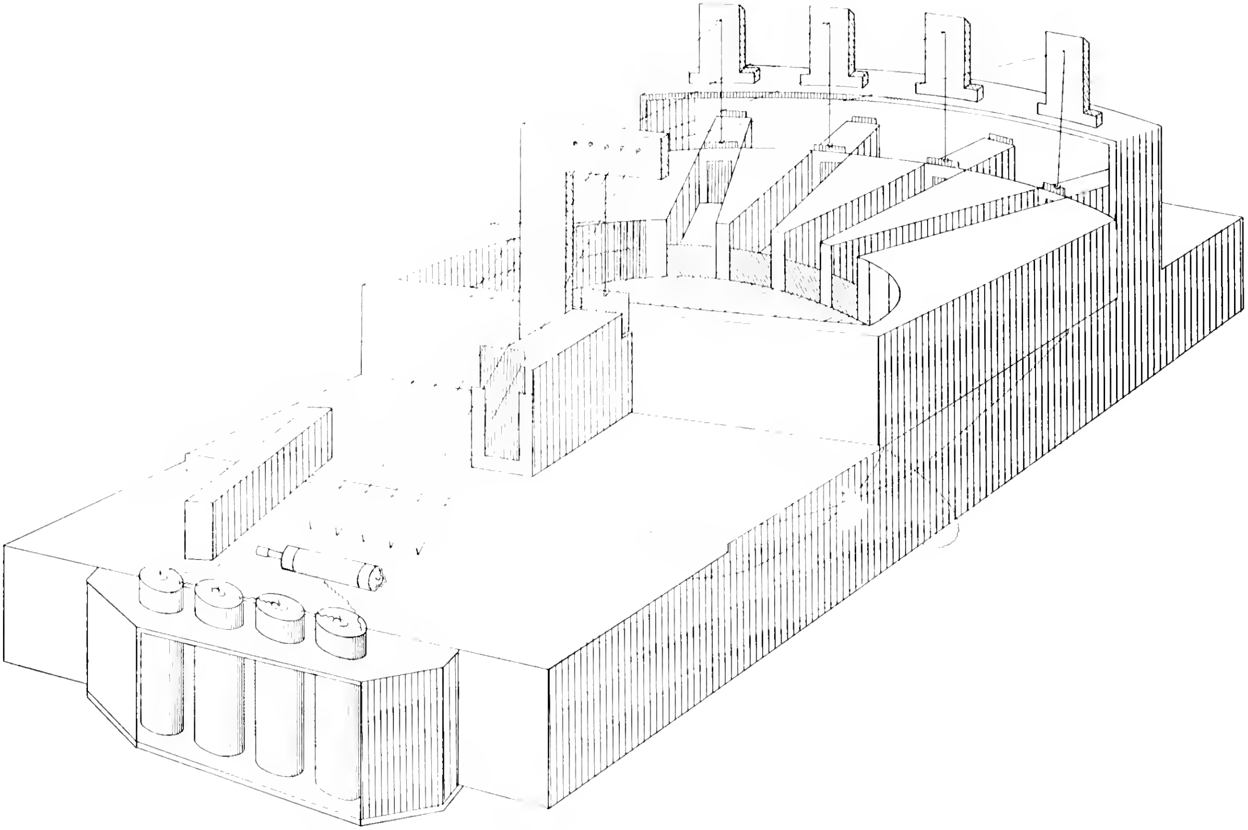
Figure 1 shows *five transfer boxes*, one of which, it will be seen, is drawn in sufficient detail to render separate representation unnecessary. These boxes are made of 1-inch Oregon pine, and are all alike. They have the following inner dimensions: top, 5.5 cm. x 18 cm.; bottom, 5.5 cm. x 20.5 cm.; sides, 9.2 cm. x 20.5 cm. Near the inner edges of the sides are vertical grooves for the reception of tin slides, which are used for opening and closing the box at either of its two ends. The box top is shorter than the bottom, hence it does not interfere with the vertical slides as they are moved up and down within their containing grooves. The slides are 5.1 cm. wide by 14.4 cm. long, and each is perforated near its upper edge for the reception of a lifting hook, to be described below.

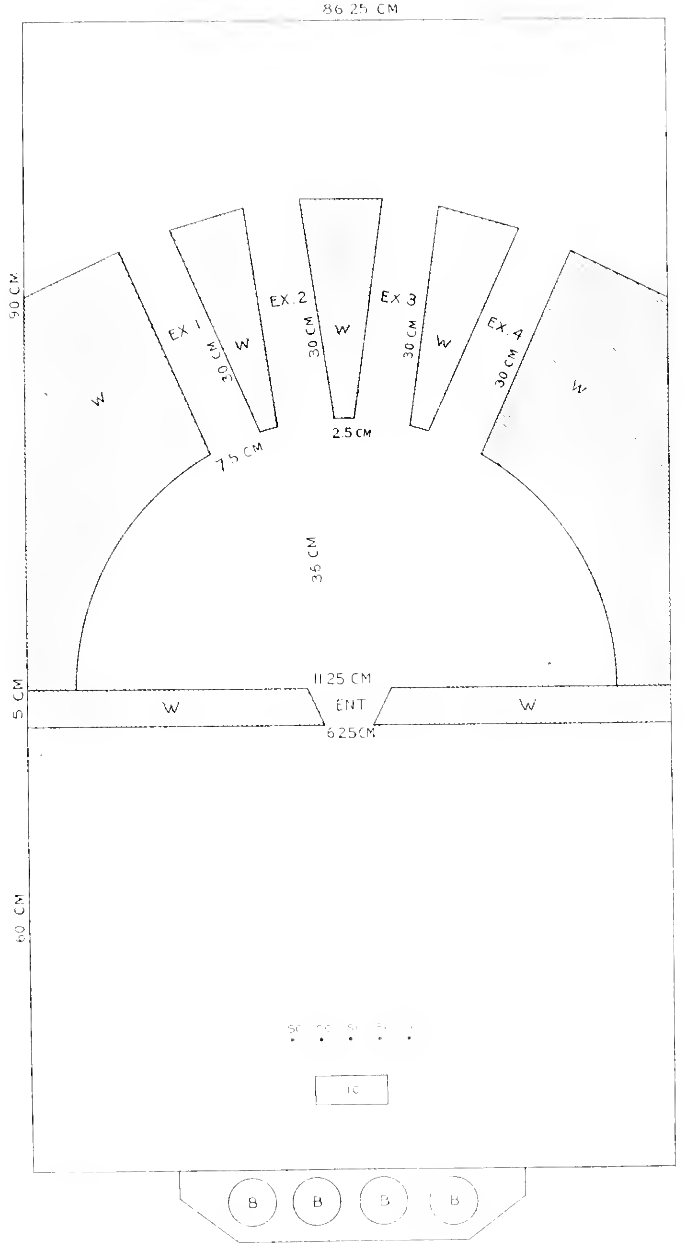
The five transfer boxes serve to block the outer apertures of the entrance and exit alleys; their vertically sliding, removable doors (tin slides) are the only ones with which the apparatus is equipped. When a subject is about to be given the first of his daily ten trials he is carried from his living quarters to the entrance end of the apparatus in one of these transfer boxes. When the box is placed in position to block the outer end of the entrance alley the slide at its inner end is elevated by means of a cord and hook device (fig. 1). The cord is permanently attached to the table top near the edge at which the experimenter is stationed, and from there it passes to the middle perforation in the horizontal arm of the standard shown in fig 1. After it passes through this perforation it hangs down and carries a hook at its end.

The subject usually refuses to leave the transfer box for the less desirable entrance alley and enclosure beyond, and his failure to do so requires the removal of the slide in the outer end of the box for the admission of a wooden plunger (fig. 1), which is used to force the animal out of the box.

Four exit alley cords, which have permanent attachments at the positions shown in figs. 1 and 2, pass from there through perforations in the horizontal arm of the standard at the entrance end of the apparatus, and from these perforations to eyes in the upright standards at the exit end of the apparatus (fig. 1). Each standard carries an eye through which passes a cord with a hook at its free end. When, now, a subject enters an exit alley from which he is to be allowed to escape into the transfer box at the exit alley mouth, the slide in the inner end of the box is elevated by means of the cord and hook device. Since the five cords have their places of permanent attachment near the end of the table at which the experimenter sits, any of the five slides that block the entrance and exit alleys can be raised or lowered from that end of the table.

When a subject completes a trial, i.e., when he has been forced from the box at the entrance end of the apparatus into the entrance alley and into the semicircular enclosure, and has passed from there through the right exit alley into the transfer box at its end, the box that has just been vacated at the entrance end and the one at the exit-alley end that contains the subject are made to exchange places. This brings the subject again to the entrance end, ready to be forced into the enclosure for another trial.




86.25 CM
90 CM
EX. 1
EX. 2
EX 3
EX. 4
30 CM
30 CM
30 CM
30 CM
W
W
W
W
W
W
W
2.5 CM
7.5 CM
36 CM
5 CM
11.25 CM
ENT
6.25 CM
60 CM
SC
IC
B
B
B
B

It is undesirable that a subject, while within the semicircular enclosure, shall be able to see whether the slides in the boxes at the outer mouths of the exit alleys are in an open or a closed position. On this account the apertures that allow passage from the semicircular enclosure into the exit alleys extend only half way from the top of the apparatus. The subject must, therefore, climb over a 7.5 cm. partition in order to get into any exit alley, and he cannot see the lower half of the slides at the exit alleys mouths from any part of the semicircular enclosure. His tendency to cling to the top of the partition in an effort to keep his feet off the apparatus floor is discouraged by means of a copper wire which runs parallel to and just in front of the upper edge of the metal partition. This wire is connected with one pole of the induction coil and the partition is connected with the other pole.

The walls of the apparatus and the surfaces of the slides that are visible from within the apparatus are battleship gray. The interior of the transfer boxes and the inner surfaces of the slides are black. The top of the apparatus is glass.

Apparatus for Primates.—This type of apparatus is described in a previous communication (1), hence it is necessary to give here merely its essential points of difference from the rodent apparatus just described. The primate subject's willingness to enter the apparatus for the sake of reward for escape therefrom renders transfer boxes and an equipment for electrical stimulation unnecessary. The mouths of the alleys are blocked by means of doors, which are hinged to the apparatus. The main enclosure is fan-shaped rather than semicircular. The only important difference between its reactive value and that of the rodent apparatus is contained in its lack of all means for reinforcing the confinement-stimulus with the more definitely disagreeable electrical one.

Method of Conducting Experiments

First Preliminary Trial.—The rodent subject is forced into the entrance alley and is given an electrical shock. The strength of this stimulus is determined for each individual by increasing it until a definite escape-response is obtained. After the initial shock the rodent is allowed to explore the apparatus, and the stimulus is not repeated until visible escape-responses cease.

The first of any of the exit alleys entered by the subject is allowed to give escape into the transfer box at its end.

The monkey subject is tolled into the enclosure and confined therein by the closure of the entrance alley door. He is allowed to use the first exit alley entered for escape into a court, where he is at once given food.

The human subject (only children were used in these studies) is told that escape from the apparatus will be rewarded by a toy credit, i.e., each escape counts one toward the number of escapes necessary to secure a reward. Four toy credits secure a reward on the day that is devoted to preliminary trials; after that ten credits secure a reward. The human subject, like the rodent and the monkey, is allowed to escape by the first exit alley entered.

Second Preliminary Trial.—The exit alley used during the first preliminary trial will not afford escape, but any one of the other three exit alleys may be used for this purpose.

Third Preliminary Trial.—Either of the two exit alleys that have not yet been used for escape will afford escape.

Fourth Preliminary Trial.—Only the one exit alley that has not yet been tried will afford escape.

These four preliminary trials, which are given to the subject in as rapid succession as is possible, are followed the next day by the first ten of a series of 100 formal trials. Ten formal trials are given daily for ten successive days. In describing the method of conducting these trials and in subsequent discussions of results I will use the following terms:

Alley No. 1—the exit alley at the subject's extreme left as he faces the row of exit alleys from within the inclosure.

Alley No. 2—the second exit alley from the left.

Alley No. 3—the third exit alley from the left.

Alley No. 4—the fourth exit alley from the left.

The Right Alley of a given trial is the only one that will afford escape.

The Impossible Alley of a given trial is the one that afforded escape during the immediately preceding trial. There is always one inferentially impossible alley for the subject who learns that it is useless, during a given trial, to try the right alley of the immediately preceding trial.

The Three Possible Alleys are the three exit alleys that would not afford escape during the immediately preceding trial. Although only one of these alleys will actually afford escape during a given trial, all three are inferentially possible avenues of escape from the adult human subject's standpoint.

The following order is observed by the experimenter in choosing right alleys for each of the 100 formal trials:

1st	trial—	alley	No.	1	will	afford	escape
2nd	"	"	"	2	"	"	"
3rd	"	"	"	3	"	"	"
4th	"	"	"	4	"	"	"
5th	"	"	"	2	"	"	"
6th	"	"	"	3	"	"	"
7th	"	"	"	4	"	"	"
8th	"	"	"	1	"	"	"
9th	"	"	"	3	"	"	"
10th	"	"	"	4	"	"	"
11th	"	"	"	1	"	"	"
12th	"	"	"	2	"	"	"
13th	"	"	"	4	"	"	"
14th	"	"	"	1	"	"	"
15th	"	"	"	2	"	"	"
16th	"	"	"	3	"	"	"

This order is repeated so that, e.g., the right alley of the first trial is also the right one for the seventeenth trial, the right alley for the second trial is also the right one for the eighteenth trial, etc. This order is such that each of the four exit alleys is the right alley for 25 of the 100 formal trials.

TABLE 1

LIST OF SUBJECTS

Subject	Age	Race or Species
Girl 1	3 years	Mexican
Girl 2	3 "	Irish
Girl 3	4 "	"
Girl 4	4 "	" —sister of Girl 7
Girl 5	5 "	Mexican
Girl 6	5 "	American
Girl 7	5 "	Irish—feeble minded, sister of Girl 4
Girl 8	5 "	" —sister of Girl 12
Girl 9	5 "	Italian
Girl 10	6 "	Mexican—feeble minded
Girl 11	7 "	"
Girl 12	7 "	Irish—sister of Girl 8
Girl 13	7 "	German
Girl 14	7 "	Italian
Girl 15	7 "	Mexican
Girl 16	9 "	"
Girl 17	9 "	"

TABLE I—*Continued*

Subject	Age	Race or Species
Girl 18	9 years	Mexican
Girl 19	9 "	Italian
Girl 20	12 "	Mexican—feeble minded
Baboon 2	Adult	Species unknown
Monkey 5	"	Pithecus rhesus
Monkey 16	"	" "
Monkey 18	"	" irus
Monkey 26	"	" "
Mouse 1	"	Mus musculus
Gray Rat 1	"	Mus rattus alexandrinus
Gray Rat 2	"	" " "
Gray Rat 3	"	" " "
Gray Rat 4	"	" " "
Gray Rat 5	"	" " "
Black Rat 1	"	Mus rattus
Black Rat 2	"	" "
Black Rat 3	"	" "
Black Rat 5	"	" "
White Rat 1	"	Mus norvegicus albinus—strain A
White Rat 2	"	" " " " "
White Rat 3	"	" " " " "
White Rat 4	"	" " " " "
White Rat 5	"	" " " litter 3-4
White Rat 6	"	" " " " "
White Rat 8	"	" " " " "
White Rat 10	"	" " " strain B
Gopher 1	"	Geomys bursarius
Gopher 2	"	" "
Gopher 3	"	" "
Gopher 4	"	" "
Gopher 5	"	" "
Gopher 6	"	" "

All but four of the subjects given in table 1 had at least 100 formal trials each: I was unable to complete the series with Gopher 6 and with Girls 2, 6 and 19. Baboon 2 had 500 trials and Monkey 26 had 200 trials.

The girls are inmates of an orphanage, a circumstance on which largely depends the dissimilarity of their records to those of the human subjects of my earlier studies (1). It is significant that three of the orphans are distinctly feeble minded. Although some children of good stock are found in our local orphanage, many of them are descended from parents who are mentally inferior, socially delinquent or shiftless and incompetent. The most inferior strains of the native Mexican population are also represented in our institution.

The marked individual differences presented by the five infrahuman primate subjects reflect a policy of selecting subjects in whom oddities of general reactive equipment had been observed. Monkey 26, for example, could be successfully bullied by even the smallest and youngest of my monkeys in spite of the fact that he was the largest but one of a band of sixteen monkeys and baboons that lived together for several months in a single band. Monkey 16, whose record is in striking contrast to that of Monkey 26, is the least distractible monkey that I have ever owned. Monkey 5, on the other hand, is not only distractible to a high degree, but unusually stupid in her social relations.

The gophers, roof rats, black rats and mouse were all wild subjects. None was used until it had been at least two weeks in captivity. The white rats belong to two different strains. White Rats 5, 6 and 8 were born in the laboratory, their parents being White Rats 3 and 4 of the list.

PRESENTATION OF RESULTS

I

Six general types of searching response were manifested by the various subjects:

1. *Response with reference to the experimenter's rule that no alley is the right alley for two successive trials.*

It is obvious that if a subject respond consistently with reference to this rule (but without inferred or other knowledge of the experimenter's order) he will have an average chance of being required to try but one of the three inferentially possible alleys during each of one-third of his trials. If he learns the always-one-impossible-alley rule during his preliminary trials, and never departs from it during his 100 formal trials he will have an average chance of effecting his escape from the apparatus 100 times by trying the various alleys 200 times.

In constructing tables of results I have employed the designation, "Type A" to indicate reactions of the kind just described. The cases in which the subject tries only one or two of the three inferentially possible alleys cannot properly be classified as Type A reactions, since one cannot be sure that an inferentially possible alley would have been chosen had a third choice been necessary. For example, alley No. 3 was the right alley for the sixteenth trial of any subject, alley No. 2 was the inferentially impossible alley and alleys No. 1, 4 and right alley No. 3 were inferentially possible ones. If a subject tried alley No. 4 first during this trial, then alley No. 1, then (successfully) alley No. 3, a Type A reaction was recorded. But if he tried only alley No. 3 or only alleys No. 4 and 3 his reaction did not enter into any of the tables that deal with the six general reaction-types now under discussion.

The statement that a subject responds with reference to a given rule and the use of such terms as "inferentially possible" and "inferentially impossible" are meant to imply neither awareness of rules nor capacity for inference on the part of the subjects. In the case of a given subject a few Type A reactions may be manifested during his series of 100 trials, even where an examination of all his reactions may disclose a tendency to favor rather than to avoid the trial-to-trial varying impossible alley.

2. *Response to the rule that it is useless to try any alley more than once during a given trial; all four alleys tried, and in an irregular order.*

This is the Type B reaction of the tables. An example of this type would be afforded by a subject if, during his sixteenth trial, he were to try the alleys in the following order: 4, 1, 2, 3; or, 1, 4. 2, 3; or, 2, 1, 4, 3, etc. If this tendency alone were operative during the formal series the subject would have an average chance of effecting his 100 escapes by trying the various alleys 250 times. Since many of the subjects, including the children, were manifestly but little influenced by the one-impossible-alley rule the reaction-type tables omit all reactions in which no alley was entered more than once during a given trial and one or more of the three inferentially possible alleys was not tried, even though the impossible alley was tried. For example, during the sixteenth trial alley No. 3 was the right one and alley No. 2 the impossible one. If the subject tried only alleys No. 1, 2 and 3 or only alleys No. 2 and 3 his reaction was not recorded in the tables. These incomplete reactions enter, however, into another reckoning of results, as will be seen in subsequent pages.

3. *When No. 4 is the right alley an effort to escape by trying alleys No. 1, 2, 3 and 4 once each, in the order given; or the reverse of this when No. 1 is the right alley.*

This reaction-type, which is recorded as Type C, is an expression of an interesting tendency to stereotype a systematic mode of searching for a varying place of escape. Some subjects have a marked tendency to try first the alley at the extreme right or extreme left of the row, and to follow this by trying the other alleys in the order of their occurrence along the row until the right one is found. Individual monkeys and a type of feeble-minded child will be found to manifest this type of reaction with especial frequency. It will be discussed at greater length in subsequent pages.

It is obvious that if a subject were invariably to initiate his reaction by trying alley No. 1 and to follow this by trying the other alleys in order until the right one were found he would manifest 25 Type C reactions per 100 trials, and would try the various alleys 250 times. The same holds true in cases where alley No. 4 is the first choice for each trial and the reverse order is followed.

4. *More than one separate effort to escape by a given alley during the same trial, but with an interruption of such efforts by an interval of effort to escape by one or more of the other alleys.*

This is the Type D reaction of the tables. In most cases it probably involves the operation of two different general tendencies, viz., (a) a tendency to vary searching activities until adjustment is effected and (b) a tendency to repeat an activity that has once been manifested. Examples of this reaction during the sixteenth trial are as follows: 4, 1, 2, 4, **3**. 2, 1, 2, **3**. 4, 1, 4, **3**.

5. *During a given trial the subject enters an alley which does not afford escape, leaves it and reenters it one or more times without having tried any other alley; or, having tried a group of two or three alleys in a certain order he reenters all alleys of the group in the same order one or more times.*

Examples of the Type E reactions during the sixteenth trial are as follows: 4, 4, 4, **3**. 1, 2, 4, 1, 1, 1, 1, 1, 1, 1, **3**. 2, 1, 4, 2, 1, 4, 2, 1, 4, **3**. 1, 2, 1, 2, 1, 2, 1, 2, 1, 2, 1, 2, 1, 2, 4, 1, 2, **3**.

6. *During a given trial the subject persistently avoids the right alley until he has tried the other alleys at least six times.*

The limitations of my method render it impossible to distinguish clearly in all cases between this "persistent avoidance" reaction-type and the reaction described above as Type E. Since the two reaction-types have a single general characteristic I have classified them together in the tables as Type E reactions. At times a subject will start to enter an alley, obviously inhibit the impulse to do so, and pass on to other alleys, which are entered without hesitation. If the "inhibited" alley happens to be the right one for that trial persistence of this inhibition may lead to the entrance into the other alleys many times, and with constant variation of choice among them. I have usually found that such episodes are characteristically manifested under conditions which contain adventitious stimuli that are conducive to either excitement or distraction, or both.

In most cases where a subject tries other alleys than the right one during a given trial many times one cannot be sure whether the subject is manifesting (1) a persistent inhibition of an originally positive impulse to enter a given alley or (2) merely a persistent positive impulse to enter a given alley or alleys, so that one alley appears to be avoided when it is not actually an "inhibited" alley.

II

Reaction-type Tables.—In table 2, which directly follows, the number of each type of reaction that was manifested by the individual subject is indicated both by a horizontal line of dashes and a numeral at the end of the line. Thus the letters "A," "B," "C," "D" and "E" appear after Girl 1's place in the table, and in line with "A" are 17 dashes, followed by the numeral 17. This is meant to indicate that this subject manifested 17 Type A reactions during her 100 formal trials. The reactions obtained during the preliminary trials do not appear in this table.

TABLE 2

Subject	Age	Number of each Reaction-Type
Girl 1	3 years	A. — — — — — — — — — — — — — — — — —17
		B. — — —3
		C. — — — —4
		D. — — — — — — — — — — —11
		E. — — — — — —6

TABLE 2—*Continued*

Subject	Age	Number of each Reaction-Type
Girl 2	3 years	A. — — —3. This subject had only 75 trials.
		B. — —2
		C. — — — — — — — — — — — — — — —13
		D. — — —3
		E. — — — —4
Girl 3	4 years	A. — — — — — — — — — — — — —12
		B. 0
		C. — — — — — — — — — — — — — — — — — —
		D. 0 [— — — — —20
		E. 0
Girl 4	4 years	A. 0. Sister of Girl 7
		B. 0
		C. — — — — — — — — — — — — — — — — — — —
		D. 0 [— — — — —22
		E. —1
Girl 5	5 years	A. — — — — — — — — — — — — — — — — — —17
		B. —1
		C. — — — — — — — — — — — — — — — — — — —
		D. — — — — — — —6 [— — — — —22
		E. — —2
Girl 6	5 years	A. — — — — —5. This subject had only 90 trials.
		B. — — — —4
		C. — — — — — —6
		D. — — — — — — — — — —9
		E. 0
Girl 7		A. —1. Sister of Girl 4—feeble-minded.
		B. —1
		C. — — — — — — — — — — — — — —
		D. 0 [— — — — —19
		E. 0
Girl 8	5 years	A. — — — — — — — — — — — — — — —14. Sister of Girl 12
		B. 0
		C. — —2
		D. — — —3
		E. — — — — — — — — — — — —11
Girl 9	5 years	A. —1
		B. — — — —4
		C. — — — —4
		D. — — — — —5
		E. — — — — — — — — — — — —12
Girl 10	6 years	A. 0. Feeble-minded.
		B. 0
		C. — — — — — — — — — — — — — — — —
		D. — — —4 [— — — — —21
		E. 0
Girl 11	7 years	A. — — — — — — — — — — — — — —14
		B. — — — —4
		C. — — —3
		D. — — — — — — — — — — — — —13
		E. 0

TABLE 2—*Continued*

Subject	Age	Number of each Reaction-Type
Girl 12.......	7 years...	A. — — — — — — — — — — — — —13. Sister of Girl 8.
		B. 0
		C. — — —3
		D. — — — — — — — — — — — — — — — — — — —19
		E. —1
Girl 13.......	7 years...	A. — — — — — — —7
		B. 0
		C. — — — — — — —7
		D. — — — —4
		E. 0
Girl 14.......	7 years...	A. — — — — — — — — — — — —12
		B. —1
		C. — — — — — — — — — — — — — — —15
		D. — — — — — — — —8
		E. 0
Girl 15.......	7 years...	A. — — — — — — — — —9
		B. 0
		C. — — — — — — — — — — —11
		D. — — — — — — — — — —10
		E. 0
Girl 16.......	9 years...	A. — — — — — — — — — — — — — — —15
		B. 0
		C. — — — — — — — — — — — — —13
		D. — — — —4
		E. 0
Girl 17.......	9 years...	A. — — — — — — — — — — — —12
		B. —1
		C. — — — — —5
		D. — — — — — — — — — — — — — —14
		E. —1
Girl 18.......	9 years...	A. —1
		B. 0
		C. — — — — — — — —8
		D. — — — — — — — — — — —11
		E. 0
Girl 19.....	9 years...	A. — — — — — —6. This subject had only 50 trials.
		B. 0
		C. — — — — —5
		D. — —2
		E. 0
Girl 20.......	12 years..	A. — — — — — — — — — — —11. Feeble-minded.
		B. — — — — — —6
		C. — — — —4
		D. — — — —4
		E. 0
Baboon 2.....	Adult	A. — — — — — — — —8. Trials 1-100.
		B. — — — — — — — —8
		C. — — — — — —6
		D. — — — — — — — —8
		E. — — — — — — —7

TABLE 2—*Continued*

Subject	Age	Number of each Reaction-Type
Baboon 2		A. — — — — — — — — — — — — — —14. Trials [101-200
		B. — — —3
		C. — — — — — — —7
		D. — — — — — — — — — — — —12
		E. —1
Baboon 2		A. — — — — — — — — — — —11. Trials 201-300.
		B. — — — — — — — — — — — — — —14
		C. — — — — — — — — — — — — — — —15
		D. — — — —4
		E. —1
Baboon 2		A. — — — — — — —7. Trials 301-400.
		B. — — — — — — — — —9.
		C. — — — — — — —7
		D. — — — — — — —7
		E. — —2
Baboon 2		A. — — — — — — — — — — — —12. Trials 401-500.
		B. — — — —4
		C. — — — — — — — — — —10
		D. — — — — — — — —8
		E. —1
Monkey 5	Adult	A. — — — — —5
		B. — — — — — — — — —9
		C. — —2
		D. — — — — — — — — — — — —12
		E. — — — — — — — — — — — — — — — — — —18
Monkey 16	Adult	A. — — — — — — — — — —10
		B. — — — — — — — — —9
		C. — — — — — — — — — — —11
		D. — —2
		E. — — —3
Monkey 18	Adult	A. — — — — — — —7
		B. — — —3
		C. — — — — —5
		D. — — — — — — — — — — — — —13
		E. — — — — — — — —8
Monkey 26	Adult	A. — —2. Trials 1-100.
		B. —1
		C. 0
		D. — — — — — — — — —9
		E. —30
Monkey 26		A. — — — — — — — —8 Trials 101-200.
		B. — — — — —5
		C. —1
		D. — — — — — — — — — — — — — — —15
		E. — — — — — — —7

TABLE 2—*Continued*

Subject	Age	Number of each Reaction-Type
Mouse 1	Adult	A. — — — —4
		B. — — — — — — —7
		C. — — — — — — —7
		D. — — — — —5
		E. —24
Gray Rat 1	Adult	A. — — —3
		B. — — — —4
		C. 0
		D. — — — — —5
		E. —33
Gray Rat 2	Adult	A. — — — — —5
		B. — — — — — — — — —9
		C. — —2
		D. — — — — — — —7
		E. — — — — — — — — — — — — — —14
Gray Rat 3	Adult	A. —1
		B. — — — —4
		C. 0
		D. — — — — — — —7
		E. —27
Gray Rat 4	Adult	A. — — — —4
		B. — — — — —5
		C. — —2
		D. — — — — — — —7
		E. —22
Gray Rat 5	Adult	A. —1
		B. — — — —4
		C. —1
		D. — — — — —5
		E. —33
Black Rat 1	Adult	A. — — — — — — — —8
		B. — —2
		C. — — — — — — — — — —10
		D. — — —3
		E. — — — — — — — — — — — — — — — —16
Black Rat 2	Adult	A. — — — — — — —7
		B. — — —3
		C. — —2
		D. — — — — — —6
		E. —23
Black Rat 3	Adult	A. — — — — — —6
		B. — — — —4
		C. 0
		D. — — — — — — — — — —10
		E. —20

TABLE 2—*Continued*

Subject	Age	Number of each Reaction-Type
Black Rat 5	Adult	A. — — — —4
		B. —1
		C. — —2
		D. — — — — — — —7
		E. —30
White Rat 1	Adult	A. — — — — — — — — —9
		B. — — — — —5
		C. — — — — — — — — — — — — — —14
		D. — — —3
		E. —20
White Rat 2	Adult	A. — — — — —5
		B. —1
		C. — —2
		D. — — — — — — — — — — — —12
		E. — — — — — — — — — — — — — — — — —17
White Rat 3	Adult	A. — — — — — — — — —9
		B. — — — — —5
		C. — — —3
		D. — — — — — — — — — — — —12
		E. — — — — — — — — — — — —12
White Rat 4	Adult	A. — — — — — —6
		B. 0
		C — — —3
		D. — — —3
		E. —40
White Rat 5	Adult	A. — — —3
		B. —1
		C. — — — — — — — —8
		D. — — — — — — — —8
		E. —23
White Rat 6	Adult	A. — — — — — — — — — — — —12
		B. — — — —4
		C. — — — —4
		D. — — — — — — — — — — — —12
		E. — — — — — — — — — — — — — — — — — —18
White Rat 8	Adult	A. — — — — —5
		B. —1
		C. — — — — —5
		D. — — — — — — —7
		E. — — — — — — — — — — — — — — — — — —18
White Rat 10	Adult	A. — —2
		B. —1
		C. 0
		D. — — — — — — — —8
		E. —26

TABLE 2—*Continued*

Subject	Age	Number of each Reaction-Type
Gopher 1	Adult	A. — —2
		B. 0
		C. — —2
		D. 0
		E. —53
Gopher 2	Adult	A. — — —3
		B. —1
		C. —1
		D. — — — — — —6
		E. —45
Gopher 3	Adult	A. — — — —4
		B. — —2
		C. — — —3
		D. — — — — — — — — — —10
		E. —34
Gopher 4	Adult	A. — — — —4
		B. —1
		C. —1
		D. — —2
		E. —51 — — — — — — — — — — — — — — — — —
Gopher 5	Adult	A. — — — — — —6
		B. — —2
		C. 0
		D. — — — —4
		E. —46
Gopher 6	Adult	A. — —2. This subject had only 80 trials.
		B. —1
		C. 0
		D. — —2
		E. —37

III

Discussion of Table 2.—This table discloses the fact that marked individual differences are elicited by the quadruple choice method. The behaviorist who is accustomed to methods which favor habit formation is apt to encounter such differences only during the first few trials with animals for whom the experimental situations are unfamiliar and complex. If, e.g., a difficult maze or puzzle box situation be presented to a half dozen mammalians of the same age and species their first-trial random movements will be found to vary markedly from individual to individual. Unfortunately, only such differences as can be tabulated in terms of reaction-time and numbers of errors are apt to be recorded in experimental studies of habit formation. One of the most important generalizations, perhaps, that can be based upon Table 2 is this:

When a mammalian is confronted by a series of situations for which he is unable to discover and stereotype a specifically adequate and invariably successful mode of response he tends to vary his responses in a manner which is less a species than an individual characteristic.

Mammalian neural organization is so complex and its functions are so plastic that there is excellent ground to assume, *a priori*, that slight individual differences of reactive equipment will be reflected by marked differences, as to detail, of behavior which conforms to the random movement type. Further exemplification of this point can be found in comparisons of the specimen individual records given in the appendix.

Although a *Type A reaction* presents the objective characteristics of a response which has reference to an inferred principle or experiment rule, its occurrence can be regarded as such with certainty only when, in the course of a large number of trials, it clearly predominates over all other types It was the most frequently manifested reaction of three-year old Girl 1, five-year old Girl 8, seven-year old Girl 11, nine-year old Girl 16, nine-year old Girl 19 and feeble-minded, twelve-year old Girl 20. No animal subject presented an excess of Type A reactions over any other type of reaction during the first series of 100 trials. Baboon 2 had an equal number of Type A and Type B reactions during his first 100 trials, and during his second and fifth series he had a plurality of Type A reactions. These reactions are wholly absent from the records of but two subjects, viz., four-year old Girl 4 and feeble-minded, six-year old Girl 10. These two girls stereotyped a method of response which, as can be seen from a glance at their records in the appendix, precluded the possibility of " by-product " or accidental Type A reactions.

With such marked individual differences to take into account we cannot attach much importance to group averages. Nevertheless, the averages given in table 3 are of some interest, as showing that only 2 (Monkey 16 and White Rat 6) of the 28 animal subjects from whom complete series were obtained equalled or exceeded the girl-group average of 9.18 Type A reactions per 100 trials.

Table 3 should be directly compared with table 4 since it will suggest that Type B reactions (which involve trying the one impossible alley as well as the three possible ones) may be manifested as an equivalent of Type A reactions in subjects who are capable of learning that it is useless to enter any one alley twice during the same trial.

It is not to be forgotten that even a few Type A reactions may express a feebly potent tendency to avoid the varying impossible alley. On the other hand a reaction of this type may be accidental in the sense that failure to try the impossible alley may be due to the operation of other tendencies than that which would lead to an avoidance of it. E.g., if a subject stereotypes a method of initiating all trials by first entering alley No. 1 and of following this activity by entering, wherever it is necessary, the other alleys in the order of their occurrence from left to right until the right one is found he will necessarily manifest an equal number of Type A and Type C reactions. The same will obtain if he adopts the reverse method, i.e., if he initiates each trial by entering alley No. 4 and working from right to left in search of the right alley.

	TABLE 3	TABLE 4
Group	Average number A-reactions per 100 trials	Average number B-reactions per 100 trials
Girls (incomplete records excluded).....	9.18	1.24
Girls (feeble-minded cases excluded)...	10.29	1.00
Baboon and Monkeys..................	6.40	6.00
(atypical Monkeys 5 and 26 excluded)..	8.33	6.67
Mouse...	4.00	7.00
Gray Rats..	2.80	5.20
Black Rats.	6.25	2.50
White Rats...	6.38	2.25
Gophers..............................	3.80	1.20

Type B Reactions.—Table 2 shows that 10 of the 20 girls manifested no Type B reactions, whilst only 2 of the 29 animal subjects—a white rat and a gopher—failed to manifest any of these reactions. The maximum number of Type B reactions for the first 100 trials of individual subjects is found in the records of Monkeys 5 and 16 and Gray Rat 2, each of whom had 9 of these reactions. Baboon 2 had five series of 100 trials each. He had 8 B-reactions in his first series, 3 in his second series, 14 in his third series, 9 in his fourth series and only 4 in his fifth series. The girls, black rats, white rats and gophers—38 subjects—afford but one case of an individual whose B-reactions are equal to or exceed the average number of B-reactions for either the baboon-monkey or gray rat group. This subject, Girl 20, is feeble minded.

Type C Reactions.—The reactions that are classified under this heading have the essential objective characteristics of a quite definite reaction-type, hence their distribution in the records of individual subjects (table 2) and in the group averages (table 5) is of considerable interest. The findings recorded in table 2 suggest that the tendency toward manifesting C-reactions is an individual rather than a species characteristic, but this is an acceptable interpretation in only a very much qualified sense. It is to be remembered that *when an animal seeks to escape from confinement his reaction to baffled effort is apt to enter as a determinant of his behavior*, and that such reaction is influenced by various individual traits, particularly by timidity, distractibility and excitability. Whenever any of these traits enters into the situation as a determinant of reaction the subject is apt to make repeated efforts to escape by way of a particular possible outlet or, at least, to return to a previously tried possible outlet before all have been tried. Individual members of each of the various mammalian species with which I have laboratory familiarity (with the possible exception of gophers) have displayed a clearly instinctive tendency toward C-reactions, hence I believe that this tendency is widely distributed among mammalian species. In my earlier studies (1) one of my dogs had 15 C-reactions in 100 trials. It was manifested by one of five cats that were used as subjects—Cat 1 of those studies had 5 C-reactions in 100 trials.

With the exception of the gopher group, each group of subjects presented one or more individuals whose records display an abrupt appearance of C-reactions, their persistence for a number of trials and their abrupt disappearance. It would seem that the mammalian, both primate and infra-primate, has a repertoire of more or less distinct reactive tendencies for meeting situations which call for efforts to escape, and that where a specifically adaptive adjustment is not learned, now one, now another of these tendencies will come to expression. From the standpoint of almost any of the behavior technologies it is highly important to explore for the conditions that characteristically set each of the various known tendencies in operation. This problem must remain an unsolved one so far as the present studies are concerned, but I believe that it will be found that C-reactions are not apt to occur under conditions which are conducive to intensity of affective response.

Some of the girls, particularly Girls 4, 7 and 10, tended to alternate a first choice of alley No. 1 with a first choice of alley No. 4 from trial to trial in starting the systematic (Type C) search for the right alley. This largely accounts for the fact that Girls 4 and 10 did not manifest a single A-reaction, although a habit of displaying C-reactions is apt to yield as many "by product" or incidental A-reactions as C-reactions. Another factor which accounts for these two girls' failure to give any A-reactions was this: whenever a given end alley proved to be the right one it was tried first at the next trial, regardless of the demands of the self-established alternating rule. Girl 7's one A-reaction occurred during her sixtieth trial, when this factor of recency failed to distract her from her habit of alternating her first choice of end alley with which to begin C-reactions.

None of the animals displayed this trial-to-trial alternation of first choice of end alley. Occasionally, however, a subject who seemed to have fixed a habit of always trying first the alley at a given end of the row would reverse his choice. E.g., all of Monkey 16's C-reactions from his sixteenth to his fifty-sixth trial were initiated by a first choice of alley No. 1, but on entering the apparatus for his fifty-seventh trial he tried No. 4 first, and since No. 1 was the right alley for that trial he followed this by entering alleys No. 3, 2 and 1 in the order given. During the remainder of his series all of his-C reactions conformed to the 4-3-2-1 order.

TABLE 5

Average number of C-reactions per 100 trials

Group	
Girls (incomplete records excluded)	10.77
Girls (feeble minded cases excluded)	9.93
Baboon and Monkeys	4.80
(atypical Monkeys 5 and 26 excluded)	7.33
Mouse	7.00
Gray Rats	1.00
Black Rats	3.50
White Rats	4.88
Gophers	1.40

Type D Reactions.—This is a form of discontinuous repetition of a non-adaptive direction of activity which enters into the typical perseverance reactions of animals and even of human adults in the presence of difficult and unusual or highly exciting situations. At this point it will be found convenient to examine the individual records of D-reactions in table 2 and the group averages in table 6:

TABLE 6

Average number of D-reactions per 100 trials

Group	
Girls (incomplete records excluded)	6.82
Girls (feeble-minded cases excluded)	7.71
Baboon and Monkeys	8.80
(atypical Monkeys 5 and 26 excluded)	7.67
Mouse	5.00
Gray Rats	6.20
Black Rats	6.50
White Rats	8.13
Gophers	4.40

Since the records of D-reactions are not easily interpreted without constant reference to those of the E-reactions, both types will be discussed together, and table 7 will follow directly:

TABLE 7

Average number of E-reactions per 100 trials

Group	
Girls (incomplete records excluded)	2.00
Girls (feeble-minded cases excluded)	2.43
Baboon and Monkeys	13.20
(atypical Monkeys 5 and 26 excluded)	6.00
Mouse	24.00
Gray Rats	26.00
Black Rats	22.25
White Rats	21.75
Gophers	45.80

Although the individual records of D-reactions (table 2) at first sight seem to be quite perplexing on account of the individual distribution of these reactions, these records and those of E-reactions serve to throw some light on the general problem that concerns the determinants of individual differences of reaction. Table 2 shows that seven-year old Girl 12 had 19 D-reactions, whilst four-year old Girls 3 and 4 and five-year old Girl 7 had none. Monkey 16 had only 2 D-reactions whilst Monkey 18 had 13; the maximum number for any individual of the black rat group is 10 and the minimum 3; for white rats the maximum and minimum numbers are respectively 12 and 3, and for gophers they are 10 and 0.

Girl 12, who gave 19 D-reactions, was bashful, easily distracted and much less attentive to the apparatus situation than to the experimenter. She and her sister, Girl 8, were two pathetic little Irish girls in a group of swarthy, self-assertive Mexican children, a circumstance which led me to establish bonds of friendship with them that had better have been delayed until the experiments were over. They were easily rattled by their own blunders, and never performed well when they were not allowed to appear in their Sunday clothes. Under these conditions Girl 12 fell into the error of reentering an already tried alley after an interval of effort to escape by an as yet (for the trial) untried alley. Her younger sister, Girl 8, under similar conditions was more apt to display the more primitive E-reactions, of which she had 11 in 100 trials.

Nine-year old Girl 17, with a record of 14 D-reactions, belongs to a highly distractible, prematurely erotic type of native (Mexican) California children who are the despair of local school teachers. Distractibility in school during the period of awakening erotic interests, unwillingness or inability to attend to things toward which there is no instinctive inclination and easy excitability are, of course, more or less characteristic of most children. But these traits are present in an extreme degree in children of the type to which I refer. Girl 18, with 11 D-reactions, belongs to this type.

Girl 11, with 13 D-reactions, was in more or less constant terror of an embarrassing enuresis which was especially apt to assert itself during the experiments. She is a naturally intelligent, attentive child, and would probably have given me records similar to those of the children of my earlier studies had she not thus been handicapped.

Girl 15, with 10 D-reactions, is unusually excitable. During her trials she would rush impulsively from alley to alley, upbraiding herself all the while for her stupidity. Girl 6, with 9 of these reactions in 90 trials, was a homesick, listless, dejected newcomer to the orphanage. She wandered through the apparatus in apparent indifference to the problem before her.

Monkey 16's variation from the average from his group is sufficiently marked to require explanation: he had only 2 D-reactions, whilst Baboon 2, the individual of the baboon-monkey group whose record of these reactions most nearly approximates his own, had 8 such reactions during his first 100 trials. Monkey 16, who was " Sobke " of Yerkes' subsequent experiments (4), is less easily distracted or excited than is any other monkey of which I have knowledge.

Type E Reactions are wholly absent from the records of 12 of the 20 girls, and only four of these subjects have more of these reactions than the low average for the group. As might have been expected, three-year old Girls 1 and 2 displayed more of the primitive Type E reactions than did any of the older girls with the exception of the two special cases to be cited. Girl 8, sister of Girl 12, has already been discussed in the paragraph devoted to an explanation of the occurrence of D-reactions in the girls' records. This five-year old girl, like her sister, was bashful, easily distracted and more attentive to the experimenter than to the apparatus. The detailed record of her reactions, which is given in the appendix, shows lapses from periods during which she quite obviously reacted with reference to the one-impossible-alley principle into periods of grossly inadequate behavior. During one of her lapses she was almost exclusively attentive to her clothing, and after her day's trials were over she wept, declaring that she felt humiliated to have appeared before me so unattractively clad. Girl 9, with 12 E-reactions, is a five-year old Italian. She is excitable, inordinately distractible and a favorite among the other children and the teachers on account of her merry ways. It was impossible to make her take the experiments seriously.

Monkey 5, with 18-E reactions, and Monkey 26, with 30 E-reactions, have already been discussed as atypical animals. In my earlier studies five monkeys gave an average of 4.8 E-reactions per 100 trials, hence the records of Baboon 2 and Monkeys 16 and 18 of these studies (an average of 6 E-reactions per first 100 trials) may be taken as fairly typical in this respect. It is important for subsequent discussions of the determinants of D and E-reactions that only three of the 25 human and infra-human primate subjects of these studies (Girl 9 and atypical Monkeys 5 and 26) have individual records of E-reaction which equal or exceed that of the rodent subject which has the minimum number of such reactions for the entire group of 24 rodents. It is clear that primitiveness of reactive equipment favors the manifestation of E-reactions.

Although the various rodent species have records which disclose marked individual differences, both the table of averages of E-reactions (table 7) and the table of individual reactions (table 2) support the view that in the presence of a situation for which they have no specifically adaptive instinct these subjects have, without exception, a marked tendency toward the repetition of unsuccessful as well as of successful attempts at adjustment. The high average of E-reactions for gophers (45.80 E-reactions per 100 trials) is consistent with my observations of the field and cage behavior of these animals. I have a small experimental garden, in one part of which a gopher persistently attacked the roots of a loganberry vine. One of the numerous mouths of his network of burrows was close to a hydrant, so that I was able to turn water into it without much trouble to myself. For several weeks it was my daily practice to remove the little mound of earth with which the animal occluded this burrow mouth and to flood the burrow with water. My persistence was equalled by the gopher, who invariably returned to the muddy burrow during the night and concealed its opening with earth. This involved much labor on the gopher's part without advantage, since these animals will not occupy a muddy burrow and will not feed on roots that are growing in well saturated ground if roots can be found in moist rather than muddy ground. In this case the gopher's burrows extended to loganberry roots in desirable feeding ground.

A much clearer example of gopher persistence in the face of disadvantage was obtained when I put 12 gophers into a large cage in which there were 12 small nests. Each gopher was driven into a nest and I hoped to avoid the slaughter that usually follows any effort to keep a number of these animals in a common cage. It was soon observed, however, that if a gopher, having left his own nest to secure food, invaded another nest after his cheek pockets were filled with grain and bits of carrot, a battle would ensue which would terminate only when one of the combatants was either dead or a helpless cripple. No matter how large and powerful the occupant of the invaded nest might be, nor how small the disputant of his occupancy, the invader did not seem to be deterred by the disadvantageous consequences of his persistence. He would dart into the nest, only to reappear in a moment, thrust out by its rightful occupant, re-enter and suffer a second expulsion, and so on, until exhaustion or death terminated his stupidly persistent repetition of the nest-seeking reaction. A little variation of reaction would have led him to his own nest or, at least, to one less well defended. Within a fortnight all but two of the 12 gophers were slain in this manner. The survivors were two large males who occupied nests at opposite ends of the cage.

During the experiments it was more or less characteristic of all rodent subjects, and particularly so of gophers, that a disadvantageous experience in a given alley did not seem to deter the animal from reentering it. After a subject had entered a given alley a number of times during the same trial he was apt to become inactive within that alley, so that electrical stimulation was necessary to bring about a resumption of activity.

This penalty for failure to continue would cause the animal to run out of the alley into the semicircular enclosure, following which he would be as apt to reenter the alley from which he was thus driven as any of the untried alleys. It might be expected that the disadvantage growing out of lack of success in attempting to escape through a particular alley and electrical stimulation for "loafing" in it would quickly lead to avoidance of that alley. On the contrary, my records contain many instances of persistent reentrance of an alley during a given trial when as many as 10 successive punishments therein failed to direct the subject's activities toward the untried alleys. I had feared that the introduction of disadvantageous stimulation (electric shock) would lead to avoidances of particular alleys, but this expectation was set at naught by actual experiment. This fact suggests that disadvantage that is not of a kind which is avoided by reason of inherent, specifically appropriate features of reactive equipment is a considerably less important determinant of reaction than the traditional assumptions of comparative psychology would lead us to suspect. I am not prepared, however, fully to subscribe to Watson's (6) view that advantage and disadvantage as such have nothing to do with habit formation. This point will be discussed in another part of these studies.

The automatic repetitions of unproductive activities and of inappropriate inhibitions that one observes in the schizophrenic psychoses are ascribed to purely psychical determinants by the psychoanalysts, but there is much of value both to psychopathology and to comparative psychology in the fact that such behavior is apt to assert itself in the mammalian under conditions that are unfavorable to the manifestation of more adequate reactive tendencies and under certain natural conditions which are characteristically encountered by infra-primate species. The observations that are recorded in these studies, supplemented by various field and clinical observations, lead me to believe that any of the following conditions are apt to precipitate reaction of the D type and E type described in preceding pages:

1. *Inherent primitiveness of reactive equipment, such as is possessed by rodents and the young of more highly developed species.*—In my earlier studies (1) a twenty-six-months old infant gave 13 E-reactions and 10 D-reactions, whilst none of 10 human subjects, whose ages ranged from eight years to maturity gave

a single reaction of this type with the exception of a fifteen-year old boy who, in a moment of embarrassment reentered an alley that he had just tried unsuccessfully. In the present studies only the two three-year old girls and two temperamental five-year old girls of a total of 20 human subjects exceeded the group average of two E-reactions per 100 trials. The individual records of the 24 rodents, on the other hand, disclose a minimum of 12 such reactions per 100 trials for any individual and a maximum of 53.

2. *Excitability.*—When a subject is directing his activities toward adjustment to a situation which has initiated reaction the repetitive tendency is favored by the concurrence of adjustive reactions and strong emotional reactions. Even the average human adult, if trapped and badly frightened in a burning hotel, will rush madly again and again to a part of the building which obviously will not afford escape from the building, thereby diverting time and effort from as yet untried possibilities of escape which would readily occur to him if he were not excited. An angry man will stupidly repeat an empty phrase instead of varying his efforts to effect a verbal adjustment to the situation. If now, any subject, either human or infra-human, is easily excited by a situation which elicits reaction by failure to effect a ready adjustment to it or by intercurrent or adventitious stimuli, he is more apt to manifest reactions of the primitive D and E types than is the subject whose affective responses to such conditions are of less intensity.

3. *Distractibility.*—Among primates and, to a certain extent, among infra-primate mammalians one is apt to encounter more or less marked individual differences of capacity for sustained *exclusive responsiveness* to an unexciting situation which precipitates adjustive reaction thereto. Distractibility consists in a tendency toward quick shifts of responsiveness from possible stimulus to possible stimulus, with consequent absence of sustained responsiveness to a particular stimulus. When a stimulus or stimulus-complex S-1 leads to adjustive activities, and intercurrent stimuli S-2, S-3, etc., cause an *incomplete shifting* of responsiveness from S-1 to S-2, S-3, etc., so that activities directed toward adjustment to S-1 do not wholly cease, we may expect the subject to manifest reactions of the D or E type.

4. *Feeble responsiveness.*—A given set of stimuli may elicit activities which are directed toward adjustment to them in a manner to suggest that the threshold of responsiveness has barely been crossed. An example has but this moment come within my own experience: I was interrupted by a nurse who wished to consult me about an unimportant matter which concerns a patient for whose care I am responsible. I responded to his question by seeking, in a rather indifferent frame of mind, an answer that would satisfy, not so much the patient's needs as the professional amenities of the situation. I found myself saying, inadequately, "The woolen cap—the woolen cap—the woolen cap." I was barely responsive to the situation, and the result was a reaction which consisted in the repetition of an unsuccessful attempt at adjustment. Had I been more responsive, attempts at verbal adjustment would have varied without repetition until a successful one had been found. Where habit serves as a guide for the direction of activity such repetition is less apt to occur, since a successful adjustment is at once effected, thereby removing the subject from the influence of the situation. In the schizophrenic psychoses the subject characteristically withdraws his interest from reality, and is therefore apt to be but feebly responsive to the outside world. There is also apt to be a deterioration of the habits that were acquired before the advent of the psychosis, hence this disorder presents conditions which we have already found to be favorable to the manifestation of D and E reactions (the cases of Girls 6, 8 and 12, already cited).

IV

Determinants of the Initial Direction of Activity.—When a subject first enters the enclosure he may first try any one of the four alleys for escape. The fact that this *first choice*, as it will hereafter be designated, was varied from trial to trial or at least every few trials by many of the subjects calls for an examination of the records for the purpose of discovering, if possible, the determinants of first choices. With this in view the record of each subject has been given a detailed analysis, the results of which are presented in table 8. This table requires the following explanations:

1. In the column that contains the list of subjects an asterisk (*) is placed opposite the name of any subject who had less than 100 trials. Girl 2 had only 75 trials, Girl 6, 90 trials,

Girl 19, 50 trials, and Gopher 6, 80 trials. All other subjects had 100 trials each, and only the first 100 trials of subjects who had more than a single series are given in table 8.

2. The heading, First Choice, is the one under which is given
1 2 3 4
the number of times that each subject entered first each of the four alleys at the beginnings of trials. E.g., the first horizontal group of figures under this heading is meant to show that Girl 1 initiated reaction in 17 of her 100 trials by entering alley No. 1; that in 28 of her trials she made first choice of alley No. 2, etc.

3. Under the heading, Alley Entrances, is shown the number
1 2 3 4 Total
of times that each of the various subjects entered each of the four alleys during his 100 trials. It also shows under the subheading, Total, the total number of separate alley-entrances that were made by each of the subjects. E.g., Girl 1, in the course of her 100 trials, entered alley No. 1 49 times, alley No. 2 83 times, etc. In her efforts to escape from the apparatus 100 times she entered the various alleys a total number of 272 times.

Possible Determinants
4. Under the heading, of First Choice is shown the
R. F. R.-F.
number of times that the individual subject's first choices sustained the relations of (a) recency, (b) frequency and (c) both recency and frequency to previous trials. If a subject began a trial by first entering the alley that was last entered during the immediately preceding trial one *recency reaction* was counted for that trial. If the alley first chosen was the one that had been most frequently entered during *all* of that subject's preceding trials one *frequency reaction* was recorded. If the alley first chosen sustained both the relation of recency and that of frequency to preceding trials one *recency-frequency reaction* was recorded, and neither a recency nor a frequency reaction was recorded in a case of this kind.

The records showed that there were a few cases where two or even three of the four alleys had been entered an equal number of times at the end of a given number of trials, and that each of these two or three alleys might be said to sustain the relation of frequency to previous trials. In such cases first choice of any of these two or three alleys was recorded as a frequency reaction for that trial.

TABLE 8

	First choice				Alley entrances					Possible determinants of first choice		
Subject	1.	2.	3.	4.	1.	2.	3.	4.	Total	R.	F.	R.-F.
Girl 1	17	28	21	34	49	83	80	60	272	8	21	7
Girl 2*	46	14	7	8	58	59	51	33	201	8	20	8
Girl 3	54	3	1	42	61	59	59	56	235	9	42	9
Girl 4	32	5	18	45	55	62	60	52	229	44	1	1
Girl 5	16	11	8	65	48	77	82	80	287	10	37	11
Girl 6*	6	35	27	22	29	68	66	44	207	12	26	7
Girl 7	58	3	1	38	70	62	50	49	231	11	34	23
Girl 8	10	19	45	26	37	77	95	50	259	4	42	1
Girl 9	26	65	6	3	49	129	114	59	351	8	46	17
Girl 10	79	21	0	0	82	82	58	29	251	5	63	14
Girl 11	20	33	32	15	54	81	68	41	244	4	31	7
Girl 12	15	32	46	7	49	89	88	40	266	10	34	9
Girl 13	22	28	30	20	39	59	55	33	186	10	17	11
Girl 14	31	16	12	41	50	67	67	56	240	14	12	1
Girl 15	25	19	25	31	48	69	69	47	233	10	16	4
Girl 16	28	21	26	25	49	73	68	40	230	14	18	9
Girl 17	18	25	42	15	52	83	79	39	253	13	17	6
Girl 18	24	27	34	15	41	62	69	40	212	6	24	6
Girl 19*	8	10	17	15	21	31	29	20	101	8	8	1
Girl 20	16	33	37	14	46	63	62	40	211	7	31	7
Baboon 2	46	25	12	17	81	83	54	49	267	16	29	10
Monkey 5	16	30	31	23	79	106	93	106	384	24	20	7
Monkey 16	64	1	1	34	77	56	56	67	256	8	48	15
Monkey 18	68	18	2	12	110	78	52	71	311	9	54	13
Monkey 26	13	24	11	52	101	98	55	239	493	10	38	12
Mouse 1.	11	24	23	42	70	96	99	96	361	15	27	7
Gray Rat 1	11	26	29	34	131	88	86	115	420	21	18	3
Gray Rat 2	26	20	13	41	75	70	61	116	322	20	29	12
Gray Rat 3	16	17	26	41	109	71	73	124	377	17	36	10
Gray Rat 4	6	8	28	58	78	97	88	116	379	10	47	9
Gray Rat 5	5	14	27	54	114	78	88	105	385	9	37	13
Black Rat 1	2	18	21	59	58	71	76	91	296	12	42	17
Black Rat 2	24	43	15	18	73	93	77	78	321	12	24	7
Black Rat 3	3	68	19	10	71	112	72	102	357	8	49	19
Black Rat 5	43	29	17	11	154	110	86	64	414	17	36	6
White Rat 1.	11	11	25	53	76	92	104	105	377	22	19	6
White Rat 2.	9	24	33	34	72	124	124	74	394	11	21	7
White Rat 3.	79	9	11	1	111	95	69	53	328	8	50	19
White Rat 4.	18	31	35	16	94	139	132	67	432	23	20	7
White Rat 5.	39	27	22	12	108	157	128	51	444	17	20	11
White Rat 6.	12	20	43	25	65	99	119	86	369	14	31	13
White Rat 8.	12	16	47	25	90	85	98	76	349	25	15	7
White Rat 10.	17	43	30	10	90	105	100	89	384	20	16	3
Gopher 1	11	20	19	50	97	115	124	189	525	11	39	10
Gopher 2	20	25	16	39	129	123	82	153	487	21	23	14
Gopher 3	7	23	32	38	77	113	132	117	439	12	34	12
Gopher 4	42	25	16	17	157	109	73	141	480	20	21	10
Gopher 5	8	13	33	46	108	181	173	113	575	22	11	5
Gopher 6*	9	26	21	24	144	132	124	128	528	12	12	3

Discussion of *Table* 8.—The figures given in this table were obtained from an analysis of 4,795 trials that were given to 49 subjects, and since the relation of each trial to the trials that preceded it was obtained by a necessarily somewhat complicated method of calculation, a presentation of illustrative specimens of this work will be reserved for the appendix. Table 8 presents in detail most of the data on which the following discussion is based, but occasional reference to what will hereafter be designated " calculations for table 8 " will be necessary.

Alley position was evidently in itself a partial determinant of some of the subject's first choices. Feeble-minded Girl 10, for example, tried alley No. 1 first during each of 79 trials, and it is probable that the mere position of this alley as the first one to the left determined her preference for it as the first one to try. Of course a marked tendency toward Type C reactions (trying alleys in the order *1-2-3-4* or *4-3-2-1*) would account for a preference of alley No. 1 or alley No. 4 because of their terminal positions with reference to the row of alleys. Nevertheless, table 8 shows that of the 4,795 first choices made by the subjects, 1199 were for alley No. 1, 1,126 for alley No. 2, 1,093 for alley No. 3 and 1,377 for alley No. 4—a finding which suggests that there was no marked disproportion between the *average* reactive values of the terminal and the middle alleys.

Twelve of the subjects have the maximum number of their first choices in the alley No. 1 column, 7 in the alley No. 2 column, 11 in alley No. 3 column and 19 in alley No. 4 column. Feeble-minded Girl 10 made no first choices whatsoever of either alley No. 3 or alley No. 4.

Recency as a factor in determining first choices is shown in table 8 to have played a less important rôle than the factor of frequency. It is to be remembered that my method is such that it is to the subject's disadvantage to initiate a trial by choosing first the alley that was last entered during the immediately preceding trial. The general tendency of mammalians to repeat the most recently manifested activity on reencountering a given situation might be expected to result in more than 25 of these " recency first choices " as we may conveniently designate them, per 100 trials in subjects who are incapable of learning to react with reference to the disadvantage of such first choices. On the other hand, a subject who has more or less capacity for learning this would be expected to manifest less than 25% of recency first choices. Table 8 shows that only 1 of the 49 subjects (four-year old Girl 4) had more than 25% of recency first choices if we exclude the cases where first choices sustained the relations of both recency and frequency to previous trials. If we do not exclude such cases, 17 subjects—2 girls, 1 baboon, 1 monkey, 2 gray rats, 2 black rats, 6 white rats and 3 gophers—had each more than 25% of recency first choices.

Frequency was apparently an important determinant of first choices. In making the calculations for table 8 a frequency reaction was recorded whenever the subject made first choice of the alley that had been most frequently entered during all preceding trials. The table shows that 27 subjects—11 girls, 1 baboon, 3 monkeys, 1 mouse, 4 gray rats, 3 black rats, 2 white rats and 2 gophers—had each more than 25% of frequency first choices if we exclude the cases where first choices sustained the relation of recency as well as of frequency to previous trials. If we do not exclude such cases, 40 of the subjects had each more than 25% of frequency first choices.

The averages for the primate and rodent groups given in table 9 are of some interest. Subjects which had less than 100 trials each are excluded from this table:

TABLE 9

AVERAGES OF RECENCY, FREQUENCY AND RECENCY-FREQUENCY FIRST CHOICES

Group	Recency	Frequency	Recency-Frequency
Primates (22)............	11.55%	30.68%	9.09%
Rodents (23)............	15.96%	28.91%	9.87%

This table suggests various interesting problems. It would be of considerable importance to have sufficient experimental data on which to base a conclusion as to whether with descent in the phyletic scale the relative importance of recency and frequency changes in favor of recency. Table 9 suggests that with ascent of the phyletic scale the rôle of recency as a determinant of habit formation decreases whilst that of frequency increases, but the individual variations from the averages are too marked to render this a legitimate interpretation.

The fact that the rodents, as a group, were not more influenced by the factor of recency under conditions which rendered the operation of this factor disadvantageous did not attract my attention until I read Watson's (7) contention that advantage and disadvantage as such have nothing to do with habit formation. It was then too late to use most of my subjects for an experimental investigation of the problem that is dealt with in subsequent pages.

The repetitive tendency that finds expression in reactions that I have classified as belonging to types "D" and "E" may play an important rôle in habit determination. If one may safely generalize to the effect that what an organism has done most frequently in past encounters with a situation it is most apt to do, all things else being equal, in reencounters with that situation it necessarily follows that whatever leads to the repetition of a given activity before a definite habit is formed must be regarded as a possible determinant of the ultimately established habit. We have found that various factors which are subject to but limited and imperfect control may operate at any time to precipitate persistent repetition of an unsuccessfully directed activity, even though no past experience and no constant reactive value of the situation could be regarded as directly accounting for such repetition. Inwardly arising adventitious stimuli or unpreventable causes of excitement or distraction that operate from without may thus account for the relative frequency of a given activity, and this frequency may in turn act as a factor in determining the composition of the ultimately established habit. Reference to the calculations for White Rat 5 (appendix) affords the following example:

At the beginning of White Rat 5's twentieth trial she had entered alley No. 1 10 times, alley No. 2 16 times, alley No. 3 22 times and alley No. 4 18 times. During the twentieth trial a quite incalculable factor precipitated a Type E reaction, so that she entered alleys No. 1 and No. 2 7 times each. At the end of her twenty-fifth trial she had entered alley No. 2 32 times and alley No. 3 only 31 times. At the completion of her series of 100 trials alley No. 2 had been entered 161 times, whilst alley No. 3 had been entered only 133 times and alley No. 4 only 56 times. In other words, during four preliminary trials (these necessarily enter into the calculations for table 8) and 19 formal trials this subject had shown a preference for alley No. 3 over all other alleys, and a preference for alley No. 4 over alley No. 2; but an episode of repetitive behavior which could not be ascribed to any change in the intrinsic reactive values of the various alleys and which suddenly increased the frequency with which alley No. 2 had been tried established a preference for this alley which was apparent at the end of the twenty-fifth trial and was maintained throughout the remainder of the series.

V

Studies of Habit Formation by the Multiple Choice Method.—These studies were limited to an investigation of the value of *recency and frequency of advantageous reaction*, and are presented here to supplement the findings of the foregoing pages, which include some information as to the value of these factors when no element of advantage is associated with their operation. Black Rat 5 and White Rats 2, 5, 6, 8 and 10 were used as subjects for these experiments. On the day following a subject's conclusion of his 100 formal trials he was given a series of habit forming trials which differed from the formal series in the following features:

1. The right alley, i.e., the only one that would afford escape from the enclosure, remained the same for a given subject throughout this series, so that the subject had only to learn always to seek this one alley for exit in order to acquire a specifically adaptive habit.
2. In most cases the alley selected by the experimenter as the right alley for the series was the one that had been entered by the subject the least number of times during his 100 formal trials. If, however, it appeared toward the end of the formal series that an episode of repetitive (E type) reaction had inclined the subject toward the alley of least frequent entrance another alley was selected by the experimenter as the right one.
3. The trials were repeated during a single session until the subject failed to try any alley but the right one during six successive trials.

The records for each of the six subjects will be presented separately:

Black Rat 5

During this subject's 100 formal trials she entered alley No. 1 154 times, alley No. 2 110 times, alley No. 3 89 times and alley No. 4 64 times. Alley No. 1 was her first choice in 43 trials, alley No. 2 in 29 trials, alley No. 3 in 17 trials and alley No. 4 in 11 trials.

Alley No. 3 rather than alley No. 4 was chosen as the one through which she would be allowed to escape during her habit forming trials since she had entered the former alley but 14 times during her last 20 formal trials, whilst she had entered alley No. 4 22 times, and had entered it 7 times during her ninety-sixth trial.

Table 10 gives her reactions during the 20 trials that she had before she ceased to try other alleys than the right alley, No. 3.

TABLE 10

Alleys Entered by Black Rat 5 During Each of 20 Habit-Forming Trials

1st	trial—	3
2nd	“	2-1-4-1-4-2-3
3rd	“	2-4-2-3
4th	“	1-4-2-4-2-4-2-4-1-4-1-4-1-3
5th	“	2-4-2-4-1-3
6th	“	3
7th	“	2-4-2-4-1-3
8th	“	1-4-1-4-1-4-1-4-1-4-2-4-3
9th	“	3
10th	“	2-4-1-4-1-4-1-4-2-3
11th	“	2-4-1-4-4-2-4-1-4-2-4-1-3
12th	“	2-3
13th	“	2-3
14th	“	4-2-3
15th	“	2-3
16th	“	2-3
17th	“	2-3
18th	“	2-3
19th	“	3
20th	“	2-3

The table shows that this subject, while forming the habit of trying first and only alley No. 3, entered alley No. 1 19 times, alley No. 2 25 times, alley No 3 20 times and alley No. 4 30 times. The repetitive episode in the ninety-sixth trial, already referred to, was probably a factor in determining the relatively frequent attempts to escape by alley No. 4. The significance of this table is contained in the following facts:

1. A habit of entering alley No. 3 first was formed under conditions which rendered such first choice invariably advantageous.

2. During this subject's 100 formal trials, when the operation of the factor of recency was invariably disadvantageous, she manifested only 17 pure recency first choices and 6 recency-frequency ones. During the 20 habit forming trials under dis-

cussion she manifested only 3 recency first choices, but after these trials, *during which the operation of the factor of recency was invariably advantageous* she manifested 100% of recency first choices.

3. During the 100 formal trials this subject entered alley No. 3 less frequently than she entered either alley No. 1 or No. 2. During her 20 habit forming trials both alley No. 2 and alley No. 4 were entered more frequently than was alley No. 3, hence the factor of frequency, as such, cannot be held to account for the formation of the habit.

These findings suggest that *frequency of an advantageous response* is a more important determinant of habit formation than is frequency of a response which carries with it no invariable advantage. This point will be given further discussion in the presentation of the records that follow:

White Rat 2

This subject displayed 11 pure recency and 7 recency-frequency reactions during his 100 formal trials. The record of his first choices is as follows: alley No. 1 was the first choice in 9 trials, alley No. 2 in 24 trials, alley No. 3 in 33 trials, alley No. 4 in 34 trials. During his 100 formal trials he entered alley No. 1 72 times, alley No. 2 124 times, alley No. 3 124 times and alley No. 4 74 times. Table 11 gives the record of the 14 trials that it was necessary to give him before he ceased to try other alleys than right alley No. 1:

TABLE 11

ALLEYS ENTERED BY WHITE RAT 2 DURING EACH OF 14 HABIT-FORMING TRIALS

1st	trial—	3-2-1
2nd	“	2-3-1
3rd	“	3-2-1
4th	“	3-2-1
5th	“	2-1
6th	“	3-2-1
7th	“	2-1
8th	“	3-2-1
9th	“	1
10th	“	1
11th	“	1
12th	“	2-1
13th	“	1
14th	“	2-1

The effects of previous frequency of efforts to escape by alleys No. 2 and 3 are reflected in this table; these alleys were evidently preferred during the first 8 habit forming trials, after which there were no further efforts to escape by alley No. 3 and only 2 efforts to escape by alley No. 2. Since alley No. 1 had been entered no more frequently than had alley No. 2 at the end of the eighth habit forming trial it is likely that frequency of advantageous response was the essential determinant of the habit. One must not forget that in this case the subject displayed only 18% of combined pure recency and recency-frequency first choices during the 100 trials in which the factor of recency always operated disadvantageously, whilst he displayed 100% recency first choices after he had been given 14 trials during which this factor invariably operated advantageously.

White Rat 5

TABLE 12

ALLEYS ENTERED BY WHITE RAT 5 DURING 5 HABIT-FORMING TRIALS

1st trial—	3-3-4
2nd "	3-3-4
3rd "	3-4
4th "	3-4
5th "	3-4

This subject had 17 recency and 11 recency-frequency first choices, which were distributed as follows: alley No. 1, 39 trials; alley No. 2, 27 trials; alley No. 3, 22 trials; alley No. 4, 12 trials. She entered alley No. 1 108 times, alley No. 2 157 times, alley No. 3 128 times and alley No. 4 51 times. It is quite clear that alley No. 4 was for her the one of least preference, both for first choice and for entrances subsequent to first choice.

The rapidity with which White Rat 5 formed the habit of trying alley No. 4 first and only is striking, and affords an example in which the factor of frequency merely as frequency and not as frequency of advantageous response could have played no important part in determining the habit. During the five habit forming trials alley No. 3 was entered 7 times, whilst alley No. 4 was entered but 5 times.

White Rat 6

This subject had 14 pure recency and 13 recency-frequency first choices, which were distributed as follows: alley No. 1, 12 trials; alley No. 2, 20 trials; alley No. 3, 43 trials; alley No. 4, 25 trials. During his 100 formal trials he entered alley No. 1 65 times, alley No. 2 99 times, alley No. 3 119 times and alley No. 4 86 times.

TABLE 13

ALLEYS ENTERED BY WHITE RAT 6 DURING 14 HABIT-FORMING TRIALS

1st	trial—	3-4-3-4-3-2-3-4-1
2nd	"	2-3-2-1
3rd	"	3-2-1
4th	"	3-2-1
5th	"	3-1
6th	"	3-2-1
7th	"	3-2-1
8th	"	3-2-1
9th	"	2-1
10th	"	2-1
11th	"	4-3-2-1
12th	"	3-1
13th	"	3-1
14th	"	3-1

In this table, as in tables 10, 11 and 12, we have an example of habit formation which evidently depended not only upon the operation of the factors of recency and frequency as such, but upon recency and frequency of advantage.

White Rat 8

This subject had 25 pure recency and 7 recency-frequency first choices, which were distributed as follows: alley No. 1, 12 trials; alley No. 2, 16 trials; alley No. 3, 47 trials; alley No. 4, 25 trials. It is significant that this subject displayed a larger number of recency first choices and a smaller number of frequency first choices than did any subject of the six that were used for the studies of habit formation. He entered alley No. 1 90 times in 100 formal trials, alley No. 2 85 times, alley No. 3 98 times and alley No. 4 76 times.

TABLE 14

ALLEYS ENTERED BY WHITE RAT 8 DURING 8 HABIT-FORMING TRIALS

1st	trial—	2-1-3-2-4
2nd	"	3-2-3-2-4
3rd	"	4
4th	"	4
5th	"	3-1-3-4
6th	"	4
7th	"	3-4
8th	"	3-4

In this case the factor of frequency as such, regardless of the advantage or disadvantage resulting from its operation, could have played only a secondary rôle since alley No. 4 was entered but once more frequently during the habit forming trials than was alley No. 3 and since, as is shown in table 8, frequency as such was a possible determinant of his first choices in only 22% (15% pure frequency and 7% recency-frequency) of his 100 formal trials.

White Rat 10

This subject was the only representative of his strain. He had 20 recency and 3 recency-frequency first choices. His first choices were distributed as follows: alley No. 1, 17 trials; alley No. 2, 43 trials; alley No. 3, 30 trials; alley No. 4, 10 trials. He entered alley No. 1 90 times, alley No. 2 105 times, alley No. 3 100 times and alley No. 4 89 times.

Although this subject had 15 habit forming trials he did not once enter any other alley than right alley No. 4. This alley is the right one for the one-hundredth formal trial, hence the factor of recency (it is evident that the factor of frequency is of secondary importance in this case) may have accounted for his first choice of alley No. 4 when he was given his first habit forming trial. During the trials that followed both recency and increasing frequency of advantage seem to have determined the habit of trying only alley No. 4.

VI

CONCLUSIONS

1. When a mammalian is confronted by a series of situations for which he is unable to discover and stereotype a specifically adequate and invariably successful mode of response he tends to vary his response in a manner which is less a species than an individual characteristic.

2. The multiple choice method that was employed in these studies operates against habit formation and favors a varying of modes of searching for a place of exit throughout an entire series of 100 trials. Such variation of response was manifested by a baboon throughout 500 trials. The following types of response were elicited from children, infra-human primates and five different rodent species:

Type A.—Response to confinement by trying three trial-to-trial varying inferentially possible exit alleys, the one trial-to-trial varying inferentially impossible exit alley not being tried by the subject.

Type B.—Response by trying all four exit alleys, but once each and in an irregular order.

Type C.—Response by trying all four alleys but once each and in regular order from left to right or from right to left.

Type D.—Response by entering a given alley more than once during a given trial, with an interval of effort to escape by another alley between entrance and reentrance of the same alley.

Type E.—(a) Response includes two or more separate efforts to escape by the same alley or group of alleys during a given trial without intervals of effort to escape by other alleys. (b) Response includes persistent avoidance of a given alley during a given trial whilst six or more separate efforts to escape by the other alleys are manifested.

3. None of the subjects of these studies manifested a consistent appreciation of the inferentially impossible, trial-to-trial varying one impossible exit alley as the one alley that would not afford escape from confinement, hence Type A reactions were not exclusively manifested by any subject. The use of an inferior class of human subjects must be held to account for the fact that the maximum number of Type A reactions was manifested by a three-year old and a five-year old girl in a group of 20 children whose ages ranged from three to twelve years. Nevertheless, the children manifested a higher average number of these reactions than did any of the animal groups. The infra-human primates average of A-reactions was higher than that of any of the rodent groups.

4. If we exclude the case of the single mouse that was used in these studies, B-reactions were of more frequent occurrence among infra-human primates than among any of the other groups. The only human subject who manifested a considerable number of B-reactions was feeble-minded twelve-year old Girl 20.

5. The girls manifested the highest average number of C-reactions. Their average in this respect is slightly lowered if

the feeble-minded cases are excluded. If the two atypical monkeys are excluded the infra-primate group has the next highest average number of C-reactions. Among rodents (the mouse being excluded) the white-rat group has the highest average of these reactions.

6. The group averages of D-reactions (table 6) do not sufficiently differ from one another to be of much significance, since these averages were much affected by extreme individual cases.

7. In spite of the influence of atypical cases on the primate averages for E-reactions, table 7 shows that the tendency toward the manifestation of these reactions is much more marked among rodents than among primates. The infra-human primate average number of E-reactions is conspicuously higher than the girl's average number, but conspicuously lower than any rodent group's average.

8. The following factors favor the manifestation of Type D and, particularly, of Type E reactions:

(a) Inherent primitiveness of reactive equipment, such as is possessed by rodent species and by the young or mentally defective of more highly developed species.

(b) Excitability.

(c) Distractibility.

(d) Feeble responsiveness to a situation which elicits reaction.

9. The first direction that is given to a subject's activities under conditions which elicit escape responses is apt to be determined by such factors as the spacial relations of the various apparently possible avenues of escape, the recency with which each of these various avenues have been previously tried for escape and the frequency with which each has been previously tried for escape.

10. Recency and frequency, in the sense in which they are used by Watson (7) are apt to be in themselves factors in determining the direction of a subject's activity, but when either of these factors repeatedly operates advantageously it is apt to act more strongly as a behavior determinant than where either no constant advantage or actual disadvantage attend its operation.

11. A comparison of the studies made under conditions which operate against habit formation with those which favor habit formation suggests that a response which has been frequently

manifested with invariable advantage is more apt to recur in the behavior of the individual than is a still more frequently manifested response which has not brought invariable advantage.

12. These studies suggest a possibility, which they by no means prove, that with descent of the phyletic scale the factor of recency increases in importance as a determinant of habit formation whilst that of frequency relatively decreases.

APPENDIX

I. Individual Records of Twenty Girls.
II. Individual Record of White Rat 5.
III. Analysis of White Rat 5's Record (calculations for table 8).

Trial number	Girl 1 (3 years) Alleys entered	Girl 2 (3 years) Alleys entered	Girl 3 (4 years) Alleys entered	Girl 4 (4 years) Alleys entered
1	1	4-3-2-1	1	1
2	4-3-2	4-3-2	1-2	2
3	3	4-3	4-3	3
4	4	2-1-4	3-4	4
5	3-2	2	1-2	1-2
6	1-2-3	3	2-3	2-3
7	4	4	4	4
8	3-2-1	2-3-4-1	1	4-3-2-1
9	4-3	2-3	1-2-3	1-2-3
10	3-2-3-4	4	4	2-3-4
11	1	1	1	1
12	4-3-2	2	4-3-2	2
13	4	3-4	1-2-3-4	3-4
14	3-2-1	1	1	4-3-2-1
15	4-3-2	3-4-1-4-3-4-3-2	4-3-2	2
16	4-3	1-2-3	1-2-3	3
17	4-3-2-1	1	1	4-3-2-1
18	4-3-2	2	4-3-2	1-2
19	4-3	3	1-2-3	3
20	3-2-1-2-3-4	4	4	4
21	2	1-2	1-2	1-2
22	4-3	3	4-3	3
23	4	2-3-4	1-2-3-4	4
24	3-2-1	1	1	4-3-2-1
25	4-3	1-2-3	4-3	1-2-3
26	4	1-2-3-4	1-2-3-4	4
27	4-3-2-1	1	1	4-3-2-1
28	4-3-2	2	4-3-2	1-2
29	4	1-2-3-4	4	3-4
30	3-2-1	2-3-4-3-4-3-2-3-1	1	4-3-2-1
31	2	1-2	1-2	1-2
32	4-3	2-1-2-3	4-3	3
33	2-1	1	1	4-3-2-1
34	2	1-2	4-3-2	1-2
35	2-1-2-3	1-2-3	1-2-3	3
36	4	1-2-4	4	4
37	2	1-3-4-1-2	1-2	4-3-2
38	3	2-3	4-3	1-2-3
39	2-1-2-3-4	1-2-3-4	1-2-3-4	4
40	1	1	1	4-3-2-1
41	3	1-2-3	1-2-3	1-2-3
42	4	1-2-3-4	4	4
43	2-3-4-3-2-1	1	1	4-3-2-1
44	2	1-2	4-3-2	1-2
45	4	2-3-4	1-2-3-4	3-4
46	3-2-1	1	1	4-3-2-1
47	2	1-1-2	4-3-2	1-2
48	1-2-3	1-2-3	1-2-3	3
49	2-1	1	4-3-2-1	4-3-2-1
50	1-2	1-2	1-2	1-2

Trial number	Girl 1 (3 years) Alleys entered	Girl 2 (3 years) Alleys entered	Girl 3 (4 years) Alleys entered	Girl 4 (4 years) Alleys entered
51	2-3	1-2-3	1-2-3	1-2-3
52	4	1-2-3-4	1-2-3-4	4
53	3-2	1-2	1-2	4-3-2
54	4-3	1-2-3	4-3	1-2-3
55	1-2-3-4	1-2-3-4	1-2-3-4	4
56	1	4-3-2-1	4-3-2-1	4-3-2-1
57	1-2-1-2-3	1-2-3	1-2-3	1-2-3
58	2-3-4	1-2-3-4	4	4
59	2-3-4-3-2-1	1	1	4-3-2-1
60	1-2	2	4-3-2	1-2
61	2-1-2-1-2-3-4	1-2-3-4	1-2-3-4	1-2-3-3-4
62	3-4-3-2-3-4-3-2-1	1	4-3-2-1	4-3-2-1
63	3-2	2	4-3-2	1-2
64	2-3	1-2-3	1-2-3	3
65	2-3-4-3-2-1	3-4-3-2-1	4-3-2-1	4-3-2-1
66	4-3-2	1-2	1-2	1-2
67	2-1-2-3	1-2-3	4-3	3
68	2-1-2-3-4	1-2-3-4	1-2-3-4	4
69	3-2	1-2	4-3-2	4-3-2
70	4-3	1-2-3	1-2-3	1-2-3
71	1-2-3-4	1-2-3-4	4	4
72	2-3-2-3-4-3-4-3-2-1	4-3-2-1	1	4-3-2-1
73	4-1-2-3	1-2-3	4-3	1-2-3
74	3-2-1-4	1-2-3-4	1-2-3-4	4
75	1	3-2-3-4-3-2-1	4-3-2-1	4-3-2-1
76	3-2		1-2	1-2
77	3-2-3-4		4	3-4
78	1		1	4-3-2-1
79	2		2	1-2
80	4-3		4-3	3
81	2-1		1	4-3-2-1
82	4-3-2		4-3-2	1-2
83	4-3		2-3	3
84	2-1-4		4	4
85	3-2		1-2	4-3-2
86	2-1-4-1-4-3		4-3	1-2-3
87	2-4		1-2-3-4	4
88	1		4-3-2-1	4-3-2-1
89	1-2-3		1-2-3	1-2-3
90	2-3-4		4	4
91	2-3-4-3-2-1		1	4-3-2-1
92	4-3-2		4-3-2	1-2
93	4		1-2-3-4	3-4
94	3-2-1		4-3-2-1	4-3-2-1
95	4-3-4-3-2		1-2	1-2
96	1-2-4-3		4-3	3
97	2-1-4		1-2-3-4	4
98	3-2-1		1	4-3-2-1
99	1-2		4-3-2	1-2
100	4-3		1-2-3	3

Trial No.	Girl 5 (5 years) Alleys entered	Girl 6 (5 years) Alleys entered	Girl 7 (5 years) Alleys entered	Girl 8 (5 years) Alleys entered
1	1	1	1	1
2	2	2	3-4-1-2	2
3	4-3	3	1-2-3	3
4	1-2-3-4	4	2-3-4	4
5	1-2	2	1-2	3-2
6	1-2-3	2-3	1-2-3	3
7	1-2-3-4	4	1-2-3-4	4
8	1	2-3-4-2-1	1	3-2-1
9	1-2-3	1-2-3	1-2-3	2-3
10	1-2-3-4	4	2-3-4	4
11	1	4-3-2-1	1	1
12	2	2	1-2	2
13	4	3-2-4	1-2-3-4	3-4
14	2-4-3-2-1	3-2-1	1	3-2-3-4-3-2-3-4-3-2 [3-2-3-4-3-2-1
15	3-4-3-2	2	1-2	2
16	2-1-3	3	1-2-3	3
17	3-4-2-1	3-4-2-1	1	4-3-2-1
18	1-2	2	1-2	3-2
19	4-3	3	1-2-3	1-2-1-2-3
20	1-2-3-4	4	4	4
21	4-3-2	4-3-2	1-2	1-2
22	4-3	2-3	1-2-3	4-3
23	4	3-4	4	3-2-1-2-3-4
24	4-3-2-1	2-3-4-3-2-1	1	3-2-1
25	4-3	2-3	1-2-3	3
26	4	3-4	4	4
27	4-3-2-1	3-4-3-2-1	1	3-2-3-4-3-2-1
28	3-2	2	1-2	2
29	4	2-3-4	1-2-3-4	3-4
30	2-1	2-1	1	3-2-1
31	4-3-2	4-3-2	1-2	1-2
32	4-3	2-3	4-3	3
33	4-3-2-1	4-3-2-1	1	4-3-2-3-4-3-2-1
34	4-3-2	2	1-2	4-3-2
35	4-3	3	1-2-3	4-3
36	4	4	1-2-3-4	4
37	4-3-2	2	1-2	3-2
38	4-3	2-3	4-3	4-3
39	4	2-3-4	1-2-3-4	2-3-4
40	4-3-2-1	3-4-2-1	1	3-2-1
41	4-3	4-3	1-2-3	1-2-3
42	3-2-1-2-3-4	2-1-2-3-4	4	4
43	4-3-2-1	2-1	1	3-2-3-4-3-2-1
44	4-3-2	2	1-2	2
45	4	3-4	4	3-2-3-4
46	4-3-2-1	3-4-3-2-1	1	3-2-3-2-1
47	4-3-2	2	1-2	2
48	4-3	3	2-3	3
49	4-3-2-1	4-3-2-1	4-3-2-1	4-3-2-3-1-3-2-1
50	4-3-2	3-2	1-2	2

Trial No.	Girl 5 (5 years) Alleys entered	Girl 6 (5 years) Alleys entered	Girl 7 (5 years) Alleys entered	Girl 8 (5 years) Alleys entered
51	4-3	4-3	1-2-3	1-2-3
52	1-2-3-4	2-1-2-3-4	4	4
53	4-1-2	4-3-2	1-2	3-2
54	4-3	2-3	4-3	3
55	4	2-3-4	1-2-3-4	4
56	3-2-1	2-3-4-2-1	4-3-2-1	3-2-3-4-3-4-3-2-3 [4-3-2-1
57	4-3	3	1-2-3	3
58	2-1-2-3-4	4	4	4
59	4-3-2-1	2-3-2-1	1	3-2-1
60	4-3-2	3-2	4-3-2	3-4-3-4-3-2
61	4	4	1-2-3-4	1-2-3-4
62	3-2-1	3-2-1	4-3-2-1	3-2-1
63	4-3-2	3-4-2	1-2	2
64	4-3	3	4-3	3
65	2-1	4-3-2-1	4-3-2-1	4-3-2-3-4-3-2-1
66	4-3-2	3-2	1-2	2
67	4-3	4-2-1-3	4-3	3
68	4	2-1-3-4	4	4
69	3-2	2	4-3-2	3-2
70	2-1-2-3	3	1-2-3	3
71	1-2-3-4	4	4	4
72	4-3-2-1	3-2-1	4-3-2-1	3-2-1
73	4-3	2-3	1-2-3	2-3
74	2-1-2-3-2-3-4	3-4	4	4
75	4-3-2-1	2-3-4-3-2-1	1	3-2-1
76	4-3-2	1-2	1-2	1-2
77	4	3-4	4	3-4
78	4-3-2-1	4-3-2-1	4-3-2-1	3-2-1
79	4-3-2	1-2	1-2	2
80	4-3	3	4-3	3
81	4-3-2-1	4-3-2-1	4-3-2-1	4-3-2-3-4-3-2-3-4 [3-2-3-4-1
82	4-3-2	2	1-2	2
83	4-3	3	1-2-3	4-3
84	2-1-2-3-2-1-2-3-2 [1-2-3-4	4	4	2-1-2-3-4
85	4-3-2	1-2	4-3-2	2
86	1-2-3	2-3	4-3	3
87	4	2-3-4	1-2-3-4	4
88	4-3-2-1	1	4-3-2-1	3-2-1
89	4-3	2-3	1-2-3	1-2-3
90	2-1-2-3-4	4	4	4
91	4-3-2-1		4-3-2-1	3-2-1
92	4-3-2		1-2	2
93	1-2-3-4		4	3-4
94	4-3-2-1		4-3-2-1	3-2-1
95	4-3-2		1-2	2
96	1-2-3		4-3	3
97	4		4	4
98	3-2-1		4-3-2-1	3-2-1
99	4-3-2		1-2	2
100	4-3		4-3	3

Trial No.	Girl 9 (5 years) Alleys entered
1	1
2	2
3	2-3
4	2-3-4
5	2
6	2-3
7	2-3-4
8	2-3-4-2-3-4-2-3-4-2-3-4-1
9	1-2-3
10	1-2-3-2-1-2-3-1-2-3-2-3-2-3-2-3-1-2 [3-1-3-2-3-2-3-2-3-2-3-2-3-4
11	1
12	2
13	1-2-3-4
14	1
15	2
16	2-3
17	2-3-4-1
18	2
19	2-3
20	2-3-4
21	1-2
22	2-3
23	4
24	2-3-4-3-2-3-4-1
25	2-3
26	4
27	3-2-3-4-3-4-1
28	4-3-2
29	3-2-1-2-3-4
30	2-3-4-3-2-3-2-3-4-3-2-3-4-3-2-3-4-3-2-1
31	1-2
32	3
33	2-3-4-1
34	2
35	3
36	2-3-4
37	1-2
38	2-3
39	2-3-4
40	2-3-4-1
41	1-2-3
42	2-3-2-3-4
43	1
44	2
45	2-3-4
46	2-3-4-3-2-1
47	2
48	2-3
49	2-3-4-3-2-3-4-3-2-1
50	2

Trial No.	Girl 9 (5 years) Alleys entered
51	1-2-3
52	2-3-4
53	2
54	2-3
55	2-3-4
56	2-3-4-3-4-3-2-1
57	2-3
58	2-3-4
59	1
60	2
61	1-2-3-4
62	2-3-4-1
63	2
64	1-2-3
65	2-3-4-2-3-4-2-3-4-2-3-4-3-2-1
66	1-2
67	1-2-3
68	1-2-3-4
69	1-2
70	2-3
71	2-3-4
72	2-3-4-3-2-3-4-2-3-4-3-2-4-3-2-3-4-1
73	2-3
74	1-2-3-4
75	1
76	1-2
77	2-3-4
78	2-3-4-3-4-3-2-1
79	1-2
80	2-3
81	3-2-1
82	2
83	2-3
84	2-3-4
85	2
86	2-3
87	3-2-4
88	1
89	2-1-2-3
90	2-3-4
91	1
92	2
93	2-1-2-3-4
94	2-3-4-3-2-1
95	2
96	2-3
97	2-3-2-1-2-3-4
98	1
99	2
100	2-3

Trial number	Girl 10 (6 years) Alleys entered	Girl 11 (7 years) Alleys entered	Girl 12 (7 years) Alleys entered	Girl 13 (7 years) Alleys entered
1	1	1	1	1
2	1-2	1-2	3-4-2	2
3	1-2-3	1-2-3	2-3	3
4	1-2-3-4	1-2-3-4	3-4	4
5	1-2	1-2	3-4-3-2	1-2
6	1-2-3	4-3	1-2-3	3
7	1-2-3-4	1-2-3-4	3-4	4
8	1	2-3-4-2-1	3-4-3-2-1	1
9	2-3	3	2-3	3
10	1-2-3-4	2-3-4	2-3-4	4
11	1	1	1	1
12	1-2	1-2	3-4-3-2	2
13	1-2-3-4	3-4	3-4	3-4
14	1	2-3-4-2-1	2-1	1
15	1-2	3-4-3-2	2	2
16	1-2-3	2-1-3	2-3	3
17	1	2-3-4-2-1	2-3-4-3-2-1	4-3-2-1
18	1-2	3-4-2	2	1-2
19	1-2-3	2-1-2-3	3	2-3
20	1-2-3-4	4	3-2-1-2-3-4	3-4
21	1-2	1-2	1-2	1-2
22	1-2-3	4-3	3	2-3
23	1-2-3-4	2-1-3-4	3-4	3-4
24	1	3-4-2-1	3-4-3-2-1	1
25	1-2-3	3	3	2-3
26	1-2-3-4	2-1-2-3-4	2-1-2-3-4	4
27	1	2-1	2-1	1
28	1-2	3-4-2	2	2
29	1-2-3-4	3-4	3-2-1-2-3-4	4
30	1	3-2-1	1	1
31	1-2	1-2	1-2	1-2
32	1-2-3	3	3	2-3
33	1	2-1	3-4-3-2-1	1
34	1-2	2	3-2	2
35	1-2-3	1-2-3	3	3
36	1-2-3-4	4	3-4	4
37	1-2	2	3-2	1-2
38	1-2-3	1-2-3	3	2-3
39	1-2-3-4	2-3-4	2-1-2-3-4	4
40	1	2-3-4-2-1	2-1	1
41	1-2-3	1-2-3	2-3	1-2-3
42	1-2-3-4	4	3-4	2-3-4
43	1	2-1	3-2-1	4-1
44	1-2	2	3-2	2
45	1-2-3-4	4	2-1-2-3-4	2-3-4
46	1	2-1	2-3-4-3-2-1	4-3-2-1
47	1-2	4-3-2	3-4-3-2	2
48	2-3	3	3	2-3
49	1	2-1	2-1	3-4-3-2-1
50	1-2	3-4-2	1-2	2

Trial number	Girl 10 (6 years) Alleys entered	Girl 11 (7 years) Alleys entered	Girl 12 (7 years) Alleys entered	Girl 13 (7 years) Alleys entered
51	1-2-3	1-2-3	3	1-2-3
52	1-2-3-4	2-1-3-4	3-2-1-2-3-4	2-3-4
53	1-2	3-2	3-2	1-2
54	1-2-3	1-3	2-1-2-3	2-3
55	1-2-3-4	2-1-4	2-1-2-3-4	4
56	1	1	2-1	4-3-2-1
57	1-2-3	2-1-2-3	3	4-3
58	1-2-3-4	3-2-1-4	2-1-2-3-4	3-2-1-2-3-4
59	1	3-2-1	3-2-1	4-3-2-1
60	1-2	2	2	1-2
61	1-2-3-4	1-2-3-4	1-2-3-4	1-2-3-4
62	1	2-1	3-2-1	3-2-1
63	2	3-2	2	2
64	1-2-3	2-1-3	1-2-3	3
65	1	1	2-1	4-3-2-1
66	2	2	3-2	2
67	1-2-3	2-1-2-3	1-2-3	3
68	1-2-3-4	4	3-4	4
69	1-2	2	3-2	3-2
70	1-2-3	2-1-3	3	2-1-2-3
71	1-2-3-4	4	1-2-3-4	1-2-3-4
72	1	3-2-1	3-2-1	3-2-1
73	2-3	4-3	2-3	2-3
74	1-2-3-4	2-1-2-3-4	1-2-3-4	3-4
75	1	2-1	3-2-1	3-2-1
76	1-2	3-2	3-2	2
77	1-2-3-4	3-2-1-2-3-4	2-1-2-3-4	3-4
78	2-3-4-3-2-1	3-2-1	3-2-1	3-2-1
79	2	4-3-2	4-3-2	3-2
80	2-3	4-3	2-1-2-1-2-3	3
81	1	1	1	1
82	2	2	2	2
83	1-2-3	3	3	3
84	2-3-4	4	2-1-2-3-4	4
85	1-2	3-2	4-3-2	3-2
86	2-3	3	2-1-2-3	3
87	2-3-4	2-1-2-3-4	4	4
88	2-3-4-3-2-1	3-2-1	3-2-1	3-2-1
89	1-2-3	4-3	3	3
90	2-3-4	3-2-1-2-3-4	2-1-2-3-4	2-1-2-3-4
91	1	1	1	1
92	2	2	2	2
93	1-2-3-4	3-4	3-4	4
94	2-3-4-3-2-1	3-2-1	3-2-1	3-2-1
95	2	3-2	4-3-2	2
96	2-3	3	1-2-3	3
97	2-3-4	4	4	4
98	3-4-3-2-1	3-2-1	3-2-1	3-2-1
99	2	3-2	4-3-2	2
100	2-3	3	4-3	3

Trial number	Girl 14 (7 years) Alleys entered	Girl 15 (7 years) Alleys entered	Girl 16 (9 years) Alleys entered	Girl 17 (9 years) Alleys entered
1	2-3-4-3-1	1	1	1
2	2	2	1-2	2
3	3	3	2-3	3
4	3-4	4	1-2-3-4	4
5	1-2	1-2	1-2	2
6	3	3	2-3	4-3
7	4	4	1-2-3-4	2-1-2-3-4
8	1	1	1	2-1
9	4-3	2-3	1-2-3	2-3
10	4	4	1-2-3-4	1-2-3-4
11	2-3-4-1	1	1	1
12	3-2	2	2	2
13	3-4	3-4	3-4	3-4
14	1	1	1	2-1
15	3-2	2	2	3-4-2
16	3	3	3	1-2-3
17	4-3-2-1	4-3-2-1	4-3-2-1	3-4-3-2-1
18	4-3-2	4-3-2	1-2	2
19	1-2-3	1-2-3	3	2-3
20	4	1-2-3-4	4	2-1-2-3-4
21	2	1-2	1-2	1-2
22	3	3	2-3	3
23	4	4	4	4
24	1	4-3-2-1	1	2-1
25	2-3	4-3	2-3	3
26	4	2-1-2-3-4	4	4
27	1	2-1	4-3-2-1	1
28	3-2	4-3-2	1-2	2
29	4	1-2-3-4	2-3-4	3-4
30	4-3-2-1	2-3-4-3-2-1	4-3-2-1	3-2-1
31	1-2	1-2	1-2	1-2
32	2-3	3	2-3	4-3
33	4-3-2-1	4-3-2-1	4-3-2-1	2-1
34	4-3-2	4-3-2	4-3-2	4-3-2
35	1-2-3	4-3	3	4-3
36	4	1-2-3-4	2-1-2-3-4	4
37	4-3-2	4-3-2	4-3-2	1-2
38	1-2-3	1-2-3	1-2-3	2-1-3
39	4	4	2-1-2-3-4	2-1-2-3-4
40	4-3-2-1	3-2-1	3-2-1	2-3-4-2-3-1
41	4-3	1-2-3	1-2-3	1-2-3
42	1-2-3-4	3-2-1-2-3-4	4	3-4
43	1	4-3-2-1	1	3-2-1
44	2	4-3-2	2	2
45	4	1-2-3-4	3-4	2-1-2-3-4
46	1	4-3-2-1	3-2-1	3-2-1
47	1-2	3-2	3-2	1-2
48	4-3	3	2-3	4-3
49	1	2-3-2-1	4-3-2-1	4-3-2-1
50	4-3-2	3-2	4-3-2	3-2

Trial number	Girl 14 (7 years) Alleys entered	Girl 15 (7 years) Alleys entered	Girl 16 (9 years) Alleys entered	Girl 17 (9 years) Alleys entered
51	1-2-3	2-3	1-2-3	1-2-3
52	4	3-2-1-2-3-4	3-4	4
53	2	3-2	4-3-2	3-2
54	1-2-3	4-3	2-1-2-3	1-2-3
55	4	2-1-2-3-4	3-4	3-2-1-2-3-4
56	1	2-3-4-3-2-1	4-3-2-1	1
57	2-3	1-2-3	4-3	3
58	1-2-3-4	3-4	3-2-1-2-3-4	3-2-1-2-3-4
59	2-3-4-3-2-1	2-3-4-3-2-1	3-2-1	3-2-1
60	3-2	3-2-1	2	3-2
61	1-2-3-4	4	1-2-3-4	1-2-3-4
62	3-2-1	3-2-1	4-3-2-1	2-1
63	1-2	4-3-2	2	1-2
64	4-3	1-2-3	3	2-3
65	1	4-3-2-1	4-3-2-1	3-2-1
66	4-3-2	4-3-2	3-2	2
67	4-3	2-1-2-3	1-2-3	4-3
68	2-1-2-3-4	4	4	4
69	1-2	3-2	3-2	3-2
70	4-3	1-2-3	1-2-3	3
71	1-2-3-4	4	1-2-3-4	1-2-3-4
72	4-3-2-1	3-2-1	3-2-1	3-2-1
73	4-3	4-3	2-3	3
74	1-2-3-4	2-1-2-3-4	4	3-2-1-2-3-4
75	4-3-2-1	4-3-2-1	3-2-1	4-3-2-1
76	4-3-2	1-2	4-3-2	3-2
77	1-2-3-4	3-4	4	3-2-1-2-3-4
78	4-3-2-1	1	3-2-1	3-2-1
79	4-3-2	2	4-3-2	3-2
80	1-2-3	3	1-2-3	3
81	1	4-1	1	1
82	4-3-2	2	2	3-2
83	1-2-3	1-2-3	3	3
84	4	4	4	2-1-2-1-2-3-4
85	1-2	1-2	3-2	3-4-2
86	4-3	3	1-2-3	3
87	2-1-2-3-4	4	4	2-1-3-4
88	4-3-2-1	1	3-2-1	3-2-1
89	4-3	3	2-3	3
90	2-1-2-3-4	4	3-4	3-2-3-4
91	1	1	1	1
92	4-3-2	2	2	3-4-2
93	2-1-2-3-4	3-4	1-2-3-4	2-1-3-2-4
94	3-2-1	1	3-2-1	3-2-1
95	4-3-2	2	2	3-2
96	2-1-2-3	3	3	2-1-2-3
97	4	4	4	3-2-1-2-3-4
98	1	1	3-2-1	3-2-1
99	4-3-2	2	2	3-2
100	2-1-2-3	3	3	4-3

Trial number	Girl 18 (9 years) Alleys entered	Girl 19 (9 years) Alleys entered	Girl 20 (12 years) Alleys entered
1	1	1	1
2	2	2	2
3	3	3	3
4	4	4	4
5	1-2	4-2	2
6	3	2-1-2-3	3
7	4	4	3-4
8	1	3-2-1	2-3-4-2-1
9	2-3	4-3	3
10	4	2-1-2-3-4	3-4
11	1	1	1
12	2	2	2
13	3-4	3-4	3-4
14	4-3-2-1	4-3-2-1	2-3-4-2-1
15	3-4-3-2	1-2	3-4-2
16	3	3	4-2-1-3
17	4-3-2-1	4-3-2-1	4-2-1
18	3-2	2	2
19	3	4-3	4-2-1-3
20	3-2-1-2-3-4	4	3-4
21	1-2	1-2	1-2
22	3	3	3
23	4	4	4
24	2-3-4-3-2-1	4-3-2-1	3-1
25	2-3	2-3	3
26	1-2-3-4	4	4
27	2-3-4-3-2-1	3-2-1	2-1
28	2	2	2
29	2-3-4	3-4	3-4
30	1	3-2-1	2-1
31	1-2	1-2	1-2
32	3	3	3
33	4-3-2-1	4-3-2-1	4-3-2-1
34	2	1-2	3-4-2
35	3	3	3
36	3-2-1-2-3-4	4	4
37	2	3-2	2
38	1-2-3	1-2-3	1-2-3
39	4	4	4
40	2-1	3-2-1	2-1
41	1-2-3	1-2-3	1-2-3
42	3-4	3-4	3-4
43	1	3-2-1	3-2-1
44	2	2	2
45	3-4	3-4	3-4
46	1	3-2-1	1
47	3-2	2	3-4-2
48	4-3	3	2-3
49	2-1	4-3-2-1	4-3-2-1
50	2	2	3-4-2

Trial number	Girl 18 (9 years) Alleys entered	Girl 20 (12 years) Alleys entered
51	1-2-3	1-2-3
52	3-4	3-4
53	1-2	2
54	3	1-2-4-3
55	4	2-1-3-4
56	1	2-1
57	3	3
58	1-2-3-4	2-1-3-4
59	2-3-4-3-2-1	2-1
60	3-2	2
61	1-2-3-4	1-2-3-4
62	1	3-2-1
63	3-4-3-2	2
64	3	3
65	1	1
66	2	2
67	3	3
68	4	2-4
69	1-2	4-2
70	3	1-3
71	1-2-3-4	1-2-3-4
72	2-3-4-3-2-1	3-1
73	3	2-1-3
74	1-2-3-4	4
75	2-3-4-3-2-1	2-1
76	4-3-2	3-4-2
77	2-1-2-3-4	2-1-3-4
78	2-1	3-2-1
79	2	2
80	3	3
81	1	1
82	2	2
83	3	3
84	3-4	4
85	4-3-2	2
86	3	2-1-3
87	3-4	3-1-3-4
88	2-1	3-2-1
89	3	2-1-3
90	2-3-4	2-4
91	1	1
92	2	3-2
93	3-4	2-3-4
94	3-4-3-2-1	2-1
95	3-2	3-2
96	3	1-3
97	4	3-2-1-3-4
98	2-1	3-2-1
99	2	3-1-2
100	4-3	4-3

Trial No.	White Rat 5 (Adult) Alleys entered
1	4-4-3-4-3-2-1
2	4-3-2
3	1-1-2-4-3
4	2-1-1-3-4
5	4-3-2
6	3
7	4
8	4-3-2-1
9	3
10	4
11	3-2-1
12	3-2
13	4
14	3-2-1
15	3-2
16	4-3
17	3-2-1
18	3-2
19	4-3
20	3-2-1-2-1-2-1-3-2-1-3-2 1-2-1-2-1-3-4
21	4-3-2
22	2-1-3
23	1-2-3-4
24	2-3-2-1
25	1-2-1-2-1-2-1-2-3
26	3-2-1-2-1-2-3-4
27	1
28	4-2
29	1-3-2-1-4
30	2-1
31	1-2
32	2-3
33	2-1
34	2
35	1-2-3
36	1-2-3-4
37	1-2
38	2-3
39	1-2-3-2-1-2-3-4
40	1
41	1-2-3
42	1-2-3-4
43	1
44	1-2
45	1-2-3-4
46	1
47	1-2
48	1-2-3
49	1
50	1-2

Trial No.	White Rat 5 (Adult) Alleys entered
51	1-2-3
52	1-2-3-4
53	1-2
54	1-2-1-2-1-2-3
55	1-2-3-2-3-2-1-2-3-4
56	2-3-4-4-3-2-1
57	1-2-3
58	3-4
59	1
60	1-2
61	3-2-3-1-2-1-2-3-1-2-3-4
62	2-3-2-3-4-3-2-3-4-3-2-1
63	1-2
64	2-1-2-3
65	3-2-1
66	1-2
67	3
68	1-2-3-2-1-2-3-2-1-2-3-4
69	1-2
70	1-2-3
71	2-3-2-3-2-3-2-1-2-3-2-3-2-3-2-3-2-3-2-1-3 1-2-1-2-3-4
72	2-3-4-3-2-3-2-3-4-3-4-3-2-3-2-3-1
73	3
74	1-2-3-4
75	2-3-4-3-4-3-2-3-2-1
76	2
77	2-1-2-3-2-3-4
78	3-2-4-3-2-1
79	2
80	2-1-2-1-2-1-2-3
81	2-3-2-3-2-3-2-3-4-3-2-3-4-3-4-3-2-1
82	3-4-3-2
83	1-2-3
84	1-2-3-4
85	4-3-1-2
86	1-2-3
87	3-1-2-3-1-4
88	2-3-2-1
89	3
90	2-2-1-2-3-4
91	1
92	2
93	2-3-2-3-2-1-2-1-2-3-1-2-3-1-2-2-3-2-1-3-4
94	3-2-2-3-1
95	2
96	2-3
97	2-3-2-1
98	3-1-2
99	3
100	2-1-3-1-2-1-3-2-1-2-1-2-1-3-4

ANALYSIS OF WHITE RAT 5'S RECORD

Trial number	Alleys				Alley first entered	Right alley	Relation of first choice to previous trials
	1	**2**	**3**	**4**			
1	1	4	5	5			
	1	1	2	3	4	1	Frequency
2	2	5	7	8			
	0	1	1	1	4	2	Frequency
3	2	6	8	9			
	2	1	1	1	1	3	
4	4	7	9	10			
	2	1	1	1	2	4	
5	6	8	10	11			
	0	1	1	1	4	2	Recency-Frequency
6	6	9	11	12			
	0	0	1	0	3	3	
7	6	9	12	12			
	0	0	0	1	4	4	Frequency
8	6	9	12	13			
	1	1	1	1	4	1	Recency-Frequency
9	7	10	13	14			
	0	0	1	0	3	3	
10	7	10	14	14			
	0	0	0	1	4	4	Frequency
11	7	10	14	15			
	1	1	1	0	3	1	
12	8	11	15	15			
	0	1	1	0	3	2	Frequency
13	8	12	16	15			
	0	0	0	1	4	4	
14	8	12	16	16			
	1	1	1	0	3	1	Frequency
15	9	13	17	16			
	0	1	1	0	3	2	Frequency
16	9	14	18	16			
	0	0	1	1	4	3	
17	9	14	19	17			
	1	1	1	0	3	1	Recency-Frequency
18	10	15	20	17			
	0	1	1	0	3	2	Frequency
19	10	16	21	17			
	0	0	1	1	4	3	

ANALYSIS OF WHITE RAT 5'S RECORD—*Continued*

Trial number	Alleys				Alley first entered	Right alley	Relation of first choice to previous trials
	1	**2**	**3**	**4**			
20	10	16	22	18			
	7	7	4	1	3	4	Recency-Frequency
21	17	23	26	19			
	0	1	1	1	4	2	Recency
22	17	24	27	20			
	1	1	1	0	2	3	Recency
23	18	25	28	20			
	1	1	1	1	1	4	
24	19	26	29	21			
	1	2	1	0	2	1	
25	20	28	30	21			
	4	4	1	0	1	3	Recency
26	24	32	31	21			
	2	3	2	1	3	4	Recency
27	26	35	33	22			
	1	0	0	0	1	1	
28	27	35	33	22			
	0	1	0	1	4	2	
29	27	36	33	23			
	2	1	1	1	1	4	
30	29	37	34	24			
	1	1	0	0	2	1	Frequency
31	30	38	34	24			
	1	1	0	0	1	2	Recency
32	31	39	34	24			
	0	1	1	0	2	3	Recency-Frequency
33	31	40	35	24			
	1	1	0	0	2	1	Frequency
34	32	41	35	24			
	0	1	0	0	2	2	Frequency
35	32	42	35	24			
	1	1	1	0	1	3	
36	33	43	36	24			
	1	1	1	1	1	4	
37	34	44	37	25			
	1	1	0	0	1	2	
38	35	45	37	25			
	0	1	1	0	2	3	Recency-Frequency

Analysis of White Rat 5's Record—*Continued*

Trial number	Alleys				Alley first entered	Right alley	Relation of first choice to previous trials
	1	2	3	4			
39	35	46	38	25			
	2	3	2	1	1	4	
40	37	49	40	26			
	1	0	0	0	1	1	
41	38	49	40	26			
	1	1	1	0	1	3	Recency
42	39	50	41	26			
	1	1	1	1	1	4	
43	40	51	42	27			
	1	0	0	0	1	1	
44	41	51	42	27			
	1	1	0	0	1	2	Recency
45	42	52	42	27			
	1	1	1	1	1	4	
46	43	53	43	28			
	1	0	0	0	1	1	
47	44	53	43	28			
	1	1	0	0	1	2	Recency
48	45	54	43	28			
	1	1	1	0	1	3	
49	46	55	44	28			
	1	0	0	0	1	1	
50	47	55	44	28			
	1	1	0	0	1	2	Recency
51	48	56	44	28			
	1	1	1	0	1	3	
52	49	57	45	28			
	1	1	1	1	1	4	
53	50	58	46	29			
	1	1	0	0	1	2	
54	51	59	46	29			
	3	3	1	0	1	3	
55	54	62	47	29			
	2	4	3	1	1	4	
56	56	66	50	30			
	1	2	2	2	2	1	Frequency
57	57	68	52	32			
	1	1	1	0	1	3	Recency

Analysis of White Rat 5's Record—*Continued*

Trial number	Alleys				Alley first entered	Right alley	Relation of first choice to previous trials
	1	**2**	**3**	**4**			
58	58 0	69 0	53 1	32 1	3	4	Recency
59	58 1	69 0	54 0	33 0	1	1	
60	59 1	69 1	54 0	33 0	1	2	Recency
61	60 3	70 4	54 4	33 1	3	4	
62	63 1	74 4	58 5	34 2	2	1	Frequency
63	64 1	78 1	63 0	36 0	1	2	Recency
64	65 1	79 2	63 1	36 0	2	3	Recency-Frequency
65	66 1	81 1	64 1	36 0	3	1	Recency
66	67 1	82 1	65 0	36 0	1	2	Recency
67	68 0	83 0	65 1	36 0	3	3	
68	68 3	83 5	66 3	36 1	1	4	
69	71 1	88 1	69 0	37 0	1	2	
70	72 1	89 1	69 1	37 0	1	3	
71	73 4	90 12	70 10	37 1	2	4	Frequency
72	77 1	102 5	80 8	38 3	2	1	Frequency
73	78 0	107 0	88 1	41 0	3	3	
74	78 1	107 1	89 1	41 1	1	4	
75	79 1	108 3	90 4	42 2	2	1	Frequency
76	80 0	111 1	94 0	44 0	2	2	Frequency

Analysis of White Rat 5's Record—*Continued*

Trial number	Alleys 1	2	3	4	Alley first entered	Right alley	Relation of first choice to previous trials
77	80	112	94	44			
	1	3	2	1	2	4	Recency-Frequency
78	81	115	96	45			
	1	2	2	1	3	1	
79	82	117	98	46			
	0	1	0	0	2	2	Frequency
80	82	118	98.	46			
	3	4	1	0	2	3	Recency-Frequency
81	85	122	99	46			
	1	6	8	3	2	1	Frequency
82	86	128	107	49			
	0	1	2	1	3	2	
83	86	129	109	50			
	1	1	1	0	1	3	
84	87	130	110	50			
	1	1	1	1	1	4	
85	88	131	111	51			
	1	1	1	1	4	2	Recency
86	89	132	112	52			
	1	1	1	0	1	3	
87	90	133	113	52			
	2	1	2	1	3	4	Recency
88	92	134	115	53			
	1	2	1	0	2	1	Frequency
89	93	136	116	53			
	0	0	1	0	3	3	
90	93	136	117	53			
	1	3	1	1	2	4	Frequency
91	94	139	118	54			
	1	0	0	0	1	1	
92	95	139	118	54			
	0	1	0	0	2	2	Frequency
93	95	140	118	54			
	5	9	6	1	2	4	Recency-Frequency
94	100	149	124	55			
	1	2	2	0	3	1	
95	101	151	126	55			
	0	1	0	0	2	2	Frequency

ANALYSIS OF WHITE RAT 5'S RECORD—*Continued-*

Trial number	Alleys				Alley first entered	Right alley	Relation of first choice to previous trials
	1	**2**	**3**	**4**			
96	101	152	126	55			
	0	1	1	0	2	3	Recency-Frequency
97	101	153	127	55			
	1	2	1	0	2	1	Frequency
98	102	155	128	55			
	1	1	1	0	3	2	
99	103	156	129	55			
	0	0	1	0	3	3	
100	103	156	130	55			
	6	5	3	1	2	4	Frequency
Total	109	161	133	56			

The above analysis requires the following explanation:

The upper one of the two horizontal rows of figures that are opposite each trial number indicates the number of times that each of the four alleys was entered during all preceding trials. For example, opposite trial No. 1 are the figures 1, 4, 5 and 5. They are meant to show that White Rat 5 had entered alley No. 1 once, alley No. 2 four times, alley No. 3 five times and alley No. 4 five times when she began her first formal trial, i.e., she had made these alley entrances during her preliminary trials. During her first formal trial she entered alley No. 1 once, alley No. 2 once, alley No. 3 twice and alley No. 4 three times. These figures appear in the second horizontal row opposite trial No. 1, and are appropriately distributed under the figures 1, 2, 3 and 4, which have the legend, "Alleys" for their heading. The two sets of figures opposite trial No. 1 and under the heading "Alleys" are added to give the uppermost horizontal row of figures opposite trial No. 2, and these sums show the number of times that each of the four alleys had been entered in all trials previous to formal trial No. 2.

The figures under the heading, "Alley first entered" indicate the subject's first choice of alley to enter during each of the 100 formal trials. The figures under the heading, " Right Alley " indicate the alley that actually afforded escape during each trial. The heading, " Relation of first choice to previous trials" is self-explanatory to readers who have followed foregoing discussions of the factors of recency and frequency in these studies.

REFERENCES

1. HAMILTON, G. V. A study of trial and error reactions in mammals. *Journal of* 1911. *Animal Behavior*, **1**, 33-66.
2. HOLT, E. B. The Freudian Wish. **vii.** New York. 1915.
3. YERKES, R. M. The study of human behavior. Contributions from the (Bos- 1913. ton) Psychopathic Hospital, 9-10.
4. YERKES, R. M. The mental life of monkeys and apes, etc. *Behavior Mono-* 1916. *graphs*, **3**, 1.
5. WATSON, J. B. Behavior. **62.** New York. 1914.
6. WATSON, J. B. Same, **257.**
7. WATSON, J. B. Same, **262.**

The Behavior Monographs

Edited by JOHN B. WATSON

The Johns Hopkins University, Baltimore, Md.

VOLUME 1

No. 1 The development of certain instincts and habits in chicks. By Frederick S. Breed. Pp. iv + 78, $1.00, postpaid.

No. 2 Methods of studying vision in animals. By Robert M. Yerkes and John B. Watson. Pp. iv + 90, $1.25, postpaid.

No. 3 An experimental study on the death-feigning of Belostoma (–Zaitha Aucct.) flumineum Say and Nepa apiculata Uhler. By Henry H. P. Severin and Harry C. Severin. Pp. iii + 47, $.65, postpaid.

No. 4 The biology of Physa. By Jean Dawson. Pp. iii + 120, $1.50, postpaid.

No. 5 The function of the vibrissae in the behavior of the white rat. By Stella Burnham Vincent. Pp. iv + 81, $1.15, postpaid.

VOLUME 2

No. 6 The delayed reaction in animals and children. By Walter S. Hunter. Pp. v + 86, $1.15, postpaid.

No. 7 The Canada porcupine: a study of the learning process. By Leroy Walter Sackett. Pp. iii + 84, $1.15, postpaid.

No. 8 Audition and habit formation in the dog. By Harry Miles Johnson. Pp. iv + 78, $1.00, postpaid.

No. 9 Habit formation in a strain of albino rats of less than normal brain weight. By Gardner Cheney Basset. Pp. iv + 46, $.65, postpaid.

No. 10 Distribution of effort in learning in the white rat. By John Linck Ulrich. Pp. iii + 51, $.65, postpaid.

No. 11 The effect of age on habit formation in the albino rat. By Helen B. Hubbert. Pp. v + 55, $.65, postpaid.

VOLUME 3

No. 12 The mental life of monkeys and apes: a study of ideational behavior. By Robert M. Yerkes. Pp. iv + 145, $1.50, postpaid.

No. 13 A study of perseverance reaction in primates and rodents. By G. V. Hamilton. iv + 65. $.75.

Regular subscription $3 per volume, postpaid.

THE BEHAVIOR MONOGRAPHS

Emerson Hall, Cambridge, Mass

BEHAVIOR MONOGRAPHS

Volume 3, Number 3, 1917 Serial Number 14

Edited by JOHN B. WATSON
The Johns Hopkins University

An Analysis of the Learning Process in the Snail, *Physa gyrina* Say

BY

ELIZABETH LOCKWOOD THOMPSON

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

The Journal of Animal Behavior

An organ for the publication of studies concerning the instincts,
habits and intelligence of organisms

The Journal contains a Department of Notes in which appear brief accounts of especially interesting and valuable observations of behavior.

Published bi-monthly at Cambridge, Boston, Mass., by Henry Holt and Company, New York. Each volume contains at least 450 pages with plates and text-figures.

The subscription price is $5.00 per volume (foreign subscription $5.50) postpaid.

Manuscripts, subscriptions, and all business correspondence should be addressed to

THE JOURNAL OF ANIMAL BEHAVIOR
Emerson Hall, Cambridge, Massachusetts

Behavior Monographs

For the publication of studies in behavior and intelligence which are
too lengthy or too expensive for acceptance
by The Journal of Animal Behavior

Published at irregular intervals at Cambridge, Boston, Mass., in connection with the Journal of Animal Behavior, by Henry Holt and Company, New York. Each volume contains approximately 450 pages with plates and text-figures.

The subscription price is $3.00 per volume (foreign subscription $3.50) postpaid.

Monographs may be purchased separately at prices varying with the cost of manufacture.

Manuscripts and inquiries concerning terms of publication should be addressed to the Editor of the Behavior Monographs,

JOHN B. WATSON,
The Johns Hopkins University,
Baltimore, Md.

Subscription to The Journal of Animal Behavior and the Behavior Monographs should be sent to Emerson Hall, Cambridge, Massachusetts.

BEHAVIOR MONOGRAPHS

Volume 3, Number 3, 1917 Serial Number 14

Edited by JOHN B. WATSON
The Johns Hopkins University

An Analysis of the Learning Process in the Snail, *Physa gyrina* Say

ELIZABETH LOCKWOOD THOMPSON

Contribution from the Zoological Laboratory of the University of Michigan No. 147
Ann Arbor, Mich.

28 FIGURES

Published
at Cambridge. Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

TABLE OF CONTENTS

AN ANALYSIS OF THE LEARNING PROCESS IN THE SNAIL, *PHYSA GYRINA* SAY

INTRODUCTION

The experiments included in this paper were begun with the purpose of studying the ability of snails to discriminate between stimuli. To this end a method was adopted analogous to the salivary reflex method of Pawlow. Its use showed, among other things, a form of modifiability of behavior that could be interpreted only as learning—the capacity to form associations. The original plan to use this capacity as a basis for studies in discrimination was then changed. The discrimination experiments were deferred and the work was continued in order to determine if the snail, which can form associations, can also solve a simple labyrinth. The paper thus falls into two parts. Part one deals with modifiability as disclosed by the method of conditioned stimuli—with the power to form simple associations. Part two takes up the further question of the ability of the snail to solve a labyrinth and discusses the relation of this to the formation of simple associations. It is the purpose of the writer to return to the study of discrimination.

To Professor Jacob Reighard the author is indebted for suggesting the problem and for advice during the progress of the work; to Professor John F. Shepard she is indebted for numerous criticisms and suggestions.

I. MODIFIABILITY AS STUDIED BY A METHOD ANALOGOUS TO THE PAWLOW SALIVARY REFLEX METHOD

1. Outline of the Problem

The snail, *Physa gyrina* Say, like many other species, has the habit of crawling suspended from the surface film of the water. Its shell is then below and the ventral surfaces of the muscular foot and the head are exposed to view from above. The mouth may then often be seen to open and close many times in succession. Dawson (1911) found that when the region within a millimeter or two of the mouth was touched with a bit of food or a clean glass rod, a chewing motion of the mouth parts began which "would continue for perhaps an hour or so, even if the animal were placed in fresh clean water." She found that the same stimuli applied to other parts of the animal produced no movements of the mouth parts. Familiarity with the mouth movements of this snail suggested that it might be a reflex of the same definite character as the salivary reflex made use of by Pawlow and his pupils in their studies of association and discrimination in the dog.

Pawlow (1904) opened the duct of the parotid gland of a dog to the outside by means of a fistula. He then measured the secretion and determined its quality (viscosity), when induced, first by the odor or sight of food unaccompanied by other experimental stimuli. Pawlow called this food stimulus an "unconditioned" stimulus. He then measured the secretion induced by food stimulation in the presence of an auxiliary (or secondary) stimulus such as sound or color. This he called a "conditioned" stimulus. After using the conditioned stimulus for a time, he omitted the primary stimulus (food) and found that the secretion was induced by the auxiliary stimulus, the tone or color, alone. He believed that he had shown conclusively that the animal had formed an association between the two stimuli since at first it had not reacted to the secondary stimulus, but after this stimulus had been used for a time in connection with the primary or food stimulus it had reacted to the secondary stimulus alone. Then by changing slightly the tone, color or other secondary stimulus used, he determined whether the animal reacted to this altered stimulus and to what extent. A change in the reaction was taken as evidence that the animal discriminated between the original and the altered stimulus. Thus power to discriminate between stimuli was tested.

Pawlow and his followers believed that the method could be used on only a limited number of mammals. "Its obvious limitation appears in the number of organisms with which it may be employed. Evidently it cannot be used for the study of animals which lack salivary glands, and even among those animals which do possess these glands there are many which surely would not lend themselves satisfactorily to the method. It seems therefore as if Pawlow's method were especially important in animal psychology as a means to the intensive study of the mental life of a limited number of mammals." (Yerkes and Morgulis, 1909.) Theoretically, however, this method is applicable to any organism that responds to a stimulation by a specific, measurable reflex of muscles or glands. Such reflexes occur in many of the lower forms. The mouth movement of Physa is an example.

It was decided then to study:

(1.) The character of the response following a single application of a food stimulus to the mouth parts of the snail. Such a response consists of one or more reactions. A single complete movement—one reaction—may be compared to the secretion of a drop of saliva in the Pawlow experiments. A determination of the number of reactions per response was deemed comparable to Pawlow's measurements of the salivary secretion induced by food alone or resulting from an "unconditioned" stimulus.

(2.) Following this the application of two stimuli together, that is, food to the mouth parts and pressure at a fixed distance from the mouth, would, it was believed, correspond to Pawlow's "conditioned" stimulus. The number of reactions constituting each response would correspond to his measurements of saliva under such conditions.

(3.) It was then planned to apply the associated or auxiliary stimulus alone in the absence of food, in order to determine from the presence or absence of reactions, whether or not an association had been formed between the two stimuli.

2. Material and Methods

The snails* used in the experiments were collected in an oxbow pond that had been completely cut off from a small creek. Several hundred were brought into the laboratory and placed in large glass aquaria containing water plants such as Elodea and Spirogyra. An effort was made to have the environment in these aquaria as nearly like the natural habitat as possible. Besides the water plants upon which snails usually feed, fresh lettuce was placed in the aquaria. They ate this in comparatively large quantities, and seemed to prefer it to other plant food.

---

* The author is indebted to H. B. Baker and Harold Cummins for the identification of the snails used in the experiments.

When the tank containing recently captured snails was jarred or disturbed in any way, they would instantly expel the air from the lung, retract into their shells, and drop from the surface film upon which they were crawling, to the bottom of the tank. Many of them frequently remained motionless on the bottom for an hour or more before they again sought the surface and refilled their lungs. It became necessary therefore, to "tame" the snails which were to be used in the experiments, to so accustom them to handling that they would remain extended and retain the air in the lung while they were being worked upon.

About twenty specimens of approximately the same size were selected for the tests. These were placed in two bacteria dishes. In each dish was about a liter of water with water plants and as much lettuce as the snails would eat. The snails were taken in the hand at intervals and moved about under water. Each was held in the hand beneath the water until it emerged from the shell and suspended itself from the surface film. They gradually became accustomed to this handling to such an extent that they could be moved from dish to dish, at the will of the operator, without retracting or expelling the air from the lung.

These "tamed" snails were then divided into two groups. Each individual was numbered by means of white water-proof paint applied to the shell after it had been thoroughly dried. These groups were worked on alternate days. Each group was starved for a period of twenty-four hours before tests were made. The snails belonging to the group were thus believed to be in approximately the same state of hunger at the time of experimentation. After a group had been worked on it was allowed to feed for twenty-four hours and was again starved, before it was used. In this way an attempt was made to obtain physiological uniformity among the snails directly under observation. The physical conditions were also kept as uniform as possible. The whole series of daily experiments was carried on at approximately the same hour. Each snail in turn was placed in a clean dish of fresh, filtered tap water. The water was of the same depth and at the same (room) temperature as that in which the snails had been living and the lighting was at all times uniform with that to which they were accustomed. Each individual was put through a series of ten tests per day with one exception (check series, Table I, p. 6) and a separate record made of each.

3. THE RESPONSE TO THE FOOD STIMULUS

In order to utilize the snails it was necessary to know:

(1) Whether the mouth reactions occur only as the result of external stimulation, and

(2) Their constancy as to the number of reactions and the duration of the response following a single stimulus. Should they occur *regularly* as the result of external stimulation and *only then*, they must be regarded as "involuntary" responses or reflexes available for the purpose of the experiments. Upon the constancy and duration of the response depends its value for quantitative uses.

A. *The response to the food stimulus a reflex.*—It was found by observation not only that the mouth movements of Physa were induced by food and mechanical stimulation of the mouth region, but that they also sometimes occurred when no such stimuli could be detected. Such apparently voluntary movements might be due to the stimuli from microscopic particles in the water. A check series of experiments in filtered *water was deemed necessary in order to determine whether or not the mouth reactions might normally occur without external stimulation and if so under what conditions and with what frequency. In these tests each snail, under the controlled conditions just mentioned, was in turn held in the hand of the operator until it suspended itself from the surface film. It was then permitted to move about, and was touched only when necessary to keep it from crawling down the sides of the dish. At such times it was pushed gently toward the center of the tank with a sterile glass rod. Each animal was thus kept crawling on the surface film for an average period of forty-five minutes under constant observation and a record was made of each mouth reaction. It was found to be impossible to keep the snail absolutely free from all chance stimulation of the mouth. It has been shown that the animal uses the same method of locomotion in crawling on the surface film that it does in moving over any substratum, that is, it crawls along a mucous path which it secretes as it moves and which remains behind it on the surface of the water. (Parker, 1912), (Dawson, 1911). The mucus thus left on the surface may reach the mouth of the snail as it moves about. In

---

*A Berkefeldt filter was used.

several instances this mucus was observed to touch the mouth parts and such contact was followed by reactions. These are noted in the following table. Stimulation of this sort is the probable cause of all other reactions that occur in the test series.

TABLE I

Showing the number of reactions of the mouth of six individuals of *Physa gyrina* Say, kept under observation in filtered tap water for 32.75 hours, when not stimulated by the application of food to the mouth region. The snails had been without food for 24 hours preceding the tests. They were divided into two groups of three each and tested on alternating days.

Snail No.	No. of days tested	Period of observation in minutes each day	No. of mouth reactions, no visible stimulus	No. of mouth reactions, mucous stimulation
18	1	60	0	0
	2	45	0	0
	3	60	0	0
	4	60	0	0
	5	60	0	0
	6	60	0	0
19	1	60	0	0
	2	45	2	14
	3	60	0	5
	4	60	1	0
	5	60	0	0
	6	60	0	0
20	1	60	0	0
	2	45	0	0
	*3	60	0	0
	4	60	0	0
	5	60	0	0
21	1	30	0	0
	2	60	0	0
	3	60	0	0
	4	60	0	0
	5	60	0	0
	6	60	0	0
22	1	30	0	0
	2	60	0	0
	3	60	0	0
	4	60	0	0
	5	60	0	0
	6	60	0	0
23	1	30	7	0
	2	60	4	0
	3	60	0	2
	4	60	3	0
	5	60	0	0
	6	60	0	0

---

* Indicates that an egg mass had been deposited within the 24 hours preceding the test.

TABLE I—(*Continued*)

Total time of observation	32.75 hrs.
Total number of reactions	38
Total number of reactions, no visible cause	17 (44.73%)
Total number of reactions, mucous stimulation	21 (55.26%)
Average number of reactions per hour	1.16
Average number of reactions, no visible cause, per hour	0.51

This table shows that when six snails were kept under observation for nearly thirty-three hours (about five and a half hours each) in filtered tap water only 38 mouth reactions were observed, an average of 1.16 per hour. If we allow for each reaction 1.25 seconds (Table II, p. 8) the duration of the 38 reactions is 47.50 seconds. Of the 38 reactions 21 or 55.26% were observed to be due to mucous stimulation and the remainder were probably due to the same cause. Owing to the fact that the mucus is colorless, and as it occurs on the surface of the water, transparent, it is exceedingly difficult to determine its presence at all times. Any attempt to bring it into view by the addition of pigment such as powdered carmine would only serve to make it more effective as a stimulus. The reactions which did occur can then be accounted for. The mouth reactions of Physa may then be regarded as a true reflex, an "unlearned response" (Watson, 1914), which occurs normally as the result of external stimulation of the mouth or the region immediately surrounding it. For experimental purposes we may say that the reaction occurs only as the result of external stimulation. If now, the snail were placed in water freed from microscopic particles by filtration and the mouth reactions were practically always induced by bringing food directly into contact with the mouth, the reactions must be regarded as reflexes, that is, involuntary. Such a series of reactions then might be interpreted as an "unconditioned reflex" (Pawlow, 1904). The number of reactions and the duration of the series should then prove an available substitute for the measurements of saliva in inquiries into the behavior of a group of animals widely separated from mammals.

B. *Number and duration of reactions in the normal food-stimulus response.*—The snails that were used in all of the remaining tests had been fed upon lettuce while in the laboratory; for this reason lettuce was used as a food stimulus in all the experiments. A small piece taken from a fresh leaf was applied to the normally inactive mouth of the snail by means of forceps and immediately withdrawn. This stimulus served to start the movements of the mouth parts, and an exact record of the number of reactions thus induced was kept for each individual. Each complete opening and closing movement is spoken of as a *reaction*. The number of reactions (whether one or several) induced by a single stimulation of the mouth parts is termed a *response*. During work a record was also made of the length of time that the reactions persisted; that is, the duration of the response (Table II).

TABLE II

Showing the number of reactions of the mouth of two individuals of *Physa gyrina* Say, resulting from a single application of food to the mouth. The tests were made in filtered water on alternating days. The snails had been starved for 24 hours preceding the tests.

SNAIL NO. 4

No. of days tested	No. for "food stimulus" series	No. of trial for day	No. of reactions per response	Duration of response in sec.
1	1	1	1	1
	2	2	0	0
	3	3	1	1
	4	4	6	10
	5–10	5–10	0	0
Totals		10	8	
Av. per response			2.66	
2	11	1	1	1
	12	2	6	10
	13	3	38	50
	14	4	*	0
	15	5	1	1
	16	6	1	1
	17	7	3	5
	18	8	1	1
	19–20	9–10	0	0
Totals		10	51	
Av. per response			7.28	
3	21	1	0	0
	22	2	1	1
	23	3	1	1
	24	4	1	1
	25	5	1	1
	26	6	1	1
	27	7	2	3
	28	8	0	0
	29	9	1	1
	30	10	0	0
Totals		10	8	
Av. per response			1.14	

---

* Cases in which lack of response was due to known mechanical disturbance.

TABLE II—(*Continued*)

SNAIL No. 4—(Continued

No. of days tested	No. for "food stimulus" series	No. of trial for day	No. of reactions per response	Duration of response in sec.
4	31	1	3	4
	32	2	2	2
	33	3	1	1
	34	4	1	1
	35	5	1	1
	36	6	2	2
	37	7	1	1
	38	8	2	2
	39–40	9–10	0	0
Totals		10	13	
Av. per response			1.62	
5	41	1	11	15
	42	2	6	8
	43	3	0	0
	44	4	3	3
	45	5	1	1
	46	6	0	0
	47–50	7–10	*	0
Totals		10	21	
Av. per response			5.25	

Total number of trials	50
Total number of stimuli followed by response	29
Per cent of stimuli followed by response	58%
Total number of reactions	101
Average number of reactions per trial	2.02
Average number of reactions in only those trials in which response occurred	3.48
Total time of reactions	2.18 min.
Average time per reaction	1.29 sec.
Average number of reactions per minute	46.51

---

* Cases in which lack of response was due to known mechanical disturbance.

TABLE II—(*Continued*)

SNAIL NO. 5

No. of days tested	No. for "food stimulus" series	No. of trial for day	No. of reactions per response	Duration of response in sec.
1	1	1	24	30
	2–4	2–4	0	0
	5–10	5–10	*	0
Totals		10	24	
Av. per response			24	
2	11	1	5	10
	12	2	1	1
	13	3	*	0
	14	4	0	0
	15	5	1	1
	16	6	1	1
	17	7	2	3
	18	8	1	1
	19–20	9–10	*	0
Totals		10	11	
Av. per response			1.83	
3	21	1	1	1
	22	2	1	1
	23	3	1	1
	24	4	2	3
	25	5	2	3
	26	6	1	1
	27–30	7–10	*	0
Totals		10	8	
Av. per response			1.33	
4	31	1	3	3
	32	2	4	5
	33	3	4	5
	34	4	2	2
	35	5	1	1
	36	6	1	1
	37	7	1	1
	38	8	1	1
	39	9	4	4
	40	10	1	1
Totals		10	22	
Av. per response			2.2	
5	41	1	19	22
	42	2	20	22
	43	3	5	6
	44	4	10	12
	45	5	4	8
	46	6	6	5
	47	7	1	1
	48	8	0	0
	49	9	3	4
	50	10	6	8
Totals		10	74	
Av. per response			8.22	

---

* Cases in which lack of response was due to known mechanical disturbance.

TABLE II—(*Continued*)

Total number of trials	50
Total number of stimuli followed by response	32
Per cent of stimuli followed by response	64%
Total number of reactions	139
Average number of reactions per trial	2.78
Average number of reactions in only those trials in which response occurred	4.35
Total time of reactions	2.81 min.
Average time per reaction	1.21 sec.
Average number of reactions per minute	45.9

Summary, Snails 4 and 5:

Total number of stimuli (trials)	100
Total number of stimuli followed by response	61
Per cent of stimuli followed by response	61%

Table II shows that when a food stimulus is applied to the mouth region of *Physa gyrina* Say, response occurs in 61% of 100 trials with two snails. The individual records show that the number of reactions making up the response varies from 1 to 38. The first 30 trials of the series of 50 trials for Snail 4 shows 67 reactions, while the following 20 trials give 34 reactions with an average of 2.83 reactions per response. In the case of Snail 5 the corresponding numbers are 43 and 96 reactions with an average per response of 3.3 and 5.05. This indicates in the case of Snail 5 a tendency to recover during the second part of the series from the inhibiting effect of the manipulation (adaptation). In Snail 4 the recovery may be delayed beyond the limits of the series. The series is in both cases too short to afford anything more than a suggestion as to the occurrence of adaptation. The absence of reaction in certain trials and the wide variation in their number in those trials in which they occur and in the differences between individual snails, are brought out in the summary (p. 31) and are discussed later. The average number of reactions per response furnishes a means of comparison of the results here obtained with those recorded in the part of this paper which deals with the simultaneous use of two stimuli.

4. RESPONSE TO THE CONDITIONED FOOD STIMULUS: SIMULTANEOUS APPLICATION OF FOOD AND PRESSURE

There are then determined the number of reactions of the mouth parts induced by a single definite stimulation and constituting a single response and the duration of the response and of the single reaction. The stimulus according to Pawlow's terminology would be "unconditioned" or primary. An attempt was next made to use a "conditioned" or double stimulus by the simultaneous application of pressure and food.

A. *Special apparatus and methods.*—This required the construction of a special apparatus which would make possible the application of a uniform pressure at a fixed distance from the food stimulus when the latter was applied to the mouth of the snail. Out of the several appliances tried, the one here pictured (fig. 1, Plate I) gave the best results.

(A) was a wire nail which served as a handle. A small strip of sheet metal (B) 5 mm. in width, and bent in the form of a square cornered U, was soldered to the end of A. B contained two smooth-edged holes, one directly above the other and both as large as would permit the free movement through them of a smooth steel rod (C) which was made from a No. 5 sewing needle. C was kept from dropping through the holes, by a fine wire (D) which was passed through the eye of the needle. To the lower end of the rod C was soldered a U-shaped piece made of No. 30 platinum wire. One limb of the U (E) 16 mm. in length, ended in a small hook. The other limb (F) 18mm. in length, ended in a three-pronged, dull-pointed platinum fork 3 mm. wide and turned at right angles to the plane of the U-shaped piece. A was held in the hand of the operator and the fork on the lower end of F was allowed to rest on the exposed ventral surface of the snail. At the same time food (a small piece of lettuce rolled between the fingers to form an inverted cone) suspended from the hook at the lower end of E was applied to the mouth of the snail. The free movement of the lower part of the apparatus permitted by the smooth-edged holes in B, made it possible for the operator to manipulate the apparatus in such a way that it remained practically stationary upon the surface of the snail during the period of stimulation in spite of the constant motion of the animal.

B. *Experiments which show lack of response of untrained snails to pressure alone.*—Before using the apparatus in experiments with the conditioned stimulus, a series of twenty trials was made on each of six snails in order to learn whether the application to the foot of the pressure fork alone induced response. The snails were starved for twenty-four hours previous to the experiments and each was tested in filtered tap water. The above described apparatus was applied without food to the upturned ventral surface of the foot. It was placed upon the animal in such a way that the food hook was just above the mouth. Pressure was thus applied to the same part of the foot that would receive it if the double stimuli, food and pressure, were being applied together. In the total of 120 trials 4 responses occurred (3.33%) and these were probably due to mucous stimulation. Pressure alone, as applied with the apparatus used is followed by response in so small a percentage of the trials that its occurrence does not vitiate the experiments which follow.

C. *Experiments to determine response to the conditioned food stimulus: first food and pressure series.*—By means of the food-pressure apparatus food and uniform pressure were next applied simultaneously in a long series of trials. Both stimuli were immediately withdrawn and the number of reactions of the mouth counted and recorded as before. Table III shows the record obtained in this attempt to form an association between two dissimilar stimuli applied simultaneously.

TABLE III

Showing the number of reactions of the mouths of two individuals of *Physa gyrina* Say, resulting from the simultaneous application of two dissimilar stimuli, food and pressure. The tests were made in filtered tap water on alternating days. The snails had been starved for 24 hours preceding the tests.

SNAIL NO. 4

No. of days tested	No. for "food and pressure" stimulus series	No. of trial for day	No. of reactions per response	Duration of response in sec.
6	1–10	1–10	0	0
7	11–20	1–10	0	0
8	21–30	1–10	0	0
9	31–40	1–10	0	0
10	41–50	1–10	0	0
11	51–60	1–10	0	0
Totals....................		60	0	
12	61	1	4	5
	62	2	1	1
	63	3	2	2
	64–70	4–10	0	0
Totals....................		10	7	
Av. per response.........................			2.33	

TABLE III—(*Continued*)

SNAIL NO. 4—(Continued)

No. of days tested	No. for "food and pressure" stimulus series	No. of trial for day	No. of reactions per response	Duration of response in sec.
13	71–80	1–10	0	0
14	81	1	0	0
	82	2	1	1
	83	3	8	10
	84–90	4–10	0	0
Totals		10	9	
Av. per response			4.5	
15	91	1	1	1
	92	2	1	1
	93	3	0	0
	94	4	2	2
	95–100	5–10	0	0
Totals		10	4	
Av. per response			1.33	
16	101–110	1–10	0	0
17	111	1	16	18
	112	2	6	8
	113	3	0	0
	114	4	1	1
	115	5	1	1
	116	6	0	0
	117	7	1	1
	118	8	1	1
	119	9	2	2
	120	10	0	0
Totals		10	28	
Av. per response			4.0	
18	121	1	1	1
	122	2	0	0
	123	3	2	2
	124	4	2	2
	125	5	5	6
	126	6	2	2
	127	7	1	1
	128	8	1	1
	129–130	9–10	0	0
Totals		10	14	
Av. per response			2.0	
19	131	1	25	27
	132	2	6	8
	133	3	30	33
	134	4	4	5
	135	5	7	8
	136–140	6–10	0	0
Totals		10	72	
Av. per response			14.4	

TABLE III—(*Continued*)

SNAIL NO. 4—(*Continued*)

No. of days tested	No. for "food and pressure" stimulus series	No. of trial for day	No. of reactions per response	Duration of response in sec.
20	141	1	19	22
	142	2	30	35
	143	3	8	9
	144	4	10	12
	145	5	3	3
	146	6	0	0
	147	7	1	1
	148	8	*	0
	149–150	9–10	0	0
Totals......................		10	71	
Av. per response..........................			11.83	
21	151	1	8	9
	152	2	8	10
	153	3	2	2
	154	4	3	4
	155	5	*	0
	156	6	2	2
	157	7	0	0
	158	8	9	10
	159–160	9–10	0	0
Totals......................		10	34	
Av. per response..........................			5.66	
22	161	1	1	1
	162	2	0	0
	163	3	1	1
	164	4	1	1
	165	5	1	1
	166	6	1	1
	167–170	7–10	0	0
Totals......................		10	5	
Av. per response..........................			1.0	
23	171	1	10	13
	172	2	12	16
	173	3	2	2
	174	4	1	1
	175	5	1	1
	176	6	4	5
	177	7	1	1
	178	8	9	12
	179	9	3	4
	180	10	1	1
Totals......................		10	44	
Av. per response..........................			4.4	

---

* Cases in which lack of response was due to known mechanical disturbance.

TABLE III—(*Continued*)

SNAIL NO. 4—(*Continued*)

No. of days tested	No. for "food and pressure" stimulus series	No. of trial for day	No. of reactions per response	Duration of response in sec.
24	181–190	1–10	0	0
25	191	1	4	5
	192	2	1	1
	193	3	4	5
	194	4	1	1
	195	5	1	1
	196	6	4	5
	197	7	1	1
	198–200	8–10	0	0
Totals.....................		10	16	
Av. per response........................			2.28	
26	201	1	2	2
	202	2	3	4
	203	3	6	8
	204	4	1	1
	205	5	1	1
	206	6	2	2
	207	7	2	2
	208	8	3	4
	209	9	2	2
	210	10	2	2
Totals.....................		10	24	
Av. per response........................			2.4	
27	211	1	3	4
	212	2	4	5
	213	3	5	6
	214	4	0	0
	215	5	1	1
	216–220	6–10	0	0
Totals.....................		10	13	
Av. per response........................			3.25	
28	221	1	4	5
	222	2	1	1
	223	3	2	2
	224	4	1	1
	225	5	1	1
	226	6	4	5
	227	7	1	1
	228	8	1	1
	229	9	2	2
	230	10	1	1
Totals.....................		10	18	
Av. per response........................			1.8	

TABLE III—(*Continued*)

SNAIL NO. 4—(*Continued*)

No. for days tested	No. for "food and pressure stimulus" series	No. of trials for day	No. of reactions per response	Duration of response in sec.
29	231	1	1	1
	232	2	1	1
	233	3	1	1
	234	4	1	1
	235	5	1	1
	236–240	6–10	0	0
Totals		10	5	
Av. per response			1.0	
30	241	1	10	12
	242	2	1	1
	243	3	1	1
	244	4	1	1
	245	5	1	1
	246	6	1	1
	247–250	7–10	0	0
Totals		10	15	
Av. per response			2.5	
31	251	1	4	5
	252	2	5	6
	253	3	5	6
Totals		3	14	
Av. per response			4.66	

Total number of trials	253
Total number of stimuli followed by response	99
Per cent of stimuli followed by response	35%
Total number of reactions	393
Average number of reactions per trial	1.55
Average number of reactions in only those trials in which response occurred	3.96
Total time of reactions	7.58 min.
Average time per reaction	1.15 sec.
Average number of reactions per minute	51.84

TABLE III—(*Continued*)

SNAIL NO. 5

	No. of days tested	No. for "food and pressure" stimulus series	No. of trials for day	No. of reactions per response	Duration of response in sec.
	6	1–10	1–10	0	0
	7	11–20	1–10	0	0
	8	21–30	1–10	0	0
	9	31–40	1–10	0	0
	10	41–50	1–10	0	0
	11	51–60	1–10	0	0
	12	61–70	1–10	0	0
	13	71–80	1–10	0	0
Totals...			80	0	0
Totals...........	14	81–90	10	14	16
Av. per response..				14.0	
Totals...........	15	91–100	10	0	0
Totals...........	16	101–110	10	0	0
Totals...........	17	111–120	10	24	28
Av. per response..				4.8	
Totals...........	18	121–130	10	11	12
Av. per response..				1.83	
Totals...........	19	131–140	10	74	85
Av. per response..				9.25	
Totals...........	20	141–150	10	23	27
Av. per response..				2.87	
Totals...........	21	151–160	10	0	0
Totals...........	22	161–170	10	0	0
Totals...........	23	171–180	10	13	16
Av. per response..				3.25	
Totals...........	24	181–190	10	3	3
Av. per response..				1.0	
Totals...........	25	191–200	10	13	15
Av. per response..				3.25	
Totals...........	26	201–210	10	17	19
Av. per response..				1.7	
Totals...........	27	211–220	10	0	0
Totals...........	28	221–230	10	0	0
Totals...........	29	231–240	10	5	5
Av. per response..				1.25	
Totals...........	30	241–250	10	28	34
Av. per response..				3.11	

TABLE III—(*Continued*)

SNAIL NO. 5—(*Continued*)

Total number of trials	250
Total number of stimuli followed by response	62
Per cent of stimuli followed by response	24%
Total number of reactions	225
Average number of reactions per trial	0.9
Average number of reactions in only those trials in which response occurred	3.62
Total time of reactions	4.33 min
Average time per reaction	1.11 sec.
Average number of reactions per minute	51.92

Summary Snails 4 and 5:

Total number of stimuli (trials)	503
Total number of stimuli followed by response	161
Per cent of stimuli followed by response	32%

TABLE III-A

Showing the distribution of responses of each 10 trials of the 250 trials shown in Table III.

SNAIL NO. 4

Trials no.	No. of responses	Total no. of reactions	Total duration in sec. of responses per 10 trials	Average duration of reactions
1– 60	0	0	0	0
61– 70	3	7	8	1.14
71– 80	0	0	0	0
81– 90	2	9	11	1.22
91–100	3	4	4	1.0
101–110	0	0	0	0
111–120	7	28	32	1.14
121–130	7	14	15	1.07
131–140	5	72	81	1.12
141–150	6	71	82	1.15
151–160	6	34	39	1.14
161–170	5	5	5	1.0
171–180	10	44	56	1.27
181–190	0	0	0	0
191–200	7	16	19	1.18
201–210	10	24	28	1.16
211–220	4	13	16	1.23
221–230	10	18	20	1.11
231–240	5	5	5	1.0
241–250	6	15	17	1.13
251–253	3	14	17	1.21

SNAIL NO. 5

1– 80	0	0	0	0
81– 90	1	14	16	1.14
91–100	0	0	0	0
101–110	0	0	0	0
111–120	5	24	28	1.16
121–130	6	11	12	1.09
131–140	8	74	85	1.14

TABLE III-A—(*Continued*)

SNAIL NO. 5—(*Continued*)

Trials no.	No. of responses	Total no. of reactions	Total duration in sec. of responses per 10 trials	Average duration of reactions
141–150	8	23	27	1.13
151–160	0	0	0	0
161–170	0	0	0	0
171–180	4	13	16	1.23
181–190	3	3	3	1.0
191–200	4	13	15	1.15
201–210	10	17	19	1.11
211–220	0	0	0	0
221–230	0	0	0	0
231–240	4	5	5	1.0
241–250	9	28	34	1.21

SNAIL NO. 6

Trials no.	No. of responses	Total no. of reactions	Total duration in sec. of responses per 10 trials	Average duration of reactions
1–110	0	0	0	0
111–120	1	6	7	1.16
121–130	0	0	0	0
131–140	2	9	11	1.22
141–150	2	2	2	1.0
151–160	0	0	0	0
161–170	0	0	0	0
171–180	0	0	0	0
181–190	3	3	3	1.0
191–200	2	2	2	1.0
201–210	5	6	6	1.0
211–220	0	0	0	0
221–230	5	5	5	1.0
231–240	4	7	7	1.0
241–250	9	25	29	1.16

SNAIL NO. 7

Trials no.	No. of responses	Total no. of reactions	Total duration in sec. of responses per 10 trials	Average duration of reactions
1– 50	0	0	0	0
51– 60	1	1	1	1.0
61– 70	1	11	13	1.18
71– 80	0	0	0	0
81– 90	2	2	2	1.0
91–100	3	10	13	1.3
101–110	0	0	0	0
111–120	4	27	31	1.11
121–130	3	6	7	1.16
131–140	5	32	41	1.21
141–150	3	38	44	1.15
151–160	3	4	4	1.0
161–170	4	11	13	1.18
171–180	4	46	55	1.19
181–190	4	4	4	1.0
191–200	10	35	44	1.25
201–210	8	11	12	1.09
211–220	10	13	13	1.0
221–230	4	7	8	1.14
231–240	5	29	33	1.13
241–250	8	50	56	1.12

TABLE III-A—(*Continued*)

SNAIL NO. 8

Trials no.	No. of responses	Total no. of reactions	Total duration in sec. of responses per 10 trials	Average duration of reactions
1– 80	0	0	0	0
81– 90	1	1	1	1.0
91–100	0	0	0	0
101–110	0	0	0	0
111–120	1	2	3	1.5
121–130	2	2	2	1.0
131–140	1	2	2	1.0
141–150	2	3	3	1.0
151–160	5	9	9	1.0
161–170	0	0	0	0
171–180	3	4	4	1.0
181–190	0	0	0	0
191–200	4	4	4	1.0
201–210	4	4	4	1.0
211–220	0	0	0	0
221–230	9	10	10	1.0
231–240	10	21	24	1.14
241–250	8	9	9	1.0
251–253	3	13	15	1.15

SNAIL NO. 14

1– 60	0	0	0	0
61– 70	3	7	8	1.14
71– 80	0	0	0	0
81– 90	1	4	5	1.25
91–100	2	3	3	1.0
101–110	2	3	4	1.33
111–120	0	0	0	0
121–130	3	13	16	1.23
131–140	5	29	35	1.2
141–150	3	4	4	1.0
151–160	2	5	6	1.2
161–170	3	8	9	1.12
171–180	0	0	0	0
181–190	1	1	1	1.0
191–200	0	0	0	0
201–210	7	25	31	1.24
211–220	6	32	38	1.18
221–230	0	0	0	0
231–240	4	31	37	1.12
241–250	5	17	19	1.11

TABLE III-B

Showing the distribution of responses of each 50 trials of the 250 trials of the "food-pressure" stimulus series.

Trials no.	No. of responses	Total no. of reactions	Total duration in sec. of responses per 50 trials	Average duration of reactions
		Snail No. 4		
1- 50	0	0	0	0
51-100	8	20	23	1.13
101-150	25	185	210	1.13
151-200	28	99	119	1.2
201-250	35	75	86	1.14
		Snail No. 5		
1- 50	0	0	0	0
51-100	1	14	16	1.14
101-150	27	132	152	1.15
151-200	11	29	34	1.17
201-250	23	50	58	1.16
		Snail No. 6		
1- 50	0	0	0	0
51-100	0	0	0	0
101-150	5	17	20	1.17
151-200	5	5	5	1.0
201-250	23	43	47	1.09
		Snail No. 7		
1- 50	0	0	0	0
51-100	7	24	29	1.2
101-150	15	103	123	1.19
151-200	25	100	120	1.2
201-250	35	110	122	1.1
		Snail No. 8		
1- 50	0	0	0	0
51-100	1	1	1	1.0
101-150	6	9	10	1.11
151-200	12	17	17	1.0
201-250	31	44	47	1.06
		Snail No. 14		
1- 50	0	0	0	0
51-100	6	14	16	1.14
101-150	13	49	59	1.2
151-200	6	14	16	1.14
201-250	22	105	125	1.19

From Table III it appears that when two stimuli were applied simultaneously, food to the mouth and pressure to the foot, there was at first an inhibitory effect upon the reflex. This is shown in the first 60 to 80 trials in which no reaction followed the application of the double stimulus. When this table is compared with Table II it is seen that a slight inhibition continued throughout this series of tests as indicated in the average number of responses per 50 trials (Table III-B) and the per cent of stimuli followed by response (Tables III and VI). The average number of reactions per response was also less than in the normal food series. For the food-pressure response the average number of reactions in the numerical order of the six snails was 3.96, 3.62, 1.96, 4.1, 1.58, 3.78, or a mean for all six snails of 3.18. The corresponding values for the normal food response (Table II) are 3.48, 4.35, 6.5, 6.5, 3.4, 10.93, with a mean value of 5.85 for the six snails. Only in the case of Snail No. 4 is there a greater average number of reactions per response to the "conditioned" stimulus than to the "unconditioned." Table III shows further that the average duration of the reactions in the food pressure response is less than that of the normal food response. Expressed in the number of reactions per minute the values are 51.84, 51.92, 65.45, 51.72, 56.7, 50.84, with a mean of 54.74. The corresponding values for the normal food response are 46.51, 45.9, 46.15, 46.51, 44.44, 48.00 with a mean of 46.25. Under food-pressure stimulation the reactions thus occur at the rate of 8.49 more per minute than under the normal food stimulus. Along with the reduction in the number of reactions there is in the food-pressure response an acceleration of the individual reaction as compared with that of the normal food response.

The distribution of the reactions over the series of 250 trials is shown for the six snails in Tables III-A and III-B and is represented in graphs (figs. 2-13, Plates II-III). Table III-A and figs. 2-7 (Plate II) show the great fluctuation in the number of reactions per response. This fluctuation causes the reaction curve to vary from the response curve although the two are in general similar in direction. The response curve shows a somewhat more uniform rise than the reaction curve. A general fact to be noted is the relative uniformity in the average duration of the individual reactions throughout the series. The method of timing the response duration by means of the stop watch is necessarily inaccurate, especially with values which, as in this case, are frequently in the neighborhood of a second. If a more accurate method were available greater uniformity might be expected. With the method employed it is noteworthy that the agreement is so close.

The food-pressure series thus shows the following characteristics: (1) An absence of reaction at the beginning, (2) a similarity in the reaction and response curves, (3) in general, a gradual rise of the response curve, (4) a less average number of responses per 50 trials and a less average number of reactions per response as compared with the normal food series, with the exception of Snail No. 4, (5) an increase in the number of reactions per minute, i. e. an acceleration of the individual response, (6) a fairly constant reaction duration. This is characteristic of all the series.

D. *Experiments with pressure alone on trained snails: association.*—The third part of the problem called for the application of the secondary stimulus alone. Any mouth reactions which might follow (beyond those attributed to chance stimulation of the mouth), would according to the Pawlow theory be evidence that an association had been formed between the essential, primary or "unconditioned" stimulus and the secondary—or in this case, between food and pressure. The series of tests was carried on in exactly the same manner as the experiments just described. The same apparatus was used but no food was placed upon the food hook. Since the food hook was shorter than the pressure fork (fig. 1, Plate I) it did not touch the mouth, which therefore received neither food nor mechanical stimulation. The pressure fork was applied to the usual place on the ventral surface of the snail. The following table (IV) shows the results obtained.

TABLE IV

Showing the number of reactions of the mouth of two individuals of *Physa gyrina* Say, resulting from the application of pressure alone as a stimulus, after an attempt had been made to establish an association between the two stimuli, food and pressure (Table III). The tests were made in filtered tap water on alternating days. The snails had been starved for 24 hours preceding the tests.

Snail No. 4

No. of days tested	No. for "pressure" stimulus series	No. of trial for day	No. of reactions per response	Duration of response in sec.
31	1	[*]4	3	4
	2	5	1	1
	3	6	1	1
	4	7	1	1
	5	8	1	1
	6	9	1	1
	7	10	0	0
Totals		7	8	
Av. per response			1.33	
32	8	1	1	1
	9	2	1	1
	10–17	3–10	0	0
Totals		10	2	
Av. per response			1.0	
33	18–27	1–10	0	0
34	28–37	1–10	0	0

Total number of trials (last 20 trials omitted)	17
Total number of stimuli followed by response	8
Per cent of stimuli followed by response	47%
Total number of reactions	10
Average number of reactions per trial	0.58
Average number of reactions in only those trials in which response occurred.	1.25
Total time of reactions in seconds	11
Average time per reaction in seconds	1.1
Average number of reactions per minute (estimated)	54.54

Snail No. 5

No. of days tested	No. for "pressure" stimulus series	No. of trial for day	No. of reactions per response	Duration of response in sec.
31	1	1	11	14
	2	2	1	1
	3	3	1	1
	4	4	12	14
	5	5	1	1
	6	6	5	6
	7	7	3	4
	8–10	8–10	0	0
Totals		10	34	
Av. per response			4.85	

---

[*] For trials 1–3 on the 31st day see Table III (31st day).

TABLE IV—(*Continued*)

SNAIL NO. 5—(*Continued*)

No. of days tested	No. for "pressure" stimulus series	No. of trials for day	No. of reactions per response	Duration of response in sec.
32	11	1	1	1
	12	2	1	1
	13–20	3–10	0	0
Totals		10	2	
Av. per response			1.0	
33	21–30	1–10	0	0
34	31–40	1–10	0	0

Total number of trials (last 20 trials omitted)	20
Total number of stimuli followed by response	9
Per cent of stimuli followed by response	45%
Total number of reactions	36
Average number of reactions per trial	1.8
Average number of reactions in only those trials in which response occurred	4
Total time of reactions in seconds	43
Average time per reaction in seconds	1.19
Average number of reactions per minute (estimated)	50.42

Summary Snails 4 and 5.

Total number of stimuli (trials)	37
Total number of stimuli followed by response	17
Per cent of stimuli followed by response	45.9%

Table IV shows that when the secondary stimulus, pressure, alone was applied to the mouth of Physa, the mouth reacted in 17 out of 37 trials with two individuals, or in 45.9% of the trials. No mucous stimulation was observed. The average number of reactions per response was for Snail 4, 1.25 and for Snail 5, 4.0 (Table IV). Snails Nos. 6, 7, 8, 14 were given a similar series of tests which are not here tabulated in detail but are summarized in Table V-C. These showed the following average number of reactions per response 4.92, 6.69, 8.0, 3.8. The mean for the six snails is 4.67. The mean for the normal food series is for six snails 5.86. For the food-pressure series it is 3.18. The average duration of reactions for the pressure response of trained snails is less than for the normal food response. The average rate per minute is 53.9, 7.65 reactions per minute in excess of that of the normal food response.

These trials followed 48 hours after the completion of the training in the food-pressure series. In the case of Snail 4, response followed the first nine trials. The last two of these are separated by an interval of 48 hours from the first seven. The effect of training thus persisted for 96 hours. Similar results were obtained from Snails 5, 6, 7, 8 and 14.

Immediately following the mouth response to pressure of trained Snail No. 4 (Table IV) seven successive applications of pressure gave no response. Similar results were obtained with the other five snails.

Snails which before training gave no response to pressure, gave, following training, a response consisting of a series of mouth reactions. This response is given on the first trial and continues to be given in trials extending over a period of 96 hours. It then suddenly ceases. There is no response of the mouth to subsequent stimulation of the foot by pressure.

E. *Experiments to determine the effect of training on food-pressure response: second food-pressure series.*—It was believed that another series of tests on the same snails, reintroducing the same "conditioned" stimulus, would be of interest in showing the ease or difficulty in re-establishing the reflex. As noted in Table IV, the snails were tested on two experimental days after pressure alone had failed to be effective as a stimulus. Allowing for the alternation of experimental days, made necessary by the twenty-four hours of starving previous to experimentation, the second series of tests using food and pressure as a stimulus was started approximately 120 hours after the disappearance of reaction upon the application of pressure as a stimulus. Fifty tests were given and the results appear in Tables V and VI-D.

TABLE V

Showing the number of reactions of the mouths of two individuals of *Physa gyrina* Say, resulting from the simultaneous application of food and pressure after the complete disappearance (Table IV) of the effect of the association established between these two stimuli (Table III). The tests were made in filtered water on alternating days. The snails had been starved for 24 hours preceding the tests.

Snail No. 4

No. of days tested	No. for "food and pressure" stimulus, second series	No. of trial for day	No. of reactions per response	Duration of response in sec.
35	1	1	11	13
	2	2	10	12
	3	3	2	3
	4–10	4–10	0	0
Totals......................		10	23	
Av. per response..........................			7.66	
36	11	1	3	5
	12	2	3	5
	13	3	7	10
	14	4	6	8
	15	5	3	5
	16	6	2	3
	17	7	1	1
	18	8	2	3
	19	9	2	3
	20	10	2	3
Totals......................		10	31	
Av. per response..........................			3.1	
37	21	1	8	11
	22	2	6	8
	23	3	3	5
	24	4	8	10
	25	5	2	4
	26	6	5	8
	27	7	6	9
	28	8	7	10
	29	9	5	7
	30	10	3	5
Totals......................		10	53	
Av. per response..........................			5.3	
38	31	1	29	33
	32	2	9	12
	33	3	9	12
	34	4	4	6
	35	5	4	6
	36	6	3	5
	37	7	6	9
	38	8	4	6
	39	9	3	5
	40	10	9	11
Totals......................		10	80	
Av. per response..........................			8	

TABLE V—(*Continued*)

SNAIL NO. 4—(*Continued*)

No. of days tested	No. for "food and pressure" stimulus, second series	No. of trial for day	No. of reactions per response	Duration of response in sec.
39	41	1	3	5
	42	2	3	5
	43	3	2	3
	44	4	2	3
	45	5	3	5
	46	6	2	3
	47	7	3	5
	48	8	2	3
	49	9	3	5
	50	10	1	1
Totals....................		10	24	
Av. per response..........................			2.4	

Total number of trials..	50
Total number of stimuli followed by response..........................	43
Per cent of stimuli followed by response..............................	86%
Total number of reactions..	211
Average number of reactions per trial................................	4.22
Average number of reactions in only those trials in which response occurred.	4.9
Total time of reactions in minutes...................................	4.9
Average time per reaction in seconds.................................	1.39
Average number of reactions per minute...............................	43.16

SNAIL NO. 5

	No. of days tested	No. for "food and pressure" stimulus, second series	No. of trial for day	No. of reactions per response	Duration of response in sec.
Totals..........	35	1–10	10	21	26
Av. per response.				4.2	
Totals...........	36	11–20	10	37	52
Av. per response.				4.62	
Totals...........	37	21–30	10	64	91
Av. per response.				6.4	
Totals...........	38	31–40	10	35	73
Av. per response.				3.5	

Total number of trials..	40
Total number of stimuli followed by response..........................	33
Per cent of stimuli followed by response..............................	82.5%
Total number of reactions..	157
Average number of reactions per trial................................	3.92
Average number of reactions in only those trials in which response occurred.	4.75
Total time of reactions in minutes...................................	4.33

TABLE V—(*Continued*)

SNAIL No. 5—(*Continued*)

Average time per reaction in seconds	1.54
Average number of reactions per minute	38.96
Summary Snails 4 and 5:	
Total number of stimuli (trials)	90
Total number of stimuli followed by response	76
Per cent of stimuli followed by response	84.4%

Table V shows that movements of the mouth occurred in 76 out of 90 trials, with two individuals, or in 84.4% of the trials. The first series of tests in which the "conditioned" stimulus was used showed reactions occurring in 61 out of 100 trials or 61%, when food alone was used as a stimulus (Table II). These differences show the wearing away of the inhibition caused by the secondary stimulus (Table III) and by the experimental manipulation. The mean number of reactions per response for snails 4 and 5 was 4.84, 0.91 less than for the normal food response and 1.01 more than in the food-pressure series with trained snails. The inhibition of the response to food by the pressure stimulus has nearly disappeared (adaptation).

TABLE VI

Showing a summary of the reactions of the mouths of four individuals of *Physa gyrina* Say, resulting from (A) food stimulus alone, cf. Table II; (B) food and pressure used simultaneously, cf. Table III; (C) pressure alone, cf. Table IV; (D) food and pressure used simultaneously, 2d series, cf. Table V. The tests were made in filtered tap water on alternating days. The snails had been starved for 24 hours preceding the tests.

	Snail 6	Snail 7	Snail 8	Snail 14
A. Stimulus Food				
Total number of trials	42	50	60	60
Total number of stimuli followed by response	8	38	47	32
Per cent of stimuli followed by reaction	19%	76%	78.3%	53.3%
Total number of reactions	52	247	143	350
Av. number of reactions per trial	1.23	5.14	2.38	5.83
Av. number of reactions in only those trials in which response occurred	6.5	6.5	3.4	10.93
Total time of reactions in minutes	1.13	5.33	3.23	7.35
Av. time per reaction in seconds	1.3	1.29	1.39	1.25
Av. number of reactions per minute	46.15	46.51	44.44	48
B. Stimulus Food and Pressure				
Total number of trials	250	250	253	250
Total number of stimuli followed by response	33	82	53	47
Per cent of stimuli followed by reaction	13.2%	32.8%	20.9%	18.8%
Total number of reactions	65	337	84	182
Av. number of reactions per trial	0.26	1.34	0.03	0.72
Av. number of reactions in only those trials in which response occurred	1.96	4.1	1.58	3.87
Total time of reactions in minutes	1.2	6.56	1.5	3.6
Av. time per reaction in seconds	1.1	1.16	1.07	1.18
Av. number of reactions per minute	65.45	51.72	56.7	50.84

TABLE VI—(*Continued*)

	Snail 6	Snail 7	Snail 8	Snail 14
C. Stimulus Pressure				
Total number of trials	30	20	17	20
Total number of stimuli followed by response	13	11	3	5
Per cent of stimuli followed by reaction	43.3%	55%	15.5%	25%
Total number of reactions	64	67	24	19
Av. number of reactions per trial	2.13	3.35	1.41	0.95
Av. number of reactions in only those trials in which response occurred	4.92	6.09	8	3.8
Total time of reactions in minutes	1.28	1.35	0.5	0.38
Av. time per reaction in seconds	1.2	1.2	1.25	1.21
Av. number of reactions per minute	64.16	64.16	48	49.54
D. Stimulus Food and Pressure (Second series)				
Total number of trials	50	50	50	50
Total number of stimuli followed by response	16	36	44	25
Per cent of stimuli followed by reaction	32%	72%	88%	50%
Total number of reactions	123	173	158	120
Av. number of reactions per trial	2.46	3.46	3.16	2.4
Av. number of reactions in only those trials in which response occurred	7.68	4.8	3.59	4.8
Total time of reactions in minutes	2.51	3.78	3.28	2.53
Av. time per reaction in seconds	1.21	1.31	1.24	1.26
Av. number of reactions per minute	49.54	45.8	48.38	47.61

5. Summary of Observations

1. All observations and experiments were made on *Physa gyrina* Say while suspended from the surface film in a dish of filtered tap-water. In this position the lower surfaces of the foot and head are exposed and the movements of the mouth are visible.

2. When food (lettuce) or an inert substance (glass rod) is quickly applied to such a snail on or near the mouth and immediately withdrawn there results a response which consists of one or more opening and closing movements of the mouth. A single opening and closing of the mouth is referred to as a reaction. A response consists of one or many reactions in succession and takes place after the withdrawal of the stimulating substance.

3. Before the snails were used they were "tamed" by handling until they no longer visibly reacted to the mechanical disturbances incidental to the experiments.

4. All experiments were conducted under approximately uniform physical conditions, that is in filtered tap water at room temperature and with constant light intensity.

5. In all tests except those of the preliminary series each snail was used at intervals of forty-eight hours. It was allowed

to feed freely during the first half of this period but was given no food during the second half of the period. By thus controlling the food an attempt was made to have all snails in the same physiological state at the time of using them.

6. As a means of detecting variations in the physiological state a record was kept of the egg masses deposited by each snail, but no relation was found between the egg laying activity and any results of the experiments.

7. Each snail was distinguished by a number painted on the shell and a separate record was kept for each.

8. After a preliminary series of experiments involving six snails (Nos. 18-23), six other snails were used in the remaining experiments. For two of these (4, 5) complete records are presented in Tables II, III, IV, V, pp. 8, 13, 25 and 28. For the remaining four (6, 7, 8, 14) a summary of results is given in Table VI, p. 30.

9. Reactions of the mouth do not occur in the absence of external stimulation (preliminary test series). Six snails (Nos. 18-23, Table I, p. 6) were kept under observation for a total period of 32.75 hours (an average of 5.47 hours each), without stimulation. During this time but eight responses occurred with a total of 38 reactions. Three of the responses were seen to follow contact of the mouth of the snail with parts of the mucous track left by the animal in its normal movements on the surface film. The remaining five responses probably followed similar contact with this nearly invisible mucus. For experimental purposes the mouth response may be said to occur only as the result of the external stimulation. It is "involuntary." The error introduced into later experiments as a result of unobserved mucous stimulation is not enough to vitiate the experiments.

10. *The normal food-response.*—When food was applied to the mouth and at once withdrawn response followed in 61% of the tests (average for Snails 4 and 5). The average response consisted of 3.93 reactions. The average time of reaction was 1.25 sec. or 46.2 reactions per minute, Table II, p. 8.

11. The average number of reactions constituting the normal food-response varied with the individual snail as follows: No. 4, 3.48; No. 5, 4.35; Nos. 6 and 7, 6.5; No. 8, 3.4; No. 14, 10.93. The mean for all six snails is 5.86.

12. The average duration of a single reaction in the normal food-response is much more nearly the same in different individuals than is the number of reactions constituting a response. This average in seconds for the six snails in the order of paragraph 11 is, 1.29, 1.21, 1.3, 1.29, 1.39, 1.25. The values are obtained by dividing the duration of the response in seconds (as obtained with a stop watch) by the number of its component reactions. At the above rate the number of reactions per minute for the six snails would be 46.51, 45.9, 46.15, 46.51, 44.44, 48.0, or a mean of 46.25 reactions per minute.

13. An apparatus (food-pressure apparatus, fig. 1, Plate I) was devised for the simultaneous application of two unlike stimuli, food to the mouth and pressure to the foot at a fixed distance from the mouth.

14. *Pressure response of untrained snails.*—By means of the food-pressure apparatus pressure was applied to the foot at a fixed distance from the mouth. No stimulus was applied to any other part of the animal. In one hundred twenty applications of pressure to six snails (20 each) but four responses were obtained. Response thus occurred to 3.3% of the applications of pressure. The responses were probably due to chance stimulation by mucus. Response to pressure on the foot with the apparatus used occurs in so small a per cent of cases that it does not affect the experiments summarized below.

15. *Food-pressure response.*—When food was applied to the mouth and pressure to the foot at the same instant no response followed in the first 60 to 110 trials, in six series of 250 trials each (Snails 4, 5, 6, 7, 8 and 14). Response followed in the remaining trials of each series. In a total of 376 responses for the six snails, the average number of reactions per response was 3.42 as compared with 5.86 with the same snails when the food stimulus alone was used (Tables III and VI-B, pp. 13 and 30). The snails are referred to as "trained."

16. *Pressure response of trained snails.*—Forty-eight hours after the completion of their training, pressure alone was applied to the foot of the six trained snails (paragraph 15). In the case of Snail No. 4 response followed the first nine trials. The last two of these were separated by an interval of 48 hours from the first seven. The effect of training thus persisted for 96

hours. Similar results were obtained from Snails 5, 6, 7, 8 and 14 (Tables IV and VI-C, pp. 25 and 31).

17. *Cessation of pressure response in trained snails.*—Immediately following the ninth response to pressure of trained Snail No. 4 (paragraph 16) seven successive applications of pressure gave no response; 48 hours later ten applications gave no response; 96 hours after the last response ten applications failed to give any response. Similar results were obtained with the other five snails. *Cessation of response to pressure after training is sudden and final* (Tables IV and VI-C, pp. 25 and 31.

18. One hundred and twenty hours after the last response to pressure of Snail No. 4 (paragraph 16) a series of 50 trials was run on this snail by the simultaneous application of food and pressure. Response followed the first three trials and occurred in 86% of the subsequent trials (cf. paragraph 15), with an average of 4.9 reactions. Similar results were obtained with Snail No. 5. The average number of reactions per response was 4.84 for the two snails, 0.91 less than for the normal food response (paragraph 10) and 1.01 more than in the first food-pressure series (paragraph 15). The interference of the pressure stimulus with the response to food has nearly disappeared (Tables IV and VI-B, pp. 25 and 30).

19. The average number of reactions in the food-pressure response is less than the number in the normal food response. The numbers for the six snails in their numerical order is 3.96, 3.62, 1.96 (Table III, p. 13), 4.1, 1.58 and 3.87 (Table VI-B, p. 30). The average for the six snails is 3.18. Application of pressure to the foot at the same time with food to the mouth shortens the response.

20. The average duration of the reaction is less in the food-pressure responses than in the normal food response. The number per minute for the six snails in numerical order is 51.84, 51.92, 65.45, 51.72, 56.7, 50.84 (Tables III and VI-B, pp. 13 and 30). This is a mean distribution of 54.74, or 8.49 reactions per minute in excess of the normal food response (paragraph 12).

21. In the food-pressure series, the average number of reactions per ten trials increases irregularly toward the end of the series, except in the case of Snail No. 4. The average number of responses per ten trials shows a more uniform rise (Table III-A, p. 19, figs. 2-7, Plate II).

22. The average number of reactions per response resulting from the application of pressure alone to the foot of trained snails (paragraph 16) is greater than the average number resulting from the application of either food or food-pressure stimulation. The numbers for the six snails in their numerical order is 1.25, 4.0, 4.92, 6.09, 8.0, 3.8, or a mean of 4.67 reactions per response. These averages differ more than those of either food or food-pressure responses (Tables II, III and VI-A and VI-B, pp. 8, 13 and 30).

23. The average duration of the reactions resulting from the pressure stimulus alone on the foot of the trained snails is less than that of the normal food response. This is shown by the average number of reactions per minute for the six snails, 53.9, which is 7.65 more than with the normal food response (Tables IV and VI-C, pp. 25 and 31). In all trials the reaction duration is notably constant.

6. Discussion

A. *Method.*—The experimental method used in this research will be called *the method of training by the use of simultaneous stimuli.* The term may be applied to any procedure in which two unlike stimuli are applied together or in immediate succession, to one of which the animal has previously responded by a definite reflex, but to the other of which it has failed to respond by the same reflex. The purpose is to establish response to the second stimulus alone. The stimuli may be referred to as primary and secondary and the primary stimulus may be called "conditioned" when used with the secondary (Pawlow's terms). The term, as thus defined, includes the salivary reflex method of Pawlow.

Comparison of the method with the salivary reflex method. The Pawlow method has been described (p. 2). It requires that the responses to the primary stimulus be a reflex, a so-called involuntary response, and that it be measurable. In order to meet the first requirement the response must, (a) occur only in response to external stimulation, (b) "always" occur in response to external stimulation; that is, it should occur with sufficient regularity to be available for experimental uses.

That the mouth response of Physa occurs only in response to stimulation of the mouth region has been shown. In six animals carefully guarded from such stimulation for a total period of nearly thirty-three hours (about five and a half hours to each animal) the response occurred but eight times. Three of these responses were due to observed accidental stimulation and the rest were doubtless due to the same cause. On the other hand, as the tables show, stimulation of the mouth region induces the response in a sufficient percentage of trials to make it available for experimental purposes. The response is therefore a reflex available for these purposes.

The response of Physa to stimulation of the mouth region unaccompanied by other stimulation will be referred to as *the normal food response.* It consists of one or more opening and closing movements of the mouth. Each complete opening and closing movement is spoken of as a reaction. The response is measurable in terms of the number of its reactions as well as in duration. An inspection of the tables shows very great fluctuation in the numerical value of the response.

B. *Probable causes of fluctuation in response* are not difficult to find. They arise in part from the accidents of manipulation and are in part due to the physiological state of the snails. It is not an easy matter to apply a stimulus to the mouth of a moving snail, delicately suspended from the surface film. The difficulty is increased when two stimuli must be accurately placed at the same time. If the pressure is applied with the pressure fork strictly vertical, it is only that of the weight of the movable part of the apparatus and should be constant. Slight deviation from the vertical is inevitable and must result in occasional variation in pressure and in the manner in which the food reaches the mouth. These accidents should shorten response. That unskilled manipulation sometimes inhibits response entirely is clear from those cases marked x in the tables. It is fair to assume that they partially inhibit other responses. They depend in part on the physiological state of the operator. Added to these personal factors the tendencies to produce irregularity in response are vibrations of an unstable building due to slamming of doors, passing of streets cars and movements of people. Other accidents prolong response. Probably at times portions of food are retained in the mouth of the snail and the response may be prolonged. At times accidental stimulation from unobserved mucus secreted by the snail, is added to that of the food on the hook and response is prolonged.

That the physiological state of the animal is in part responsible for fluctuations in response is indicated by the no-response shown in Tables II and III, pp. 8 and 13. Some of these were known to be due to mechanical disturbance and are marked x in the tables. Others were probably due to disturbance. Others again may be attributed to the physiological state of fatigue, although it is believed that the method of testing the snails alternately practically eliminated this element. It is noteworthy that a large percentage of the no-response trials occurs at the end of the day's series. This may be attributed to fatigue or adaptation—in either case a changed physiological state.

With a more delicate apparatus, and particularly with the elimination of external disturbances such as the vibrations caused by sudden jars beyond the limits of the experimental room, responses of more constant value might be obtained. It is quite possible that responses obtained by more refined methods would equal those of the salivary reflex method in constancy.

Throughout each of the tables it is to be noted that the duration of the individual reaction is reasonably uniform. It fluctuates mainly between 1.0 and 1.25 seconds. The fluctuations may be attributed to the method of timing with a stop watch. Were a better method available for the purpose, much greater uniformity in the reaction duration would doubtless appear. The uniformity actually found is striking and indicates that the use of the single reaction as a unit for measuring the response is justified. The unit is probably as accurate as the drop of saliva.

That the method described in this paper would yield results if used in the study of discrimination in the snails the writer does not doubt. It seems to offer for this purpose whatever advantage the Pawlow method affords although possibly in a less degree. It is also available in working out many of the details of the association process in a statistical way.

The data presented in this paper show that the behavior of the snail is modified by two processes, adaptation and learning.

Adaptation appears throughout the experiments and needs no extended discussion. It is enough to cite the following instances. Others will doubtless appear from a study of the tables.

(a) The snails became "tame." When first brought into the laboratory they expel the air from the lung and drop from the surface film when in the least disturbed. After a time they no longer drop from the surface film under any moderate stimulation. To secure the expulsion of air from the lungs it is then necessary to use mechanical means.

(b) The snails recover from the inhibition of the mouth response due to manipulation. This appears in Table III, p. 13. Here inhibition is more marked in the first 60-80 trials, after which the snails recover and the number of reactions per response increases as the trials proceed until it reaches a maximum toward the middle of the series.

(c) A waning of response occurs in the series of trials shown in Table III, p. 13. Following a maximum number of reactions per response toward the middle of the series, the number gradually diminishes to the end of the series (figs. 2-7, Plate II). The snails are becoming adapted to the stimulus which is not followed by its wonted reward. The same thing occurs in the pressure response of trained snails shown in Table IV, p. 25. Here response is entirely lacking toward the end of the series.

(d) The absence of the response in the final trials on many days' series appears in Table III, p. 13. This may be due to fatigue, but is more likely the result of adaptation.

Other cases of adaptation in the snail appear in the paper of Dawson (1911) and in the work of Nagel (1894).

Learning, modifiability by association. Snails which gave no mouth response to pressure on the foot, were so modified by the simultaneous application of pressure to the foot and food to the mouth, that they then gave the mouth response to pressure on the foot. The effect of training with the simultaneous stimuli persisted for ninety-six hours after the cessation of training. Objectively this persistence of the training effect simulates memory. Its psychological implication is not discussed here.

The training process was accompanied by a shortening of the average duration of the individual reactions, in other words in an increase in their rate, as compared with the rate obtaining when the food stimulus alone was used. This acceleration is no doubt due to the unwonted stimulus applied to the foot and may be taken as evidence (if any is needed) that this stimulus was effective. The training process was accompanied by a reduction in the average number of reactions per response and this again may be attributed to the unwonted foot-stimulus and may be accepted as evidence of its effectiveness.

Notable in the training series of trials of Snails 4 and 5 is the increase and subsequent decline in the average number of reactions per response after the initial inhibition. The average number of reactions per ten trials rises to a maximum in the neighborhood of the 150th trial and then declines. An examination of the graphs for these two snails (figs. 2, 3, 8 and 9, Plates II-III) suggests that the training has produced its maximum effect in the neighborhood of the 150th trial. If this were true a longer response to pressure only should be obtained from snails that had been trained for 150 trials than from those of longer training and this response might persist for a longer period. Whether this is actually the case could only be determined by comparing the response to pressure alone in a considerable number of snails with training periods of different lengths. A comparison of the graphs for Snails 4 and 5 with those for 6, 7, 8 and 14 shows that the latter are quite irregular. They show numerous maxima one of which commonly lies in the region of trial 150, but they do not support the view of a maximum training effect at a particular region in the series of trials. Thus an interpretation that seems wholly tenable from a study of two snails becomes untenable when the study is extended to a larger number of individuals. The maxima in the graphs for Snails 4 and 5 probably have their explanation in some undiscovered variation in the physiological state of these individuals. That taken by itself, it is interpretable in terms of learning may serve as a warning against generalizations based on experiments on one or two individuals. Nevertheless the relation of length of training and training effect in the snail may be worthy of further investigation.

II. MODIFIABILITY STUDIED BY THE LABYRINTH METHOD

1. Introduction

The evidence of learning in the snail, *Physa gyrina* Say, obtained by the use of the method of simultaneous stimuli suggested the query whether the snail could solve a simple labyrinth. Failure to solve the labyrinth would be evidence that this requires a capacity beyond that of forming simple associations. The labyrinth method has been used extensively in nearly all groups of vertebrates, less extensively among invertebrates. Notable among the work on the latter group is that of Yerkes (1902) on the crayfish *Carcinus granulatus*, Yerkes and Huggins (1903) on habit formation in the crayfish *Cambarus affinis*, Fielde (1901) and Shepard (1916) on ants, and Yerkes (1912) on earthworms.

Just as the natural habits of rats in running about in narrow horizontal passages suggested to Small (1899) a series of such passages for experimental work, so the structure and habits of Physa suggested an upright or vertical path. Physa breathes air, but remains under water indefinitely when the so-called lung is full of air. When a new supply of oxygen is required it commonly seeks the surface of the water, although bubbles below the surface are sometimes "tapped" for air by the snail (Dawson, 1911). In order to gain the surface it ordinarily crawls up the stems of water plants growing in its natural habitat. It seemed then as though the tests might be placed on a purely natural basis by mechanically depriving the snail of air and placing it at the base of an upright path leading to the surface of the water. The animal's need for air would furnish the motive necessary to action. This need also suggested a natural punishment and reward for a correct or incorrect choice of paths, that is, the attainment of air at the end of the path or failure to get it.

2. Apparatus and Material*

The labyrinth used had the form of a rectangular U on an upright stem (fig. 14, Plate IV). One arm of the U reached the surface of the water while the other was so much shorter that the snail could not reach the surface from its tip. Various materials were tried out in the construction of the apparatus, such as cardboard coated with paraffin to protect it from the water, metal, mica and celluloid. The latter was finally selected because the surface of this material is like that of glass upon which snails crawl readily and also because it could be easily cut and manipulated should it be decided to alter the form of the path. Transparent celluloid 2.5 mm. in thickness was used in constructing the path (fig. 14, Plate IV). The upright

---

* The author is indebted to Professor George R. La Rue for advice in the mechanical construction of the labyrinth.

arms A and B were 8.5 mm. by 4.9 cm., the base of the U, C was 8.5 mm. by 4.25 cm. This rested on an upright stem D, 8.5 mm. by 3.6 cm., the base of which was cemented into a block of lead E, 2.1 cm. by 1.6 cm. by 6 mm. The celluloid part of the apparatus was carefully squared to the lead base E, before the cement was allowed to harden. Physa in crawling up anything but a broad surface has a tendency to crawl in a spiral direction, as may be readily demonstrated by allowing it to crawl up a glass rod placed upright in the aquarium. Should the animal ascend the stem of the labyrinth in a spiral path it would have the long or correct arm sometimes on its right and sometimes on its left. In order that the snail might have a choice of right and left paths on the labyrinth it was necessary to keep it confined to one side of the apparatus. In an attempt to accomplish this, the celluloid path was backed with galvanized, 0.5 inch-mesh wire netting, so cut that the entire path was bordered by a "picket fence" of wire, the pickets extending outward in the same plane as the path rather than at right angles to it. By cutting out every other "picket" the remaining wires were just far enough apart to make it mechanically impossible for the snail to pull its shell between them. This device proved an effective but not an absolute barrier, inasmuch as the animal occasionally managed to crawl over the end of a "picket," but the number of crossings became so reduced by its use, that this source of error was practically eliminated. The wire was cemented to the path by means of celluloid dissolved in acetone. The wires themselves and the lead base were also coated with this material, so that all parts of the completed apparatus with which the snail could come in contact were either made of celluloid or coated with it. This apparatus when in use was placed in the exact center of the experimental tank, thus bringing it directly under the centered light, so that the rays falling on each branch of the path A and B (fig. 14, Plate IV) were of equal intensity and thus the possibility of phototropism directing the course of the snail was avoided. Tap water at room temperature was used in the experimental tank. This temperature varied within the limits of a degree or two but it will appear later in the records of the experiments that the variations in temperature were the cause of no irregularities in the results.

In order to conduct the experiments under controlled lighting conditions and thus avoid any possible phototropic influence it was deemed advisable to create special conditions governing the light and keep them uniform throughout the experiments (fig. 15, Plate V). An experimental tank consisting of a battery jar 6 inches high and 4 inches in diameter was painted black on the outside and a level floor was obtained by means of a false bottom made of weighted paraffin. The center of the bottom was marked in the paraffin. This tank, during experimentation, was placed in the center of a " house " constructed from gray beaver-board fastened together with strips of gummed paper and painted dead black on the inside. The walls were 26 cm. by 38 cm. with a strip 19 cm. by 5 cm. cut from the lower edge on three sides for ventilation. For this purpose also small holes uniformly spaced were punched 2 cm. from the upper edge of the four sides. The fourth wall, the one next the operator, had a horizontal slit 2 cm. wide cut in it at 10 cm. from the top. The part of this wall below the slit was made removable. With the lower part of the fourth wall removed the operator could prepare experiments without taking the tank from the " house." When the movable part of the fourth wall was replaced there remained a slit 2 cm. in width, through which observations could be made. The top of the house was ground glass above which at a distance of 4 cm. was an 8 candle power electric light. This was centered over the house and held in a stationary position by means of a wooden frame. Two thicknesses of ordinary medium weight white paper were placed on top of the ground glass to further weaken and diffuse the light. The room in which the apparatus was located was darkened during experimentation. This apparatus was used without change throughout the experiments. The labyrinth itself was varied to conform with the requirements of each set of experiments. All snails used were tamed, individually numbered and kept under the same conditions as in the experiments with simultaneous stimuli.

3. EXPERIMENTS ON PREFERENCE

Two different kinds of preference tests were carried out. The first was to find whether a tendency to turn to the right or left existed. The asymmetry of the animal suggested the possibility.

The second test was to find any possible preference for an ascending or descending path.

A. *Experiments to test right or left preference.* In the *first series* an unpainted glass dish of beaker type, containing tap water at room temperature was placed in the centre of the experimental "house" and the ventilating spaces at the bottom of the walls of the "house" were blocked to exclude light. The air was forced from the lung of the snail by pressing its body gently back into its shell. It was then placed against the side of the dish at the four points of the compass in turn. It was allowed in each case to crawl up the side of the dish for air. The path was sketched and its right or left divergence from a perpendicular noted. The snails were handled with forceps and always placed on the horizontal bottom of the dish at right angles to the surface to be traveled. The mucous trail left on the sides of the dish was broken up after each trial by brushing the traveled surface with a clean camel's hair brush. Three snails, Nos. 15, 16 and 17, were used and four tests given each on two consecutive days. The record follows.

TABLE VII

Showing the deviation from a perpendicular shown by three individuals of *Physa gyrina* Say, in following an unconfined path up the side of a cylindrical glass dish in search of air.

	Trial No.	Snail 15	Snail 16	Snail 17
First day	1	Left	Right	Right
	2	Right	Left	Right
	3	Left	Right	Right
	4	Left	Left	Right
Second day	1	Left	Left	Right
	2	Left	Left	Left
	3	Right	Right	Right
	4	Right	Left	Right
Total		5L 3R	5L 3R	1L 7R

Total number of trials	24
Total number of deviations to the right	13 approximately 54%
Total number of deviations to the left	11 approximately 45%

This experiment was based upon the normal behavior of the snail in its search for air; its path from the bottom of the dish to the surface was not limited in any way. Besides the record just given, three points of general interest were observed.

1. At no time during these experiments did the snail appear to make any effort to obtain air except at the surface of the water. Inasmuch as the depth of the water in the experimental tank was greater than that in the home aquarium, the observations conflict with those of Dawson (1911) which refer, however, to young snails.

2. The touching of the tentacles to the surface of the dish, at intervals, on either side of the path of the snail appeared in no case to alter the direction of the path.

3. There appeared to be no effort on the part of the snail under observation in these experiments, to re-distribute its weight by shifting the shell from side to side. Such shifting of the shell might exert a pull upon the body of the animal, strong enough to turn it in one direction more than another.

In the *second series* of preference tests the U-shaped labyrinth was used. Both of the upright arms reached the surface of the water so that a choice of either did not result in punishment. Six snails were used. They were divided into two groups of three each and tested on alternate days. This division was made because six snails could not be worked each day during the time at the disposal of the operator, and it was deemed advisable to have records from more then three snails. Each snail in each group of three was given ten trials per day. Enough fresh tap water at room temperature was placed in the experimental tank to reach the top of the arms of the U apparatus resting in the center of the dish. The snail was taken from the bacteria dish, in which it was kept at room temperature, and held in the hand while the body was gently pressed back into the shell by means of a dull metal seeker. In this way the snail was compelled to expel air from its lung. The animal, now negatively geotropic, was placed on the base of the U, E (fig. 14, Plate IV) by means of long forceps. The apex of the shell rested on base E while the opening of the shell touched the stem D. Thus when the snail came out of its shell, which it usually did almost immediately, it at once started to crawl up the stem D to the surface. At the top of D it turned either to the right or left on the cross bar C and then moved up either arm A or B to the surface of the water where it was permitted to obtain air. It was then lifted off with the hand as gently as possible and returned to its individual dish where it was allowed to feed undisturbed while the other two snails of the group were tested in a similar way. After each test the celluloid apparatus was removed to a separate dish of water and thoroughly cleaned with a stiff brush in order to remove the mucous trail left by the tested snail and thus make it impossible for the next snail to follow the trail.

A complete record was made for each test given to each individual snail. Yerkes' method (1912), modified to suit the experiments, was adopted in making the records. Outlines of the labyrinth were drawn on separate slips of paper and the course chosen by each snail traced on one of these. The time required for the trip was taken with a stop-watch. The additional data of date, time of day, and temperature were added to the slip, and a record was also made of the eggs, if any, which had been deposited within twenty-four hours previous to the tests. It was thought that a later analysis of the records might show some relation between the physiological condition of the animal caused by ovulation, and the results of the experiments.

The following table shows the results obtained from a series of forty tests on each of six snails..

TABLE VIII

Showing the right and left preference of six individuals of *Physa gyrina* Say, which had been deprived of air and placed at the base of a vertical U-shaped path resting on an upright stem. Both arms of the U reached the surface. The horizontal of the U necessitated a choice of paths (fig. 14, Plate IV)

Snail No.	No. of days tested	No. of trials per day	No. of left choices	No. of right choices	Eggs
15	1	10	8	2	0
16		10	7	3	0
17		10	7	3	0
24	2	10	2	8	0
25		10	1	9	1
26		10	2	8	1
15	3	10	5	5	0
16		10	5	5	1
17		10	8	2	1
24	4	10	3	7	0
25		10	5	5	0
26		10	4	6	0
15	5	10	6	4	0
16		10	1	9	0
17		10	5	5	0

TABLE VIII—Continued

Snail No.	No. of days tested	No. of trials per day	No. of left choices	No. of right choices	Eggs
24	6	10	3	7	1
25		10	2	8	1
26		10	3	7	1
15	7	10	8	2	0
16		10	6	4	0
17		10	9	1	1

Total number of trials	240
Total number of left turns	116 approximately 48%
Total number of right turns	124 approximately 52%

B. *Experiments to test preference for an ascending or descending path.* If the cross bar of the U should not be horizontal, the snail after ascending the stem would have a choice of a descending path on one side and an ascending path on the other. In a set of preference tests an attempt was made to determine whether the animal showed preference for either of these paths. In order to tilt the labyrinth at various angles the experimental tank was placed on a small board, 8 1-2 in. by 10 in. by 1-4 in., made of three layers to insure an unwarped surface. One end of the board was pierced by two screw eyes set one inch from the edge. By turning these screws the board, together with the apparatus resting upon it, could be readily adjusted to any angle. The angle used in each experiment was measured with a Sterret spirit level. The slant of the right and left sides of the path was frequently and irregularly reversed in order to avoid any possible right and left habit formation during experimentation.

These tests were performed in the same way as those already described and the records were similar. The tabulated results follow.

TABLE IX

Showing the tendency of six individuals of *Physa gyrina* Say to follow an ascending or a descending path when the apparatus was tilted. The U-shaped apparatus was used and the angle made by the cross bar of the U with a horizontal measure. Both arms of the U reached the surface.

Snail No.	Angle of inclination to horizontal of cross bar of U-shaped labyrinth									
	0°–1°		1°–2°		3°–4°		5°–6°		7°–8°	
	up	down	up	down	up	down	up	down	up	down
15	37	23	3	7	7	3	5	5	8	2
16	23	27	2	8	4	6	5	5	6	4
17	38	22	6	4	5	5	6	4	6	4
24	36	24	6	4	5	5	6	4	10	0
25	32	28	4	6	4	6	7	3	8	2
26	32	28	4	6	6	4	6	4	5	5
Totals.......	208	152	25	35	31	29	35	29	43	17
	58%	42%	42%	58%	52%	48%	58%	42%	72%	28%

Table IX shows that when six individuals were given a choice of an ascending or a descending path, both of which ended at the surface, and when the angle of deviation from a horizontal at the point of selection was not greater than 8 degrees, the ascending path was chosen in 342 trials out of 600 or in 57% of the trials. There appears to be a strong tendency to follow the upward path only at the angle 7-8 degrees. In further experiments the cross bar of the U was kept horizontal.

4. EXPERIMENTS ON LEARNING THE U-SHAPED LABYRINTH; CHOICE OF RIGHT AND LEFT PATH; PUNISHMENT FAILURE TO GET AIR

Inasmuch as the snails showed a slight tendency to turn toward the right in the preference tests, the right arm of the U-shaped path was now shortened by cutting 2 cm. from the upper end. A choice of this arm would lead to punishment (failure to get air) and would be a wrong choice. When a wrong choice was made the snail had to retrace its path to the point of choice where it usually continued its course along the horizontal bar and up the long arm to the surface of the water. This was the only modification made in the apparatus for the learning experiments. The method of handling the animals and the apparatus was the same as in the preceding tests. Three snails were used in these experiments. They were at first tested every other day. The three snails were worked in turn and as many trips given each as was possible in the time at the disposal of the experimenter. The number of trips per day varied considerably because it was found that sometimes individual snails refused to start up the labyrinth. At such times they were frequently allowed to remain at the base of the stem for a considerable length of time in the hope that the tests might be continued in regular order. Sometimes, however, work on the individual snail so affected had to be discontinued for the day. When a snail lost its hold on the labyrinth and dropped to the bottom of the tank during its test, it was immediately started up again and this second trip was given the same serial number as the previous one with a small letter after it. The same method of recording the observations was used as that described for the preference tests. A record was also made of the "latent" period, that is, the time between the placing of the animal at the base of the stem and the actual start on the upward path. Although not noted in the tabulations, this record appears to have no bearing on the results of the experiments, since fluctuations were probably due in part to the fact that the snail still retained a small amount of air in its lung, so that its need for a fresh supply was less urgent. In constructing curves based upon these records the percentage of errors or wrong choices for each snail per day was used. An analysis of the records appears to show no relation between the latent periods, oviposition, the time required for the trip and the number of errors.

TABLE X

Showing the paths followed by three individuals of *Physa gyrina* Say in attempting to reach the surface of the water for air. U-shaped labyrinth used. The right arm was so short that the snail could not obtain air by following it (punishment), while air might be obtained at the top of the left arm (reward). The apparatus was cleaned between trials.

SNAIL NO. 1

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
*1	1	1	1	0
	2	2	1	0
	3	3	0	1
	4	4	1	0
	5	5	1	0
	6	6	1	0
Totals		6	5 (83%)	1 (16%)

TABLE X—(*Continued*)

SNAIL NO. 1—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
*2	7	1	1	0
	8	2	1	0
	9	3	0	1
	10	4	1	0
	11	5	1	0
	12	6	0	1
	13	7	1	0
	14	8	1	0
Totals........................		8	6 (75%)	2 (25%)
*3	15	1	1	0
	16	2	1	0
	17	3	1	0
	18	4	1	0
	19	5	1	0
	20	6	1	0
	21	7	0	1
Totals........................		7	6 (85%)	1 (14%)
*4	22	1	1	0
	23	2	0	1
	24	2a	1	0
	25	2b	1	0
	26	3	1	0
	27	4	1	0
	28	5	1	0
Totals........................		7	6 (85%)	1 (14%)
5	29	1	1	0
	30	2	1	0
	31	3	1	0
	32	4	1	0
	33	5	0	1
	34	5a	1	0
Totals........................		6	5 (83%)	1 (16%)
*6	35	1	1	0
	36	2	1	0
	37	3	1	0
	38	4	0	1
	39	4a	0	1
	40	4b	0	1
	41	4c	1	0
	42	5	0	1
Totals..		8	4 (50%)	4 (50%)

TABLE X—(*Continued*)

SNAIL NO. 1—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
7	43	1	1	0
	44	2	0	1
	45	3	1	0
	46	4	1	0
	47	5	1	0
	48	6	0	1
	49	6a	0	1
	50	6b	1	0
Totals......................		8	5 (62%)	3 (37%)
8	51	1	1	0
	52	2	1	0
	53	3	1	0
	54	4	1	0
	55	5	1	0
	56	6	0	1
	57	7	0	1
Totals......................		7	5 (71%)	2 (28%)
*9	58	1	1	0
	59	2	1	0
	60	3	1	0
	61	4	1	0
	62	5	1	0
Totals......................		5	5 (100%)	0
10	63	1	1	0
	64	2	0	1
	65	3	0	1
	66	4	1	0
	67	5	1	0
Totals......................		5	3 (60%)	2 (40%)
*11	68	1	1	0
	69	2	1	0
	70	3	1	0
	71	4	0	1
	72	5	0	1
	73	6	0	1
	74	6a	1	0
Totals......................		7	4 (57%)	3 (42%)
*12	75	1	0	1
	76	2	0	1
	77	3	0	1
	78	4	0	1
	79	5	1	0
	80	6	0	1
	81	7	1	0
Totals......................		7	2 (28%)	5 (71%)

TABLE X—(*Continued*)

Snail No. 1—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
13	82	1	1	0
	83	2	0	1
	84	3	0	1
	85	4	0	1
	86	5	1	0
	87	6	0	1
	88	7	0	1
Totals........	....	7	2 (28%)	5 (71%)
*14	89	1	1	0
	90	2	0	1
	91	3	0	1
	92	3a	0	1
	93	3b	0	1
	94	3c	0	1
	95	3d	0	1
	96	3e	0	1
	97	4	0	1
	98	5	0	1
	99	6	0	1
Totals........	....	11	1 (9%)	10 (90%)
*15	100	1	0	1
	101	2	0	1
	102	3	0	1
	103	3a	1	0
Totals........	....	4	1 (25%)	3 (75%)
16	104	1	1	0
	105	2	1	0
	106	3	1	0
	107	4	1	0
	108	5	1	0
	109	6	0	1
	110	7	0	1
	111	7a	1	0
Totals........	....	8	6 (75%)	2 (25%)
*17	112	1	1	0
	113	2	0	1
	114	3	1	0
	115	4	0	1
	116	5	0	1
	117	6	1	0
	118	7	1	0
Totals........	....	7	4 (57%)	3 (42%)
18	119	1	1	0
	120	2	1	0
	121	3	1	0
	122	4	0	1
	123	5	0	1
Totals........	....	5	3 (60%)	2 (40%)

TABLE X—(*Continued*)

SNAIL NO. 1—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
19	124	1	0	1
	125	2	0	1
	126	3	0	1
	127	4	0	1
	128	5	1	0
Totals......................		5	1 (20%)	4 (80%)
20	129	1	0	1
	130	1a	1	0
	131	2	1	0
	132	3	0	1
	133	4	1	0
	134	5	1	0
Totals......................		6	4 (66%)	2 (33%)
*21	135	1	1	0
	136	2	0	1
	137	2a	1	0
	138	3	1	0
	139	4	1	0
	140	5	0	1
Totals......................		6	4 (66%)	2 (33%)
*22	141	1	1	0
	142	2	1	0
	143	3	1	0
	144	4	1	0
	145	5	0	1
	146	5a	1	0
Totals......................		6	5 (83%)	1 (16%)
23	147	1	1	0
	148	2	0	1
	149	3	1	0
	150	4	1	0
	151	5	1	0
	152	6	1	0
	153	7	0	1
	154	7a	1	0
Totals......................		8	6 (75%)	2 (25%)
24	155	1	0	1
	156	2	0	1
	157	3	0	1
	158	3a	0	1
	159	3b	1	0
	160	4	0	1
Totals......................		6	1 (16%)	5 (83%)

TABLE X—(*Continued*)

SNAIL NO. 1—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
25	161	1	0	1
	162	2	0	1
	163	3	0	1
	164	4	0	1
	165	5	0	1
Totals		5	0	5 (100%)
26	166	1	0	1
	167	2	1	0
	168	3	0	1
	169	3a	0	1
	170	3b	1	0
Totals		5	2 (40%)	3 (60%)
*27	171	1	1	0
	172	2	0	1
	173	3	0	1
	174	3a	0	1
	175	4	0	1
Totals		5	1 (20%)	4 (80%)
28	176	1	0	1
	177	2	0	1
	178	3	0	1
	179	4	0	1
	180	5	0	1
Totals		5	0	0 (100%)
29	181	1	0	1
	182	2	1	0
	183	3	0	1
	184	4	0	1
Totals		4	1 (25%)	3 (75%)
30	185	1	0	1
	186	2	0	1
	187	3	0	1
Totals		3	0	3 (100%)
31	188	1	1	0
	189	2	1	0
	190	3	1	0
	191	4	0	1
	192	5	0	1
Totals		5	3 (60%)	2 (40%)
32	193	1	0	1
	194	2	0	1
	195	3	1	0
	196	4	1	0
	197	5	1	0
Totals		5	3 (60%)	2 (40%)

TABLE X—(*Continued*)

SNAIL NO. 1—(*Continued*

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
*33	198	1	0	1
	199	1a	0	1
	200	2	0	1
	201	3	0	1
	202	4	0	1
Totals		5	0	5 (100%)
34	203	1	0	1
	204	1a	0	1
	205	2	0	1
	206	2a	1	0
	207	3	0	1
Totals		5	1 (20%)	4 (80)%
35	208	1	1	0
	209	2	0	1
	210	3	1	0
	211	4	1	0
Totals		4	3 (75%)	1 (25%)
*36	212	1	1	0
	213	2	1	0
	214	3	0	1
	215	4	0	1
	216	5	0	1
Totals		5	2 (40%)	3 (60%)
37	217	1	0	1
	218	2	0	1
	219	2a	0	1
	220	3	1	0
	221	4	0	1
Totals		5	1 (20%)	4 (80%)
*38	222	1	0	1
	223	1a	1	0
	224	2	1	0
	225	3	0	1
	226	4	0	1
Totals		5	2 (40%)	3 (60%)
39	227	1	1	0
	228	2	0	1
	229	3	0	1
	230	3a	1	0
	231	4	1	0
Totals		5	3 (60%)	2 (40%)

TABLE X—(*Continued*)

SNAIL NO. 1—(*Continued*

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
40	232	1	1	0
	233	2	0	1
	234	3	0	1
	235	3a	0	1
	236	4	1	0
Totals		5	2 (40%)	3 (60%)
41	237	1	1	0
	238	2	1	0
	239	3	0	1
	240	4	0	1
	241	5	0	1
Totals		5	2 (40%)	3 (60%)
42	242	1	1	0
	243	2	0	1
	244	3	0	1
	245	4	0	1
	246	5	1	0
Totals		5	2 (40%)	3 (60%)
*43	247	1	1	0
	248	2	1	0
	249	3	1	0
	250	4	0	1
	251	5	0	1
Totals		5	3 (60%)	2 (40%)
44	252	1	1	0
	253	2	1	0
	254	3	1	0
	255	4	0	1
	256	5	0	1
	257	6	1	0
	258	7	0	1
	259	8	0	1
Totals		8	4 (50%)	4 (50%)
45	260	1	0	1
	261	2	1	0
	262	3	0	1
Totals		3	1 (33%)	2 (66%)
*46	263	1	0	1
	264	2	1	0
	265	3	0	1
	266	3a	0	1
	267	4	0	1
	268	5	0	1
	269	6	0	1
	270	6a	0	1
Totals		8	1 (12%)	7 (87%)

TABLE X—(*Continued*)

Snail No. 1—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct or left choices	No. of incorrect or right choices
47	271	1	1	0
	272	2	0	1
	273	2a	0	1
	274	2b	0	1
	275	3	0	1
	276	4	0	1
Totals........................		6	1 (16%)	5 (83%)
48	277	1	1	0
	278	2	0	1
	279	3	0	1
	280	4	1	0
	281	5	1	0
	282	6	1	0
Totals........................		6	4 (66%)	2 (33%)

Summary—Snail No. 1

No. of days tested	No. of trials	Correct or left choices	Incorrect or right choices
*1	6	5	1
*2	8	6	2
*3	7	6	1
*4	7	6	1
5	6	5	1
*6	8	4	4
7	8	5	3
8	7	5	2
*9	5	5	0
10	5	3	2
*11	7	4	3
*12	7	2	5
13	7	2	5
*14	11	1	10
*15	4	1	3
16	8	6	2
*17	7	4	3
18	5	3	2
19	5	1	4
20	6	4	2
*21	6	4	2
22	6	5	1
23	8	6	2
24	6	1	5
25	5	0	5
26	5	2	3
*27	5	1	4
28	5	0	5
29	4	1	3
30	3	0	3

TABLE X—(*Continued*)

SUMMARY—SNAIL NO. 1—(*Continued*)

No. of days tested	No. of trials	Correct or left choices	Incorrect or right choices
31	5	3	2
32	5	3	2
*33	5	0	5
34	5	1	4
35	4	3	1
*36	5	2	3
37	5	1	4
*38	5	2	3
39	5	3	2
40	5	2	3
41	5	2	3
42	5	2	3
*43	5	3	2
44	8	4	4
45	3	1	2
*46	8	1	7
47	6	1	5
48	6	4	2

Total number of trials	282
Total number of correct or left choices	136 approximately 48%
Total number of incorrect or right choices	146 approximately 52%

SUMMARY—SNAIL NO. 2

No. of days tested	No. of trials	Correct or left choices	Incorrect or right choices
1	5	4	1
*2	8	8	0
*3	7	3	4
*4	5	3	2
5	6	2	4
*6	5	5	0
7	6	3	2
8	6	3	3
*9	5	2	3
10	6	1	5
*11	9	3	6
*12	8	4	4
13	7	4	3
14	7	4	3
*15	3	2	1
16	9	4	5
17	7	4	3
*18	7	2	5
19	5	4	1
20	5	2	3
21	5	3	2
*22	7	1	6
23	7	6	1

TABLE X—(*Continued*)

SUMMARY—SNAIL NO. 2—(*Continued*)

No. of days tested	No. of trials	Correct or left choices	Incorrect or right choices
24	4	0	4
25	5	2	3
26	4	2	2
*27	5	1	4
28	5	0	5
29	4	1	3
*30	3	0	3
31	4	2	2
32	5	1	4
33	5	0	5
*34	5	1	4
*35	4	1	3
36	5	2	3
37	5	0	5
*38	5	1	4
39	5	1	4
*40	5	0	5
41	5	2	3
42	5	3	2
43	5	4	1
*44	5	0	5
45	8	4	4
46	3	0	3
47	7	4	3
48	6	1	5
49	6	1	5
*50	10	2	8
51	5	1	4
52	7	2	5
53	6	0	6
54	6	1	5

Total number of trials	307
Total number of correct or left choices	117 approximately 38%
Total number of incorrect or right choices	190 approximately 62%

SUMMARY—SNAIL NO. 3

No. of days tested	No. of trials	Correct or left choices	Incorrect or right choices
*1	2	1	1
*2	7	4	3
3	6	5	1
*4	5	0	5
5	6	1	5
6	7	2	5
7	5	2	3
8	6	1	5
9	5	4	1
10	5	2	3

TABLE X—(*Continued*)

SUMMARY—SNAIL NO. 3—(*Continued*)

No. of days tested	No. of trials	Correct or left choices	Incorrect or right choices
*11	6	3	3
*12	7	4	3
13	7	2	5
14	7	3	4
*15	3	1	2
16	7	3	4
17	7	3	4
*18	5	1	4
19	5	2	3
20	5	2	3
21	4	2	2
22	4	1	3
*23	8	6	2
24	4	3	1
*25	7	2	5
26	5	2	3
*27	5	2	3
28	5	2	3
*29	5	1	4
30	5	1	4
31	4	3	1
32	5	1	4
*33	5	1	4
34	5	2	3
35	4	0	4
36	5	3	2
37	5	0	5
*38	5	2	3
*39	5	4	1
40	5	1	4
*41	5	4	1
42	5	1	4
43	5	1	4
44	5	1	4
45	8	2	6
46	3	1	2
47	7	6	1
48	5	3	2
49	6	1	5
*50	10	3	7
51	8	0	8
52	7	3	4
53	6	0	6
*54	6	2	4

Total number of trials	299
Total number of correct or left choices	113 approximately 38%
Total number of incorrect or right choices	186 approximately 62%

---

* Indicates that an egg mass was deposited by the snail during the 24 hours immediately preceding the tests on the day marked.

TABLE X-A

SUMMARY OF LEFT-RIGHT DISCRIMINATION TESTS

	Snail No. 1	Snail No. 2	Snail No. 3
Total number of trials	282	307	299
Per cent of incorrect choices	52	62	62

Table X shows that in a total of 888 trials on three individuals of Physa, in a series of tests offering a choice of right and left paths, with a reward for the correct choice (left), the incorrect path (right) was followed in 58.8% of the trials. The graphs (figs. 16-18, Plate VI) show very great irregularity in the time distribution of the right and wrong choices. There are days when the record is perfect, other days when all choices are wrong. Between these is every intermediate condition. A slight excess of incorrect choices is to be expected on account of the possible tendency to turn to the right shown in the right-left preference tests. *These records show no indication of learning.*

5. EXPERIMENTS WITH THE Y-SHAPED LABYRINTH: ROUGHNESS OF PATH AS A WARNING SIGN: ELECTRIC SHOCK PUNISHMENT

It seemed either that the problem itself was too difficult or that the immediate demand for air (punishment) was not great enough to force its solution. In its natural habitat the snail often depends on plant stems as a means of reaching the surface of the water when a fresh supply of oxygen is needed. The stems must frequently be too short. The problem is then familiar to the snail and should be easy. To make the problem still easier it was decided to determine whether or not the snail could form a simple association between two stimuli upon a labyrinth that it had failed to solve. The labyrinth was changed as follows. 1. A Y-shaped labyrinth was used so that the snail could make a choice immediately upon reaching the top of the stem of the Y. The Y was made of glass tubing. 2. The snail was not confined to one side of it. Owing to the spiral course followed by the ascending animal, there was no right and left choice. 3. One side of the Y was made rough and the other left smooth. The training series had shown that the snail feels the delicate pressure on the foot of the three-pronged pressure fork of the food-pressure apparatus (fig. 1, Plate I). It should therefore feel the difference between a rough and smooth path. 4. For the failure-to-get-air punishment with the U-shaped labyrinth there was substituted the electric shock. Reward was the getting of air.

The problem resolved itself then, into an attempt to determine whether or not the snail could form a simple association between two dissimilar stimuli one of which (punishment) followed the other (warning). It has already been shown that the snail can form a weak association between two stimuli simultaneously applied (food-pressure series). Can it then bridge over an interval between two stimuli one of which follows the other almost immediately?

Glass tubing, 5 mm. in diameter, in the form of a Y was used. One arm of the Y, 4.5 cm. in length, reached the surface of the water. The other arm, 3.5 cm. in length, was short enough so that the animals could not reach the surface. The stem of the Y was 3 cm. long. Two (No. 23) silk-insulated copper wires were passed through the stem to the upper end of the short arm, where they connected with two fine platinum wire (No. 26) rings passing around the end of the short arm close enough together so that an electric current could be passed between them, when proper connections were made. The upper ends of both arms were then closed with sealing wax to keep out the water. A hollow base, 4 cm. by 2 cm. by 1.5 cm., of lead was made, in order to permit an outlet for the wires. These passed from the lower end of the stem of the Y into a rubber tube which carried them, protected from the water, through the metal base, along the floor of the experimental tank and upward to the outside. A direct current of 0.2 amperes was taken from 220 volt mains through a lamp of 1100 ohms in series with 8 ohms of other resistance. From the terminals of 8 ohms a second circuit was taken including a rheostat of 1800 ohms, a receiving switch, a key and the Y apparatus. When no current was drawn through this second circuit the voltage between the two platinum wires would be 1.6 volts. The maximum current which could be drawn through the circuit by short-circuiting the two platinum wires was slightly less (actually 16–18) than 0.001 amperes or 1 milliampere*. It was

---

*The author is indebted to Professor A. W. Smith for measuring the electric current used in the experiments.

found that this current caused the snails to draw backward into their shells for a little way. They usually resumed their normal movements in a short time. Rarely they retracted so much that they lost their hold on the glass tubing and dropped to the bottom of the tank. The short arm of the Y from the point of divergence to within a millimeter or so of the platinum wires was roughened by means of a band of block tin, 2 mm. in width and 1 mm. thick, passing around it in a spiral. The metal was then insulated with a coat of liquid celluloid and further roughened by a coat of coarse cutting sand imbedded in the celluloid before it hardened. These precautions appeared desirable because it seemed possible that enough mucus might be secreted by the snail in crawling over sand alone, to fill in the spaces between the grains and thus make a smooth surface upon which to travel. This hardly seemed possible over the ridges of metal used, inasmuch as they were far enough separated so that the snail had to sink down into the space between, in passing over them. If the snail turned toward the short arm at the top of the stem it immediately came in contact with the rough ridges and a little later with the bare electric wires, at which point the punishment was administered by closing the circuit. No attempt was made to keep the snail on one side of the apparatus. It sometimes crawled in an irregular spiral but frequently kept on one side. The whole apparatus was made shorter than the celluloid labyrinth previously used, in order to shorten the time used in each test and thus make a greater number of tests possible in a given time. The method of manipulation and cleaning the apparatus was the same as in the previous tests, and in order to avoid any possible tendency of the snail to follow the same path in consecutive trips, the labyrinth was frequently turned so that the short arm was as often toward the left as the right. The "map" method before described was adopted in making the records. Four snails were used and a total of 664 trials given. The curves (figs. 19-22, Plate VII) were based upon the percentage of errors for each snail per day.

TABLE XI

Showing the number of correct and incorrect turns made by three individuals of *Physa gyrina* Say, in their attempts to reach the surface of the water on the Y apparatus. Error was punished by an electric shock. The warning stimulus was roughness. The apparatus was cleaned between trials.

SNAIL NO. 9

No. of days tested	No. of trial for series	No. of trial for day	No. of correct choices	No. of incorrect choices
1	1	1	1	0
	2	2	0	1
	3	3	0	1
Totals........................		3	1 (33%)	2 (66%)
2	4	1	0	1
3	5	1	1	0
	6	2	1	0
	7	3	1	0
Totals........................		3	3 (100%)	0
4	8	1	1	0
	9	2	0	1
	10	2a	0	1
	11	2b	0	1
	12	3	0	1
Totals........................		5	1 (20%)	4 (80%)
*5	13	1	0	1
	14	1a	1	0
	15	2	0	1
	16	3	0	1
	17	3a	0	1
Totals........................		5	1 (20%)	4 (80%)
*6	18	1	0	1
	19	1a	1	0
	20	2	0	1
	21	3	0	1
Totals........................		4	1 (25%)	3 (75%)
7	22	1	1	0
	23	2	0	1
	24	2a	0	1
	25	2b	1	0
	26	3	0	1
	27	3a	0	1
	28	4	0	1
Totals........................		7	3 (42%)	4 (57%)
8	29	1	1	0
	30	2	0	1
	31	3	0	1
	32	3a	0	1
	33	3b	1	0
	34	4	1	0
Totals........................		6	3 (50%)	3 (50%)

TABLE XI—(*Continued*)

SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct choices	No. of incorrect choices
9	35	1	1	0
	36	2	1	0
	37	3	1	0
	38	4	0	1
	39	5	0	1
	40	5a	0	1
	41	5b	1	0
Totals......................		7	4 (57%)	3 (42%)
10	42	1	1	0
	43	2	1	0
Totals......................		2	2 (100%)	0
11	44	1	0	1
	45	2	0	1
	46	2a	0	1
	47	3	0	1
	48	3a	0	1
Totals......................		5	0	5 (100%)
12	49	1	1	0
	50	2	1	0
	51	3	0	1
	52	4	0	1
	53	4a	0	1
	54	4b	1	0
	55	5	0	1
	56	5a	0	1
Totals......................		8	3 (37%)	5 (62%)
13	57	1	1	0
	58	2	1	0
	59	3	1	0
Totals......................		3	3 (100%)	0
14	60	1	0	1
	61	1a	1	0
	62	2	0	1
	63	2a	0	1
Totals......................		4	1 (25%)	3 (75%)
15	64	1	1	0
	65	2	0	1
	66	2a	0	1
	67	2b	1	0
	68	3	1	0
Totals......................		5	3 (60%)	2 (40%)

TABLE XI—(*Continued*)

SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct choices	No. of incorrect choices
16	69	1	1	0
	70	2	0	1
	71	2a	0	1
	72	2b	0	1
	73	2c	1	0
	74	3	1	0
Totals		6	3 (50%)	3 (50%)
17	75	1	1	0
	76	2	1	0
	77	3	1	0
	78	4	0	1
	79	4a	0	1
Totals		5	3 (60%)	2 (40%)
18	80	1	0	1
	81	1a	1	0
	82	2	0	1
	83	3	1	0
	84	4	0	1
	85	4a	1	0
	86	5	0	1
Totals		7	3 (42%)	4 (57%)
19	87	1	0	1
	88	2	1	0
	89	3	0	1
	90	3a	0	1
	91	3b	1	0
Totals		5	2 (40%)	3 (60%)
20	92	1	1	0
	93	2	0	1
	94	2a	0	1
	95	2b	1	0
Totals		4	2 (50%)	2 (50%)
21	96	1	0	1
	97	1a	1	0
	98	2	1	0
	99	3	1	0
	100	4	0	1
Totals		5	3 (60%)	2 (40%)
22	101	1	0	1
	102	2	1	0
	103	3	0	1
	104	3a	1	0
	105	4	1	0
	106	5	1	0
Totals		6	4 (66%)	2 (33%)

TABLE XI—(*Continued*)
SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct choices	No. of incorrect choices
23	107	1	0	1
	108	1a	1	0
	109	2	1	0
	110	3	0	1
	111	3a	0	1
	112	3b	1	0
	113	4	1	0
Totals........................		7	4 (57%)	3 (42%)
24	114	1	0	1
	115	1a	0	1
	116	1b	0	1
	117	1c	1	0
	118	2	1	0
Totals........................		5	2 (40%)	3 (60%)
25	119	1	0	1
	120	1a	1	0
	121	2	1	0
	122	3	1	0
	123	4	1	0
	124	5	1	0
	125	6	0	1
	126	6a	0	1
	127	6b	0	1
	128	6c	1	0
Totals........................		10	6 (60%)	4 (40%)
26	129	1	1	0
	130	2	1	0
	131	3	1	0
	132	4	1	0
	133	5	0	1
	134	5a	0	1
	135	5b	1	0
Totals........................		7	5 (71%)	2 (28%)
27	136	1	0	1
	137	2	1	0
	138	3	0	1
	139	3a	0	1
	140	3b	0	1
	141	3c	0	1
	142	3d	1	0
	143	4	1	0
Totals........................		8	3 (37%)	5 (62%)
28	144	1	0	1
	145	1a	0	1
	146	1b	0	1
	147	1c	0	1
	148	2	0	1
	149	2a	0	1
	150	2b	1	0
Totals........................		7	2 (28%)	5 (71%)

TABLE XI—(*Continued*)

SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct choices	No. of incorrect choices
29	151	1	0	1
	152	1a	0	1
	153	1b	0	1
	154	1c	1	0
Totals........................		4	1 (25%)	3 (75%)
30	155	1	0	1
	156	1a	1	0
	157	2	1	0
	158	3	0	1
	159	3a	0	1
	160	4	1	0
	161	5	1	0
Totals........................		7	4 (57%)	3 (42%)
31	162	1	0	1
	163	2	1	0
	164	3	1	0
	165	4	0	1
	166	4a	0	1
	167	4b	0	1
	168	4c	0	1
Totals........................		7	2 (28%)	5 (71%)
32	169	1	0	1
	170	1a	0	1
	171	1b	1	0
	172	2	0	1
	173	2a	1	0
	174	3	1	0
	175	4	1	0
	176	5	1	0
	177	6	0	1
	178	6a	0	1
Totals....		10	5 (50%)	5 (50%)
33	179	1	0	1
	180	1a	0	1
	181	1b	0	1
	182	1c	1	0
	183	2	1	0
	184	3	1	0
	185	4	0	1
	186	4a	1	0
Totals........................		8	4 (50%)	4 (50%)
34	187	1	1	0
	188	2	0	1
	189	2a	0	1
	190	2b	0	1
	191	2c	0	1
	192	2d	1	0
Totals........................		6	2 (33%)	4 (66%)

TABLE XI—(*Continued*)

SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct choices	No. of incorrect choices
35	193	1	0	1
	194	1a	0	1
	195	1b	1	0
	196	2	0	1
	197	2a	0	1
	198	2b	1	0
Totals........................		6	2 (33%)	4 (66%)
36	199	1	0	1
	200	1a	0	1
	201	1b	0	1
	202	1c	0	1
	203	1d	1	0
Totals........................		5	1 (20%)	4 (80%)
37	204	1	0	1
	205	1a	0	1
	206	1b	0	1
	207	1c	0	1
	208	1d	0	1
	209	1e	0	1
	210	2	1	0
Totals........................		7	1 (14%)	6 (85%)
38	211	1	1	0
	212	2	0	1
	213	2a	0	1
	214	2b	0	1
	215	2c	1	0
Totals........................		5	2 (40%)	3 (60%)

SUMMARY—SNAIL NO. 9

No. of days tested	No. of trials	Correct choices	Incorrect choices
1	3	1	2
2	1	0	1
3	3	3	0
4	5	1	4
*5	5	1	4
*6	4	1	3
7	7	3	4
8	6	3	3
9	7	4	3
10	2	2	0
11	5	0	5
12	8	3	5
13	3	3	0

---

* Indicates that an egg mass had been deposited by the snail during the 24 hours immediately preceding the tests on the day marked.

TABLE XI—(*Continued*)

SUMMARY—SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trials	Correct choices	Incorrect choices
14	4	1	3
15	5	3	2
16	6	3	3
17	5	3	2
18	7	3	4
19	5	2	3
20	4	2	2
21	5	3	2
22	6	4	2
23	7	4	3
24	5	2	3
25	10	6	4
26	7	5	2
27	8	3	5
28	7	2	5
29	4	1	3
30	7	4	3
31	7	2	5
32	10	5	4
33	8	4	5
34	6	2	4
35	6	2	4
36	5	1	4
37	7	1	6
38	5	2	3

Total number of trials	215
Total number of correct choices	95 approximately 44%
Total number of incorrect choices	120 approximately 56%

SUMMARY—SNAIL NO. 10

No. of days tested	No. of trials	Correct choices	Incorrect choices
1	6	1	5
2	3	2	1
3	6	3	3
4	1	1	0
5	3	1	2
6	3	1	2
7	6	3	3
8	5	1	4
9	2	1	1
10	5	2	3
11	5	3	2
12	9	3	6
13	10	3	7
14	6	1	5
15	6	2	4
16	8	4	4

TABLE XI—(*Continued*)

SUMMARY—SNAIL NO. 10—(*Continued*)

No. of days tested	No. of trials	Correct choices	Incorrect choices
17	11	3	8
18	9	5	4
19	5	1	4
20	14	4	10
21	9	1	8
22	7	3	4
23	5	1	4
24	3	2	1
25	11	2	9
26	6	3	3
27	15	5	10
28	5	4	1
29	8	2	6
30	7	1	6
31	5	2	3
32	6	3	3
33	4	1	3

Total number of trials.................................... 214
Total number of correct trials.......................... 75 approximately 35%
Total number of incorrect trials....................... 139 approximately 65%

SUMMARY—SNAIL NO. 11

No. of days tested	No. of trials	Correct choices	Incorrect choices
1	4	3	1
2	6	1	5
3	5	4	1
4	9	4	5
5	7	3	4
6	5	0	5
7	8	1	7
8	7	5	2
9	5	5	0
10	3	2	1
11	9	7	2
12	6	4	2
13	9	3	6
14	6	4	2
15	5	1	4
16	8	5	3
17	7	0	7
18	10	3	7
19	7	3	4
20	9	2	7
21	7	2	5
22	7	1	6
23	7	3	4
24	8	2	6
25	14	7	7

TABLE XI—(*Continued*)

SUMMARY—SNAIL NO. 11—(*Continued*)

Total number of trials	178
Total number of correct trials	75 approximately 42%
Total number of incorrect trials	103 approximately 58%

SUMMARY—SNAIL NO. 27

No. of days tested	No. of trials	Correct choices	Incorrect choices
1	4	2	2
2	3	1	2
3	5	1	4
4	6	1	5
5	5	1	4
6	4	0	4
7	4	2	2
8	7	0	7
9	5	4	1
10	6	2	4
11	5	3	2
12	3	1	2

Total number of trials	57
Total number of correct trials	18 approximately 32%
Total number of incorrect trials	39 approximately 68%

TABLE XI-A

SUMMARY OF ROUGH-SHOCK-SMOOTH TESTS

	Snail No. 9	Snail No. 10	Snail No. 11	Snail No. 27
Total number of trials	215	214	178	57
Per cent of incorrect choices	56	65	58	68

Table XI shows the trials of four snails on the rough-shock-smooth Y-shaped labyrinth. These cover periods of 38, 35, 25, and 12 days and involve 215, 214, 178, and 57 trials, or a total of 664 trials. Of these 401 or 60.3% give wrong choices. The curves (figs. 19-22, Plate VII) show great irregularity, especially in the first third of their courses. Nearly all the days on which the choices were either all right or all wrong fall in this part of the curves. After this the curves fluctuate less from the fifty per cent level, but rise slightly as they proceed. In this series the snails climb a labyrinth of cylindrical glass and are free to move on any side of it; there is no question of right or left. The choice is made between rough and smooth. No rough-smooth preference tests were made, but there is evident in this series a possible tendency to prefer the rough side. The punishment had no apparent effect upon this tendency. It seemed as though this might be due to the fact that owing to the construction of the apparatus the punishment (shock) was delayed too long after the warning stimulus (roughness) had been received,

6. Experiments With Y-Shaped Labyrinth: Mechanical Stimulation as Warning Signal

The results treated in Table XI and in the group of figures (19-22, Plate VII) show no indication of learning. It is believed that the large percentage of error might have been lowered in a greater number of tests, but this would probably not account for the type of curve obtained. It was then suggested by Dr. John F. Shepard that while the snails were unable to solve the labyrinth they might nevertheless be able to form a simple association under the conditions of the labyrinth experiments. They might show on the labyrinth a capacity equal to that developed under tests with simultaneous stimuli. A series of tests was therefore carried out, using the Y apparatus with the electric shock punishment but modified as follows: 1. In place of roughness on the shock side a mechanical warning stimulus was used which could be observed to affect the snail. 2. The wrong arm of the labyrinth was shortened so that the shock followed immediately on the warning stimulus. The time relation of the two stimuli was similar to that in Yerkes' experiments (1912), with earthworms and approached that of the method of simultaneous stimuli. The Y apparatus just described was used in a slightly modified form. All traces of the sand and metal were removed. The long arm of the Y was shortened to 3 cm. in order to bring the reward nearer the point of divergence of the paths, while the short arm was reduced to 1.5 cm. in length in order to bring the punishment nearer to the point of the path which necessitated choice on the part of the snail. The snail was placed at the base of the path in the usual way. If at the top of the stem the short arm was chosen, as soon as the turn was made the tentacles and head were stroked with a hair. This was fastened to the end of a small stick so that it could be handled by the operator. This tactile stimulus was immediately followed by the usual electric shock. The fact that the tentacles were withdrawn and that the animal recoiled slightly when touched with the hair, showed conclusively that the tactile stimulus was effective. A series of check tests in which this stimulus alone was used showed that the irritation thus caused was not severe enough to make it serve as a punishment. Six snails were used and the usual method of recording and curve plotting was followed.

TABLE XII

Showing the number of correct-by-chance, correct-by-association, and incorrect trials in a series of tests in which an electric shock was used as punishment in connection with a mechanical warning stimulus. The Y apparatus was used with the arms shortened to 3 cm. and 1.5 cm. In case of error in choice the snail was stroked across the dorsal head region with a hair and then given an electric shock. The apparatus was cleaned between tests. Four individuals of *Physa gyrina* Say were used.

SNAIL NO. 9

No. of days tested	No. of trial for series	No. of trial for day	Choices correct by chance	Choices correct by association	Incorrect choices
1	1	1	0	0	1
	2	2	1	0	0
	3	3	0	0	1
	4	3a	0	0	1
	5	4	0	0	1
	6	5	0	0	1
	7	6	0	0	1
	8	7	1	0	0
	9	8	0	0	1
Totals..................		9	2 (22%)	0	7 (77%)
2	10	1	0	0	1
	11	2	0	1	0
	12	3	1	0	0
	13	4	0	1	0
	14	5	0	1	0
	15	6	1	0	0
Totals..................		6	2 (33%)	3 (50%)	1 (16%)
3	16	1	1	0	0
	17	2	1	0	0
	18	3	0	0	1
	19	3a	0	1	0
	20	4	0	0	1
	21	5	0	0	1
	22	5a	0	0	1
	23	6	1	0	0
Totals..................		8	3 (37%)	1 (12%)	4 (50%)
4	24	1	0	1	0
	25	2	1	0	0
	26	3	0	1	0
	27	4	1	0	0
	28	5	1	0	0
	29	6	1	0	0
	30	7	1	0	0
Totals..................		7	5 (71%)	2 (28%).	0

TABLE XII—(*Continued*)

SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	Choices correct by chance	Choices correct by association	Incorrect choices
5	31	1	0	0	1
	32	2	0	0	1
	33	3	0	0	1
	34	4	1	0	0
	35	5	0	0	1
	36	6	1	0	0
	37	7	0	1	0
Totals..................		7	2 (28%)	1 (14%)	4 (57%)
6	38	1	0	1	0
	39	2	1	0	0
	40	3	1	0	0
	41	4	1	0	0
	42	5	0	0	1
	43	6	1	0	0
	44	7	1	0	0
	45	8	1	0	0
Totals..................		8	6 (75%)	1 (12%)	1 (12 %)
7	46	1	0	0	1
	47	2	0	0	1
	48	3	1	0	0
	49	4	1	0	0
	50	5	1	0	0
	51	6	0	0	1
	52	7	1	0	0
	53	8	0	1	0
Totals..................		8	4 (50%)	1 (12%)	3 (37%)
8	54	1	1	0	0
	55	2	0	0	1
	56	3	1	0	0
	57	4	0	0	1
	58	4a	1	0	0
	59	5	1	0	0
	60	6	0	1	0
	61	7	0	1	0
	62	8	1	0	0
Totals..................		9	5 (55%)	2 (22%)	2 (22%)
9	63	1	0	0	1
	64	1a	1	0	0
	65	2	0	0	1
	66	2a	1	0	0
	67	3	0	0	1
	68	4	0	0	1
	69	4a	0	0	1
	70	4b	0	0	1
Totals..................		8	2 (25%)	0	6 (75%)

TABLE XII—(*Continued*)

SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	Choices correct by chance	Choices correct by association	Incorrect choices
10	71	1	0	0	1
	72	2	0	1	0
	73	3	0	0	1
	74	3a	0	1	0
	75	4	0	1	0
	76	5	0	1	0
	77	6	0	1	0
Totals..................		7	0	5 (71%)	2 (28%)
11	78	1	1	0	0
	79	2	1	0	0
	80	3	0	0	1
	81	4	0	0	1
	82	5	1	0	0
	83	6	0	0	1
	84	6a	0	0	1
Totals..................		7	3 (42%)	0	4 (57%)
12	85	1	1	0	0
	86	2	1	0	0
	87	3	1	0	0
	88	4	1	0	0
	89	5	0	1	0
	90	6	1	0	0
Totals..................		6	5 (83%)	1 (16%)	0
13	91	1	1	0	0
	92	2	0	0	1
	93	3	0	1	0
	94	4	1	0	0
	95	5	1	0	0
	96	6	1	0	0
Totals..................		6	4 (66%)	1 (16%)	1 (16%)
14	97	1	0	0	1
	98	2	1	0	0
	99	3	1	0	0
	100	4	0	1	0
	101	5	1	0	0
	102	6	0	1	0
Totals..................		6	3 (50%)	2 (33%)	1 (16%)
15	103	1	0	1	0
	104	2	1	0	0
	105	3	1	0	0
	106	4	1	0	0
	107	5	0	1	0
	108	6	0	1	0
	109	7	1	0	0
Totals..................		7	4 (57%)	3 (42%)	0

TABLE XII—(*Continued*)

SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	Choices correct by chance	Choices correct by association	Incorrect choices
16	110	1	0	1	0
	111	2	0	1	0
	112	3	1	0	0
	113	4	0	1	0
	114	5	1	0	0
	115	6	1	0	0
Totals..................		6	3 (50%)	3 (50%)	0
17	116	1	0	1	0
	117	2	1	0	0
	118	3	1	0	0
	119	4	0	1	0
	120	5	1	0	0
	121	6	1	0	0
	122	7	1	0	0
	123	8	1	0	0
Totals..................		8	6 (75%)	2 (25%)	0
18	124	1	1	0	0
	125	2	1	0	0
	126	3	0	1	0
	127	4	1	0	0
	128	5	1	0	0
	129	6	1	0	0
	130	7	1	0	0
Totals..................		7	6 (85%)	1 (14%)	0
19	131	1	0	1	0
	132	2	0	1	0
	133	3	1	0	0
	134	4	1	0	0
	135	5	1	0	0
	136	6	1	0	0
Totals..................		6	4 (66%)	2 (33%)	0
20	137	1	0	0	1
	138	1a	0	1	0
	139	2	1	0	0
	140	3	1	0	0
	141	4	0	1	0
Totals..................		5	2 (40%)	2 (40%)	1 (20%)
21	142	1	1	0	0
	143	2	1	0	0
	144	3	0	1	0
	145	4	1	0	0
	146	5	0	0	1
	147	5a	1	0	0
Totals..................		6	4 (66%)	1 (16%)	1 (16%)

TABLE XII—(*Continued*)

SUMMARY—SNAIL NO. 9—(*Continued*)

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
1	9	2	0	7
2	6	2	3	1
3	8	3	1	4
4	7	5	2	0
5	7	2	1	4
6	8	6	1	1
7	8	4	1	3
8	9	5	2	2
9	8	2	0	6
10	7	0	5	2
11	7	3	0	4
12	6	5	1	0
13	6	4	1	1
14	6	3	2	1
15	7	4	3	0
16	6	3	3	0
17	8	6	2	0
18	7	6	1	0
19	6	4	2	0
20	5	2	2	1
21	6	4	1	1

Total number of trials	147
Total number correct by chance	75 approximately 51%
Total number correct by association	34 approximately 23%
Total number incorrect	38 approximately 26%

SUMMARY—SNAIL NO. 10

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
1	7	3	0	4
2	5	5	0	0
3	11	7	0	4
4	10	4	0	6
5	7	3	0	4
6	6	3	0	3
7	8	5	0	3
8	5	3	0	2
9	9	4	2	3
10	10	2	1	7
11	7	4	1	2
12	7	5	1	1
13	6	2	1	3
14	9	4	1	4
15	9	4	2	3
16	8	4	0	4
17	7	5	1	1
18	10	2	0	8

TABLE XII—(*Continued*)

SUMMARY—SNAIL NO. 10—(*Continued*)

No. of days tested	No. of trial for series	No. of trial for day	No. of correct choices	No. of incorrect choices
19	7	5	2	0
20	6	2	4	0
21	6	1	4	1
22	7	3	1	3
23	6	4	1	1

Total number of trials	173
Total number correct by chance	84 approximately 48%
Total number correct by association	22 approximately 13%
Total number incorrect	67 approximately 39%

SUMMARY—SNAIL NO. 13

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
1	5	0	0	5
2	5	3	0	2
3	10	6	0	4
4	5	3	0	2
5	6	3	0	3
6	5	4	0	1
7	10	5	1	4
8	5	4	0	1
9	10	5	1	4
10	7	2	1	4
11	7	3	1	3
12	8	4	0	4
13	8	6	0	2
14	9	5	2	2
15	8	3	4	1
16	6	3	2	1
17	7	3	2	2

Total number of trials	121
Total number correct by chance	62 approximately 51%
Total number correct by association	14 approximately 12%
Total number correct by chance	45 approximately 37%

SUMMARY—SNAIL NO. 19

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
1	9	2	0	7
2	8	6	2	0
3	8	4	3	1
4	6	1	3	2
5	6	3	1	2

TABLE XII — (*Continued*)

SUMMARY—SNAIL NO. 19—(*Continued*)

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
6	5	4	1	0
7	6	2	2	2
8	5	4	1	0
9	7	5	1	1
10	8	3	3	2
11	7	5	1	1
12	7	6	0	1
13	8	4	1	3
14	8	4	3	1
15	6	3	2	1
16	6	5	1	0
17	6	4	2	0
18	6	4	2	0
19	6	2	2	2
20	6	4	1	1
21	7	3	2	2

Total number of trials	141
Total number correct by chance	78 approximately 55%
Total number correct by association	34 approximately 24%
Total number incorrect	29 approximately 21%

SUMMARY—SNAIL NO. 21

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
1	6	5	0	1
2	8	6	0	2
3	8	3	0	5
4	7	2	1	4
5	6	2	0	4
6	8	2	2	4
7	7	5	0	2
8	8	6	1	1
9	8	8	0	0
10	5	1	0	4
11	4	3	0	1
12	5	3	2	0
13	6	3	2	1
14	5	5	0	0
15	6	3	2	1
16	7	6	0	1
17	7	4	0	3
18	7	6	0	1
19	6	3	1	2
20	6	4	0	2
21	5	4	1	0
22	6	4	1	1
23	8	4	0	4

TABLE XII—(*Continued*)

SUMMARY—SNAIL NO. 21—(*Continued*)

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
24	6	2	2	2
25	5	2	2	1
26	8	4	1	3
27	6	3	2	1

Total number of trials	174
Total number correct by chance	103 approximately 59%
Total number correct by association	20 approximately 12%
Total number incorrect	51 approximately 29%

SUMMARY—SNAIL NO. 22

No. of days tested	No. of trials	Choices correct by chance	Choices correct by association	Incorrect choices
1	6	1	0	5
2	8	2	1	5
3	7	4	0	3
4	6	3	1	2
5	8	1	0	7
6	8	5	1	2
7	8	3	1	4
8	12	1	1	10
9	7	2	1	4
10	5	0	0	5
11	5	4	0	1
12	6	4	0	2
13	7	4	0	3
14	5	2	0	3
15	9	3	2	4
16	7	4	0	3
17	9	4	1	4
18	6	5	0	1
19	7	4	1	2
20	7	2	0	5
21	5	2	1	2
22	5	1	4	0
23	7	3	2	2
24	6	3	2	1
25	6	4	1	1
26	6	4	1	1
27	6	4	1	1

Total number of trials	174
Total number correct by chance	79 approximately 45%
Total number correct by association	22 approximately 13%
Total number incorrect	73 approximately 42%

TABLE XII-A

Summary of tests in which a mechanical stimulus was used in connection with electric shock.

	Snail No. 9	Snail No. 10	Snail No. 13	Snail No. 19	Snail No. 21	Snail No. 22
Total number of trials.........	147	173	121	141	174	174
Per cent of choices correct by chance.....................	51	48	51	55	59	45
Per cent of choices correct by association..................	23	13	12	24	12	13
Per cent of incorrect choices..	26	39	37	21	29	42

Table XII shows that in a total of 930 trials on six individuals of Physa, 51.2% were correct by chance, that is, the correct path was chosen without contact with the warning stimulus. In 15.6% of the trials the correct path was chosen after contact with the warning stimulus, but without punishment, showing that a weak association had been formed between the two stimuli. In 32.5% of the trials, the warning stimulus was disregarded and punishment received. This shows that the behavior of the snails tested was modified during the experiments inasmuch as they apparently learned to recognize the warning stimulus and turned back before receiving the electric shock. The snails profited by experience. The slightly downward tendency of the jagged curves based on these records (figs. 23-28, Plate VIII) shows an erratic decrease in error, weak association and limited associative memory.

7. Summary of Observations

1. All observations and experiments were made on the snail *Physa gyrina* Say while in tap water at room temperature and under uniform lighting conditions.

2. When the air is pressed from the lung of Physa and the snail is placed in the water at the base of a U-shaped labyrinth supported on an upright stem, it crawls up the stem and along the arms of the U to the surface of the water. Fig. 14, Plate IV.

3. *Experiments to test right or left preference: first series.*—In a series of 24 trials in which three individuals of Physa were deprived of air and placed on the bottom near the wall of a glass dish filled with water, and allowed to crawl unhindered to the surface of the water, the path chosen deviated from a

perpendicular toward the right in 54.1% of the trials and toward the left in 45.8% of the trials. This showed that the natural path chosen by the three snails tested, in traveling over a flat unrestricted substratum to the surface of the water for air, in a comparatively small number of trials deviated somewhat to the right. Table VII, p. 43.

4. *Experiments to test right and left preference: second series.* When the U apparatus (paragraph 1) is bordered by a " picket fence " of wires, the snail is forced to travel on one side. When it reaches the top of the stem it must turn toward either the right or left end of the horizontal bar of the U to reach the surface of the water. Fig. 14, Plate IV.

5. In a series of 240 trials in which the two arms of the U were identical and both reached the surface of the water, the three snails tested followed the right arm in 51.6% of the trials and the left arm in 48.3%, showing a slight right preference for the snails tested. Table VIII, p. 45.

6. *Experiments to test preference for an ascending or descending path.*—In order to determine whether any preference existed for an ascending or a descending path, the U-shaped labyrinth was so tilted that the bar of the U deviated from a horizontal at angles varying from 0–1 degree to 7–8 degrees. In 600 trials the three snails tested chose the descending arm in 43% of the trials and the ascending arm in 57%, showing a slight preference for an ascending path. Table IX, p. 47.

7. In subsequent experiments in which the U labyrinth was used the horizontal bar was kept level.

8. In the experiments to test learning, complete records are presented for two individuals in each series of tests and summaries of results are given for additional snails tested.

9. *Experiments on learning the U-shaped labyrinth.*—In a series of 888 tests on three individuals of Physa, using the U-shaped labyrinth with the right arm shortened so that a snail could not reach the surface of the water from its upper end, thus making failure to obtain air serve as a punishment incident to the choice of this arm, 58.8% of the turns were toward the right, that is, incorrect, and 41.2% toward the left or correct. There is then shown no evidence of ability in the snail to distinguish right or left in connection with failure to

obtain air (punishment) and the attainment of air (reward). Table X, p. 48.

10. The curves based on these trials show irregularity at the beginning and a slightly upward tendency toward the end, indicating that the possible right preference was covered in the early part of the series by disturbance such as manipulation and the change in apparatus. Figs. 16-18, Plate VI.

11. *Experiments with Y-shaped labyrinth: roughness of the path as a warning stimulus; electric shock as punishment.*—In a series of 664 tests a Y-shaped path of glass tubing, unlimited by wires, was used. One arm of the Y gave access to the surface of the water and the other was shortened and supplied with wires over which an electric current could be sent. The space between the point of divergence of the two arms and the wires was roughened. The four snails tested chose the short arm or punishment in 60.3% of the trials. This series of tests then, shows no indication of learning. Table XI, p. 63.

12. The curves based on these trials show less irregularity than those in the preceding tests (paragraph 10). The latter part lies slightly above the 50% level indicating a possible preference for the rough side, masked by disturbance in the first part of the series, combined with punishment too long delayed after the warning stimulus had been received. Figs. 19-22, Plate VII.

13. *Experiments with Y-shaped labyrinth: mechanical stimulation as warning signal.*—In a series of 930 tests the Y apparatus was used but instead of roughness in connection with the electric shock, the snail was stroked on the tentacles and dorsal head region with a hair, preceding the shock, if a wrong choice were made. Check tests showed that the irritation caused by the warning stimulus was not sufficient to cause it to serve as a punishment. The six snails tested chose the short arm and punishment in 32.5% of the trials, the long arm, by chance in 51.2% and the long arm through association in 15.6%. Modification of behavior through the formation of an association between the two stimuli, one serving as punishment, the other as a warning signal, is thus shown. Table XII, p. 73.

14. The curves based on these trials show irregularity throughout but a downward tendency toward the latter end, indicating

the formation of a weak association between the two stimuli used. Figs. 23-28, Plate VIII.

15. The last series of experiments (paragraph 13) shows results comparable to those obtained in the first part of the present work which indicated an association formed between two dissimilar stimuli, food and pressure, in a series of tests based on the Pawlow salivary method. It does not show ability to learn the labyrinth.

8. DISCUSSION

The foregoing attempt to learn whether the snail can solve a simple vertical U- or Y-shaped labyrinth falls into three series.

1. In the *first* of these there was used a U-shaped labyrinth supported on a stem and bordered by a paling of wires so that the snails could crawl up but one side of it. At the top of the stem they had a choice of a right or left path. If the right path were chosen it led to the right vertical bar of the U from the top of which the snail could not reach the surface of the water. If the left path were chosen it led to the vertical bar of the U, longer than the right, from the top of which the snail could fill its lung with air. The upward course of the snail is in search of air. Choice of the right path resulted in failure to get air which was assumed to act as punishment. Such a choice was regarded as incorrect. Choice of the left path led to the attainment of air, which was regarded as reward and such choice was registered as correct.

Preliminary tests showed that the snails have a slight preference for the right path, a preference possibly related to the asymmetry of the animal. It was to be expected that this right preference would appear at the beginning of the series of learning tests, but that it would be soon corrected by the result of the learning process. If the snails actually learned and a curve were plotted showing their percentage of errors from each successive day's experience, this curve would start somewhere above the 50% level on account of the right preference, which would result in an excess of incorrect choices. The curve would then gradually descend, through learning, toward the zero level with decrease in error present. If the snail did not learn and were in no way affected by its experiences with the labyrinth the curve of errors might be expected to follow the 50% level or somewhat above it, while fluctuating above and below.

An inspection of the graphs (Plate VI) shows that the curves are quite irregular and with the progress of time there is a slight tendency to rise above the 50% level. The snails not only do not learn but their percentage of errors apparently increases as the series of trials lengthens. Nothing has been detected in the physical environment to direct the snail to either side of the path. It is in a symmetrical environment as to structure of labyrinth, surrounding tank and house, lighting and temperature. It seems probable that the disturbance due to manipulation and the change in conditions incident to the experimental work probably caused irregularities in the first part of the series which covered the slight tendency to turn to the error (right) side of the path, evident in the latter part of the curves. The graphs show then, a probable masking of the "right" preference in the early part of the experiments, due to disturbance rather than increase in this preference as the series lengthens. *There is no evidence of learning.*

2. In the *second series* of labyrinth tests the U-shaped labyrinth was discarded and in its place was used a Y-shaped labyrinth of cylindrical glass tubing. Owing to the snail's tendency to crawl in a spiral such a labyrinth offers no choices of right or left and the snail is free to move in any direction in order to reach the top. One arm of this labyrinth was made rough while the other remained smooth. Thus roughness served as a means of discrimination between the two. At the upper end of the rough arm the snail received an electric shock. The roughness served as a warning signal of the approaching shock. No rough-smooth preference tests were made.

The curves (Plate VII) again show much irregularity, but not so much as those with the U-shaped labyrinth. There are again days on which all choices are correct and other days on which all are incorrect. Nearly all of these days, however, fall in the first third of the curves, with the result that this part is much more irregular than that which follows. After their first thirds the curves remain nearer the 50% level, but on the whole tend to lie above it. In other words, the percentage of error again appears to increase as the series lengthens. It seems possible that a greater number of trials might have brought the curves again to the 50% line or even slightly below it, indicating that the upward tendency was due to a wave of error and that a corresponding wave showing fewer errors might follow. It is more probable however, that the greater irregularity evident in the first thirds of the curves was due in part to disturbance following the change of apparatus and that the slight increase in error toward the end was the result of a possible rough preference linked with a too long delayed punishment.

3. The *third series* of labyrinth tests was planned to test the capacity of the snail to form associations on the labyrinth that it had failed to solve. The Y labyrinth was used, but all traces of roughness were removed. The short arm was shortened still more making the interval between the warning stimulus and the punishment less than in the preceding tests. The warning stimulus adopted was the irritation caused by stroking the dorsal head region and tentacles of the snail with a hair. That this stimulus was an irritation was evidenced by the fact that its use caused the snail to withdraw its tentacles, and at times to partially retract its head toward the edge of its shell. Check tests, however, showed that the irritation was not severe enough in itself to act as a punishment. The labyrinth as before offered no choice of right or left.

The curves (Plate VIII) of error percentage show much irregularity but a slight downward tendency. In 15.6% of the total of 930 trials, the snails changed their course from the wrong to the right path after contact with the warning stimulus but before the shock (punishment) was received. Since the warning stimulus was known to irritate the animal, there could have been no preference for the wrong side of the labyrinth to offset the associative effect of the shock (punishment). The 15% of correct-by-association trials, then, represents the strength of the association formed between the warning stimulus and the punishment, no part of which could have been masked through preference for the signal stimulus used. This series of tests thus shows the formation of a weak association between two stimuli, one used as a warning of the punishment to follow if the course be not changed. Physa then, profits by experience, but cannot be said to solve a labyrinth. The work with the modified apparatus is merely confirmatory of that by the method of simultaneous stimuli. It shows no greater capacity on the part of the snail than was made evident by that method except that to form an association between stimuli separated by a small time interval.

Observational work on mollusks has been reported, but little experimental work has been done on this group of animals, none along the lines of the present work. Kollmann (1877) attributed "consciousness" to *Octopus vulgaris*, basing his conclusions, however, on observation rather than experiment. In his analysis of this report, Schneider (1909) explains the behavior observed as reflexive, not "conscious," and hence typical of any of the lower animals. Piéron (1909) concludes that the octopus is capable of "learning," and although Kafka (1914) reports this work he does not analyze it. Modifiability of behavior then, may be attributed to members of the Cephalopod group. Piéron (1909), (1911) experimented on the waning of the effect of stimuli inducing reflexive action in *Limnaea stagnalis* and Littorina, which he terms memory. Associative memory as observed in the present work was believed to be an added proof of learning, but no attempt was made to determine its exact duration nor the way in which it developed. The work of Piéron is here of interest only in that it deals with so-called memory (adaptation) in closely related forms.

III. CONCLUSIONS

1. The present work on the snail, *Physa gyrina* Say, gives much evidence of capacity for adaptation—the getting-used-to-the-stimulus. This is quite apart from evidence for fatigue which appears in some places.

2. In addition to adaptation its behavior is modifiable by experience in another way. As the result of prolonged training with two simultaneous stimuli it is found to have acquired a response to one of these stimuli which it did not give before. It has formed an association between the two stimuli.

3. This definite acquired response is retained for four days beyond the completion of the training (memory).

4. When put through a long series of tests with the simplest form of labyrinth, under controlled conditions it shows no capacity to solve the labyrinth.

5. On a similar labyrinth it gives evidence of the capacity to form associations.

6. The capacity to form associations does not then suffice for the solution of the simplest labyrinth, that is, "selective" ability is apparently lacking.

IV. LITERATURE CITED

1. Dawson, J., 1911. The biology of Physa. *Behavior Monogr.*, I, 4.
2. Fielde, A. M., 1901. A study of an ant. *Proc. Philadelphia Acad. Nat. Sci.*, vol. 53, p. 425.
3. ——— 1901. Further study of an ant. *Ibid.*, vol. 53, p. 521.
4. Jennings, H. S., 1906. Behavior of the lower organisms. New York.
5. Kollmann, J., 1877. Aus dem Leben der Kephalopoden. *Vierteljahrssch. wiss. Philos.*, Bd. I, S. 136.
6. Kafka, G., 1914. Einführung in die Tierpsychologie. Leipzig.
7. Morgulis, S., 1914. Pawlow's theory of the function of the central nervous system and a digest of some of the more recent contributions to this subject from Pawlow's laboratory. *Jour. of An. Behav.*, vol. 4, no. 5, p. 362.
8. ——— 1914. The auditory reactions of the dog studied by the Pawlow method. *Ibid.*, vol. 4, no. 2, p. 142.
9. Nagel, W. A., 1894. Beobachtungen über den Lichtsinn augenloser Muscheln. *Biol. Cent.*, Bd. 14, S. 385.
10. Nicolai, G. F., 1907. Die physiologische Methodik zur Efforschung der Tierpsyche. *Jour. f. Psych. u. Neur.*, Bd. 10, S. 1.
11. ———, 1908. Das Lernen der Tiere (auf Grund von Versuchen mit Pawlowscher Spiechelfistel). *Cent. f. Physiol.*, Bd. 22, S. 362.
12. Oberli, L. A., 1909. Reflexes conditionnels du côté de l'oeil chez le chien. *Arch. de Sci. Biol.*. t. XIV, p. 31.
13. Parker, G. H., 1912. The mechanism of locomotion in gastropods. *Jour. of Morph.*, vol. 22, p. 155.
14. Pawlow, I. P., 1904. Sur la secretion psychique des glandes salivaires. *Arch. intern. de Physiol.*, t. I, p. 119
15. ———, 1906. The scientific investigation of the psychical faculties or processes in the higher animals. (Huxley Lecture for 1906.)
    Brit. Med. Jour., 1906, p. 871 (Pub. in part).
    Lancet, 1906, p. 911 (Pub. in full).
    Science, *N. S.*, vol. 24, p. 613 (Pub. in part).
16. Piéron, H., 1909. Sens de l'Orientation et memoire topographique de la limnée. *Compt. rend. Acad. Sci.* Paris, 148, p. 530.
17. ———, 1909. La loi d'evanouissement des traces mnemoniques en fonction du temps chez la limnée. *Ibid.*, 149, p. 513.
18. ———, 1911. Les curbes d'evanouissement des traces mnemoniques. *Ibid.*, 152, p. 1115.
19. ———, 1911. Sur la determination de la periode d'établissement dans les acquisitions mnemoniques. *Ibid.*, 152, p. 1410.
20. Schneider, K. C., 1909. Vorlesungen über Tierpsychologie. Leipzig.
21. Shepard, J. F. Unpublished, referred to by permission.
22. Small, W. S., 1899. An experimental study of the mental processes of the rat, I. *Am. Jour. of Psych.*, vol. 11, p. 80.
23. Spaulding, E. G., 1904. An establishment of association in hermit crabs, *Eupagurus longicarpus*. *Jour. of Comp. Neur. and Psych.*, vol. 14, p. 49.
24. Washburn, M. F., 1908. The animal mind. New York.
25. Watson, J. B., 1914. Behavior. An introduction to comparative psychology. New York.
26. Yerkes, R. M., 1902. Habit formation in the green crab, *Carcinus granulatus*. *Biol. Bull.*, vol. 3, p. 241.
27. ———, 1912. The intelligence of the earthworm. *Jour. of An. Behav.* vol. 2, p. 332.
28. Yerkes, R. M., and Huggins, G. E., 1903. Habit formation in the crawfish, *Cambarus affinis*. *Harvard Psych. Studies*, vol. 1, p. 565.
29. Yerkes, R. M., and Morgulis, S., 1909. The method of Pawlow in animal psychology. *Psych. Bull.*, vol. 6, no. 8, p. 257.

LIST OF ILLUSTRATIONS

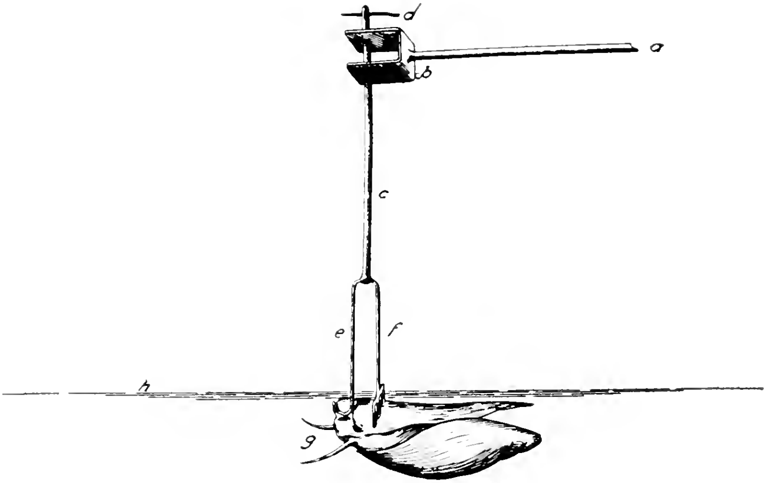
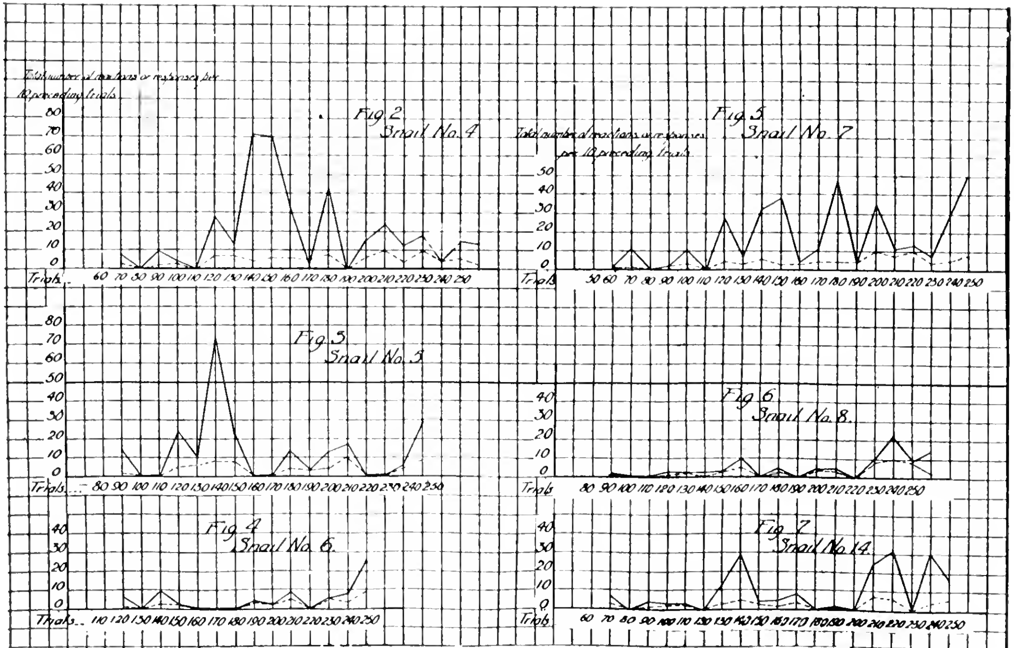
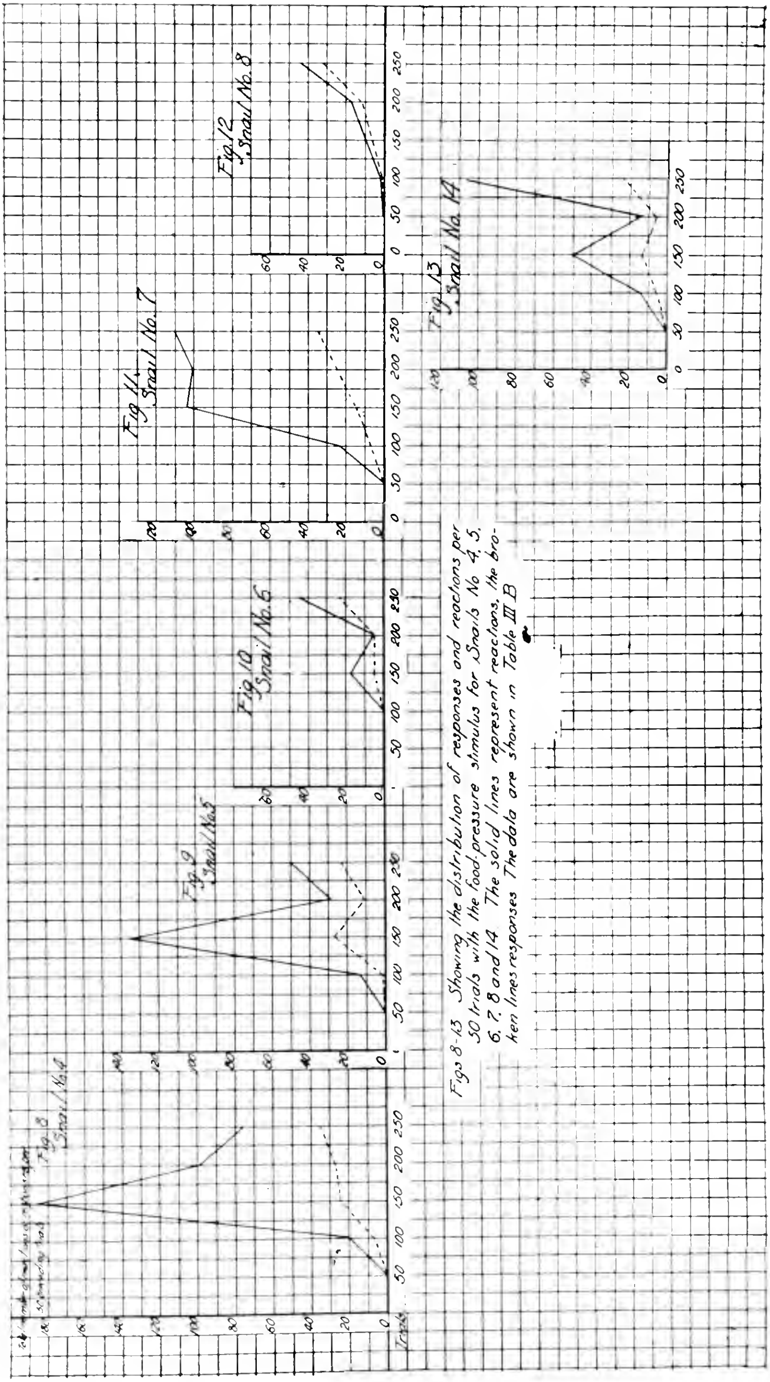


Fig. 1.

Apparatus used in experiments which required the simultaneous application to the snail of food and pressure and pressure alone. a, handle; b, sheet of metal; c, steel needle; d, wire through eye of needle; e, food hook; f, pressure fork; g, snail; h, surface of water.



Figs. 2-7 Showing the distribution of responses and reactions per 10 trials with the food-pressure stimulus for Snails No 4, 5, 6, 7, 8, and 14. The solid lines represent reactions, the broken lines responses. The data are shown in Table III A.



Figs 8-13 Showing the distribution of responses and reactions per 50 trials with the food-pressure stimulus for Snails No 4, 5, 6, 7, 8 and 14. The solid lines represent reactions, the broken lines responses. The data are shown in Table III B

Plate IV

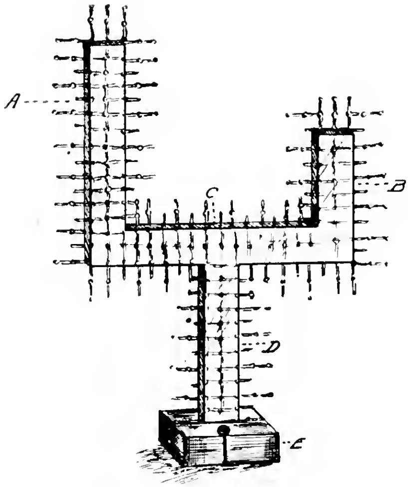


Fig. 14. – U shaped labyrinth with its picket fence of wires. A, long arm of the U giving access to the surface of the water; B, short arm of the U from the upper end of which air could not be obtained; C, horizontal bar of the U; D, upright stem upon which the U rested; E, lead base which anchored the labyrinth to the bottom of the tank.

PLATE V

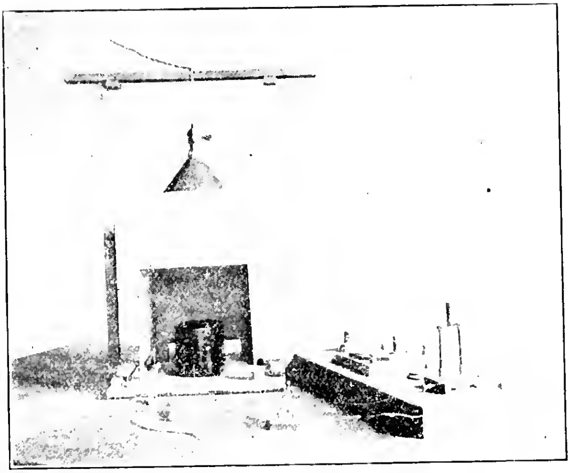


Fig. 15

Showing the apparatus used in an attempt to control light conditions, experimental tank, Y shaped labyrinth used in later experiments, and rheostat used in controlling the electric current used in administering the shock.

Fig 16-18 Distribution of error for Snails No 1, 2 and 3 in series of 282, 307 and 299 right-left discrimination tests.

PLATE VII

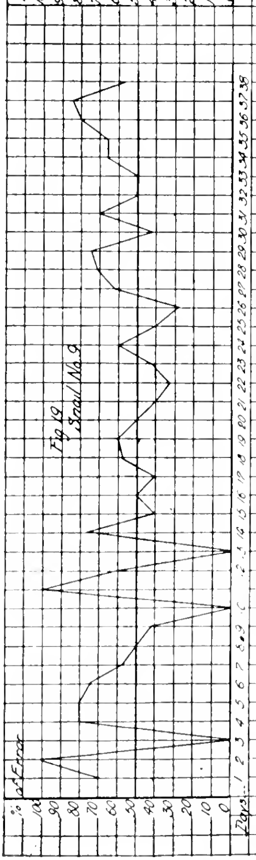


Fig 19 Showing the distribution of error in the rough-shock-smooth tests for Snail No 9, in a total of 215 trials extending over a period of 38 days

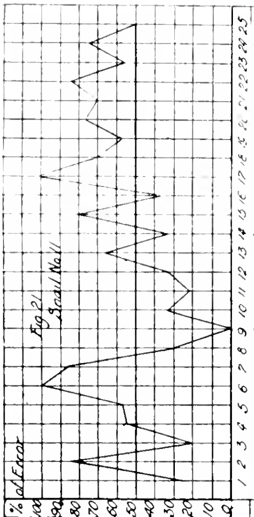


Fig 21 Showing the distribution of error in the rough shock-smooth tests for Snail No. 11, in a total of 178 trials extending over a period of 25 days

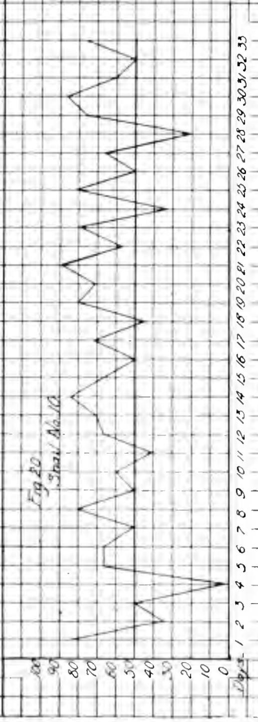


Fig 20. Showing the distribution of error in the rough-shock-smooth tests for Snail No 10, in a total of 214 trials, extending over a period of 33 days

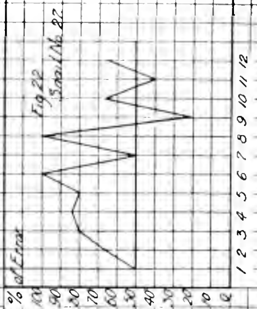


Fig 22 Showing the distribution of error in the rough-shock-smooth tests for Snail No 27, in a total of 57 trials extending over a period of 12 days

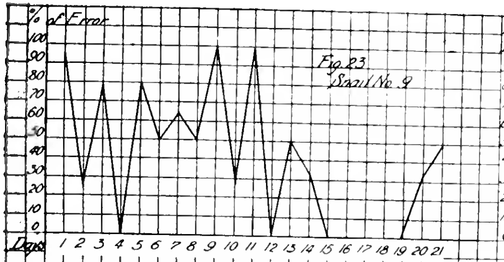


Fig 23. Showing distribution of error for Snail No 9 in a series of 147 tests, extending over a period of 21 days, in which a mechanical stimulus was used in connection with the electric shock

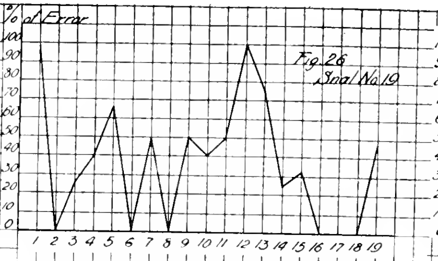


Fig 26. Showing the distribution of error for Snail No 19, in a series of 141 tests, extending over a period of 19 days, in which a mechanical stimulus was used in connection with the electric shock

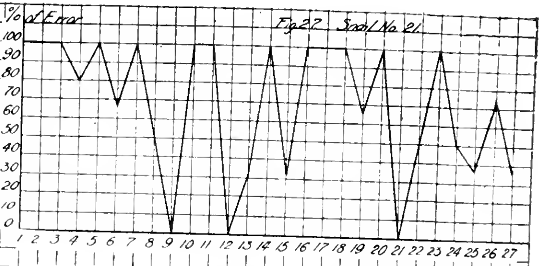


Fig 27 Showing the distribution of error for Snail No 21, in a series of 174 tests, extending over a period of 27 days, in which a mechanical stimulus was used in connection with the electric shock

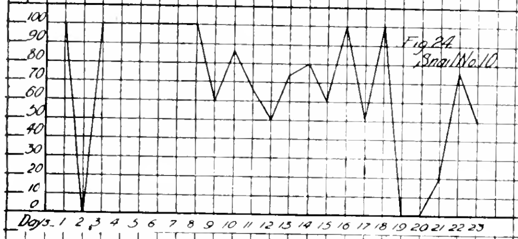


Fig 24 Showing the distribution of error for Snail No 10, in a series of 173 tests, extending over a period of 23 days in which a mechanical stimulus was used in connection with the electric shock

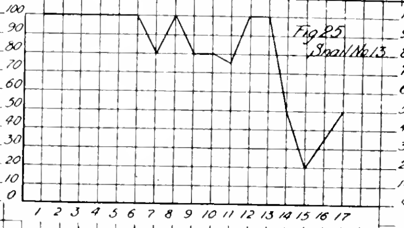


Fig 25 Showing the distribution of error for Snail No 13 in a series of 121 trials, extending over a period of 17 days in which a mechanical stimulus was used in connection with the electric shock

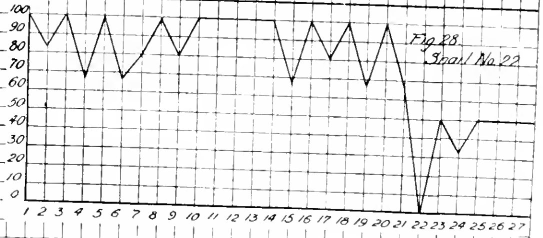


Fig 28. Showing the distribution of error for Snail No 22, in a series of 184 tests, extending over a period of 27 days, in which a mechanical stimulus was used in connection with the electric shock

The Behavior Monographs

Edited by JOHN B. WATSON

The Johns Hopkins University, Baltimore, Md.

VOLUME 1

No. 1 The development of certain instincts and habits in chicks. By Frederick S. Breed. Pp. iv + 78, $1.00, postpaid.

No. 2 Methods of studying vision in animals. By Robert M. Yerkes and John B. Watson. Pp. iv + 90, $1.25, postpaid.

No. 3 An experimental study on the death-feigning of Belostoma (-Zaitha Aucct.) flumineum Say and Nepa apiculata Uhler. By Henry H. P. Severin and Harry C. Severin. Pp. iii + 47, $.65, postpaid.

No. 4 The biology of Physa. By Jean Dawson. Pp. iii + 120, $1.50, postpaid.

No. 5 The function of the vibrissae in the behavior of the white rat. By Stella Burnham Vincent. Pp. iv + 81, $1.15, postpaid.

VOLUME 2

No. 6 The delayed reaction in animals and children. By Walter S. Hunter. Pp. v + 86, $1.15, postpaid.

No. 7 The Canada porcupine: a study of the learning process. By Leroy Walter Sackett. Pp. iii + 84, $1.15, postpaid.

No. 8 Audition and habit formation in the dog. By Harry Miles Johnson. Pp. iv + 78, $1.00, postpaid.

No. 9 Habit formation in a strain of albino rats of less than normal brain weight. By Gardner Cheney Basset. Pp. iv + 46, $.65, postpaid.

No. 10 Distribution of effort in learning in the white rat. By John Linck Ulrich. Pp. iii + 51, $.65, postpaid.

No. 11 The effect of age on habit formation in the albino rat. By Helen B. Hubbert. Pp. v + 55, $.65, postpaid.

VOLUME 3

No. 12 The mental life of monkeys and apes: a study of ideational behavior. By Robert M. Yerkes. Pp. iv + 145, $1.50, postpaid.

No. 13 A study of perseverance reactions in primates and rodents. By G. V. Hamilton. Pp. iv + 65. $.75, postpaid.

No. 14 An analysis of the learning process in the snail, *physa gyrina* Say. By Elizabeth Lockwood Thompson Pp. iii + 97, $1.25, postpaid.

No. 15 The effect of length of blind alleys on maze learning: An experiment on twenty-four white rats. By Joseph Peterson. Pp. iii + 53, $.75, postpaid.

Regular subscription $5 per volume, postpaid.

THE BEHAVIOR MONOGRAPHS

Emerson Hall, Cambridge, Mass.

Directory of American Psychological Periodicals

American Journal of Psychology—Worcester, Mass.: Florence Chandler.
Subscription $5. 600 pages annually. Edited by G. Stanley Hall.
Quarterly. General and experimental psychology. Founded 1887.

Pedagogical Seminary—Worcester, Mass.: Florence Chandler.
Subscription $5. 575 pages annually. Edited by G. Stanley Hall.
Quarterly. Pedagogy and educational psychology. Founded 1891.

Psychological Review—Princeton, N. J.: Psychological Review Company.
Subscription (with Psychological Bulletin) $5. 480 pages annually.
Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin—Princeton, N. J.: Psychological Review Company.
Subscription $2.75. 480 pages annually. Psychological literature.
Monthly. Founded 1904. Edited by Shepherd I. Franz.

Psychological Monographs—Princeton. N. J.: Psychological Review Company.
Subscription $4. 500 pages per vol. Founded 1895. Ed. by James R. Angell.
Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.: Psychological Review Company.
Subscription $1. 200 pages. Founded 1895. Edited by Madison Bentley.
An annual bibliography of psychological literature.

Journal of Philosophy, Psychology and Scientific Methods—New York:
Sub-Station 84. Bi-weekly. 728 pages per volume. Founded 1904.
Subscription $3. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Substation 84, New York: Archives of Psychology.
Subscription $5. 600 pp. ann. Founded 1906. Edited by R. S. Woodworth.
Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology—Boston: Richard G. Badger.
Subscription $4. 480 pages annually. Edited by Morton Prince.
Bi-monthly. Founded 1906. Entire field of abnormal psychology.

Psychological Clinic—Philadelphia: Psychological Clinic Press.
Subscription $1.50. 280 pages annually. Edited by Lightner Witmer.
Monthly (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Training School Bulletin—Vineland, N. J.: The Training School.
Subscription $1. 160 pages annually. Ed. by E. R. Johnstone. Founded 1904.
Monthly (10 numbers). Psychology and training of defectives.

Journal of Religious Psychology—Worcester, Mass.: Louis N. Wilson.
Subscription $3. 480 pages annually. Founded 1904. Edited by G. Stanley Hall.
Published without fixed dates. 4 numbers constitute a volume.

Journal of Race Development—Worcester, Mass.: Louis N. Wilson.
Subscription $3. 460 pages annually. Founded 1910.
Quarterly. Edited by George H. Blakeslee and G. Stanley Hall.

Journal of Educational Psychology—Baltimore: Warwick & York.
Subscription $2.50. 600 pages annually. Founded 1910.
Monthly (10 numbers). Managing Editor, J. Carleton Bell.
(Educational Psychology Monographs. Edited by Guy M. Whipple.
Published separately at varying prices. Same publishers.)

Journal of Animal Behavior—Cambridge, Mass.: Emerson Hall.
Subscription $5. 450 pages annually. Founded 1911.
Bi-monthly. Robert M. Yerkes, Managing Editor.

The Behavior Monographs—Cambridge, Mass.: Emerson Hall.
Subscription $5. 450 pages per volume. Edited by John B. Watson.
Published without fixed dates, each number a single research.

Psychoanalytic Review—New York: 64 West 56th Street.
Subscription $5. 500 pages annually. Psychoanalysis.
Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J.: Psychological Review
Company. 480 pages annually. Experimental. Founded 1916.
Subscription $3. Bi-monthly. Edited by John B. Watson.

Journal of Applied Psychology—Worcester, Mass.: Florence Chandler.
Subscription $4. 400 pages annually. Quarterly. Founded 1917.
Edited by G. Stanley Hall, John Wallace Baird and L. R. Geissler.

THE BRANDOW PRINTING COMPANY
ALBANY, N. Y

BEHAVIOR MONOGRAPHS

Volume 3, Number 4, 1917 Serial Number 15

Edited by JOHN B. WATSON
The Johns Hopkins University

The Effect of Length of Blind Alleys on Maze Learning:

An Experiment on Twenty-Four White Rats

BY

JOSEPH PETERSON

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

The Journal of Animal Behavior

An organ for the publication of studies concerning the instincts, habits and intelligence of organisms

The Journal contains a Department of Notes in which appear brief accounts of especially interesting and valuable observations of behavior.

Published bi-monthly at Cambridge, Boston, Mass., by Henry Holt and Company, New York. Each volume contains at least 450 pages with plates and text-figures.

The subscription price is $5.00 per volume (foreign subscription $5.50) postpaid.

Manuscripts, subscriptions, and all business correspondence should be addressed to

THE JOURNAL OF ANIMAL BEHAVIOR
Emerson Hall, Cambridge, Massachusetts

Behavior Monographs

For the publication of studies in behavior and intelligence which are too lengthy or too expensive for acceptance by The Journal of Animal Behavior

Published at irregular intervals at Cambridge, Boston, Mass., in connection with the Journal of Animal Behavior, by Henry Holt and Company, New York. Each volume contains approximately 450 pages with plates and text-figures.

The subscription price is $3.00 per volume (foreign subscription $3.50) postpaid.

Monographs may be purchased separately at prices varying with the cost of manufacture.

Manuscripts and inquiries concerning terms of publication should be addressed to the Editor of the Behavior Monographs,

JOHN B. WATSON,
The Johns Hopkins University,
Baltimore, Md.

Subscription to The Journal of Animal Behavior and the Behavior Monographs should be sent to Emerson Hall, Cambridge, Massachusetts.

BEHAVIOR MONOGRAPHS

Volume 3, Number 4, 1917 Serial Number 15

Edited by JOHN B. WATSON
The Johns Hopkins University

The Effect of Length of Blind Alleys on Maze Learning:

An Experiment on Twenty-Four White Rats

JOSEPH PETERSON
Assistant Professor of Psychology, University of Minnesota

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

THE EFFECT OF LENGTH OF BLIND ALLEYS ON MAZE LEARNING: AN EXPERIMENT ON TWENTY-FOUR WHITE RATS

THE GENERAL PROBLEM

"*How far are pleasurable results able to burn in and render predominant the association which led to them?* This is perhaps the greatest problem of both human and animal psychology." So wrote Thorndike in 1898. The problem is not yet solved. The problem arises from the fact, clearly pointed out by Thorndike, that "the connection thus stamped in is *not contemporaneous* [*with*], *but prior to* the pleasure."[1] "*There is no pleasure along with the association. The pleasure does not come until after the association is done and gone.*"[2] This problem, though raised by Lloyd Morgan[3] in connection with experiments on learning by the "trial and error" method, has received very little but theoretical attention from psychologists to the present time.[4] Its importance for the education process, including the informal moral development by general social conditions, is certainly such as not to be overlooked. The dearth of experimentation

---

[1] Thorndike, E. L. *Psychol. Mon.*, Ser. No. **8**, p. 103. After nineteen years of extensive work on certain phases of learning Professor J. B. Watson, who has himself taken a considerable part in this experimental work, says practically the same thing. In a review of Holt's *The Freudian Wish and Its Place in Ethics*, in which he considers a few artificial and inadequate illustrations of learning with but slight attention by the author to the neural processes involved, Watson says: "In these few experiences a genuine *learning process* is involved and the explanation of this learning process—regardless of whether the act is acquired in few or many trials—is what I consider one of the chief problems in psychology." *Jour. of Phil. Psychol.*, etc., 1917, **16**,, p. 89.

[2] *Ibid.*, p. 104.

[3] *Introduction to Comparative Psychology*, 1894, Ch. 12. *E.g.*, on page 213 Morgan says: "The successful response is repeated because of the satisfaction it gives; the unsuccessful response fails to give satisfaction, and is not repeated."

[4] See, *e.g.*, Smith, S. Limits of Educability in Paramecium. *Jour. Comp. Neurol. and Psychol.*, 1908, **18**, 499-510. Meyer, Max, *The Fundamental Laws of Human Behavior*, 1911. Thorndike, E. L. *Animal Intelligence*, 1911, Ch. 4. Haggerty, M. E. The Laws of Learning. *Psychol. Rev.*, 1913, **20**, 411-422. Carr, Harvey A. Principles of Selection in Animal Learning. *Ibid.*, 1914, **21**, 157-165. Watson, J. B. *Behavior*, 1914, Ch. 7. Peterson, Jos. Completeness of Response as an Explanation Principle in Learning. *Psychol. Rev.*, 1916, **23**, 153-162.

on the problem is likely due to the mind-body relations implied in the early form of its statement.

It is desirable to rescue any problem as to how learning goes on from mere theoretical discussions. Professor Watson has already attempted this for the problem in question, though not yet with marked success.[5] Two groups of rats were allowed to solve individually a certain problem; in the one group each animal was fed immediately after the successful movements that brought it into the food box, while in the other group each rat was not allowed to take food for thirty seconds after entrance to the food box. No difference in the learning of the two groups was found. The experiment is regarded as only preliminary to a further study of the matter. Two criticisms may be offered on this experiment. In the first place, it is not on a wholly objective basis. As reported the experiment did not seem to be free from the assumption that the question at issue is whether the *pleasure of the eating* had a " stamping-in " effect, to use a term of Thorndike's, on the processes leading up to the eating. " Successful movements " seem to be regarded as movements bringing about this pleasure. If, in the second place, this is not the true meaning of the author, it may be suggested that the test of the effect of " successful movements " is not adequate, since precisely the same kind of acts was necessary for both groups of animals to get out of the situation presented by the problem. Experience with rats will certainly suggest that after an animal has once been fed in the food box it will for a time work energetically and learn to run the maze without further feeding of the kind, particularly if the odor is not carefully excluded. As the habit becomes partly fixed it is questionable whether the feeding, or even the smell of food, has very much to do with the energy that the animal displays. So far as the writer's own experience goes—though he has made no definite test of the matter—it appears that once the habit is well under way the animal will display great energy in the usual way as soon as placed into the entrance box; that the habit will unwind itself on the basis of the numerous other stimuli which have accompanied the process before. However, any criticism of Watson's experiment on the basis of his report of the preliminary

---

[5] An abstract of the experiment, which was reported in the Chicago convention of the American Psychological Association, is printed in *Psychol. Bull.*, 1916, **13**, p. 77.

procedure is unfair, and our only purpose here is to point out that there are real difficulties involved such as may give trouble to even an experienced behaviorist.

The problem must be conceived in some other way, and in terms of *stimulus and response* as Watson has rightly insisted.[6] Elsewhere the writer has attempted a statement of the general problem in a form more acceptable for experimentation.[7] The general thought in mind, whatever the degree of success of its statement may have been, was this: Response is never, in the case of *learning*, at least, a reaction to a *single stimulus.* The afferent impulse never begins at a given receptor as the result of stimulus by a single object and thence passes into motor channels from only one particular afferent fiber. The situation in all learning is vastly more complex. A complication of external stimuli is nearly always to be reckoned with; then again, the afferent impulses from these stimuli are greatly determined in their relative effects on response by impulses from the proprio- and the entero-ceptive systems; and, in addition, the responses resulting are to a large extent determined by the general conformation of the organism. Different forms of animals have different action systems, for example. The pleasantness or unpleasantness of an act is only an inner indication as to whether the response, forced by the complex inner organization (inherited and acquired) and the outer circumstances, or stimuli, is or is not in general harmony with the conformation of the organism. The question of explanation may resolve itself, then, wholly into one of the physical and physiological circumstances. It was then suggested that all such factors as recency, frequency, and intensity of stimuli, which may be conceived as involving only a single tract, are in themselves inadequate to account for learning. Indeed, they may serve in all cases outside of mere chance associative connections only as secondary aids to learning. In the usual cases certain stimuli and their immediate effects continue for a time and operate synchronously with others so that the response is a resultant of these various circumstances. It may tentatively tend this way and that, but will complete itself in the way that is on the whole most consistent, when everything is taken into

---

[6] *Behavior*, p. 257.

[7] Peterson, Jos. Completeness of Response as an Explanation Principle in Learning. *Psychol. Rev.*, 1916, **23**, pp. 153-162.

consideration. The most complete response possible, in this sense,—the most consistent—has the advantage and will, other things equal, survive over others. The various tentative beginnings of acts this way and that, moreover, are not to be regarded as separate *acts*: they may easily, at a later juncture, be resolved into the "completest" act. Such conditions, it was maintained, must be taken into consideration to account for the selectiveness manifest in learning. This is a complex "principle" both to state and to test out in experiment; but the organism and the behavior of an animal are inconceivably complex, and over-simplification for the sake of clearness of conception and of explanation is often a positive disadvantage to progress in the biological sciences. Numerous evidences of this statement might be given.

The experiment reported in the following pages was planned in its main features when the article above referred to was written, and it is there suggested in the concluding paragraph. It was thought that varying the lengths of certain *cul de sacs* in identical mazes might show a difference in behavior not explicable on the basis of frequency, recency, and intensity of stimulation. If, for instance, a tendency to enter a short *cul de sac* is overcome with fewer errors in that particular case, or in fewer runs through the maze, than are required when the same *cul de sac* is lengthened somewhat, it would appear that some other explanation than that based on the principles named is necessary. On the basis of frequency and recency the animal would stand the same chance, on emerging from the blind alley, either of turning back toward the entrance of the maze, on the one hand, or of going toward the food box, on the other, that it would with the blind alley longer. This would certainly be true if acts are the individual and disparate affairs in trial and error processes that they are usually assumed to be, each being complete as a rule before the next is begun.

Watson says: "This factor (frequency) alone is probably sufficient to account for the maze habit. Apparently it is difficult to obtain any explanation based upon other factors."[5]

---

[5] *Op. cit.*, pp. 267, 268. In a footnote he says: "If it happens by chance that any *cul de sac* is entered as frequently as any segment of the true pathway, it becomes as firmly fixed as the true segment." I cannot understand what the warrant is for this statement. A careful tabulation of the detailed movements of some of my rats in the maze shows that it is altogether contrary to the actual facts. In records

After pointing out, successfully to the writer's mind, the difficulty in the way of Thorndike's principle of "satisfiers," he contends that there is no immediate connection backwards between the obtaining of food and the elimination of errors. Watson attempts on the basis of the probability doctrine, suggested in another relation by Stevenson Smith, to show how frequency alone may suffice in the acquiring of maze habits. He argues that an animal, having started along the maze path A, has an equal chance on coming to a *cul se sac* X, all other factors equal, either of taking B, the true path beyond the blind alley, or of going into X; that on returning from X, in case of the wrong choice having been made, it again has an equal chance of taking B. It thus has a probability of 3/4 (or 1/2 + 1/2 of 1/2) of keeping the right path.

If no other factor than frequency operates in such a case we should expect an animal to continue entering the *cul de sacs* indefinitely; for on turning back from any point toward the starting place in the maze the same law must apply. The chances are again 1/2 that any *cul de sac* passed will be entered, and 3/4 that the animal will continue in its general direction, now toward the starting point in the maze. In a maze with several blind alleys, each of which has a chance of 1/4 of turning any rat reaching it back toward the maze entrance, the probability would be very slim that the animal would at the first trial reach the food. The returns would therefore tend to fix the habit of entering *cul de sacs* as strongly as that of going toward the food. Mere probability explains truly enough how the animal gets to the food each time, but *that is not the problem of learning*; it does *not* explain how it happens that on the whole the second trial is better than the first, the third better than the second, and so on. Frequency based on probability does not bring such a result: *it fails utterly to explain learning*, even in the simple case of the maze.[9] The real issue has been over-

---

picked at random, instances occur in numerous places of violations of the principle stated. A detailed presentation of these instances will be reserved for a later article, as proper attention to them here would lead us too far away from the main purpose of the present paper. Instances are very frequent when the animal takes certain blind alleys entirely contrary to the expectations based upon either frequency or recency or of both combined.

[9] This statement is based on actual data of a supposed case of a rat in a maze of six *cul de sacs* whose "choice" at each bifurcation of the trail is determined by the flipping of a coin. After considerable data by this method has accumulated—after most any number of trials—it becomes very evident that if the frequency

looked as a rule. Watson does not try out his suggestion, or follow it far enough to get to the real difficulty. It is not easy, as Watson rightly admits,[10] to see how the recency principle can help out the situation. No one has given more on this than the mere name. Both recency and frequency fail to explain learning as a gradual change in the way of doing something, involving the elimination of random acts. They do not show what controls an act, but only that *if it is controlled, or directed, alike each successive trial it will become easier and more rapid in performamce.*

On the other hand, if different "acts" in a random trial and error process are only more or less tentative expressions of the one general act of getting food, for example, comparable to the out-reachings of the pseudopodia of the ameba, and if in all their changing forms these are related to the main performance by numerous in-going and out-going impulses, it would seem reasonable to suppose that errors of entering blind alleys would be overcome, other things equal, in something like a direct proportion to the length of the latter. This might be expected to hold within certain limits of length, at least. It is not at all implied in this view of learning, let it be clearly kept in mind, that any conscious states, whether or not they are present, are controlling or directing the animal. Indeed, it is just this view that we regard as unfruitful, and for which we are seeking a successful substitute. Instead of covering up the problem by assuming that the animal "perceives relations," or makes "practical judgments," or "has ideas," we are attempting to meet it squarely and to state schematically *how* the complexity of stimuli in the situation favoring learning can function so that the animal may "learn by results." There can be little question in fact that somehow the animal *does* learn by results. Our problem is to understand how and by what kind of results. Its solution would seem to have valuable bearings in the way of substituting for current erroneous "social forces" factors (including "pleasure and pain") used in explaining human conduct, in the absence of better conceptions.

(or the recency, or both) of running through any unit of the maze be the determining factor in subsequent choices the rat never would learn the maze. As the previous note states a similar tabulation of actual choices by an animal likewise shows the inadequacy of the principles in question.

[10] *Op. cit.*, p. 269. The writer is working, however, with encouraging prospects upon a method of testing the influence of recency, and he is finding that influence much less potent than he had supposed.

THE EXPERIMENT

The experiment was carried out in the University of Chicago during the months of July and August, 1916.[11] Twenty-four white rats, ranging in ages from about five to six weeks at the time of the beginning of the experiment, were used. Of these, nine were males and fifteen females. These were at first divided into two main groups, the one consisting of the fifteen females and one small male[12] and the other of eight males. The first group began as untrained animals in the B-mazes, to be described, and the second in the A-mazes. They were ear-marked and grouped about eleven days before the experiment began, during which time they were habituated to handling, and were fed daily in the food box of the maze (in the separate groups) except a couple of days while the maze was out of the laboratory for remodeling. The food was bread soaked in milk, a definite quantity being given each day to insure uniformity of bodily conditions and of hunger. During the entire period of preparation and experimentation not a single rat showed any signs of illness. The two main groups were again divided into control groups, as will be explained later in "The General Schedule of Experiments." These sub-groups were caged separately for convenience of experimentation, but they were fed together daily in the food box of the maze throughout the time of the experiment and were also interchanged daily in the cages, *i. e.*, each sub-group was on any given day put into the cage occupied by its control group during the previous twenty-four hours. The purpose of these interchanges was to prevent the development of group odors.

Only one maze in the laboratory was available. This was,

---

[11] I desire to express my thanks here to Professors Angell and Carr for the privileges of the laboratory and for the animals used in the experiment. With the exception of aid from my brother, John C. Peterson, a graduate student in the University of Chicago, I am wholly responsible for the experiment, both as to problem and method. My brother helped me plan the modifications of the maze available, and to get started with the experiment, which help I gratefully acknowledge. We had planned to carry on the experiment together, but it was found after the second day of experimentation that one person could record all the movements satisfactorily and could secure greater uniformity in the conditions of the experiment than was possible to two.

[12] Through an oversight at the time of the segregation and ear-marking of the animals the small male, No. 10, was classed as a female. The error was noticed on the fourteenth day of the experiment, and after this time the rat was caged with the males, 5, 8, 1, and 7, but it continued to run the 1B maze with the females. No difference from this change was noted in the behavior of the male or of any of the other animals. No. 10 did not continue with the females in any other maze, as will be seen in the schedule.

however, converted into two mazes as shown in figure I, by means of a rearrangement of the partitions. Both mazes have the same food box, and therefore only one of them can be used at any one time in experimentation. IB (figure I) is a maze with ten blind alleys, numbered from one to ten. The broken line from the entrance, E, indicates the correct path to the food. IA is another maze having but six *cul de sacs*, the entrance being at E′. This maze is shown in the figure in heavier outlines. Whenever the one maze was in use the entrance to the food box from the other was, of course, closed. The mazes were made of soft wood, and were stained black just before being used in the present experiment. The alleys were uniformly four by four inches in cross dimensions, and the partitions were approximately one-half inch thick. By means of a number of easily removable shutters, braced with triangular supports from behind, the *cul de sacs* could be shortened as desired for the purposes of the experiment. By this means each maze could be converted quickly into a maze of a slightly different type, having the same blind alleys but of relatively different lengths. These shutters were also of soft wood stained black and had the same cross dimensions as the alleys of the maze, so as to fit tightly. In the figure these shutters are indicated by dotted cross-lines in the blind alleys. Thus in Maze IB the blind alleys 2, 4, 6, and 9 are shortened so that the ten blind alleys together have a total length (about eleven feet) approximately equaling the ten in Maze IIB, of which 1, 3, 5, 7, 8 and 10 have been shortened as indicated.

Maze IA differs from Maze IIA on another principle: all the *cul de sacs* in the first are of full length, as indicated, while the second has them *all shortened*. In IA the total length of the blind alleys is about eight feet, while in IIA it is about four feet.

The mazes were supplied with glass covers, with wooden drop-shutters at the entrances and tin side-sliding shutters at the food box entrances. In the experiments each animal was first put into the food box and allowed to taste the food before the first run, or trial. This was not only to strengthen the incentive but also to insure uniformity in incentive and in handling of the animal in all trials. In presenting the animal to the entrance of the maze the experimenter was always seated, with the entrance slightly at his right.

The groupings of the animals made in the experimental procedure are clearly and concisely shown in the general schedule following, arranged according to the mazes used. Thus in Maze IB the first rats used are those called Group M*u*, the *u* indicating that the animals were *untrained.* The rats of Group S*t* were trained, as indicated by the *t*; *i. e.*, they had learned another maze. Frequent reference to figure I, in connection with the study of this schedule, will make clear which maze was in use for any group in question, and the exact modification of the blind alleys.

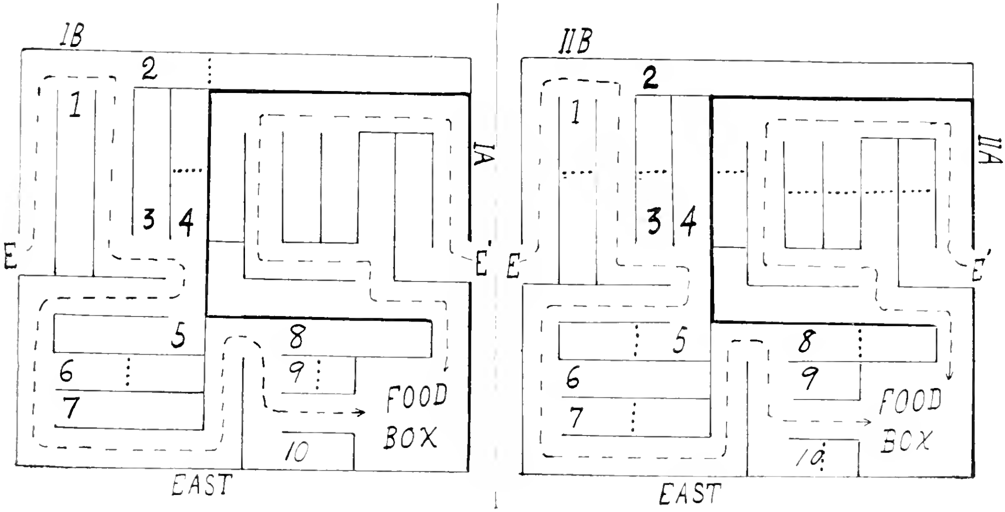


FIGURE I.—The four mazes used, IB, IIB, IA, and IIA. The heavy lines mark the division between the B- and the A-mazes. Dotted lines across blind alleys show position of the shutters. E and E′ are the entrances to the mazes.

GENERAL SCHEDULE OF EXPERIMENTS

Rats used	Practice distributions
	MAZE IB
M*u* Group	7 females (9, 11, 12, 13, 14, 16, 18) and 1 male (10). Two tests, or runs, daily for ten days, then four daily for three days, then three daily for each rat until eight runs out of ten were correct. (Rat 12 did not complete the habit in time available.)
S*t* Group	4 males (2, 3, 4, 6), trained on Maze IIA. Three runs daily for three days, then an intermission for sixteen days (see explanation on next page), then by the intensive method the rats were run Aug. 29th and 30th each three times at the following periods of day: 9-9:20, 9:40-9:55, 3-3:15, 3:30-

3:40, and 8:30-8:40,—total runs, twenty-four for each rat. All records were left incomplete, but all rats were equally practiced to the point of discontinuance. All animals were eager and active.

Maze IIB

N*u* Group.......8 females (15, 17, 19, 20, 21, 22, 23, 24).

Rat 20 was blind in left eye. Distribution of practices precisely same as for Group M*u*, in Maze IB, same days.

R*l* Group........4 males (1, 5, 7, 8) trained in IA.

Distributions of practice same as for S*l*, Maze IB, same days, Practice periods, Aug. 29th and 30th: 9:25-9:35, 9:55-10:10, 3:15-3:30, 3:40-3:55, and 8:45-9. All records left incomplete; rats eager and active.

Maze IA

R*u* Group.......4 males (1, 5, 7, 8).

Practice distributions same as for M*u*, Maze IB.

Nl_1 Group.......4 females (15, 20, 21, 22), trained on Maze IIB.

Three runs daily for each rat until habit was completed, eight runs of ten correct.

Ml_1 Group.......3 females (9, 13, 14), trained on Maze IB.

By intensive method: the three animals were given three runs each, alternating with short periods of rest, during the forenoon of Aug. 28th. Rat 14 completed habit in twenty-eight runs, or trials; rat 9, in twenty-four runs; rat 13, in forty-one runs, eleven of which were made early the morning of the following day. All rats were eager and active, except 13 on the last run of first day, when it took sixteen seconds following two runs of two seconds each.

Maze IIA

S*u* Group.......4 males (4, 6, 3, 2).

Practice distributions same as for the R*u* Group in Maze IA.

Nl_2 Group.......4 females (17, 19, 23, 24), trained on Maze IIB.

Practice distributions same as for Nl_1 in Maze IA.

Ml_2 Group.......3 females (11, 16, 18), trained on Maze IB.

Practice distributions same as for Ml_1, and alternating after each three trials with them. Each rat completed the habit, getting eight out of ten runs correct, in a total of twelve runs. All were very active and eager throughout.

This schedule is given as actually carried out, not exactly as originally planned. It will be noted that the programs for the two B-mazes are precisely alike, and that the same is true of the A-mazes. This affords means of control of a number of factors which otherwise might favor one or the other of the control groups. Temperature conditions changed considerably; it was also necessary to modify occasionally, to suit the time at the disposal of the experimenter, the number of runs per day by each animal. At the early stages of the learning there was not enough time to give each animal more than two runs daily. Later four runs daily were tried, but the eagerness of the animals seemed in one or two cases to diminish in the last run. Three runs a day proved to be very satisfactory. It was originally supposed that each rat could learn both one of the B- and one of the A-mazes during the time available for the experiment—July 18th to August 30th—but a difficulty arose, which had been underestimated in the planning. When the male rats had finished their more simple problems—the A-mazes—and were started on the B-maze problems, signs of trailing the females appeared. To prevent this possibility the male and the female groups of animals had been made to occupy the same cages alternately in successive days. It was impracticable to wash the maze thoroughly before each experiment for each group. The first day that the S*t* and the R*t* male rats were run in the B-mazes, after the runs of the females, there was no difficulty. On the second and the third day, however, there seemed to be evidences of trailing and of excitement, and some of the rats deposited urine drops in the maze from the second to the fifth blind alleys. This seemed to influence, as a guide, later members of the same groups (*i. e.*, also males), and to stimulate them to make similar deposits along the trail. Thorough washing of the entire maze with soap water and Creolin-Pearson, a disinfectant, did not change the behavior materially. Consequently, after the third day the practices of these males were discontinued for sixteen days, until the females had completed their problem. This experience with the males seemed in only one (questionable) case to influence in the least the runs of the females whose habits had been already reduced to the stage of proprioceptive control. The mazes, moreover, had been carefully washed after the second and the third day of the experience with the males already described.

The postponement of the experiment with the males in the B-mazes made it necessary to run them by the intensive method described in the schedule, if at all. It was found that if each rat was given three runs and then put back into the cage without feed it could again be run soon after with no loss of eagerness. In fact, the method worked surprisingly well. The fact that the records had nevertheless to be left incomplete on this maze so far as these rats were concerned does not affect the data so far as they go, as the two comparable groups had identical experiences.

Since the A-mazes were cleared earlier than the B-mazes, it was possible to put into them, as indicated in the schedule, some of the females—four on each A-maze—which first completed their original problem.[13] Finally,—leaving out the female 12, which did not complete its original problem until the maze was taken over for the males, and the small male 10, which had been running with the female group M*u*—three females were practiced on each of the A-mazes by the intensive method. The results of these two groups are, for obvious reasons, strictly comparable only to the twelfth trial inclusive, when the rats in the IIA maze had completed their problem.

All comparable, or control groups were then run on the same days, the same number of times, and as nearly as possible the same time of day. Moreover, to give no possible advantage of trailing to either group—and aside from the cases noted, not between control groups, no such behavior was observed—the group which was practiced first one day was second the next.

Both time and error results were noted. The experimenter devised a system of short signs with which to record the complete gross behavior of each animal. Returns in the maze were noted as accurately as possible; only minor ones not reaching *cul de sacs* or corners of the various maze alleys before being corrected were left out of the records. Entrances into the blind alleys were all classified by means of appropriate signs, into three classes,—complete entrances, entrances about half way in, and beginning entrances bringing the animal's head and fore part into the blind alley while the hind feet remained in the true path. In the table of results these entrances constituting the last class are in the column headed "Start." It was also noted, as the animal emerged from the *cul de sac*, whether it continued forward toward the food or turned back toward the place of beginning. Hesitancies were also noted. Of these a peculiar and amusing kind was frequent. Occasionally, an

---

[13] To be sure that the two groups were of approximately equal ability the animals composing them were selected as follows: The first, third, fifth, and seventh rats that completed Maze IIB were selected for Maze IA, with the long *cul de sacs*; and the second, fourth, sixth, and eighth were taken for Maze IIA. In case of any slight difference in the groups this would put the better animals into the more difficult maze, so that the better results expected for IIA could not be due to superior animals.

animal would stop quickly at the entrance to a blind alley while the head would vibrate very rapidly between the direction of the true path and that of the tempting by-way. The record here and there shows, for instance, that an animal would stop at *cul de sac* **1**, after having nearly inhibited the tendency to enter this blind alley, and make, say, three double vibrations (3 v. d.) with the head. This behavior is very suggestive and will be considered later. On the whole it was found that the full description of the animal's behavior was much more valuable for the present problem than the mere recording of time and errors. Time records were, however, also kept.

TABLE I

General Summary of Results in the B-Mazes

Blind alley........		First									Second								
Degree............		Compl.			Half			Start.			Compl.			Half			Start.		
Direction..........		E	R	F	E	R	F	E	R	F	E	R	F	E	R	F	E	R	F
Runs	Group																		
1- 2	*Mu* IB	19	10	9	2	0	2	12	4	8	33	19	14	9	3	6	6	3	3
	St IB	6	3	3				1		1	14	4	10				1	1	
	Nu IIB	18	9	9	3		3	5	3	2	28	19	9	10	4	6	1		1
	Rt IIB	4		4	1		1	1		1	7	3	4	1		1	1		1
1- 5	*Mu* IB	37	15	22	4		4	15	5	10	44	23	21	17	6	11	11	4	7
	St IB	12	4	8				2	1	1	16	4	12	1	1		4	1	3
	Nu IIB	27	15	12	10	2	8	7	3	4	41	27	14	18	4	14	2		2
	Rt IIB	8	3	5	6		6	6	1	5	9	4	4	3		3	1		1
6- 15	*Mu*	70	16	54	5	2	3	2		2	8	5	3	5		5	13		13
	St	30	4	26	4		4	7		7	5	1	4	2		2	8		8
	Nu	25	5	20	26	2	24	21		21	6	2	4	10	1	9	5	1	4
	Rt	4		4	6	1	5	30	3	27	5	3	2	3	1	2	6		6
16- 25	*Mu*	56	2	54	18	2	16	4		4	5	2	3	5	1	4	6		6
	St	12		12	15		15	7		7	1		1	4	1	3	5		5
	Nu	9	1	8	10		10	22	1	21	8	1	7	3		3	11	1	10
	Rt	3	2	1	3	1	2	7	1	6	7	6	1	10	3	7	3		3
26- 35	*Mu*	38	4	34	21		21	7		7	3		3	7		7	4		4
	Nu	4		4	4		4	10		10	5		5	10	1	9	10	1	9
36- 45	*Mu*	40	1	39	17	1	16	7		7	2	1	1	3		3	4		4
	Nu	1		1	2		2	8		8	1	1		7		7	16		16
46- 55	*Mu*	30		30	21		21	9		9	5	1	4	2		2	3		3
	Nu	1		1	4		4	6		6	2		2	5		5	7		7
56- 65	*Mu*	19		19	19		19	12		12	3		3	2		2	5		5
	Nu				1		1	1		1				1		1	4		4
66- 75	*Mu*	21		21	9	1	8	5		5	2		2	2		2	4	1	3
	Nu							1		1	1		1	1		1			
76- 85	*Mu*	9		9	9		9	7		7	2		2	2		2	4		4
	Nu							1		1									
86- 95	*Mu*	4		4	4	1	3				7		7						
	Nu																		
96-105	*Mu*	3		3	4		4				2		2						
	Nu																		
106-115	*Mu*	1		1	3		3	2		2	1		1	2		2			
	Nu																		
116-125	*Mu*										1		1	2		2			
	Nu																		
Totals IB		382	46	336	153	7	146	86	6	80	107	37	70	56	9	47	71	6	65
Totals IIB		82	26	56	72	6	66	120	9	111	85	41	44	71	10	61	65	3	62
Total number of entrances into *cul de sacs*, IB,										621									234
Total number of entrances into *cul de sacs*, IIB,										275									221

TABLE I—*Continued*

GENERAL SUMMARY OF RESULTS IN THE B-MAZES

Blind alley.......		Third									Fourth								
Degree...........		Compl.			Half			Start.			Compl.			Half			Start.		
Direction.........		E	R	F	E	R	F	E	R	F	E	R	F	E	R	F	E	R	F
Runs	Group																		
1- 2	M*u* IB	**10**	**4**	**6**	**5**	**0**	**5**	**10**	**3**	**7**	23	8	15	4	3	1	2		2
	S*l* IB	**6**	**2**	**4**				**5**		**5**	5	3	2	2	2		7	2	5
	N*u* IIB	10	2	8	3	2	1	4	2	2	**10**	**3**	**7**	**3**		**3**	**1**		**1**
	R*t* IIB							3	1	2	**4**	**2**	**2**	**4**		**4**			
1- 5	M*u* IB	**16**	**5**	**11**	**6**		**6**	**12**	**3**	**9**	30	10	20	6	4	2	4	1	3
	S*l* IB	**7**	**2**	**5**				**6**		**6**	8	3	5	2	2		7	2	5
	N*u* IIB	13	3	10	6	3	3	9	2	7	**15**	**5**	**10**	**5**		**5**	**2**		**2**
	R*t* IIB							4	1	3	**4**	**2**	**2**	**6**	**1**	**5**			
6- 15	M*u*	**2**		**2**				**2**		**2**	1	1		1	1				
	S*l*	**2**	**1**	**1**	**1**		**1**				2	2							
	N*u*							1	1					**3**		**3**	**2**	**1**	**1**
	R*t*	2	1	1				1		1	**5**	**3**	**2**				**2**		**2**
16- 25	M*u*	**2**	**1**	**1**	**1**		**1**				1	1		2		2	1		1
	S*l*													1		1			
	N*u*	1		1							**2**	**1**	**1**	**5**		**5**	**2**		**2**
	R*t*							1		1	**3**	**1**	**2**				**2**		**2**
26- 35	M*u*	**1**		**1**	**1**	**1**		**3**	**2**	**1**	1		1	1	1		2		2
	N*u*							1		1	**1**		**1**				**3**	**1**	**2**
36- 45	M*u*																1		1
	N*u*							2		2				**1**	**1**		**2**		**2**
46- 55	M*u*	**5**	**3**	**2**							3	1	2	2	1	1			
	N*u*	1	1					1	1		**1**		**1**						
56- 65	M*u*	**1**	**1**								3		3				1		1
	N*u*										**1**		**1**				**1**		**1**
66- 75	M*u*	**1**		**1**							1		1						
	N*u*							1		1	**1**	**1**							
76 -85	M*u*	**1**	**1**								2	1	1				2		2
	N*u*																		
86- 95	M*u*	**2**		**2**							2	1	1						
	N*u*																		
96-105	M*u*				**1**	**1**													
	N*u*																		
106-115	M*u*																		
	N*u*																		
116-125	M*u*																		
	N*u*																		
Totals IB.......		**40**	**14**	**26**	**10**	**2**	**8**	**23**	**5**	**18**	54	20	34	15	9	6	18	3	15
Totals IIB.......		17	4	13	6	3	3	21	5	16	**33**	**13**	**20**	**20**	**2**	**18**	**16**	**2**	**14**
Total number of entrances into *cul de sacs*, IB,										**73**									86
Total number of entrances into *cul de sacs*, IIB,										44									**69**

TABLE I—*Continued*

GENERAL SUMMARY OF RESULTS IN THE B-MAZES

Blind alley		Fifth									Sixth								
Degree		Compl.			Half			Start.			Compl.			Half			Start.		
Direction		E	R	F	E	R	F	E	R	F	E	R	F	E	R	F	E	R	F
Runs	Group																		
1- 2	*Mu* IB	30	18	12	1		1	9	4	5	7	3	4				1	1	
	St IB	12	5	7				5	3	2	1		1				1		1
	Nu IIB	21	8	13	1		1	4	2	2	5		5	4	2	2	4	1	3
	Rt IIB	8	6	2				4	1	3	1		1						
1- 5	Mu IB	46	27	19	1		1	13	5	8	10	5	5	1		1	2	1	1
	St IB	19	6	13				5	3	2	2		2	1		1	1		1
	Nu IIB	31	10	21	1		1	6	3	3	9	2	7	5	2	3	7	2	5
	Rt IIB	8	6	2	1		1	4	1	3	2		2	1		1	1		1
6- 15	Mu	5	2	3							5		5	4	1	3	4		4
	St	6	2	4	5	1	4	3		3	1		1				2		2
	Nu	3	2	1	1		1	4	1	3	2		2	3		3	6		6
	Rt	4	2	2	2		2							1		1			
16- 25	*Mu*	3	2	1				1		1	1		1				3		3
	St																		
	Nu	1		1				1		1				2		2			
	Rt	2	1	1				1		1	1	1					1		1
26- 35	Mu	7	5	2													1		1
	Nu	2		2															
36- 45	*Mu*	2	1	1													1		1
	Nu				1	1		1		1	1		1						
46- 55	*Mu*	4	2	2				1	1					1		1			
	Nu	1	1					1		1	1		1						
56- 65	*Mu*	3		3							1	1							
	Nu				1		1												
66- 75	*Mu*	1		1	2	1	1												
	Nu	1		1															
76- 85	*Mu*	2	1	1	1		1	2		2	1		1				1		1
	Nu																		
86- 95	*Mu*	2	1	1							1		1						
	Nu																		
96-105	*Mu*																		
	Nu																		
106-115	*Mu*	1		1															
	Nu																		
116-125	*Mu*																		
	Nu																		
Totals IB		101	49	52	9	2	7	25	9	16	22	6	16	7	1	6	15	1	14
Totals IIB		53	22	31	7	1	6	18	5	13	16	3	13	12	2	10	15	2	13
Total number of entrances into *cul de sacs*, IB,										135									44
Total number of entrances into *cul de sacs*, IIB,										78									43

TABLE I—*Continued*

General Summary of Results in the B-Mazes

Blind alley		Seventh									Eighth								
Degree		Compl.			Half			Start.			Compl.			Half			Start.		
Direction		E	R	F	E	R	F	E	R	F	E	R	F	E	R	F	E	R	F
Runs	Group																		
1- 2	M*u* IB	1	1					2	1	1	13	7	6				1		1
	S*t* IB	1		1				1		1	3		3	1		1			
	N*u* IIB	3	1	2	1		1	3	2	1	14	3	11	1		1			
	R*t* IIB	1		1	1		1				3		3	2		2			
1- 5	M*u* IB	3	1	2				3	1	2	19	9	10	2		2	1		1
	S*t* IB	2		2				2		2	9	1	8	1		1	1		1
	N*u* IIB	6	1	5	3		3	3	2	1	18	4	14	2		2	1		1
	R*t* IIB	2		2	1		1	3		3	5		5	2		2	1		1
6- 15	M*u*	9	1	8	6		6	2		2	6		6				2		2
	S*t*	2		2				2		2	2		2	2		2			
	N*u*	4		4	6		6	1	1								1		1
	R*t*							1		1	1		1				1		1
16- 25	M*u*	4		4	3		3	7		7	1		1	1		1	1		1
	S*t*																		
	N*u*	2		2	1		1				1		1						
	R*t*							1		1	1		1						
26- 35	M*u*	1		1	1		1	4		4				1		1	1		1
	N*u*	1		1				3		3							1		1
36- 45	M*u*				2		2	3		3				1		1			
	N*u*							1		1				1		1			
46- 55	M*u*	1	1		1		1				2	1	1						
	N*u*																1		1
56- 65	M*u*																		
	N*u*																		
66- 75	M*u*										1		1						
	N*u*																		
76- 85	M*u*																		
	N*u*																		
86- 95	M*u*										2		2						
	N*u*																		
96-105	M*u*																		
	N*u*																		
106-115	M*u*																		
	N*u*																		
116-125	M*u*																		
	N*u*																		
Totals IB		22	3	19	13		13	23	1	22	42	11	31	8		8	6		6
Totals IIB		15	1	14	11		11	13	3	10	26	4	22	5		5	6		6
Total number of entrances into *cul de sacs*, IB,										58									56
Total number of entrances into *cul de sacs*, IIB,										39									37

TABLE I—*Continued*

General Summary of Results in the B-Mazes

Blind alley		Ninth									Tenth								
Degree		Compl.			Half			Start.			Compl.			Half			Start.		
Direction		E	R	F	E	R	F	E	R	F	E	R	F	E	R	F	E	R	F
Runs	Group																		
1- 2	M*u* IB	**5**		**5**	**2**		**2**	**1**		**1**	7	2	5				1		1
	S*t* IB	**2**	**1**	**1**							3		3				1		1
	N*u* IIB	5		5	1	1		2		2	4	2	**2**						
	R*t* IIB	3		3				1		1	**2**		**2**				**1**		**1**
1- 5	M*u* IB	**9**		**9**	**5**		**5**	**2**		**2**	15	3	12				1		1
	S*t* IB	**4**	**1**	**3**							5		5				1		1
	N*u* IIB	14		14	4	1	3	2		2	**7**	**2**	**5**	**2**		**2**	**1**		**1**
	R*t* IIB	5		5				4		4	**3**	**1**	**2**				**1**		**1**
6- 15	M*u*							**3**		**3**	4		4	3		3			
	S*t*	**1**		**1**							1		1						
	N*u*	1		1	1		1				**1**	**1**		**2**		**2**	**2**		**2**
	R*t*							1		1							**1**	**1**	
16- 25	M*u*				**1**		**1**	**1**		**1**	2		2						
	S*t*																		
	N*u*							2	1	1									
	R*t*																**1**		**1**
26- 35	M*u*	**1**		**1**				**2**		**2**				1		1			
	N*u*							2		2									
36- 45	M*u*							**1**		**1**									
	N*u*	1		1	1		1							**1**		**1**			
46- 55	M*u*							**1**		**1**									
	N*u*							1		1	**1**		**1**						
56- 65	M*u*																		
	N*u*																		
66- 75	M*u*																		
	N*u*																		
76- 85	M*u*																		
	N*u*																		
86- 95	M*u*																1		1
	N*u*																		
96-105	M*u*																		
	N*u*																		
106-115	M*u*																		
	N*u*																		
116-125	M*u*																		
	N*u*																		
Totals IB . .		**15**	**1**	**14**	**6**		**6**	**10**		**10**	27	3	24	1		4	3		3
Totals IIB		21		21	6	1	5	12	1	11	**12**	**4**	**8**	**5**		**5**	**6**	**1**	**5**
Total number of entrances into *cul de sacs*, IB,										**31**									34
Total number of entrances into *cul de sacs*, IIB,										39									**23**

TABLE II

General Summary of Results in the A-Mazes

Blind alley.......			First			Second								
Degree of entrance			Compl.	Half	Start.	Compl.			Half			Start.		
Direction.........			E	E	E	E	R	F	E	R	F	E	R	F
Runs		Group												
1- 2	IA	R*u*	15	3	6	3	2	1	1	1		14	5	9
		Nt_1	10	1	2	5		5	3	2	1	3	1	2
		Mt_1	1		1	1	1		1		1			
	IIA	S*u*	11	2	2	4	3	1	2	2		1		1
		Nt_2	5	2		2		2	1		1	1		1
		Mt_2	2	1					1		1	2		2
1- 5		R*u*	22	4	9	10	3	7	3	2	1	18	5	13
		Nt_1	13	4	4	8	2	6	4	2	2	3	1	2
		Mt_1	3	1	1	9	6	3	1		1			
		S*u*	30	3	2	10	6	4	4	3	1	5		5
		Nt_2	8	4		2		2	2		2	3		3
		Mt_2	3	3					1		1	1		1
6- 15		R*u*	14	6	2	13	4	9	4	1	3	13		13
		Nt_1	3	2	2	2	1	1	3	1	2	4	1	3
		Mt_1	5	2	1	10	6	4	3		3	2		2
		S*u*	2	4	3	2	1	1	4		4	3		3
		Nt_2	2	1	2							2	1	1
		Mt_2			1									
16- 25		R*u*	4	4		4	4		1		1	3		3
		Nt_1	1	1		1	1		1	1		3	1	2
		Mt_1		1					4	1	3	5		5
		S*u*		2								6		6
		Nt_2												
		Mt_2												
26- 35		R*u*	3	4	1				1		1			
		Nt_1							1		1			
		Mt_1										1		1
		S*u*		3	1	1	1					4		4
36-45		R*u*				2	1	1						
		S*u*												
46- 55		R*u*							1	1				
		S*u*												
56- 65		R*u*												
		S*u*												
Totals IA.......			68	29	20	59	28	31	27	9	18	51	8	43
Totals IIA.......			45	20	9	15	8	7	11	3	8	24	1	23
Total number of entrances into *cul de sacs*, IA,					117									137
Total number of entrances into *cul de sacs*, IIA,					74									50

TABLE II—*Continued*

GENERAL SUMMARY OF RESULTS IN THE A-MAZES

Blind alley			Third									Fourth								
Degree of entrance			Compl.			Half			Start.			Compl.			Half			Start.		
Direction			E	R	F	E	R	F	E	R	F	E	R	F	E	R	F	E	R	F
Runs	Group																			
1- 2	IA	R*u*	4	1	3				6	3	3	12	8	4	2	2		7	5	2
		Nt_1	3	1	2				2	1	1	15	9	6						
		Mt_1	1	1		1		1	1		1	4	1	3						
	IIA	S*u*	2		2							3	1	2						
		Nt_2	1		1				1		1	5	2	3	2	1	1	2		2
		Mt_2	1		1				1		1							1		1
1- 5		R*u*	6	1	5				7	3	4	14	9	5	5	5		11	6	5
		Nt_1	6	2	4				3	1	2	18	9	9	3	1	2	3		3
		Mt_1	8	6	2	2		2	2		2	10	1	9	1	1		1		1
		S*u*	7	3	4				2		2	12	6	6	3	1	2	5	1	4
		Nt_2	2		2	1		1	3		3	5	2	3	2	1	1	2		2
		Mt_2	1		1				1		1							2		2
6-15		R*u*	15	6	9	5		5	4		4	20	11	9	3	1	2	2		2
		Nt_1	7	2	5	5		5	4		4	6	1	5	1		1	6		6
		Mt_1	10	3	7	2		2	1		1	7	2	5	1		1	6		6
		S*u*	6	3	3	6		6	1		1	8	3	5	4		4	8		8
		Nt_2	1		1				5		5	1		1	1		1	4	1	3
		Mt_2	1		1															
16-25		R*u*	5	1	4	5		5	6		6	7	1	6	2	1	1	6		6
		Nt_1	3	2	1				1		1							6		6
		Mt_1	1	1		6		6	4	1	3	1	1		1	1		1		1
		S*u*	1		1	5	1	4	10		10				2		2	4		4
		Nt_2							2		2				1		1			
		Mt_2																		
26-35		R*u*				3		3	4		4	4		4	1	1		3		3
		Nt_1																		
		Mt_1				2		2	2	1	1				1		1			
		S*u*	5	3	2	1		1	8		8	1		1	4		4	6		6
36-45		R*u*	3	1	2	4		4	4		4				1		1	1		1
		S*u*				1		1	1		1							8		8
46-55		R*u*				1		1	1		1	3		3	1		1	1		1
		S*u*	1	1								1	1					3		3
56-65		R*u*																		
		S*u*													1		1			
Totals IA			64	25	39	35		35	43	6	37	90	35	55	21	11	10	47	6	41
Totals IIA			25	10	15	14	1	13	33		33	28	12	16	18	2	16	42	2	40
Total number of entrances into *cul de sacs*, IA,											142									158
Total number of entrances into *cul de sacs*, IIA,											72									88

TABLE II—*Continued*

GENERAL SUMMARY OF RESULTS IN THE A-MAZES

Blind alley			Fifth									Sixth								
Degree of entrance			Compl.			Half			Start.			Compl.			Half			Start.		
Direction			E	R	F	E	R	F	E	R	F	E	R	F	E	R	F	E	R	F
Runs	Group																			
1- 2		Ru							**4**		**4**							**3**		**3**
	IA	Nt_1							**6**	**1**	**5**	**3**		**3**						
		Mt_1										**1**		**1**						
		Su	2		2	1		1	1		1	2		2						
	IIA	Nt_2				2		2				2		2				1		1
		Mt_2																1		1
1- 5		Ru							**10**		**10**	**1**		**1**	**1**		**1**	**4**		**4**
		Nt_1							**9**	**2**	**7**	**4**		**4**						
		Mt_1										**1**		**1**						
		Su	3		3	6		6	2		2	8		8						
		Nt_2				2		2				2		2				1		1
		Mt_2																1		1
6-15		Ru	**3**		**3**	**3**		**3**	**4**		**4**	**4**		**4**				**1**		**1**
		Nt_1	**1**		**1**				**1**		**1**									
		Mt_1																		
		Su	4		4	2	1	1	2	1	1	3	2	1						
		Nt_2	1		1							1	1							
		Mt_2																		
16-25		Ru				**1**		**1**	**1**		**1**									
		Nt_1																		
		Mt_1																		
		Su																		
		Nt_2																		
		Mt_2																		
26-35		Ru	**1**		**1**							**1**		**1**				**1**		**1**
		Nt_1																		
		Mt_1																		
		Su																		
36-45		Ru							**1**		**1**									
		Su																		
46-55		Ru	**1**		**1**															
		Su	1		1				1		1							1		1
56-65		Ru																		
		Su																		
Totals IA			**6**		**6**	**4**		**4**	**26**	**2**	**24**	**11**		**11**	**1**		**1**	**6**		**6**
Totals IIA			9		9	10	1	9	5	1	4	14	3	11				3		3
Total number of entrances into *cul de sacs*, IA,											**36**									**18**
Total number of entrances into *cul de sacs*, IIA,											24									17

RESULTS

Tables I and II give in a condensed form the main results of the entire experiment. In the separate larger divisions are given the reactions to the several blind alleys. These reactions are classified in a manner most easily made clear by taking up a concrete case. In table I the words " First," " Second," etc. at the top stand for the blind alleys of the B-mazes of the corresponding numbers. The results of the first blind alley, for example, are then divided into three parts, " Complete," " Half," and " Start," meant to designate the degree of entrance by the rats into the blind alleys, as already explained. Complete entrance means going entirely to the end of the blind alley, or so near the end that the animal might reach the end by means of the vibrissae. Frequently the rats ran against the end with considerable force. Half entrance means approximately half way, or all entrances between complete and beginning. Those marked " Start " include cases in which the animal either just put the head in or entered with the fore half of the body. In such cases the hind feet of the animal usually remained in the true path, so that the general orientation was not completely given up as in the other two cases. The three columns coming again under each of these rubrics show respectively, the number of entrances into the blind alley in question, E, the number of returns toward the place of starting in the maze on the rats' emerging from the blind alley, R, and the number of times the animals kept the general orientation, *i. e.*, continued toward the food box, F. The totals for R and F must therefore equal the number under E.

The figures in the left column of the table indicate the number of the run, or of the test, of the animals, while the letters M*u*, S*t*, etc. stand for the group, as Group M *un*trained, Group S trained, and so on. The description of each group and of its practice distributions are given in detail in the schedule, pages 9 and 10, to which frequent reference is advisable. Now, to illustrate in a concrete case, in the first line of the data, giving results of the first two trials of the animals, we find that Group M*u* (eight rats, untrained, running in Maze IB) made nineteen complete entrances into the blind alley No. 1, with ten returns and nine cases in which the rat continued forward toward the food box. There were two entrances half way, with two forward runs and no returns; thirteen beginning entrances, with four returns and eight cases of the animal keeping its general forward orientation; and so on, through the results for all the blind alleys in order.

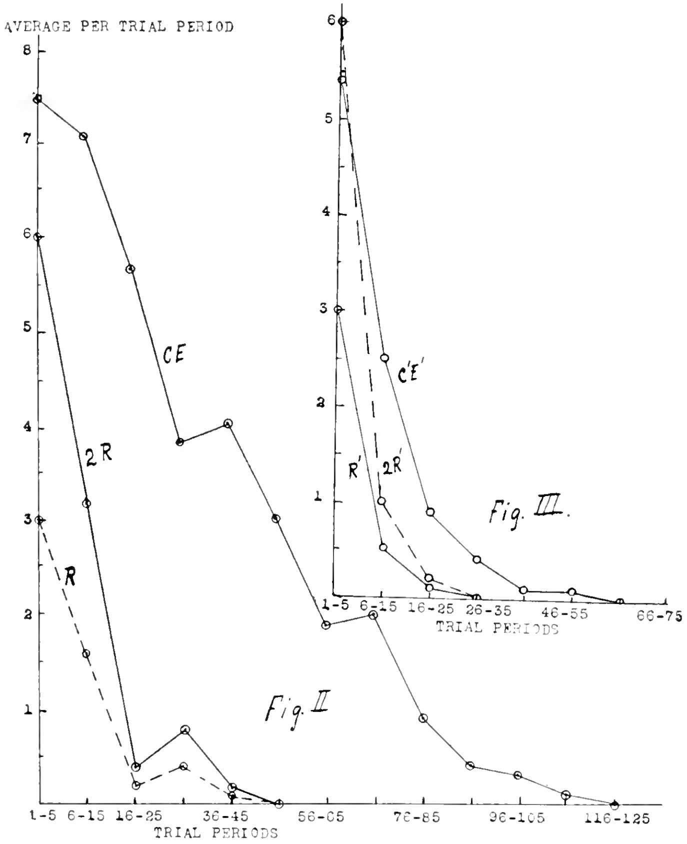
Note that the figures for groups M*u* and S*t* are in bold face for blind alley 1, and not for 2, and that these relations are just reversed for the groups of animals running in Maze IIB. The bold face designates full length *cul de sacs*, and the figures not in bold face indicate that the blind alley was shortened. The amount of shortening in any case is shown in figure I, as already explained. Careful attention to all these matters will greatly aid the reader in getting quickly and conveniently the general results of numerous reactions. Without such attention the tables are meaningless. The results cannot so well be effectively and accurately shown in graphs.

The totals at the foot of the columns must not be taken too seriously, as will be evident in subsequent discussion. These are totals only of *changing* comparative quantities. For this reason the results of the experiments have been classified for different periods of the training. The results of the first two trials are given separately—and are not added in the totals because they are again included in the data for the 1st to 5th trials—as they are least affected by the animals progressive training. They show us approximately whether mere chance, or probability laws, can explain the direction that an animal beginning in the maze takes on emerging from a *cul de sac*, whether it returns or continues forward keeping its general orientation,—not accurately, however, for learning begins from the very first experiences in the maze. The progression of the learning in the case of each particular *cul de sac* is shown by a gradual decrease in entrances in the summaries of the 1st to 5th, the 6th to 15th, 16th to 25th, etc. trials; also by the gradual decrease in returns and the increase, correspondingly, in the number of cases of keeping the general forward orientation. These two kinds of changes are very interesting and illuminating toward showing, in a manner not hitherto done with data on learning, just *how* the *cul de sacs* are eventually eliminated.[14] This is our main concern in this paper.

---

[14] Professor Carr has pointed out that the *extent* of entrance to *cul de sacs* gradually decreases, as well as the *number* of entrances. (Hicks, V. C., and Carr, H. A. Human Reactions in a Maze. *Jour. Animal Behav.*, 1912, **2**, 98-125. See particularly page 116.)

Three important features of the results are to be noted. The first is the rapid decrease in the proportion of the returns to forward runs, on the rat's emergence from blind alleys. With the exception of blind alleys **2** and **5** in the B-mazes few such returns were made after the 15th trial, though the animals continued to enter some of the blind alleys beyond the 75th, some even beyond the 100th. These *cul de sacs*, noted as



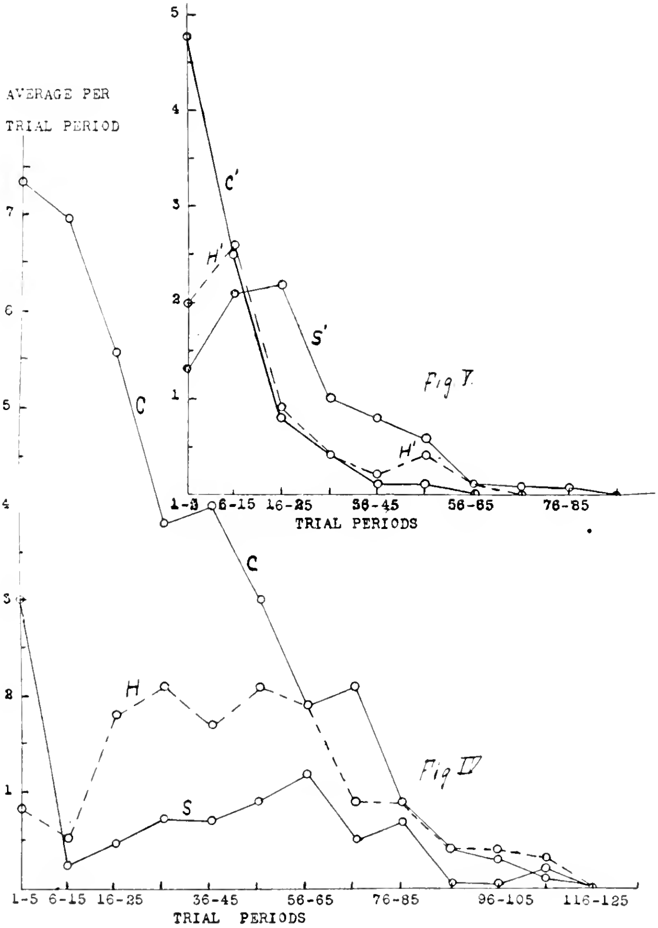
FIGURES II AND III.—CE is the curve of decrease in average number of complete entrances per trial to Blind Alley **1** full length, Maze IB; R indicates the decrease in returns, and 2R twice the returns, from this *cul de sac*. C′E′, R′ and 2R′ show corresponding data for Blind Alley **1** shortened, Maze IIB. Eight untrained rats in each case.

exceptions, have directions such as to favor returns in the case of a rat emerging from them. This more rapid decrease in returns than in entrances to *cul de sacs* is least complicated, and also shown most emphatically, in the case of the complete entrances to *cul de sac* **1**, which is encountered before the rat could be confused by running into any other blind alleys. Figure II shows the matter graphically. Curve CE represents the number of complete entrances of eight untrained rats to the first blind alley at full length, as in Maze IB; curve R, the returns; and 2R, twice the returns. 2R is a better curve for comparison with CE because originally, *i. e.* before an animal is at all practiced, about half of the entrances are followed by returns; twice the returns, therefore, gives a number initially about equal to the total number of entrances. Figure III gives corresponding curves, C′E′, and 2R′, respectively, for the same number (eight) of untrained rats in *cul de sac* **1**, shortened from 22 inches to 8.5 inches. Here the same result is evident: while the elimination of entrances is far more rapid than in the case of the longer blind alley, the returns are still more rapidly reduced as shown by the 2R′ curve.

It may also be noted here under our first point that the returns in both the B-mazes persisted longer in the cases of the blind alleys farther from the food box than of those near it. That is, returns from blind alleys first encountered were less easily eliminated, as were also entrances, than from those further along the true path. This is true even in cases of blind alleys nearer the food box that were comparatively long, as **7** and **8**, even though, as in the case of **8**, the direction of movement in emerging from the *cul de sac* favored returns. It is barely possible that an odor factor may have entered in case of **8**. The mazes IA and IIA are not so well adapted to show these relationships, as there are fewer blind alleys of various individual differences of complexity, but the same conclusions as those given for the B-mazes may also be made for them.

A second important point to note is, that the *nature of the response* to a blind alley gradually changes with practice, as well as the relative number of entrances into it. This change in the nature of the response is more marked in longer than in shorter blind alleys, particularly in those whose elimination was most difficult. It is illustrated best in the data from *cul de sac*

1 of Maze IB. Many of the entrances to **2**, as the observation of the animals in their responses and also their individual records showed, are clearly due to confusions resulting from entrances to **1**. The experimental notes supply many evidences. As a rule the rat in the early stages of response to such a blind alley runs rapidly into it the entire distance, usually coming into



FIGURES IV AND V.—C shows decrease in average number of complete entrances per trial to Blind Alley **1** full length in the B-mazes; H same for half-way entrances, and S for beginning entrances. C′, H′, and S′ show corresponding results for the blind alley shortened. Eight untrained rats in each case.

contact one way or another with the end; but with succeeding trials the entrance is less and less complete, until finally the impulse to enter is wholly inhibited. Thus in the records of responses of two groups each of eight untrained rats to the first blind alley in the B-mazes (table I) the large numbers in the E-columns shift gradually from the " Complete " through the " Half " to the " Start " column. This shift is graphically shown in figure IV and figure V for first blind alley of mazes IB and IIB, respectively. C and C′ are the curves representing the rate of elimination of complete entrances, H and H′ of half entrances, and S and S′ of beginning entrances. Note that while the C-curves fall rapidly from the first, especially the one (C′) from the shortened *cul de sac*, there is a decided rise in the H- and the S-curves. Specifically, in the case of Maze IB (the *cul de sac* long), C falls gradually, with two minor exceptions, all the way at a nearly uniform rate; H rises almost uniformly to the 35th trial, then it keeps almost a uniform height to the 65th trial, and finally gradually declines; and S, after a rapid initial decline, gradually rises again until the 65th trial is reached, when it gradually declines and reaches zero before the other two curves. In the case of Maze IIB (*cul de sac* shortened) the same relationship between these respective curves is shown, though all these curves drop earlier in the process than with the longer blind alley, except that in this case the S′ curve holds out longer than either of the other curves. A cursory examination of the data for other blind alley records shows that this type of transition from complete to only partial entrance and then to final elimination is a general feature of the results for the different groups of animals in the various mazes. A few exceptions only, in cases of very short *cul de sacs*, are noticeable. This is a phenomenon of learning in the maze to which little attention has previously been given, and which seems to the writer to be inexplicable on the basis of mere frequency and recency laws. Several impulses working together, some facilitating others inhibiting one another, gradually result in the survival of the most consistent, or complete acts. No hesitancies in the rats' behavior in these cases were present, such as might be secured from persons in similar circumstances. The rats evidently did not have time, nor adequate sense organs and conscious memories as a person would have, to recognize and take note of external stimuli, but resembled automatic machines in the quickness and uniformity of their responses.

This change appears more significantly in the results of most of the individuals than in those of all averaged. Here are some examples. Rat 18, of Group M*u* made entrances to the first blind alley (full length) in this order: 12c (complete), 1h (half), 1c, 1h, 5c, 1s (started), 4c, 1h, 2c, 1h, 5c, 1h, 4c, 1h, 1c, 4h, 2c, 3h, 3s, 1c, 1h (total 55 entrances). Rat 10's record, same group, is 1s, 1c, 1h, 9c, 1h, 7c, 2h, 3c, 1h, 1c, 2h, 1s, 2c, 1s, 2c, 1h, 1s, 1c, 1h, 2c, 1h, 1c, 1h, 1c, 1h, 1c, 3h, 1s, 1c; the next time on passing this *cul de sac* there was a momentary pause with three very rapid in and forward vibrations of the head, causing a confusion in which the animal made eleven errors in the other nine *cul de sacs* none of which it had entered, with but one exception, for twelve trials; then 1s, 2c, 1s, 2c (after another such vibrating pause before the *cul de sac*), 2s, 1c, 1h, 1s, 2c, 1h, 1c, 1h, 1c, 1s (total 70 entrances). These results are typical. In numerous cases when the habit of avoiding the *cul de sac* was nearly complete, so that the animal usually made the " s " type of entrance, the peculiar rapid vibration of the head noted above took place. The pause was, however, but for an instant. This response seems to indicate that the impulse to go forward at the critical place is still partly checked or impeded by one to enter, not quite eliminated. It is important to note, moreover, that when finally the rat *does* succeed in passing the *cul de sac*, even when this hesitant, vibrating behavior does not take place, it very frequently runs headlong into some neighboring *cul de sac* which had long since been inhibited, and thereby gets considerably confused. Frequently, after such an experience it makes a complete entrance into the *cul de sac* in question the next trial, just as a child " speaking a piece " must bow again and start over when she goes wrong. This is one reason why a few complete entrances continue to occur. More than once an animal which had successfully passed *cul de sac* **1** for several trials would, without any hesitancy, run into it with great speed and against the closed end with terrific impact. In one such case the animal's whole maze habit, just on the finishing stage, seemed to have been temporarily jolted wholly out of gear, its next trial being much like that of a beginner. All this makes it very plain that maze habits are not to be explained on the basis of individual, disparate " acts," following in their occurrence some law of chance. On the contrary, the various impulses in the random activity of the early trials are gradually and collectively woven into one matrix of successive responses, each setting off the next succeeding one, and all shaped by the whole

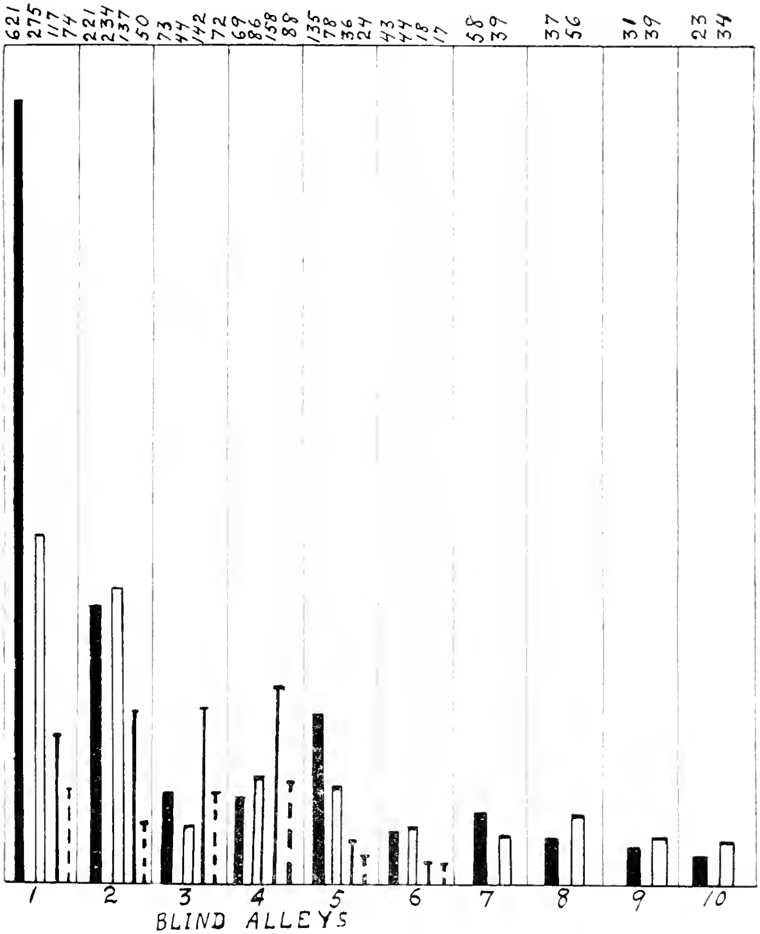


FIGURE VI.—Heavy columns, double lines, single lines, and discontinuous lines show, respectively, total entrances by all animals to full length and shortened blind alleys in the B-mazes, and to full length and shortened blind alleys in the A-mazes. Figures above columns give the totals represented.

circumstance of the maze environment.[15] This seems to imply that the effect of one stimulus holds over into and conditions effects of later stimuli.

The third point to note in our results is that when any given *cul de sac* is shortened it is eliminated more readily than when left at full length. That is to say, other things equal, and within certain limits, a long *cul de sac* is eliminated less readily than a short one. This statement is amply borne out in our data both from the A- and the B-mazes. The general results of all our experiments are shown roughly in the accompanying diagram, figure VI, representing the total number of entrances to each of the blind alleys in the various mazes. The heavy black columns and the double lines represent the totals for the full length and the shortened blind alleys, respectively, in the B-mazes; the single continuous and the broken lines stand for the corresponding totals for the A-maze blind alleys. In the B-mazes the total entrances to the full length *cul de sacs* is 1311, while the total number of entrances to the same *cul de sacs* when shortened, by an equal number of animals under the same conditions, is 929, a decrease of 29%. This decrease would doubtless be considerably greater but for the fact that confusions by the long blind alleys resulted in random behavior which increased the totals for the shortened *cul de sacs*. For instance, table I shows that more entrances were made into **2** short than into **2** full length. This was very clearly due to the fact that as long as the habit to avoid **1** was incomplete the animals in the confusion also entered **2**. It will be recalled that in the A-mazes the *cul de sacs* were all full length in the one and all shortened to about half their length in the other case. Here we do not have the confusion noted in the B-mazes. The shortened blind alleys were entered 47% fewer times than those of full length. This bears out the conclusions drawn from the B-mazes.

The effect of shortening the *cul de sacs* was most noticeable in the case of **1** in the B-mazes, which was by all means the most difficult to eliminate. Being the first to encounter, it was

---

[15] On this point our results agree with some aspects of those by Peckstein, L. A. Whole vs. Part Methods in Motor Learning: a comparative Study. *Psych. Mon.*, Ser. No. 99, 1917. "Each aspect of the course is no doubt associated with and located in reference to all the details of the course and to the entire objective environment as well." P. 30.

likely the least complicated by the results of entering other blind alleys. Table III and figure VII show comparatively the rate of elimination of all entrances to this *cul de sac* full length and shortened, 22 and 8.5 inches respectively, by the two groups of rats, M*u* and N*u*. While the two curves start near together the one, S, representing the entrances to the shortened *cul de sac* drops rapidly after the 15th trial; the other one, L, after the initial decline keeps nearly the same height to the 55th trial. The percentage eliminations are shown for the long and for the shortened *cul de sac*, respectively, by curves E and E′.

TABLE III

ELIMINATION OF ALL ENTRANCES TO BLIND ALLEY **1**. TWO GROUPS OF EIGHT RATS EACH. MAZES IB AND IIB

Trials	1–2	1–5	6–15	16–25	26–35	36–45	46–55
Av. No. of entrances to blind alley **1**, long	16.5	11.1	7.7	7.8	6.6	6.4	6.0
Per cent	100.0	67.3	46.6	47.3	40.0	38.8	36.4
Av. No. of entrances to blind alley **1**, short'd.	13.0	8.8	7.2	4.1	1.8	1.1	1.1
Per cent	100.0	67.7	55.4	30.8	13.8	8.5	8.5

TABLE III—*Continued*

ELIMINATION OF ALL ENTRANCES TO BLIND ALLEY **1**. TWO GROUPS OF EIGHT RATS EACH. MAZES IB AND IIB

Trials	56–65	66–75	76–85	86–95	96–105	106–115	116–125
Av. No. of entrances to blind alley **1**, long	5.0	3.5	2.5	.8	.6	.6	0
Per cent	30.3	21.2	15.2	4.8	3.6	3.6	0
Av. No. of entrances to blind alley, **1** short'd.	.2	.1	.1	0	0	0	0
Per cent	1.2	.8	.8	0	0	0	0

The results from *cul de sac* **2** are 221 entrances to the full length (40 inches) and 234 to the shortened form. This would appear to contradict our general conclusion. However, it must be remembered that the rats for which **2** was shortened made

346 more entrances to **1** (long) than did the control animals for which **2** was left full length; they also made 18 more returns to the starting place in the maze. This not only required that **2** shortened be passed more times than **2** long, but also with greater probability of entrance for each time. It was noted that rats entering **1** were likely thereby to be thrown out of orientation and to make other errors. There can therefore be little doubt that if **2** had been the only blind alley in the maze, it would have formed no exception to the general rule. The greater number of entrances to **4** shortened than to **4** long (86 to 69) is due to the fact that rats emerging from **5** had a strong tendency to run into **4**. A glance at the maze will show why this is to be expected. There were 32 more returns from **5** long than from **5** short; furthermore, entrance to **5** long had the greater tendency to disorient the animal so that entrance to **4** would be an increased probability. Just why **8** short should have been entered 51% more than **8** long is not easy to

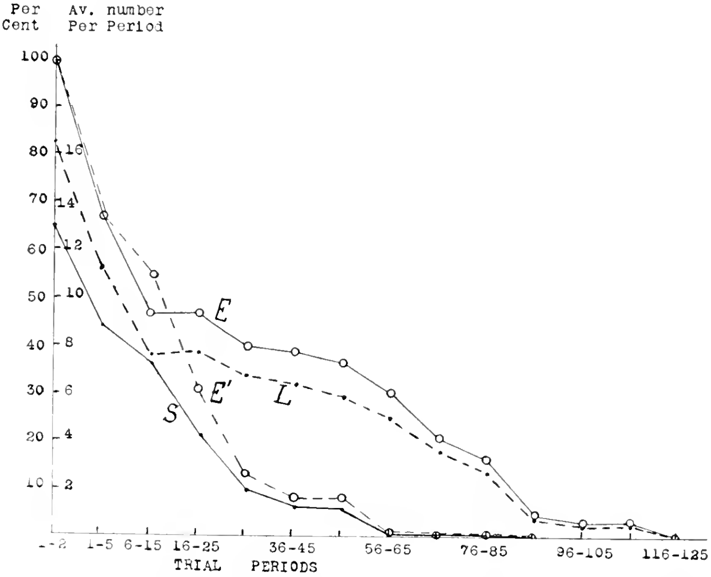


FIGURE VII.—L shows rate of elimination of all entrances to *cul de sac* **1** long, Maze IB, and S same for **1** short, Maze IIB. E′ and E show corresponding percentage eliminations.

determine. There were, moreover, 14% more returns from the entrances in the former than in the latter case. It is possible that the rats entering 8 at full length, which runs along side the food box, had time and opportunity to get sufficient odor from the food to influence them against returning. Accidental factors may have been the cause in part; half of the entrances were made in the first two trials, and the total numbers are too small to indicate with much probability the actual trends.

On the whole there can be no question that, other things equal, entrances to short *cul de sacs* are more easily eliminated than entrances to long ones.

The results from both types of mazes used in this experiment (see tables I and II) show that on the whole *cul de sacs* first encountered in the maze were entered more frequently, and that the impulses to enter them were overcome with more difficulty, than were those occurring further along the true path, or nearer the food. In this respect our results are in agreement with those of Miss Vincent[16] and contrary to those of Miss Hubbert.[17] While in the present experiment, not intended especially to test this point, the bearing of the results is necessarily complicated by an inequality of the lengths of the various blind alleys, there is no evidence to show that results would have been different with *cul de sacs* of equal lengths and of equal direction difficulties. In the B-mazes, for example, **6** and **7** were much less troublesome than **3** and **4**, in many respects similarly located with respect to the correct path, and all of equal length. By all means the most difficult *cul de sac* to avoid entering was **1**, even when shortened to 8.5 inches. The total entrances to **6** and **7** long are 101, against 142 to **3** and **4** long; to **6** and **7** shortened 83, against 130 to **3** and **4** shortened. The total number of entrances to **1** short are 275, whereas the totals to **6**, **7**, **8**, **9**, **10** full length amount only to 192. It seemed that the rats got rather firmly registered in their proprio-ceptive system of controls the tendency to make two successive turns of 90 degrees each to the right, beginning

---

[16] Vincent, Stella B. The White Rat and the Maze Problem—IV. The Number and Distribution of Errors: a Comparative Study. *Jour. Animal Behav.*, 1915, **5**, 367-374. "The final members of the *cul de sacs* were entered less frequently and eliminated first." P. 374.

[17] Hubbert, Helen B. Elimination of Errors in the Maze. *Jour. Animal Behav.*, 1915, **5**, 66-72.

at the corner of the maze before *cul de sac* **1**, and that since the turns were so close together they tended very persistently to fuse together into a single turn of 180 degrees, thus taking the rat into the blind alley. It was very interesting to see certain rats continue to run into **1** with almost monotonous regularity for three weeks, three trials each day, while other errors, errors of entering other *cul de sacs*, occurred very seldom. Thus from the 10th to the 79th trial, inclusive, rat 9 made 60 errors of entering **1** with only 11 entrances to all the other nine blind alleys; rat 11 from the 24th to the 83rd trials made corresponding errors of 47 to 15.

In the A-mazes *cul de sacs* **5** and **6** were likewise entered fewer times and eliminated more easily than **1**, **2**, and **3**, all of length equal to that of **5** and shorter than **6**. It is, of course, not contended here that the two sets of blind alleys compared are of equal difficulty in all respects other than that here considered. At the same time, they may be approximately equal; that is a matter which can be determined only empirically.

The accompanying table (table IV) shows that not only is the number of entrances to blind alleys first to be passed along the true path greater than that nearer the food box, but also that the percentage rate of elimination is greater in the latter. This is shown by comparing the number of entrances to the different groups of *cul de sacs* in question for different successive periods in the learning process from the first to the last trial. In the first five trials of all the animals, trained and untrained, the average number of entrances per trial into *cul de sacs* **1-4** of the B-mazes is twice that of entrances into **6-10**. Calling these numbers for the first period (the average of the 1st to the 5th trial) 100% each, to get a common basis for comparison, we find that there is a much more rapid percentage drop of elimination of entrances in the case of the blind alleys nearer the food box. Since the trained rats discontinued the experiment with the 25th trial without finishing the habit, the percentages for the two groups in the B-mazes are not correct after the 25th trial, though they are strictly comparable. An additional line is given, in the case of each of these groups, of the accurate percentages of elimination of entrances for the untrained rats (eight in each group) alone. It will be noted that in the case of the five *cul de sacs* nearest to the food box the percentage of elimination is considerably more rapid than in that of the first four blind alleys encountered. In the case of the A-mazes the percentage elimination is considerably greater for *cul de sacs* **5** and **6** than for **2** and **3**. Figures VIII and IX represent graphically the data of Table IV.

There is no room to doubt that the blind alleys first to be passed along the true paths in the mazes used are both more frequently entered and more slowly eliminated than are those further along the trail.

TABLE IV

	Periods of trials	1–5	6–15	16–25	26–35	36–45	46–55
Maze B	Blind alley **1**	134	210	166	84	75	71
	Blind alley **2**	147	76	68	39	33	24
	Blind alley **3**	79	11	5	6	2	7
	Blind alley **4**	89	16	19	8	6	6
	Totals	449	313	258	137	116	108
	Av. per trial	89.8	31.3	25.8	13.7	11.6	10.8
	Per cent, 24 rats	100.0	34.7	28.6	15.2	12.9	12.0
	Per cent, 16 untr'd rats	100.0	27.6	25.5	19.7	16.7	15.6
Maze B	Blind alley **6**	42	28	9	1	2	2
	Blind alley **7**	28	33	18	10	6	2
	Blind alley **8**	62	15	5	3	2	3
	Blind alley **9**	49	7	4	5	3	2
	Blind alley **10**	36	14	3	1	1	1
	Totals	217	97	39	20	14	10
	Av. per trial	43.7	9.7	3.9	2.0	1.4	1.0
	Per cent, 24 rats	100.0	21.3	9.0	4.6	3.2	2.3
	Per cent, 16 untr'd rats	100.0	15.6	6.8	4.0	2.8	2.0
Maze A	Blind alley **2**	84	65	28	8	3	1
	Blind alley **3**	51	73	49	25	13	3
	Totals	135	138	77	33	16	4
	Av. per trial	27.0	13.8	7.7	3.3	1.6	.4
	Per cent	100.0	46.9	26.5	11.2	5.6	1.4
Maze A	Blind alley **5**	32	21	2	1	1	3
	Blind alley **6**	23	9	0	2	0	1
	Totals	55	30	2	3	1	4
	Av. per trial	11.0	3.0	.2	.3	.1	.4
	Per cent	100.0	27.3	1.8	2.7	.9	3.6

TABLE IV—*Continued*

	Period of trials	56–65	66–75	76–85	86–95	96–105	106–115	116–125
Maze B	Blind alley **1**	52	36	26	8	7	6	0
	Blind alley **2**	15	10	8	7	2	3	3
	Blind alley **3**	1	2	1	2	1	0	0
	Blind alley **4**	6	2	4	2	0	0	0
	Totals	74	50	39	19	10	9	3
	Av. per trial	7.4	5.0	3.9	1.9	1.0	.9	.3
	Per cent, 24 rats	8.2	5.6	4.3	2.1	1.1	1.0	.3
	Per ct., 16 untr'd rats	10.6	7.2	5.6	2.7	1.4	1.3	.4
Maze B	Blind alley **6**	1	0	1	1	0	0	0
	Blind alley **7**	0	0	0	0	0	0	0
	Blind alley **8**	0	1	0	2	0	0	0
	Blind alley **9**	0	0	0	0	0	0	0
	Blind alley **10**	0	0	0	1	0	0	0
	Totals	1	1	1	4	0	0	0
	Av. per trial	.1	.1	.1	.4	0	0	0
	Per cent, 24 rats	.2	.2	.2	.9	0	0	0
	Per ct., 16 untr'd rats	.2	.2	.2	.8	0	0	0

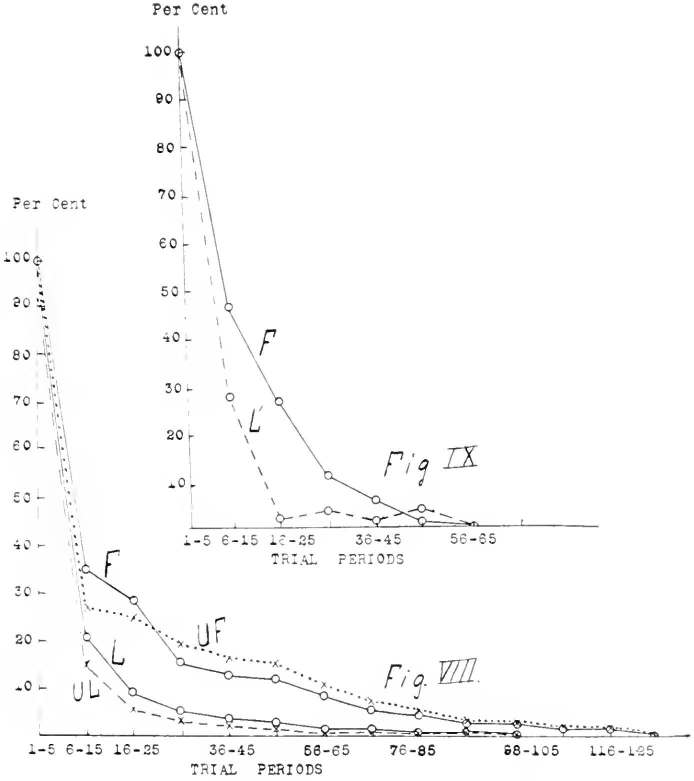
It may be that the odor of the food is a factor that at least partly explains the more easy elimination of the *cul de sacs* nearer the food box. However, there is very little, if any, real evidence that such is the case. A crucial test would be to use anosmic rats, though other means of controlling the odor factor are easily possible. Some facts in the present experiment count against the influence of odor as suggested. For example, errors of entrance into *cul de sac* **10** are nearly as numerous as those of entrance into **9**, although to get to **10** the animal had to pass a short alley of 8.5 inches leading directly into the food box. Moreover, all the rats, with occasional exceptions,[18] ran so rapidly after the first trial that it is improbable that food odor had any immediate direct influence in the behavior in the maze. There was no evidence in the behavior of the animals that they were attracted to the food box by such odors.[19] In the cases of supposed trailing, already noted, the animal which appeared

---

[18] Occasionally, without any apparent external condition to explain the behavior, an animal would sneak slowly and cautiously all the way through the maze. In a few cases such activity seemed to be due to recent fights with other rats or to noises from fights between other animals.

[19] An exception should be made here of the case of returns from *cul de sac* **8**, already discussed. The floor of the food box was covered with paper (double thickness) during the feeding each day, and during the experiment the food was kept in a dish in the extreme corner of the food box away from *cul de sacs* **8**, **9**, and **10**.

to be following a scent of any kind moved perceptibly more slowly, holding the nose continuously or frequently to the floor. The writer does not believe that the more rapid elimination of the *cul de sacs* nearer the food is to be explained on the basis of scenting the food. The matter, however, needs further test.



FIGURES VIII AND IX.—F and L show percentage elimination of all entrances to *cul de sacs* **1-4** and **6-10** combined, respectively, by twenty-four rats in the B-mazes; UF and UL the corresponding data for sixteen untrained rats. F′ and L′ show the percentage elimination, respectively, of all entrances to blind alleys **2** and **3**, and **5** and **6** combined, by twenty-two rats in the A-mazes.

Do pure probability laws govern the returns of the rat on emergence from blind alleys? In the tables of results (tables I and II) the totals of the first two trials have been kept separate so that the percentage of returns from blind alleys toward the starting place in the maze could be found for a period little influenced by the effects of training. The following table (table V) classifies for easy comparison the results of all the rats on the first two trials. The entrances to *cul de sac* **1** in the A-mazes are not included as all emergences from this blind alley brought the rat to the place of the entrance to the maze.

TABLE V

Full Length *Cul De Sacs*

Rats	Maze	Compl. Ent.		Half Ent.		Start. Ent.		% Ret. of all E's.
		Ent.	Ret.	Ent.	Ret.	Ent.	Ret.	
8 untr'd	B	170	76	35	6	54	16	38
4 tr'd	B	44	16	7	0	14	3	29
4 untr'd	A	51	24	7	4	46	16	42
7 tr'd	A	33	13	5	2	12	3	36
Average per cent returns								33.75

TABLE V—*Continued*

Shortened *Cul De Sacs*

Rats	Maze	Compl. Ent.		Half Ent.		Start. Ent.		% Ret. of all E's.
		Ent.	Ret.	Ent.	Ret.	Ent.	Ret.	
8 untr'd	B	182	72	27	11	48	18	39
4 tr'd	B	42	13	4	2	20	5	30
4 untr'd	A	23	10	6	3	12	0	32
7 tr'd	A	11	2	6	1	10	0	11
Average per cent returns								23

It will be noted that in the B-maze the per cent of returns from the shortened *cul de sacs* are practically equal to those from the full length ones, both for the trained and for the untrained rats. The returns for untrained rats are not far short of 50%. The shortage is mostly due, no doubt, to the small degree of learning that took place in the process of the first two trials, during which there was considerable random activity and reduction of excess movements. It would seem that at first—before any learning has taken place—the chance of a rat's returning on emergency from a blind alley is about one to one. There may be a greater tendency to go forward, keeping the general orientation rather than to return; if so, the excess forward tendency is but slight. The returns from *cul de sacs* first to be passed seem slightly to exceed in percentage those from blind alleys further on toward the food box. In the B-mazes the returns from *cul de sac* **1** (both full length and shortened) are 44% of the total number of entrances; the corresponding percentages for the other blind alleys in order from **2** to **10** are 55, 31, 32, 48, 33, 50, 34, 13, 33. These figures are taken, of course, only from the records of the untrained rats, sixteen altogether. Those most favorably situated for returns, so far as the rat's keeping the general direction on emergence from the blind alley is concerned, are **2** and **5**. This judgment is supported by the data. It is not clear why the returns from **7** should run so high. The percentage of returns by the eight untrained rats in the A-mazes are, for the 2nd to the 6th blind alley, in order: 36, 33, 67, 0, and 0. The large number for **4** was to be expected. The greater number of returns from the *cul de sacs* first encountered is likely due to the fact that the animals had already learned something of keeping the general orientation before the other blind alleys were entered.

In the B-mazes there appears to be a slight decrease in the returns of the first two trials by the trained rats as compared with the untrained. This seems to be due to a sort of "transfer of training." It is likely, as the writer suggested in the earlier article already referred to, due to a tendency of animals with experience in mazes to proceed with less whole-souled response into *cul de sacs*. Let us suppose that as an animal enters a *cul de sac* it also receives certain stimuli of various kinds from the true path from which it departs. These stimuli may produce a weak partial response, or tendency to response, which does not immediately fade away. If this tendency persists until the rat emerges from the *cul de sac* it will, of course, enhance the impulse to take the true path and thus increase the probability of continued forward movement. It is not inconceivable that a trained animal may have developed a habit of keeping the correct general orientation by some such means as this. Such habits would then have common factors for all *cul de sacs*, and in mazes of different kinds. It would seem, too, that on some such basis as this the returns would be eliminated more readily than entrances to the blind alleys, as has already been shown to be the case. This explanation may involve an interaction of sensory and motor impulses in the nerve fibres—each system, sensory and motor, interacting upon and stimulating the other—in such a manner as to make comprehensible how the effect of stimuli may be carried over into later responses and partly condition them as suggested below.

Possibly the animals also learn with training to utilize better such factors as vague visual stimuli of the closed end of the *cul de sac*. Certainly the speed of the rat running into the blind alley would make one cautious in assuming that such factors are explicitly reacted to by the animal. That there was a real transfer of some kind is, in any event, a conclusion which also finds support in the results of the A-mazes. For the full length and the shortened *cul de sacs*, the per cent returns for untrained rats are 42 and 32, respectively, agreeing rather closely with the B-maze results, whereas the corresponding percentage returns by the trained rats—seven in each A-maze—are 36 and 11, a decrease from that of the untrained animals of 14% for the full length and of 66% for the shortened *cul de sacs*. In the B-mazes the percentage returns from the full length *cul de sacs* by trained rats is 24% less than that by untrained rats; for the shortened blind alleys the percentage returns by the trained rats is 21% less than that by untrained rats.

THE SIGNIFICANCE OF THE RESULTS

It may be urged by the reader that the more rapid elimination of entrances to the shortened *cul de sacs* than to the full length ones is due to the fact that the rat, in the case of the short blind alleys at least, *sees* the closed end and thereby avoids entering so frequently, or so completely. In one sense this begs the whole question. Seeing is not some *thing* that stimulates or directs the animal; it is only a mode of being stimulated. Its possibility in the present study is not at all denied. The whole question with which we are concerned is: *How do all possible* *kinds of stimuli operate, directly or indirectly, toward the learning to avoid entering cul de sacs?*

That the rat is not wholly blind has been demonstrated in a number of cases,[20] but there is no clear evidence to show that the presence of such visual factors as are possible to the rat could operate on the principle of frequency, recency, or intensity, or all combined, in such a manner as to eliminate the impulses to enter the *cul de sacs* under the conditions of the present problem. They might, of course, aid the rat in getting to the food at any one time, but how could they operate toward cutting short the random processes in successive trials, *i.e.*, in bringing about what is called learning? A brief review of the work on visual controls in the rat's behavior is to be found in Miss Vincent's paper. Waugh found[21] that though the mouse could perceive the distance of objects "within a range of 15 cm.," it nevertheless seemed not to make use of the "visual perception of depth" in getting past two partitions each from opposite sides reaching half way across the problem box, the one being nearer than the other.

In the present experiment, it will be recalled, the interior of the maze was stained black, and even if it be granted that the rat could see the ends of some of the shortest *cul de sacs* there would be but little difference in the visual stimuli between the "blind" and the open alleys, in as much as both were obstructed alike in the further end and the side opening of the latter was not directly visible. Differences in brightness would be irregular and but slight, as the room was lighted from three sides—south, west, and slightly from the north—and an electric light was directly over the maze. It should be said that no difference in behavior between the rat blind in the left eye—No. 20—and the other rats was noticeable though the experimenter kept watch for such difference. More careful visual controls are of course desirable.

But the real question is how any stimulus, visual or otherwise, must operate together with other stimuli so as to inhibit unsuccessful acts and to cause to survive those acts which bring

---

[20] See Richardson, Florence. A Study of the Sensory Control of the White Rat. *Psychol. Mon.*, Ser. No. **48**, 1909. Vincent, Stella B. The White Rat and the Maze Problem—I. The Introduction of a Visual Control. *Jour. Animal Behav.*, 19 15, **5**, 1-24.

[21] Waugh, K. T. The Rôle of Vision in the Mental Life of the Mouse. *Jour. Comp. Neurol. and Psychol.*, 1910, **20**, 549-599.

success, in this case those acts which bring the animal to the food box. The results follow the series of stimuli and responses which take the animal through the maze. How can the result work backwards? The writer believes that in the foregoing pages he has presented plausible reasons and data to show the absolute inadequacy of frequency and recency laws as the directing factors in maze learning. Frequency fails to give any basis not only for this kind of learning in general but particularly for the specific kinds of results obtained in the experiments considered. In a complex situation like this, frequency explains only how within a certain probability the rat will finally reach the food, but it fails to explain why subsequent trials should be improvements on the first one. It is not clear how recency, as ordinarily understood, can aid the learning. The principle of intensity needs re-interpretation. When several stimuli act on an animal bringing about a series of responses as in this case, the final one of which is the successful one, it appears that somehow, not well understood yet, the various effects of these stimuli hold over into that of the final stimulus and that all together simultaneously act to direct the energy of the animal into the most consistent channels. In the large, these channels offer the least resistance and afford the most complete response. It is in this sense that the successful acts are more intense than others, and thus their effect is greater toward shaping the neural pathways for their repetition and for the gradual elimination of the more inconsistent and tentative responses leading up to them. On this assumption it becomes somewhat comprehensible why the maze is learned to a large extent "as a whole," so that small errors may throw the animal out completely, or at some other part of the maze, when the habit is nearly perfected. The specific results of the present experiment are also intelligible. These various hold-over effects in the extero- and the proprio-ceptive systems afford the basis of imagery in human behavior, and supply the " large situation " to which one reacts ideally. They may function, so far as we can know, wholly unconsciously or with but vague consciousness in the case of the rat. In the human being habits of responding to separate groupings of these factors may be acquired, and such exciting factors may be aroused indirectly by association. Nothing is gained in psychological explanation by assuming " ideas " to explain behavior, unless in such cases we understand how the ideal dispositions themselves are acquired. The use of the term *idea* in the higher forms of behavior is justified then only on the basis of simplicity of statement. There is none but questionable evidence thus far that ideational behavior is different in any way but degree from sensori-motor, or the well known trial and error, behavior. "Ideas" can function only when the somewhat detachable dispositions, of which they are the imperfect, subjective aspects, have been built up by experience, and such dispositions require a rather complex nervous mechanism. It is needless to say that no evidence of ideational behavior has been found in the white rat. While, as has been pointed out in the foregoing, there are likely some hold-over effects of stimuli in the case of the rat, these likely operate more or less mechanically and *en masse* so that the animal enjoys little independence of action and is subject rather completely to the dominance of the group of stimuli present or immediately past. That is to say, the animal can respond only to present situations though with a considerable number of random variations, until the most consistent responses to that situation have fixed themselves to the exclusion of all others, after many repetitions of trials. Then the response becomes uniform and mechanical to a high degree.

The more advanced behavior as we see it in the case of man—ideational behavior—differs from the lower forms illustrated in the present study in that it is less fixed and less dependent upon immediate situations. Stimulus-response organizations, or tendencies, are more detachable in their separate smaller functional components; and the latter have richer possibilities of combinations among themselves, on the one hand, and on the other there is less dependence for their functioning upon direct or immediate stimulation. Various indirect and vicarious stimuli come to serve adequately. Thus various systems of stimulus-response mechanisms may become organized into inconceivably complex relationships about certain symbolic stimuli, such as written or spoken words, various kinds of gestures and attitudes of the stimulating individuals, associated objects, sounds, contacts, and so on. It then becomes practically impossible to predict which of the various aspects of the situation will succeed in calling out its particular response. We shall not here enter into further consideration of this complex behavior, except to point out that when the various stimulus-response mechanisms have become sufficiently well associated with certain muscular strains or neural excitations, the revival of the latter by favorable stimuli will call out the acts themselves. Thus a stimulus may have entirely ceased to play upon the sense organs from without and long periods of time may have elapsed, and yet, because of this acquired organization, the recurrence of any significant aspect of the outer situation, even such as a sound associated with it, may revive the crucial excitation and thus call out the act. Something of this kind—stimulated, however, by the original situation minus the light when the animal is allowed to respond—likely takes place in the delayed reactions of animals, though this assumption leaves entirely open the question as to whether or not the animals *have ideas*, a rather infertile question for science, it must be confessed. More elaborate systems of acquired associations make possible the continual thinking of absent situations which we *know* that *we ourselves* experience. In these more advanced forms of behavior groups of response systems may come so to interact upon one another by associations and by stimulation from the inward bodily conditions that rehearsal of a problem mentally may take place long after actual practice has ceased, thus changing behavior materially between practices. It is yet questionable whether there are any such cases in animal behavior.[22]

In the foregoing pages we have called into question the principles of frequency, recency, and intensity of stimulation as usually understood in relation to the fixing of associations, so far as their value in explaining learning is concerned. They do not seem to account for the *change* in successive trials called learning. This seems to be true at any rate for maze learning; probably it holds for all kinds of learning. All that these factors do is, likely, to make more and more easy *any* associations and acts brought about by the real directing factors. That is, they tend to fix any series of acts in the order that they are gone through, *not to change the order of the acts.* Some other directing factors and some *vis a tergo* must be found to account

---

[22] Cf. Yerkes, R. M. The Mental Life of Monkeys and Apes: A Study of Ideational Behavior. *Behav. Mon.*, 1916, 3, No. 1. Yerkes thinks Julius, an orang-utan, solved a problem ideationally; see particularly pp. 68 and 131.

for the changes in behavior which gradually make response more and more direct and which gradually eliminates the useless random acts. We must not forget that the numerous internal life processes, *e.g.*, the contractions of the muscles of the stomach with hunger, serve as the motivation to activity. They determine the stimulating value, as do also modifications in the proprio-ceptive system by past behavior, of various outside factors. The organism continues to respond by varying behavior until successes are attained which modify these internal conditions and change the inner motivating factors. But the failures also change the organism. The directing factors of the response seem to be the inner organic processes and the total combination of stimuli from external conditions and from muscular contractions, all these overlapping in their several effects as has been suggested. The neural channels involved in the most consistent acts become the most operative through the compelling effects of all these factors, and these acts, or directions of response, in time survive over all others and gradually acquire an ease and automaticity of functioning characteristic of habits. The stimuli to action even in as simple an organism as a rat are infinitely more complex than usually imagined in our "neural explanations." Mere contingency in the combinations of acts of a rat brought about in the maze, or in other problem boxes, for that matter, cannot be regarded as the important factor that it has sometimes been supposed to be. It is true that some useless acts may occasionally survive with the more consistent ones by chance associations, but such acts are really not vital parts of the system of learned acts.

The precise nature of the hold-over effects of various stimuli posited in the explanations of learning here suggested must be left to physiology and neurology. There is undoubtedly a close connection between sensory and motor impulses. Sensory stimuli bring about responses which in their different stages of expression set up new afferent impulses, or either facilitate or tend to inhibit old ones; these again modify the motor tendencies. We are a long way yet from a satisfactory knowledge of nerve impulses and their effects upon one another,—Are they periodic or continuous? What relations obtain between stimulus changes and nerve impulse changes? What is the nature of inhibition and of facilitation? These and many other problems not yet solved have important bearings upon our knowledge of the learning act. But psychologists cannot wait for the solution of these problems before attempting to formulate more satisfactory conceptions of the processes with which they must deal at every turn. It must be apparent that chaos now reigns with respect to this matter. Some writers invoke *imitation* to explain most modifications in behavior; others use *pleasure and pain* for the same purpose; while *ideas*, *purposes*, the *effects of random acts*, and so on, are freely used directly or indirectly by most writers. All of these factors may have real parts to play in the learning process, in some one or more of its various aspects, but they are all more or less vaguely conceived and frequently erroneously referred to, almost as some sort of original or spontaneous causes, rather than complex aspects of the very thing that is to be better understood and analyzed. Popular, educational, and sociological writers may be forgiven for their own sins in this particular so long as psychologists have nothing more satisfactory to offer than at present. The great problem of how learning takes place is yet largely unsolved.

For the best progress, experiments in behavior modification must go hand in hand with physiological investigations into the nature of the nerve impulse. A few rather suggestive studies have been carried out by psychologists upon the mutual effects of successive acts on one another. It appears that while one particular kind of act is being learned a second contrary one is inhibited by it more than after the first has been completed.[23] The extensive investigations of Professor T. G. Brown,[24] on the physiological side, have shown a summation of successive liminal stimuli (facilitation) of intervals up to about ten seconds. Such neural overlapping effects may well function to bring about a

---

[23] Pillsbury, discussing experiments on associative inhibition by Müller and Schumann (1891), concludes that "where several things are to be learned in the same connection, it is found that inhibition ceases to be effective if the first is thoroughly learned before the second is begun." *Fundamentals of Psychology*, 1916, p. 359. See also p. 365. Especially interesting in this regard is a study recently reported by Hunter,—Hunter, W. S., and Yarbrough, Jos. U. The Interference of Auditory Habits in the White Rat. *Jour. Animal Behav.*, 1917, **7**, 49-65. See especially pages 60 ff. One must be careful not to generalize too much from these experiments on contrary acts.

[24] Brown, T. G. On the Phenomenon of Facilitation. 1. Its Occurrence in Reactions Induced by Stimulation of the "Motor" Cortex of the Cerebrum in Monkeys. *Quart. Jour. of Exper. Physiol.*, 1915, **9**, 81-99. Other articles by the same authority in the same journal.

simultaneous operation to some extent of the various experiences that a rat has in finding the food box in the maze. In many types of learning we have been much in the dark as to how later *effects* of the successful results could work back and stamp in these successful acts to the exclusion of the various unsuccessful ones. By the conception of the overlapping of effects of successive nerve functionings may we not be getting a start in the right direction?

DISCUSSION

Peckstein[25] has recently tried to explain the transfer effects found in his experiments on the basis of factors which the writer finds extremely vague, subjective, and otherwise technically objectionable. The *general* factors of his explanation are: (1) "General maze habits"—reduction of tendency to return, knowledge of the nature of errors, improved sense of direction; (2) "consciousness of power;" and (3) "proper emotional attitude." Specific factors are such as common specific identities, or near-identities, in the different mazes. We are told that return is due to the general "dominance of the familiar." "The return pathway is known to be safe. The rats seem natively inclined to return to the closed entrance." This return tendency—due to knowledge or instinct?—is actually inhibited by any maze for any other. The knowledge of the nature of errors is a "concept," we are told, developed in the earlier sections of the total maze. A *cul de sac* "ceases to be a detail that must be cautiously explored," and "comes to mean a detail that must be left as soon as possible." At first—now we are at the "sense of direction" factor—some learners "have almost a 'going ahead' instinct," while others have a greater tendency to return. This latter tendency is gradually overcome. This seems, then, only to be another name for the factor mentioned under "returns." "In subsequent mazes, the truly sophisticated learner will enter the *cul de sac*, but will proceed along the forward pathway when he returns to the true course."

The "consciousness of power" in the rats seems to manifest itself, after all, in some objective behavior change, such as increased activity. "In subsequent mazes, however, the consciousness of power is clearly seen (!). No 'warming-up' period

---

[25] *Op. cit.*, pp. 50-54.

is needed. There is no delay at the entrance. Work has come to mean invariable accomplishment and reward. The entire attack upon the new problem is aggressive. The learner has learned to do by doing." The proper emotional attitude, the last of the general factors, means the overcoming of an attitude complex, "a mixture of fear, indecision, curiosity, and perhaps anger." All this after some really valuable experiments! Surely this is only a complication in subjective terms of *facts to be accounted for*, which facts practically all authorities are willing to accept more or less completely. These "factors" of transfer do not take us anywhere.

Dr. Peckstein finds[26] that the difficultness of mazes is not proportional to their lengths, nor to the number of their blind alleys. But, why should it be? As to the number of *cul de sacs*, it is obvious that if *at first* a rat tends in its "choices" at bifurcations to follow chance laws—and our present results point that way—difficultness ought to increase on some other principle. The probability of passing any single *cul de sac* successfully is 3/4, as Watson has pointed out, i. e., if we mean by "successfully" that the animal either goes on in the correct path, or, if it enters the blind alley, that on emergence from it it keeps the general forward direction. The chance of passing two blind alleys is therefore 9/16 (= 3/4 x 3/4); that of passing three *cul de sacs*, $(3/4)^3$; and so on. On this basis the chance of getting through one unit of Peckstein's maze—3 blind alleys—is $(3/4)^3$, while that of getting through the entire maze without a return is $(3/4)^{12}$. In the former case the probability of a success without returns is therefore over thirteen times that in the latter. Complications from returns at any *cul de sac* will be brought about by additions of other forward movements beyond the point to which return is made, but each of these additional forward runs may again be assumed to follow, before any training sets in, the same probability law at each *cul de sac* that is followed in the original run. Thus the above calculation may stand roughly as approximately correct. Its results—a difficultness of the whole maze of over thirteen times that of the quarter maze—agrees more closely with Peckstein's actual results, as estimated by him, than do those based on the assumption of a direct proportionate increase in difficultness

---

[26] *Ibid.*, pp. 55-57.

with distance through the maze and number of *cul de sacs*. Peckstein found the whole maze over twenty times more difficult than the average of the four quarter mazes, whereas such a direct proportionate increase of difficultness as he assumed should make it but four times more difficult. Of course, many other factors in any such calculations must be taken into consideration. With Peckstein's rats the relative degrees of difficultness of the four sections of the maze were found, in order from the first to the fourth quarter, to be 15, 1, 3, 2, as determined by his combined trials-time-error formula. This does not look much like equality. The excess-distance run by the rats is certainly a factor as well worth while as any to consider. Why was it not included?

So far, then, Dr. Peckstein's results, as presented by himself, seem roughly to agree with our own in supporting the view that mere probability laws account for the original " choices " of the untrained rat at the several bifurcations in the maze. This is our own interpretation of his data, not his, it should be stated. He merely seems to hold that there is some law of diminishing returns, which he does not clearly state, that determines the degree of energy expended for the learning of mazes of varying complexity.

Dr. G. V. Hamilton in his interesting study of perseverance reactions, by the multiple choice method as he has developed it, finds frequency and recency of an advantageous response more strongly effective toward learning than either frequency or recency with no advantage or with actual disadvantage.[27]. He finds that frequency with invariable advantage is stronger in effect toward the building of a habit than even greater frequency without an invariable advantage. But, as in most experiments on learning, he has his conditions so arranged that the animal can end up only with the " successful " act.[28] E. g., Hamilton says: " During the twenty habit forming trials under discussion she [Rat No. 1] manifested only three recency first choices, but after these trials [that is, when the habit was learned] *during which the operation of the factor of recency was invariably advantageous* she manifested 100% of recency first choices."[29]

---

[27] A Study of Perseverance Reactions in Primates and Rodents. *Behav. Mon.*, Ser. No. 13, 1916.

[28] *Ibid.*, pp. 38-46.

[29] *Ibid.*, p. 40.

How could she do otherwise after learning is accomplished? Here the success of the final act was inevitable by the conditions of the experiment. The final high degree of success is the *result* of the learning, how, then, can it get around to come in at the front door as one of the causative factors?

SUMMARY AND CONCLUSIONS

The "principles of learning" frequency, recency, and intensity, in their usually accepted meaning, have been found inadequate to account for learning in the maze. Probability laws alone make possible a sufficient number of right choices for the rat to reach the food box finally in the ordinary maze. The probability of reaching the food box by mere chance rapidly decreases with the increase in the number of *cul de sacs* in the maze. But it is found that on laws of pure fortuity there is no explanation for the *elimination* of *cul de sacs*; for since the probability of entering any blind alley on returns as well as on forward runs is 1/2, the habit of continuing to enter them should be as strong as that of keeping the right trail toward the food box. For learning to be possible, some sort of short-circuiting process must take place by which the true path may be suggested for the line of action when the animal gets to the entrance of any blind alley. It is not clear how any of the usually accepted laws of learning—frequency, recency, and intensity—can operate to bring this about. Frequency and recency fail entirely to account for the behavior of the rat in the maze. The real process of learning, the gradual elimination of unsuccessful random acts, such as entrances to *cul de sacs* and returns toward the entrance place in the maze, must be accounted for on the basis of some entirely different principle. The principles named show only how an act, directed by some other factor, becomes gradually more mechanically reflex.[30]

---

[30] Statistically the statement in this paragraph, as well as the one in the first part of the monograph, is inaccurate. An animal coming the first time to a blind alley has a probability of 1/2 of entering it; a probability of 3/4 of continuing in the right direction, whether or not the blind alley is entered; and a probability of 1/4 of entering the CUL DE SAC and, from it, returning toward the starting place in the maze. If the animal actually gets by the blind alley in question, enters one farther on and returns in the maze, the conditions are reversed at the first blind alley. Now the probability of continuing back to the starting place in the maze, i.e., either of not entering the blind alley at all or of entering and then continuing in the return direction, is 3/4; that of getting reöriented in the right direction toward the food is 1/4; and that of entering the blind alley is 1/2. Adding these fractions to those above for the respective directions in which the animal can possibly go

The present experiment was devised to present conditions which might test the efficiency of the "completeness of response" theory outlined recently by the writer, suggesting a means of learning based on the overlapping and thereby simultaneously operative effects of successive stimuli. Identical mazes were used for separate groups of animals, but they were so arranged that their several *cul de sacs* could be conveniently varied in length. By this means control groups of rats were run in mazes differing only in the relative lengths of their *cul de sacs*, certain of these being long for one group and short for the other. Two modifications of such differences were used; the one pair of control groups having all the blind alleys short in the one maze and all long in the other, while the other pair of controls each had some long and some short blind alleys making the total length of blind alleys equal for both members of the pair. In all, twenty-four rats were used. Groups of rats were interchanged in the mazes, after the first problems were completed, so that each problem was tried both by trained and by untrained animals.

Detailed records were kept of the behavior of the animals. Complete entrances, half way entrances, and beginning entrances to *cul de sacs* were indicated; complete returns and returns to blind alleys already passed were noted; and the direction of the rat's movement on emerging from blind alleys, whether forward or back, were recorded. The exact time for each trial was kept but not used in the present report.

---

we have: forward from the blind alley $3/4 + 1/4 = 1$; into the blind alley $1/2 + 1/2 = 1$; and return toward the starting place in the maze $1/4 + 3/4 = 1$. This gives equal exercise to the acts in all these three directions, on pure probability laws, when returns are considered. But the animal is taken out of the maze only at the food end of the trail; hence for each trial there must be one more forward run at any given place in the maze than return runs. This gives the forward direction an advantage statistically of $1/4$ runs for each trial over the entrance to the *cul de sac* and of $1/2$ over returns. This advantage is proportionately small where many returns are made, as near the maze entrance, and large where this is not the case, as near the food and later in the learning process at any given point. No returns will be made to the last *cul de sac*, and when entrance to it has been elimnated none will be made to the one next to it; and so on. This condition, then, affords a fine theoretical basis for explaining learning in the maze, and also for the backward elimination of errors of entrance to blind alleys. There are, however, serious flaws in this argument when given in support of frequency, either alone or combined with recency, as the only principle operative in the learning of the maze. Frequency and recency factors really operate against this explanation, rather than in favor of it; for they favor the mere repetition of the choices first made at any of these critical positions in the maze, and therefore the strengthening of the impulses to enter blind alleys rather than their weakening. The force of this point will be shown concretely in the paper now in preparation.

1. The decrease in the percentage of returns by the animal emerging from blind alleys is very rapid in the early part of the learning, and as a rule the rat continues to enter blind alleys, even to their full length, long after returns are discontinued; i. e., the curve of returns from blind alleys drops much more rapidly than that representing the number of entrances to blind alleys. These returns persist longer in the case of *cul de sacs* encountered along the first part of the correct path than in that of those nearer the food box.

2. The elimination of entrances to blind alleys does not come about mainly by a decrease in the *number* of entrances, but principally, especially in the case of the longer *cul de sacs*, by a gradual decrease in the *degree*, or the distance, of entrance. Just before entrance is eliminated completely, there frequently occurs a peculiar and very rapid vibration of the rat's head between the direction of the true path and that of the tempting blind alley. Frequently, after the first success in passing any such blind alley, the rat runs headlong into some *cul de sac* farther along the correct trail, which it had previously learned to avoid. These and other facts of similar import indicate that the maze is learned "as a whole" to a large extent, and that entrances to blind alleys are not properly to be regarded as *separate acts*, as is frequently done in speculations on learning.

3. Entrances to short *cul de sacs* are eliminated more readily, other things equal, than entrances to long ones. Not only are the total entrances to the short blind alleys fewer than to the longer ones, but the percentage elimination of them is greater. The curve of decrease of entrances drops more rapidly in the case of short than in that of long *cul de sacs*.

4. Blind alleys first to be passed along the true path are entered more frequently than those further along—nearer the food box—and their percentage rate of elimination is less. That is, entrances to the first *cul de sacs* encountered are more persistent, harder to overcome, than those to *cul de sacs* nearer the food box.

5. With untrained rats the number of returns toward the entrance door in the maze, on the rats' emergence from blind alleys, nearly, if not quite, equals in the beginning of the experiment the number of cases of keeping the general forward direction toward the food box. It appears that at the beginning stage of learning in the maze mere probability determines whether

the animal goes forward or back on emerging from a blind alley. In this respect, however, as in many others, there are rather large individual differences. From the very first experience in the maze, and this makes the test of the probability law rather difficult, the learning factors enter in and rapidly decrease the returns in favor of the general forward orientation.

6. It has been found desirable in work of this kind to study individual reactions in detail. Mere averages do not show the significant aspects of the behavior in many cases. A detailed report of individual "choices" in the maze, by a method which promises to be fruitful, is being prepared to justify further the statement in the present paper regarding the inadequacy of frequency and recency laws as explanations of the rat's maze-learning.

7. Responses to stimuli cannot take place instantaneously, neither do stimulation effects fade away momentarily. Besides this, response tendencies and muscular strains, maintained for a shorter or longer time, constantly set up new sensory impulses (proprio-ceptive stimuli) which again stimulate reactions. It is suggested that by such means as these, and possibly by others not yet known, the effects of successive stimuli, such as an animal encounters in getting through a problem box to food, operate in a measure simultaneously, and the resulting response is on the whole the most consistent or complete one under the whole circumstance. The channels to this most complete response are gradually forced most open or permeable; their greater consistency of operation (facilitation) brings about an intensity of activity through them which in repeated trials gradually short-circuits through the infinitely numerous pathways involved and thus brings about the gradual elimination of useless random acts. It is suggested that learning comes about by this means. It is hoped that this suggestion may be fruitful toward an understanding of how a final success can operate back (as it appears externally to do) upon the random acts leading to it so as gradually to bring about their elimination. This is the theory which the writer has called the "completeness of response" principle in learning, and it seems to him to account for the results obtained in the present experiment as well as for others which have been uncritically attributed to the *stamping-in effects of pleasantness.*

The Behavior Monographs

Edited by JOHN B. WATSON

The Johns Hopkins University, Baltimore, Md.

VOLUME 1

No. 1 The development of certain instincts and habits in chicks. By Frederick S. Breed. Pp. iv + 78, $1.00, postpaid.

No. 2 Methods of studying vision in animals. By Robert M. Yerkes and John B. Watson. Pp. iv + 90, $1.25, postpaid.

No. 3 An experimental study on the death-feigning of Belostoma (–Zaitha Aucct.) flumineum Say and Nepa apiculata Uhler. By Henry H. P. Severin and Harry C. Severin. Pp. iii + 47, $.65, postpaid.

No. 4 The biology of Physa. By Jean Dawson. Pp. iii + 120, $1.50, postpaid.

No. 5 The function of the vibrissae in the behavior of the white rat. By Stella Burnham Vincent. Pp. iv + 81, $1.15, postpaid.

VOLUME 2

No. 6 The delayed reaction in animals and children. By Walter S. Hunter. Pp. v + 86, $1.15, postpaid.

No. 7 The Canada porcupine: a study of the learning process. By Leroy Walter Sackett. Pp. iii + 84, $1.15, postpaid.

No. 8 Audition and habit formation in the dog. By Harry Miles Johnson. Pp. iv + 78, $1.00, postpaid.

No. 9 Habit formation in a strain of albino rats of less than normal brain weight. By Gardner Cheney Basset. Pp. iv + 46, $.65, postpaid.

No. 10 Distribution of effort in learning in the white rat. By John Linck Ulrich. Pp. iii + 51, $.65, postpaid.

No. 11 The effect of age on habit formation in the albino rat. By Helen B. Hubbert. Pp. v + 55, $.65, postpaid.

VOLUME 3

No. 12 The mental life of monkeys and apes: a study of ideational behavior. By Robert M. Yerkes. Pp. iv + 145, $1.50, postpaid.

No. 13 A study of perseverance reactions in primates and rodents. By G. V. Hamilton. Pp. iv + 65. $.75, postpaid.

No. 14 An analysis of the learning process in the snail, *physa gyrina* Say. By Elizabeth Lockwood Thompson Pp. iii + 97, $1.25, postpaid.

No. 15 The effect of length of blind alleys on maze learning: An experiment on twenty-four white rats. By Joseph Peterson. Pp. iii + 53, $.75, postpaid.

Regular subscription $5 per volume, postpaid.

THE BEHAVIOR MONOGRAPHS
Emerson Hall, Cambridge, Mass.

Directory of American Psychological Periodicals

American Journal of Psychology—Worcester, Mass.: Florence Chandler.
Subscription $5. 600 pages annually. Edited by G. Stanley Hall.
Quarterly. General and experimental psychology. Founded 1887.

Pedagogical Seminary—Worcester, Mass.: Florence Chandler.
Subscription $5. 575 pages annually. Edited by G. Stanley Hall.
Quarterly. Pedagogy and educational psychology. Founded 1891.

Psychological Review—Princeton, N. J.: Psychological Review Company.
Subscription (with Psychological Bulletin) $6. 480 pages annually.
Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin—Princeton, N. J.: Psychological Review Company.
Subscription $3.25. 480 pages annually. Psychological literature.
Monthly. Founded 1904. Edited by Shepherd I. Franz.

Psychological Monographs—Princeton, N. J.: Psychological Review Company
Subscription $4 per vol. 500 pages. Founded 1895. Ed. by James R. Angell.
Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.: Psychological Review Company.
Subscription $1.25 200 pages. Founded 1895. Edited by Madison Bentley.
An annual bibliography of psychological literature.

Journal of Philosophy, Psychology and Scientific Methods—New York:
Sub-Station 84. Bi-weekly. 728 pages per volume. Founded 1904.
Subscription $3. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Substation 84, New York: Archives of Psychology.
Subscription $5. 600 pp. ann. Founded 1906. Edited by R. S. Woodworth.
Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology—Boston: Richard G. Badger.
Subscription $4. 480 pages annually. Edited by Morton Prince.
Bi-monthly. Founded 1906. Entire field of abnormal psychology.

Psychological Clinic—Philadelphia: Psychological Clinic Press.
Subscription $1.50. 280 pages annually. Edited by Lightner Witmer.
Monthly (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Training School Bulletin—Vineland, N. J.: The Training School.
Subscription $1. 160 pages annually. Ed. by E. R. Johnstone. Founded 1904.
Monthly (10 numbers). Psychology and training of defectives.

Journal of Race Development—Worcester, Mass.: Louis N. Wilson.
Subscription $3 460 pages annually. Founded 1910.
Quarterly. Edited by George H. Blakeslee and G. Stanley Hall.

Journal of Educational Psychology—Baltimore: Warwick & York.
Subscription $3. 600 pages annually. Founded 1910.
Monthly (10 numbers). Managing Editor, J. Carleton Bell.
(Educational Psychology Monographs. Edited by Guy M. Whipple.
Published separately at varying prices. Same publishers.)

Journal of Animal Behavior—Cambridge, Mass.: Emerson Hall.
Subscription $5. 450 pages annually. Founded 1911.
Bi-monthly. Robert M. Yerkes, Managing Editor.

The Behavior Monographs—Cambridge, Mass.: Emerson Hall.
Subscription $5. 450 pages per volume. Edited by John B. Watson.
Published without fixed dates, each number a single research.

Psychoanalytic Review—New York: 64 West 56th Street.
Subscription $5. 500 pages annually. Psychoanalysis.
Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J.: Psychological Review
Company. 480 pages annually. Experimental. Founded 1916.
Subscription $3.25 Bi-monthly. Edited by John B. Watson.

Journal of Applied Psychology—Worcester, Mass.: Florence Chandler.
Subscription $4. 400 pages annually. Quarterly. Founded 1917.
Edited by G. Stanley Hall, John Wallace Baird and L. R. Geissler.

The Brandow Printing Company
Albany, N. Y

BEHAVIOR MONOGRAPHS

Volume 3, Number 5, 1919 Serial Number 16

Edited by JOHN B. WATSON
The Johns Hopkins University

An Experimental Study of Transfer of Response in the White Rat

BY

HARRY H. WYLIE

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

[*Publication temporarily suspended*]

The Journal of Animal Behavior

An organ for the publication of studies concerning the instincts, habits and intelligence of organisms

The Journal contains a Department of Notes in which appear brief accounts of especially interesting and valuable observations of behavior.

Published bi-monthly at Cambridge, Boston, Mass., by Henry Holt and Company, New York. Each volume contains at least 450 pages with plates and text-figures.

The subscription price is $5.00 per volume (foreign subscription $5.50) postpaid.

Manuscripts, subscriptions, and all business correspondence should be addressed to

THE JOURNAL OF ANIMAL BEHAVIOR
Emerson Hall, Cambridge, Massachusetts

Behavior Monographs

For the publication of studies in behavior and intelligence which are too lengthy or too expensive for acceptance by The Journal of Animal Behavior

Published at irregular intervals at Cambridge, Boston, Mass., in connection with the Journal of Animal Behavior, by Henry Holt and Company, New York. Each volume contains approximately 450 pages with plates and text-figures.

The subscription price is $5.00 per volume (foreign subscription $5.50) postpaid.

Monographs may be purchased separately at prices varying with the cost of manufacture.

Manuscripts and inquiries concerning terms of publication should be addressed to the Editor of the Behavior Monographs,

JOHN B. WATSON,
The Johns Hopkins University,
Baltimore, Md.

Subscription to The Journal of Animal Behavior and the Behavior Monographs should be sent to Emerson Hall, Cambridge, Massachusetts.

BEHAVIOR MONOGRAPHS

Volume 3, Number 5, 1919 Serial Number 16

Edited by JOHN B. WATSON
The Johns Hopkins University

An Experimental Study of Transfer of Response in the White Rat

BY

HARRY H. WYLIE

Published
at Cambridge, Boston, Mass.
HENRY HOLT & COMPANY
34 West 33d Street, New York
G. E. STECHERT & CO., London, Paris and Leipzig, Foreign Agents

ACKNOWLEDGMENT

As in every work of this sort, the real source of inspiration, of guidance and of criticism lies in the Instructors of the Department in which the work was done. In this particular case, the author wishes to acknowledge his indebtedness and gratitude to Professor Angell and Dr. Carr. Their helpfulness has extended far beyond the immediate assistance given in this thesis. No one can be under their tutelage without being influenced for the better by their ideals and practice. In particular, Dr. Carr's constant and patient oversight and criticism was a large element in carrying this present work to a successful issue.

Acknowledgment is also due to Mrs. Wylie for assistance in performing the experiments and in preparation of the manuscript.

HARRY H. WYLIE

TABLE OF CONTENTS

SECTION ONE

INTRODUCTION

The experiments reported in this paper were inspired by the conviction that almost all of the work previously done on the problem of the transfer of response or the transfer of learning lacked exact definition and control of the factors involved. But in spite of this lack two generally accepted conclusions have been fairly well established: (1) Transfer, *negative* as well as *positive*, in varying degrees has been found; (2) A satisfactory theory of explanation must find its basis in the " something in common " or the " identical elements " in the problems or situations between which the transfer occurs. However, to get at the actual conditions under which transfer, both positive and negative, takes place, and to state the explanatory principle in a definite and usable way, it is necessary *to define the two problems between which transference is tested accurately and completely in terms of each other as to their similarities and differences.* One can then find by concomitant variation what factors and conditions are correlated with positive and negative transfer and the relative influence of these factors and conditions upon one another. This, of course, has been attempted. One needs but to glance at Coover's(1) whole page of " identical elements " to be convinced of this. But to facilitate this analysis two conditions are favorable: (1) Simplicity of problems. This, too, has been attempted. It is the keynote of most attacks on the problem. Instead of trying the effect of a certain course of study upon practical success in some chosen business or profession, the experimentation has taken up simple cases of memory, discrimination, etc. But these are not sufficiently simple. One element of complexity is the central or subjective factors involved in what goes under the names " attention," " general ideas," " ideals," etc. Hence comes our second condition suited to facilitate analysis,—i. e. (2) the reduction of the problem to perceptible and definitely measurable objective terms—*sensory* stimulus and *motor* response. This condition has not been obtained and can not easily be obtained with human subjects. It can presumably be best obtained with animals. Without attempting any exhaustive survey of cases, let us notice briefly the problem as studied in animal psychology.

It is well to bear in mind at this point that whatever study has been given this problem up to the present time by those experimenting with animals has been largely of an incidental character. Yet the incidental results have often been interesting and suggestive.

The problem has presented itself to most of the animal psychologists in this form: What is the effect of previous habits upon the formation of new habits? Yerkes(2) study of the Dancing Mouse has furnished some data bearing on the question as stated. Animals which had not been previously trained on a simple labyrinth (C) succeeded in making the trip correctly for the first time on the average after 19.7 trials. Animals which had previously learned the labyrinth (B) succeeded in making the first correct trip in (C) after 7 trials. The learning of (B) without previous training required 8.2 trials. After previous training on (C), the learning of (B) required 5 trials. Thorndike(3) reports in his experiments with cats in problem boxes, that " previous experience makes a difference in the quickness with which the cat forms the associations. After getting out of six or eight boxes by different sorts of acts the cat's general tendency to claw at loose objects within the box is strengthened and its tendency to squeeze through holes and bite bars is weakened; accordingly it will learn associations along the general line of the old more quickly." He cites three animals, Nos. 10, 11, 12, which had learned the problem box A (0 at front) and were afterwards taught the problem box B1 (0 at back). He remarks, " It naturally takes a cat much longer time to accidentally claw a loop in the back than in the front, yet a comparison of these curves with those—shows the opposite to have been the case with 10, 11 and 12." A parallel series of controls is needed here, however, to make sure the statement. Richardson's(4) experiments on white rats also furnish some data. Two sets of rats were obliged to learn to respond correctly to box III. One set had previously learned to respond correctly to boxes I and II. The curves for the two sets show a decided advantage for the trained rats. Yoakum's(5) experiments with squirrels show that training is sometimes a disadvantage. Thus animals trained on box I and box II took a longer time to learn to open box III than animals which had had no training with boxes I and II. The responses required in the first two boxes, such as scraping, butting up the latch with the nose, were carried over to box III where a different response was required. Hunter(6) offers two sets of experiments bearing on this question, only one of which will be mentioned here, the other being discussed later. In his work on pigeons he found that there was no difference as to number of trials required for trained and untrained animals in learning a given maze where training had been on another maze. The excess time and errors during early trials were greater for the trained, yet the excess effort was eliminated in a smaller number of trials by the trained.

Bogardus and Henke(7) also made some tests upon the question of transfer in the case of rats learning a maze. The animals were taught a certain maze. Then by use of doors for blocking the true pathway at certain points and for opening up the pathway at new points, the correct pathway was altered in certain respects. Then the animals were taught to run the new pathway. An effort was made to make the maze different, each time any alteration was made, in but one respect. These were arranged according to what was judged to be their relative difficulty in order I, II, IV, III, V, number I being the original maze. The actual order of difficulty in learning these five proved to be, as shown by the table of results, V, IV, III, I, II. This leads to the conclusion that the disadvantages of the old habits rather overshadow their advantages, that is, that responses to the previously learned maze were carried over to the new maze and actually proved a hindrance to the formation of the new habit. In Watson's work on Animal Education where animals were taken from one problem to another, the results were generally found to be advantageous.

Whatever advantage, however, is shown by previous training in any of these cases can be explained in part at least by saying that the animal carries over the food seeking reaction, that fear and emotional disturbances have been eliminated, and that a definite manner of attacking the problem has carried over. In so far as these factors enter into the new problem positive transfer was shown. Likewise where the responses have some similarities postive effects obtain. But we also find negative effects in varying degrees. In Bogardus' work the negative overbalanced the positive effects. In his work the responses were similar in some respects and different in others. It was attempted however, to make the maze different in but one respect each time a change was made. But upon examination we discover perhaps three important changes which entered in each time: (1) a change in the direction or amount of turning at the critical point, (2) a change in the length of the runway at the critical point, (3) a change in the order of succession of the runways of different lengths at the critical point. Of these perhaps the first would be most important, although the third has been shown by Watson to be very important also. In fact the maze reaction has never been carefully enough analyzed to be used successfully in transfer tests. At least it is too complex to serve for pioneer tests. So our conclusion must be that in none of the experiments mentioned has there been a careful analysis of situation and response into their various factors and then a careful control and variation of those factors. Both situation and response have been varied in a lump or grossly. This is just the criticism that has been offered on a previous page upon the experiments on the same problem in human psychology

Thus while the taking over of the problem from the human field into the animal field has secured simplicity in as large a measure as seems possible by the elimination of the central or subjective factors, yet there is still too much complexity of objective factors and a lack of accurate analysis and definition of the relation of the two problems in terms of similarities and differences.

It seems to the writer, therefore, that if the experiments upon this problem are to have any finality and secure for us the solution of the problem they will have to begin with such conditions as will either keep the response as a whole constant and vary the stimulus or situation one factor at a time, or keep the stimulus or situation as a whole constant and vary the response one factor at a time. From some such simple beginning we could then pass, as experience and technique indicate, to tests where more than one factor of the stimulus or situation was varied while the response remained constant, or to tests where more than one factor of the response was varied while the stimulus or situation remained constant, or to tests where one or more factors in the stimulus or situation and one or more factors in the response were varied while all others in the *stimulus* and in the *response* remained constant. Most experiments which have been performed would presumably fall under the fourth class mentioned, although no careful analysis, especially of the response side, has been made in any of them, and in many no careful analysis of the stimulus or situation. We have already cited Bogardus and Henke's tests. For further example, how much, and what change is made in the stimulus and how much and what change is required in the response in passing from one of the boxes used by Yoakum to another, or from one of Hunter's mazes to another? Shall we, following Thorndike's principle of identical elements, decide upon the amount of likeness or difference of the two problems by the amount of transfer shown when one is learned after the others? The true course would require first such an analysis and control of factors as is suggested above and a long series of experiments in which control was present, in order to furnish the only sound basis for Thorndike's principle, or any other.

It appears to the writer, moreover, that after all, the problem back of the transfer of learning finds one of the best points of attack, as suggested above, in the form in which the response remains constant while the stimulus is varied under proper control. Evidently such a situation is in mind when the problem of transfer is spoken of as the problem of general training, of generalized response, of generalized habit. Is there such a thing as generalized response? Will a response which has been learned for one stimulus or situation be found to carry over in any degree for a stimulus or situation which is different in a definite way from the first stimulus or situation? If so, under what conditions does such a transfer take place? The experiments reported in this paper were designed to answer partially these questions. How successfully they have done so will appear in the issue. They represent an attempt to secure both the greatest simplicity possible and an accurate definition of the relation of the two problems. Both have the same motor response, both present the same sensory situation except in one definite particular and both presumably have a minimum of central factors. Before presenting the writer's experiments, however, let us notice a few theoretical considerations and some further experiments that bear upon the question in the form in which this paper expects to deal with it.

Theoretically it is possible for a "generalized response" to occur under two widely different conditions: (1) under circumstances in which discrimination of the stimuli is not possible for the organism called upon to make the response, or if possible, has not yet been learned; (2) under circumstances where discrimination of the stimuli is possible and is known to be made by the organism. Thus under number one would fall such cases as those where an animal makes the same response to all colors owing to an apparent inability to discriminate differences in colors, making the response wholly on the basis of brightness. Also the case of the child calling all men "papa" might be mentioned. These are to be looked upon not as cases of "transfer of response" or "generalized response" between previously discriminated particulars, but as cases merely of the absence of discriminated particulars. They represent a primitive and unspecialized stage, not an advanced state. Under number two would fall what psychologists are pleased to call, in their advanced stage in human psychology, " concepts," " generalizations," and " abstractions." Psychologists have not hesitated to apply the term " generalized response " to what is included, on the behavior side, under the terms mentioned. Thus to quote Angell(8): " Concepts of adults may also be considered as forms of generalized motor activity." Gore(9) sets forth something of the same idea. The idea presented is that the essence of such types of behavior after all lies in the fact that one response serves for a multitude of stimuli, situations, objects. The word " man," for instance, serves as a proper and efficient response to a whole group of objects and to each member of that group. The question before us in this paper is, when looked at from this point of view, whether we can get experimental data bearing upon this second type of " generalized response " or " transfer." Can we actually get well controlled situations where a response learned for one stimulus is transferred to another different stimulus? If so, what are the conditions of such transfer?

We find simple experimental illustrations in human psychology in the reaction experiments. Here one learns to make a simple response, pressing a key, upon the reception of a certain stimulus, say a visual stimulus. The same response is required upon the reception of a second stimulus of a different kind, say an auditory stimulus. So likewise with a touch stimulus. While such experiments furnish us little data on the conditions of transfer, yet they do suggest methods of procedure. In animal work we find also suggestive data. In Hunter's work(10) on the auditory sensitivity of the white rat he reports that his animals reacted properly (a given, required, controlled response in all cases the same) when either of the following noises were substituted for the standard whistle: the rush of air through the whistle; sound of the rush of air made with lips, and clapping hands. He found also that animals trained to react to hand clapping reacted successfully to the following noises when these were substituted: rattling of paper, dropping sunflower seed on tin, scratching on wood, drumming on table with fingers, etc. Here it may be contended that the response is a " generalized " one, but one belonging to the first type mentioned above. We have no definite ground for believing that the stimuli mentioned—all belonging to the group called noises—were in any way actually different stimuli for the animals tested, so we can not maintain that this furnishes an illustration of transfer between " different " stimuli, the second type mentioned above.

Better illustrations, perhaps, can be found among cases which are said to illustrate " substitution." Here one feature of the situation being the dominant one in arousing the response to the situation comes to be replaced by another as the dominant element. Watson(11) in his book on Behavior defines substitution as follows: " By substitution we mean that a stimulus which originally did not call out a given response comes later to call it out." He cites two examples. One is from the Pawlow type of experiment on dogs, in which a green light came to call out the salivary secretion in large amounts when the sight of the food had been the dominant stimulus for that reaction. The other is the experiment with cats which Thorndike(12) offered as a tentative proof of the presence of ideas in animals. Here the clapping of hands came to be the dominant stimulus in calling out the desired reaction, although the laying of fish on the top of the cage had been the dominant stimulus. Further examples might be taken from experiments with rats learning the maze. In the work of Bogardus and Henke(13) it was shown that rats make considerable use of tactual stimuli in learning the maze, although when once thoroughly learned the response was largely in kinaesthetic and organic terms, as Watson contended. It is evident then that here we have a substitution of kinaesthetic and organic stimuli for tactual as dominant in securing the required response. In the later work of Vincent(14) we have admirable illustrations of the same sort of thing. She has showed that the maze is probably learned in tactual, visual, and olfactory terms, when the maze is so constructed as to call these into play. Later the kinaesthetic and organic come to be substituted for them. Yet the kinaesthetic never becomes accurate enough to maintain correctness but needs almost constantly some sort of cutaneous guidance.

Cases of " substitution " are, in the opinion of the writer, after all cases of " transfer " of a certain sort. The essential characteristics of one type of transfer are all present, i. e., a certain definite, required response which is kept constant and a definite variation in the nature of the stimuli or situation calling out the response. While it can not be maintained that in the maze problem certain stimuli were removed and others, which had not been present before, put in their place, yet there is evidence that as far as effectiveness of the stimuli goes, such was actually the case. In the experiments to be reported in this paper the conditions were so made that one stimulus which was shown to be the dominant one was actually removed and another, which had not been present before, was substituted for it so that it became the dominant one in place of the former. These dominant stimuli were taken from the different sense fields, light, sound, and pain, so that a stimulus from one sense field was replaced by a stimulus from a different sense field. In this way it was hoped to secure results which were based on stimuli which were simple, but which were also as different as it was possible to secure. Of course these dominant stimuli had for their background or setting the entire situation composed of various stimuli from the box, room, etc. All these were conditioning stimuli, it is true, but not the dominant ones. All these conditioning stimuli were kept constant, the dominant stimuli alone being changed. The response also was kept the same. By such a method we hoped to secure some reliable and important results bearing on the possibility of one and the same response serving for two or more different sorts of stimuli, or on the possibility of a " generalized " response of the second type mentioned above.

SECTION TWO

GENERAL FEATURES OF THE EXPERIMENTS

A. *Animals Used.*

The animals were white rats of the same species as used by Watson, Carr, Vincent, and others in work done in the same laboratory in previous years. The rats were for the most part young animals, beginning the work when some three or four months old on the average. When young animals were not used the fact will be found indicated in the detailed reports of the experiments. In all, close to one hundred and seventy-five animals were used. They were divided into groups of five or six or seven to the group, and the record for such a group was taken as representing what the normal animal would do on the average under like conditions. The food used was sunflower seeds. This allowed better control of amount secured at each trial than other types of food at hand.

B. *The Two Types—Positive and Negative*

Two series of experiments were planned and carried out. In the first series the attempt was made to teach the animals a definite *positive* response to one sort of sense stimulus. Then a second sort of stimulus was substituted for the first. The purpose was to see whether and to what extent such a response would carry over from the first to the second. This series falls into two groups: (a) where the response was first learned to the light stimulus, and then the sound stimulus was substituted for the light; (b) where the response was first learned to the sound stimulus, and the light stimulus substituted for the sound.

In the second series a negative response was required in all cases. The purpose was the same as in the positive series. This series falls into six groups: (a) where the response was first learned to the light and the sound substituted, (b) where the response was first learned to the light and the electric shock substituted, (c) where the response was first learned to the sound and the light substituted, (d) where the response was first learned to the sound and the electric shock substituted, (e) where the response was first learned to the electric shock and the light substituted, (f) where the response was first learned to the electric shock and the sound substituted.

C. *Positive*

(1) Apparatus.—The apparatus consisted of a wooden box whose plan is shown in figure I. The base LMNO upon which the box rested was approximately two feet by three. The food box (F) was 13 inches wide and 13 inches long on one side but longer on the other because of the entrance of the two return alleys as shown in the figure. ED is a door which leads from the food box to the main part of the box (B) through the entrance alley (EA). This entrance alley was four inches in length. X and X′ are the entrances to the return alleys (RA and R′A′). RD and R′D′ are doors from return alleys to foodbox.

S and S′ are sounders hung just outside of the box close to three-quarter inch holes through the side of the box into the heads of the return alleys. Lt and Lt′ are two small electric light bulbs just inside the wall of the box and just above the holes mentioned above. K and K′ are two keys which control the left and right sounders and the left and right lights respectively. Sw and Sw′ are two switches which allow the use of the same keys (K and K′) for both the sounders and the lights, either singly or simultaneously. Thus the key (K) could be used to work the sounder (S) or the light(Lt) or both the sounder (S) and the light (Lt) simultaneously. The sounders were small electric buzzers so fixed that with one pressure of the key but one click was produced, instead of the rapid succession of clicks ordinarily produced by such an instrument. The lights were small electric bulbs of approximately one and one half candle power. The current necessary was produced by a cell of dry batteries indicated in the figure by the letters DB. The box and all partitions were six inches deep. The alleys were five inches wide. All the alleys were

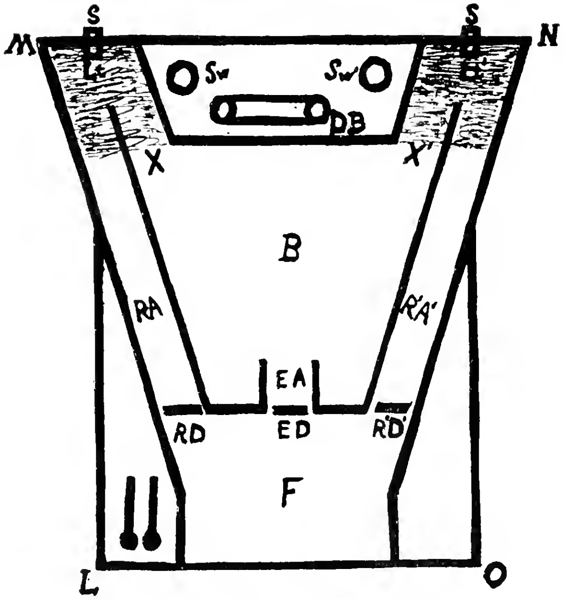


FIGURE I

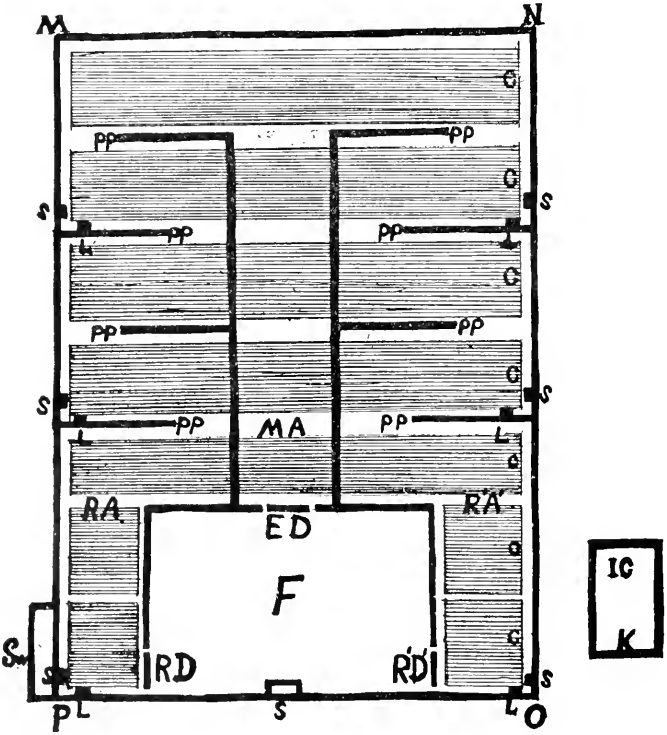


FIGURE II

uncovered excepting the heads of the alleys where the sound and light stimuli were presented. The covered portions are indicated in the figure by shading.

(2) Method of Procedure,—The animals were first tamed by being fed in the experiment boxes, by being handled freely by the experimenters, and by being allowed free run of the box until all emotional factors were removed as far as possible. The regular day's work for any animal was a series of twenty trials. However, when just beginning to learn, the animal was given fewer trials and gradually brought up to the full stint.

The standard adopted in this series as indicating that the animal had learned the problem was 95 correct responses out of 100 chances. Since a daily series consisted of 20 trials, the standard required the completion of five successive series of 20 trials each with not more than one error on the average to each series of 20 trials.

Normal records were secured first for the sake of comparision. These served to indicate how many trials and series of trials were necessary for the average untrained rat to learn the response to each of the various sorts of stimuli used in the experiments. Then rats which had learned to respond to one stimulus, say light, for example, were required to learn the same response to another, sound, for example. Then this 'sound' record was compared with the normal 'sound' record to see whether there was any advantage or disadvantage in first learning the response to the light stimulus. Also, in some cases, after the response to one stimulus had been learned, this stimulus was presented simultaneously with another sort to determine the effect of simultaneity of presentation upon the learning of the response to the second sort.

In regard to 'controls,' the simple expedient was adopted of omitting the stimulus and observing the effect upon the behavior. The series of 20 trials in which the stimulus had been omitted was compared with the series in which the stimulus had been presented, and the effect of the presence of the stimulus inferred from the comparision.

Two sorts of stimuli were used in the positive series — light and sound. The particular method of procedure with any one animal was to place it in the food box (F), allow it to secure a morsel of food, open the door (ED) into the alley (EA), then press the key K or K′, as the case demanded, and thus require the animal to choose the lighted or the noisy return alley. In case the animal entered partly or wholly into the unlighted or the quiet return alley it was accounted an error. The door from the unlighted or the quiet return alley to the food box was kept closed. The light stimulus was not always given as a continuous stimulus in any one trial until the choice of correct return alley had been made. The stimulus was given sometimes continuously and sometimes brokenly. Care was taken that the action of the key (K or K′) was noiseless. In case of the sound stimulus it was at first attempted to sound the clicks regularly until the correct choice had been made. This plan, however, was not followed in all the work reported. Whenever a different course was followed it will be indicated. The following order of choices between right and left return alleys was required in each series of 20 trials: L, R, L, R, L, L, R, R, L, R, L, R, L, L, R, R, L, L, R, R.

D. *Negative*

(1) Apparatus,— The apparatus for the negative response consisted of a box approximately two feet by three, MNOP in figure II. F represents the food box approximately 12 inches by 14 inches. This food box had three openings or doors, ED being the entrance door from food box into main alley, MA; RD and R′D′ being return doors from return alleys, RA and R′A′, to food box. All the alleys had a uniform width of approximately five inches and a uniform depth of six inches. From the main alley, MA, two winding alleys led back to the food box. These are marked RA and R′A′ in the figure and are spoken of as return alleys. PP are partial partitions obstructing the view of the return doors and causing the animal to take a devious course back to the food box. C, C, C, etc., are brass plates practically covering the entire width of the bottom of the alleys and are joined to batteries and an induction coil, IC in figure. S, S, S, etc., are small electric sounders of the same type as used in the positive experiments. They are here hung just inside the

walls of the box close to the partial partitions, excepting the one found on the wall of the food box. These sounders are also joined to the batteries. L, L, etc., are small electric lights of the same size as used in the positive experiments. They are attached to the partial partitions near the sounders and are also joined to the batteries. In one box used for these experiments a switch was attached to the side of the box, marked Sw in the figure. This switch was used to turn the current into the right or left sounders or lights as the case demanded. A later box had a slightly different arrangement. Two keys were used on the battery and induction box, one for the right and one for the left. This allowed of more rapid and less noticeable action on the part of the experimenter when giving the stimulus or combination of stimuli required in any particular case. The sounders, lights, and brass plates were so wired as to allow them to be used singly or in combination. Thus the lights on any one side could be used alone or in combination with the sounders of that side or in combination with the brass plates for pain stimuli. Also it was arranged at first so that one, two or all three of the sounders might be used at any one time. This was found to be unnecessary, however, and so it was finally arranged that all of the sounders on any one side would come into play by pressing the key controlling the sounders on that side. The sounder found on the wall of the food box was the first one used in attempting to teach the negative response to the sound, but it was found necessary to place the sounders along the side of the box as indicated above before the animals would pay sufficient attention to them to learn the response required in any reasonable number of trials. So the sounder in the food box was abandoned and the sounders along the side were substituted. Since the wiring required was rather intricate no detailed description of it will be given here. Any electrician can make the proper connections to secure the working combinations of sound, light and pain required in the experiments.

(2) Method of Procedure, — The animal was put through the entrance door (ED) into the main alley (MA) and was required to go up the main alley and back to the food box through either the right or the left return alley (RA or R′A′).

When the animal had entered either of these return alleys the stimulus was given, that is, the light or sound or electric shock, or a combination of any two of them as the case might require, and the animal was required to go back and come down the other return alley to the food box. In fifteen out of every series of twenty trials the proper stimulus was given and the animal was required to turn back and enter the food box by the other alley. In the other five trials the animal was allowed to continue on its chosen way and return to the food box, no stimulus of any kind having been given. These five where no stimulus was given were scattered irregularly through the series of 20 to prevent any possibility of the animal's learning a definite fixed order in the series. The purpose was to train the animal to stop at a given stimulus, to turn around, and to go back to the food box through the other return alley but, when given no stimulus, to proceed on to the food box through the alley first chosen. When the animal was first learning, the stimulus was given more or less irregularly as to loudness and continuity from the time the animal had fully entered the return alley until it had turned back, even if the animal persisted in its course until it found the door at the end of that alley closed. The fact is that the usual course was for the animal at first to ignore completely the stimulus other than a short delay for examination and to proceed clear down to the closed door at the end of the alley. After the first strangeness had worn itself away, which happened after a few trials, there was little indication, if any, to show that the stimulus was sensed at all until actual learning of the required reponse began. However, as accurate records as possible were kept of hesitations as well as of the actual times of turning back, when the hesitations were apparently due to the effect of the stimulus. Sometimes also in the learning process the speed of the reaction was apparently much lessened by the disturbing effect of the stimulus. This was particularly true when the light stimulus was used, so a record of these was also kept. Both doors, RD and R′D′, were kept closed until a choice had been made, when the proper one was opened as noiselessly as possible. An attempt was made to prevent the experimenter's movements from being so evident, as to become cues for the animal. A response was counted correct provided the animal had fully entered one or the other of the return alleys and turned back, when the stimulus was given, before it had reached a point where there was any possibility of its seeing the closed doors of the alley. After the animal had begun to learn it was given the stimulus when at various places to prevent the formation of position habits. Thus sometimes the stimulus was given just when the animal had entered the alley, sometimes when it was half way down the alley, and sometimes when it had come almost to the point where the closed doors could be seen. By such a variation the animal would not learn to stop at any particular place but only when the stimulus was given. The omission of the stimulus five times out of twenty trials also prevented the formation of the turning habit regardless of the presentation of stimulus and served as an effectual control. The standard adopted for the negative series was 100 correct responses out of 105 chances to respond to the stimulus given. Since the stimulus was given but 15 times out of each series of 20 trials it required the completion of seven such series with but five errors all told to meet the standard.

SECTION THREE

FEATURES BEARING ON LEARNING

A. *Positive*

(1) Light—Two groups, I and II, were trained with the light stimulus and were then transferred to the sound. The following tables give the results as to the number of trials and series of trials necessary for learning the light sufficiently well to meet the standard of 95 correct choices out of 100 chances.

GROUP I

Animal	Trials	Series
No. 15........	680	34
No. 16........	880	44
No. 22........	640	32
No. 23........	460	23
No. 26........	340	17
Average.......	600	30

GROUP II

Animal	Trials	Series
No. 17........	920	46
No. 18........	580	29
No. 19........	580	29
No. 20........	600	30
No. 21........	540	27
No. 25........	560	28
Average.......	630	31.5

Average for 11 animals, 616.36 trials, 30.81 series.

This average for eleven animals can be taken as a normal record for learning a positive reaction to a light stimulus under the conditions of the experiment.

It will be noticed there is great variation in the number of series and trials required for the individual animals to learn the response. Thus number 26 needed only 17 series or 340 trials, while number 17 took 46 series or 920 trials. For this reason group curves would not show the characteristics of the learning process. So two typical individual curves have been given instead. The curves for number 22 of Group One, (Figure III) and for number 25 of Group Two (Figure IV) are given. These curves show not only the learning of the response to the light stimulus but they also indicate the record for the control series in each case.

These curves indicate neither a rapid initial rise nor a long period of final perfecting of response. They rather show a gradual, though very irregular, rise from the beginning to the end of the learning process.

The unusually large number of trials required by Numbers 16 and 17, for learning the light finds its explanation, apparently, largely in terms of distracting olfactory stimuli. The males in these two groups were kept in a cage by themselves and were given their daily stint of work immediately after the females of the two groups. The two individuals mentioned, being unusually vigorous animals, were constantly attracted apparently by the odors left by the females and would often wander about the box sniffing here and there to the neglect of the light stimulus. So even after the response had been apparently learned, it took a greater number of series to meet the required standard.

The important thing about learning the light was, of course, to be as sure as possible that the animals were responding to the light and not to other stimuli. The order of presentation of right and left pathways followed in the regular and control series has been given already on page 29. It will easily be seen that position habits might account for a rather large percentage of correct choices in such a series. Of the position habits most likely to be learned the most advantageous would be a double alternation between the right and left pathways. Such a position habit, if acquired, would account for a record in which 80% of the responses were correct. The next most advantageous position habit would be a simple alternation between right and left. That, if acquired, would account for a record in which 70% of the responses were correct. Chance, of course, in any series where two possible choices must be presented an equal number of times, will account, in the long run, for 50% of the responses being correct. A position habit of always going to one side would also give a record in which 50% of

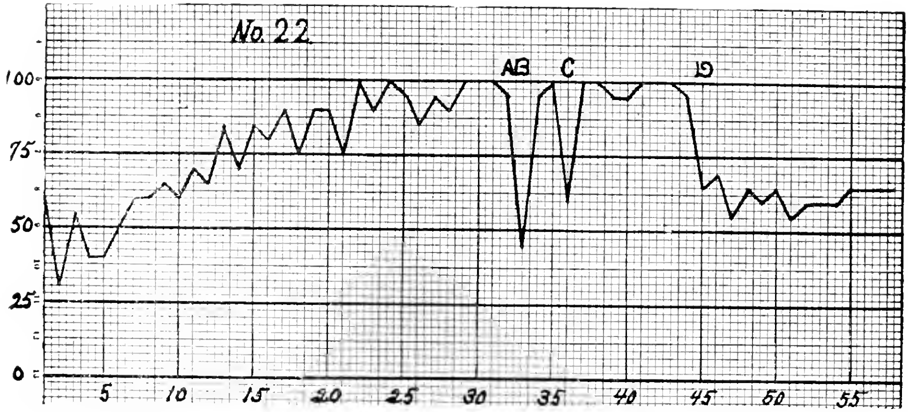


FIGURE III

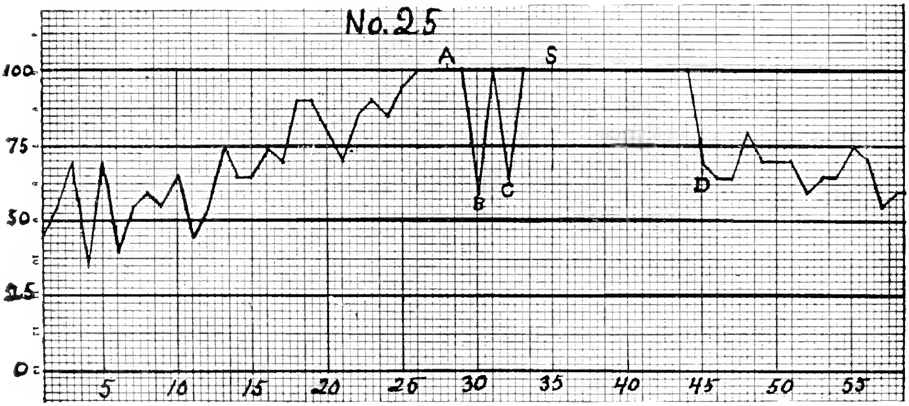


FIGURE IV

Curve 22 (Figure III) is an individual curve from Group One. Curve 25 (Figure IV) is an individual curve from Group Two. The horizontal lines represent the percentage of correct responses. The vertical lines represent the number of series of 20 trials each. In lettering,—A is the end of the learning process with light, B is the first control series, C is the second control series, D is the beginning of training with the sound, S (in Curve 25) is the beginning of simultaneous light and sound.

the responses would be correct. From a study of the daily records it was found that there is no evidence in any case for believing that the animals at any time used the double alternation position habit. A study of the records does show, however, that at first the choice of response was practically a matter of chance. In a very few instances the position habit of always going to one side was picked up after a series or two but was not maintained for longer than one day at a time. Then in most cases there seems to be a gradual shift to the simple alternation position habit, but in none of the cases in this list of eleven animals was such a habit actually learned and consistently maintained *while learning the light. A careful study of the daily records did not reveal a single series where a perfect simple alternation was carried clear through the series while learning the light.* This was not true, however, after the animals had learned the light and had been transferred to the sound. There the animals seemed to be working largely on a basis of simple alternation but not with any degree of consistency. Out of 215 series where sound was given as a stimulus after light had been learned only 20 showed a perfect simple alternation. From a study of the records, however, one must conclude that there is a strong tendency toward using simple alternation in a series of the type used, but that there is little ability to maintain it consistently.

In order to make it as certain as possible that the animals were actually responding to the light stimulus when the required response was considered learned, first, a high standard of excellence was chosen and second, control series were introduced. The standard adopted as indicating that the problem had been mastered was 95 correct choices out of 100 chances or five series in which there was not more than one mistake to a series on the average. The actual fact was that most of the animals met even a higher standard than that, as a glance at the sample individual curves will show. It was not considered probable that the animals used would be able to develop a position habit of sufficient complexity to enable it to respond correctly, 95 times out of 100 even with a series apparently as simple as the one adopted. As indicated above the results justified this assumption. But to be certain control series were used. After the animal had apparently learned the problem, it was given one or two series without the light stimulus. The result was a drop in efficiency of from 25% to 70% with an average drop of 40%, leaving an average of 60% correct. This is not much above what chance would enable the animal to do. Upon the light being restored again an immediate return to the required efficiency appeared again. This was taken as conclusive evidence that the light stimulus was the dominant stimulus in making the correct response when the percentage of efficiency had reached 95.

(2) Sound—Five groups of animals, III, IV, V, VI, VII, were trained with the sound stimulus preparatory to transferring them to the light. In all 32 animals were trained in these groups. The response required was the same simple positive response required in the case of the experiments with light. Groups III and IV used the sounder mentioned in the description of the apparatus. Group V was trained with the telephone receiver as sounder in place of the regular electric sounder. Groups VI and VII were trained with the telephone receiver as sounder and with pain as punishment for a wrong choice. Groups III and V were older animals than those used in the other groups and had had training on the maze. They were all practically full grown animals.

The results can be summarized very briefly. All the members of group III were given 85 series or 1700 trials without learning the response. All the members of group IV received 60 series or 1200 trials without learning the response. All the members of group V received 40 series or 800 trials without learning the response. Three members of group VI received 40 series or 800 trials, one 35 series or 700 trials, and two 30 series or 600 trials. None learned the response. Four members of group VII received 60 series or 1200 trials, two 55 series or 1100 trials, and none learned the response.

In order to bring to light the significant features of these results we need to notice the method and results more in detail.

In the main, the method with the sound stimulus was the same as with the light. The stimulus was given in a more or less regular way, that is, the sounder was 'clicked' with more or less regularity until the animal had made a choice. When the choice had been made, whether right or wrong, the stimulus ceased to be given. When the choice was wrong and the animal had returned to the main portion of the box (B in figure I) the stimulus was again given until correct choice was made. However, the animals in groups III, IV, and V, where no punishment for wrong choice was, given, soon learned to work so rapidly that one scarcely had time to give any stimulus, after the animal had been put into the entrance alley (EA in figure I), before a choice had been made. The animal would dash into one or the other of the return alleys (X or X′ in figure I) and, after glancing about the turn at the head of the return alley, dash toward the door into the food box if the choice happened to be correct, or if wrong, whirl quickly and dash into the other return alley and into the food box. When the reaction became of this type, the stimulus was given from the time the animal entered EA until the correct return alley had been entered.

With group V, beginning with the 26th series, the stimulus was given irregularly in place of regularly, that is, the number of times the key was pressed and the sounder 'clicked' varied greatly from one unit of time to another during the time the stimulus was being given. This was done with the idea of attracting the animal's attention. A like change of procedure was made with the animals in groups VI and VII where pain was given as punishment for wrong choice.

Another change of procedure introduced was punishment for wrong choice. The punishment given was an electric shock. The return alleys (RA and R′A′) beginning at X and X′, had small copper plates placed in them and these plates were attached to an induction coil. Two keys were used to connect the circuits, one for RA and one for R′A′. With the introduction of punishment the behavior of the animals was quite different. Considerable hesitation appeared as a rule before choice of return alley was made. This hesitation gave more time to attract the animal's attention by means of the sounder than when the animal rushed pell-mell into one of the return alleys as soon as placed in the entrance alley (EA).

While some improvement appeared, as will be shown later, yet the main thing to be noticed in the results is that not a single animal out of the 32 trained learned to choose the 'noisy' alley even though some were given as high as 1600 trials and even though pain was used as punishment for wrong choice in the case of a number. Also as will appear later in our study of Transfer effects, even those animals which had learned the positive response to the light failed to learn the same response to the sound. A number of explanations or reasons may be offered for this.

First, it may be claimed that the animals could not hear the sound presented. That is disproved, however, by the fact that other animals did learn the *negative* response to the very same type of sound.

Second, it may be that the reaction required lay outside the animal's ability to learn. However, eleven animals did learn the same response, when a light stimulus was used, with much smaller number of trials than were given with the sound stimulus.

Third, perhaps the rats were negative to the types of sound used and so would not learn a positive response to such sounds. The records, however, do not give any convincing evidence to support this contention. Since the rats had free opportunity to enter either the 'noisy' or the 'quiet' alleys, if they were negative to the sound, we should expect some evidence of it in the early part of the records. The rats should have made many more than 50% of errors until they had become accustomed to the sound. In Group III the averages for the first 200 trials were 46, 49, 51, 53, 50, 51, 48, 54, 49, 56; just about what chance would give in the long run. The records for the other groups show about the same results. No signs appeared in the general behavior of the rats, except in one or two individual cases, that would go to support the contention that they were negative to the sounds presented. Rat number 7 in group III was one of the exceptions. It was, however, an extremely nervous animal and objected to being handled. Its daily record often contains such a statement as the following; " Would not work well today. Seemed frightened." With practically all the animals, however, one could not tell from their general behavior that they even .heard the sound at all. As stated above, the responses became so rapid, where no punishment was given for a wrong choice, that one would be led to think that the sound was wholly ignored. Further, if the rats were negative, the omission of the sounds should show an increase in percentage of correct choices. Let us notice the last 100 trials with the sound stimulus in groups III and IV, and the five control series or 100 trials where the sound was omitted. The last 100 trials with the sound in Group III average 68, 66, 66, 67, 67. The 100 trials without the sound average 69, 69, 69, 64, 64. A slight advantage is shown for those without the sound. In group IV the last 100 trials with sound average 64, 68, 62, 66, 68. The 100 trials without sound average 67, 68, 63, 68, 63. No advantage is shown for either.

Although no convincing evidence can be found in the records that the rats did not learn because they were negative to the sound presented, yet to give the matter a further trial a less metallic sound was sought, if that should prove to be the difficulty in the way of learning. For that reason a telephone receiver was substituted for the electric buzzer used with groups III and IV. A duller, more wooden sound was thus secured. No one of the six rats in Group V gave any evidence of learning the response after 800 trials. Neither the records nor the general behavior showed that the rats were negative to the sound. Groups VI and VII were trained with the same sound as Group V but were given pain as a punishment for wrong choice, but 1200 trials for four of them brought no convincing results.

As a last resort a short test with one rat was tried with a sound to which from general behavior we had every evidence for believing the rats to be positive. This sound was the dropping of sunflower seed in a dish. Any one who has handled rats knows that such a sound will cause in a hungry rat a very active hunting for the source of the sound. The method of procedure was as follows: One experimenter was placed behind a screen at the end of the box marked MN in figure I. A dish of sunflower seed was placed at the position of each of the sounders, S and S′. The other experimenter took the regular position in front of the box. When the animal was placed in the entrance alley (EA) the experimenter behind the screen would rattle the seeds in the dish at the head of the open return alley. Punishment was given for wrong choice. Rat number 52 in group VII was given 280 trials under such conditions. The record by series runs: 60, 80, 75, 65, 50, 75, 70, 65, 55, 70, 75, 50, 75, 65. Since no signs of improvement were appearing the work was dropped.

Because of the above considerations the conviction was reached that the failure to learn the positive response to the sound stimulus was not due to the animals' being negative to the sounds presented.

The fourth possible explanation for the failure that suggested itself was that the rat could not localize the sounds well enough under the circumstances to enable it to guide its responses by them. This does not mean that the rat can not localize sounds, but that, if it can, the conditions under which the experiments were conducted were not suited to call localization into play. When the experiments began it was assumed that the rat could localize sufficiently well under the conditions presented to enable it to guide its response by the sound after a certain period of training.

The records for Groups VI and VII contain some significant things that require some comment and explanation. It will be recalled that pain for a wrong choice was given in these two groups. The records show that Numbers 42, 46, 47, 48, and 50 gave considerable evidence that they were actually learning the response. The fact is, Numbers 42, 47 and 50 actually met the standard required, that is, 95 correct responses out of 100 chances. Why was it that these records were not accepted as indicating that those animals had learned the sound? The fact was that their general behavior did not indicate definitely enough that they were actually responding to the sound stimuli when they were making enough correct responses to come close to meeting the standard required. The experimenter became convinced that some other factor was present in these cases as the determining factor. To make plain the basis for this conviction let us notice again the method of procedure. The keys which closed the circuit containing the induction coil and controlled the electric current used for punishment were located just to the left of the keys (K and K′) used for working the sounders. The method of procedure had regularly been to use the left hand for working the 'sounder' keys and the right for opening and closing the doors of the box whenever necessary and for handling the animal. To have the left hand free to work the 'sounder' keys the right was also used to work the 'punishment' keys whenever necessary. The various movements made by the experimenter were usually as follows: The left hand remained resting at the 'sounding' keys while the right was used in adjusting the return doors (RD and R′D′ in figure I) and in putting the animal into the entrance alley (EA). Then the proper 'sounder' key was manipulated by the left hand and the sound stimulus given until a choice had been made, the right hand in the meanwhile resting upon the edge of the box. If a wrong choice had been made the sound was discontinued and the right hand was stretched across to the 'punishment' key and punishment given. In case of the animals mentioned above as showing evidence by the records of learning the required response the experimenter became convinced from observation of the general behavior that it was this very evident motion of the right hand or perhaps of his whole body that in some way served as a warning when about to enter the wrong alley. So a change was made in procedure. Instead of using the right hand for pressing the 'punishment' key, the right hand was allowed to remain lying on the edge of the box and the left was slipped as unobtrusively as possible from the position of the 'sounder' keys to that of the 'punishment' keys. A comparison of the records for five series before this change was made and for five series after the change was made will be surprising. Practically every animal which gave evidence that the response was being learned showed a considerable decrease in efficiency of response after this change was made. For the sake of comparison I have gathered them together in the following table:

	Before					After				
No. 39....	65	60	80	80	90	45	85	80	55	50
No. 40....	65	50	55	80	80	55	65	65	70	40
No. 41....	85	75	80	80	95	75	70	70	55	40
No. 42....	100	90	90	95	100	50	60	80	70	65
No. 43....	80	85	80	90	95	75	55	45	60	70
No. 45....	65	45	80	75	65	55	50	45	55	60
No. 46....	95	95	100	95	80	50	40	70	60	80
No. 47....	95	100	100	100	100	85	60	65	70	65
No. 48....	80	95	95	90	80	20	40	65	55	60
No. 49....	80	70	95	95	65	50	55	60	70	70
No. 50....	90	85	100	100	100	60	65	75	70	80
No. 51....	85	90	65	80	75	70	60	40	45	65
No. 52....	55	75	70	80	80	35	55	50	..	..

These records indicate that it is highly probable that the rats were depending in part at least upon certain movements made by the experimenter rather than upon the sound stimulus which was to serve as the guide in making the choice.

This complete failure to get any rats, either trained previously with the light or wholly untrained, either with punishment for wrong choice or without punishment, to learn the positive response to any sound stimulus presented was one of the most surprising results of the experiments. As suggested above the most plausible reason for this failure seems to be that the rats were unable to localize the sound sufficiently well under the conditions of the experiment to enable them to utilize the sound in learning the response required.

However, it would not be correct to say that these animals trained with the sound made no improvement at all. The fact is that all of them actually made improvement during the first 300 trials. To what was this improvement due? To answer that question we need first to know the amount of improvement made. We find that Group III in the first 100 trials made 49.8 per cent of correct responses, while in the fourth 100 trials the same group made 61 per cent of correct responses. Group IV in the first 100 trials made 51.2 per cent of correct responses, while in the fourth 100 trials the same group made 60 per cent of correct responses. Group V in the first 100 trials made 53.6 per cent of correct responses, while in the fourth 100 trials the same group made 65.8 per cent of correct responses. Thus roughly for the three groups the gain amounted to about 10 per cent.

We have already called attention to the fact, that with the series of choices required, chance would account for 50 per cent of correct responses and simple alternation between left and right would account for 70 per cent of correct responses. We have also called attention to the fact that the records show a tendency toward simple alternation in the case of the sound stimulus, but that there was no consistency at all in maintaining such an alternation. We are led to conclude, therefore, that *whatever gain was made by those animals trained with the sound stimulus was due to a partial perfecting of the simple alternation position habit.* In the case of those which learned the positive response to the light stimulus, since there is no direct evidence in the records of such a position habit having been acquired while learning the light, we would be led to believe that the light stimulus itself became the increasingly dominant factor right from the beginning of the learning process. However, when those same animals were transferred to sound, the records immediately show, as we have pointed out, that such a position habit had been partially formed. Thus the evidence seems conflicting. However, the conclusion seems warranted that chance and simple alternation, imperfectly carried out, will account for 60 per cent of correct responses when using such a series as was used in the experiments with positive response. Since such improvement is made in the first 300 trials any improvement beyond that amount, whether during the first 300 trials or later, must be accounted for in other ways. Thus Group I required only 120 trials with the light stimulus to surpass 60 per cent. Group II needed only 180 trials to surpass 60 per cent. Our conclusion must be that the light stimulus began to be effective in both cases not later than during the second 100 trials and reached its maximum influence at the end of 500 trials.

But we still have the question why it required those trained with the light only 120 and 180 trials to reach an efficiency of 60 per cent whereas it required those trained with the sound 300 trials to reach the same point. We have already found that the sound stimulus had no influence since it could not be localized. We have, therefore, to take account of the light only. In one case it was present and in the other case it was absent. Therefore we must account for the rapidity with which the 60 per cent standard was reached in the first case as compared with the second by means of the effect of the light stimulus. Therefore we can conclude that the light stimulus was a factor practically from the beginning in learning the response, although upon the removal of the light the position habit, gained partly through the influence of the light and partly by mere repetition, still remained effective.

(3) Summary,—

(*a*) White Rats do not localize sounds sufficiently well to make use of them as a guide in learning under the conditions of these experiments.

(*b*) Movements of the experimenter can be utilized by white rats as cues which aid materially in the learning process.

(*c*) White rats will not learn and maintain consistently, under the conditions of these experiments, a simple alternation position habit even though given 1700 trials.

(*d*) In learning such a series as the one used in these experiments where a simple alternation position habit is of considerable value in getting the correct order of responses, such a position habit is not formed first independently of the light stimulus and then the learning process completed in terms of the light stimulus.

Rather there is evidence that the light stimulus is more or less effective in helping to establish whatever position habit enters into the order of responses and is wholly effective in carrying the learning process beyond the point where position habit alone could carry it.

(*e*) The learning process in this sort of problem is evidently very gradual, though somewhat irregular, having neither a rapid initial rise nor a long final perfecting of the response.

B. *Negative*

(1) Light,—Six groups of animals, VIII, IX, X, XI, XII, XIII, were taught the negative response to the light stimulus. The results have been summarized in the following tables:

Group VIII

Animal	Trials	Series
No. 53	255	17
No. 54	255	17
No. 55	240	16
No. 56	195	13
No. 57	255	17
No. 58	225	15
Average	237.5	15.8

Group IX

Animal	Trials	Series
No. 77	210	14
No. 78	255	17
No. 79	225	15
No. 80	285	19
Average	243.75	16.25

Group X

Animal	Trials	Series
No. 93	210	14
No. 94	195	13
No. 95	180	12
No. 102	210	14
No. 99	180	12
Average	195	13

Group XI

Animal	Trials	Series
No. 84	120	8
No. 85	165	11
No. 86	195	13
No. 91	150	10
No. 97	165	11
No. 101	120	8
Average	152.5	10.2

Group XII

Animal	Trials	Series
No. 81	225	15
No. 82	225	15
No. 89	225	15
No. 90	165	11
No. 98	135	9
Average	195	13

Group XIII

Animal	Trials	Series
No. 83	195	13
No. 87	195	13
No. 88	165	11
No. 92	210	14
No. 96	135	9
Average	180	12

Average for 31 animals, 198.87 trials, 13.26 series.

To understand these tables it is necessary to recall that with the negative response the stimulus was given in but 15 trials out of each series of 20 trials, the other five trials in each series being used for control tests as explained under method of procedure.

The important question here would be whether we can be sure that the responses were actually made to the light. As far as the writer could tell the other possible types of stimuli which might have been functioning were the sight of the experimenter's movements, the mechanical jarring of the apparatus produced by working the keys, and the kinaesthetic which would show itself in turning habits due to regularity of position and order of the required turning. As to the first mentioned, as much care was exercised as was possible to make such movements inconspicuous. The keys were not in the range of the animal's vision and the finger movement or forearm movement necessary to work them was also below the line where vision might function. This result was secured mainly by having the alleys six inches deep, making it practically impossible for the animals to see any extraneous object or movement except where it appeared immediately over the alley. The rest of the experimenter's body was practically immobile during the time in which the reaction was to be made. As to the mechanical jarring of the apparatus, the extremely light touch necessary to work the keys would certainly render the jarring negligible. In the writer's opinion the most likely to manifest itself of the three possible types of things mentioned above is the third. When an animal is required to turn back fifteen times out of every twenty, it is extremely probable that it will acquire a habit of turning back, even when no stimulus has been given. As has been stated the series of trials used was particularly arranged to meet such a contingency. The possibility of "order in the series" functioning was eliminated by introducing irregularly the trials when the stimulus was omitted. The possibility of "position in the alley" functioning was eliminated by constantly varying the point at which the sound or light stimulus was given. Finally in cases where the animal did turn back and entered the other return alley when no stimulus was given by the experimenter or before such stimulus had been given, the stimulus was given in the other alley and the animal required to return to the alley first chosen to get back to the food box. Since such cases were confined practically to the work with the sound stimulus we shall omit the discussion of them here. These cases were so rare when using the light that they can well be neglected in considering the results.

The outstanding feature of the learning process here is the shape or form of the learning curve. To bring out the peculiar features more plainly, a number of curves are presented. These curves show the record not only for learning the light but also for the transfer to sound and pain. See Figures V to XIII inclusive.

It will be noticed that these curves show a period at first,

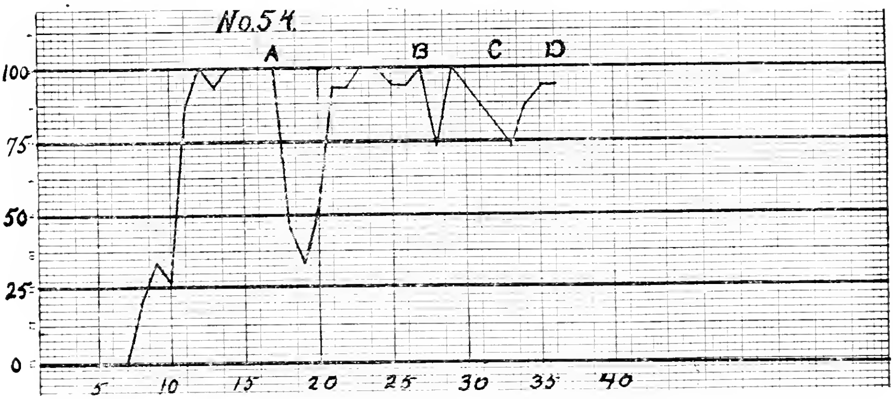


FIGURE V

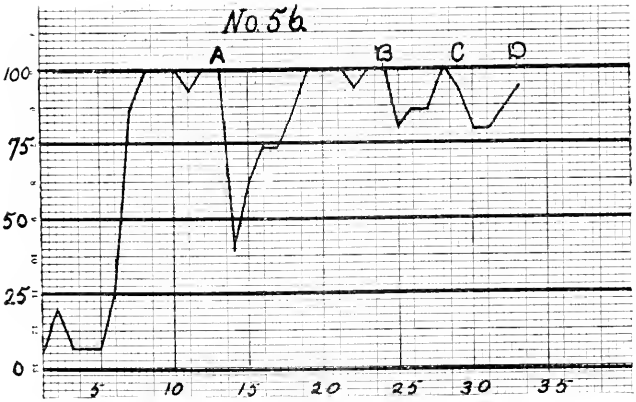


FIGURE VI

Curves 54 (Figure V) and 56 (Figure VI) are individual curves from Group Eight. The horizontal lines represent the percentage of correct responses. The vertical lines represent the number of series of 20 trials each. In lettering,—A is the end of the learning process with light, B is the end of the learning process with the regular sound stimulus, C is the end of the learning process with the bell, D is the end of the learning process with the pipes.

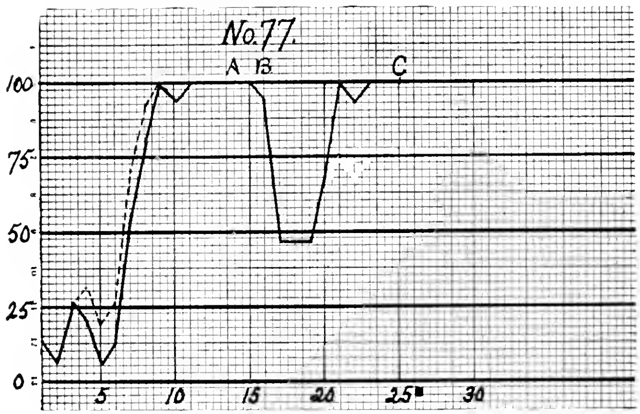


FIGURE VII

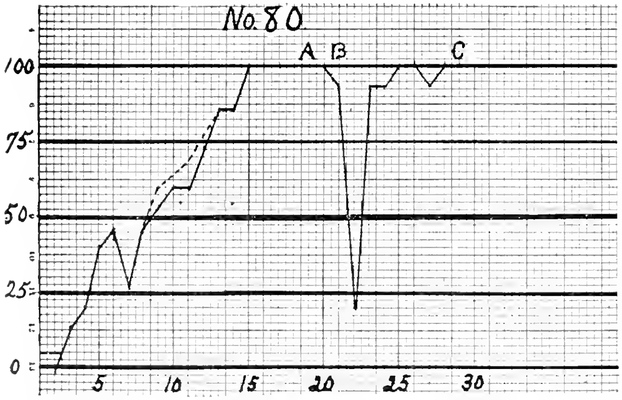


FIGURE VIII

Curves 77 (Figure VII) and 80 (Figure VIII) are individual curves from Group Nine. The horizontal lines represent the percentage of correct responses. The vertical lines represent the number of series of 20 trials each. The dotted lines represent the record for correct responses and for hesitations. In lettering,—A is the end of the learning process with the light, B is the end of the simultaneous series, C is the end of the learning process with the sound.

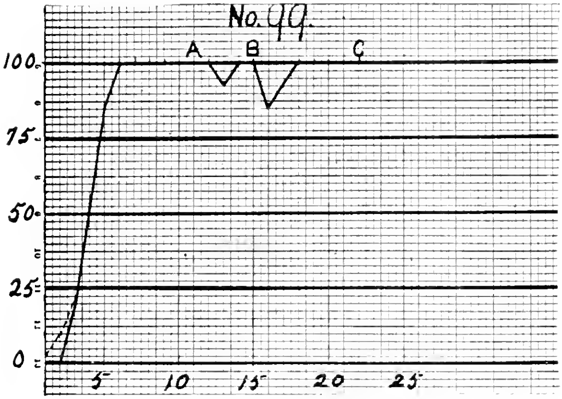


FIGURE IX

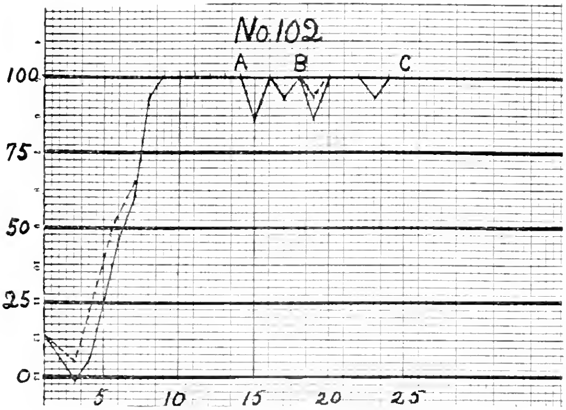


FIGURE X

Curves 99 (Figure IX) and 102 (Figure X) are individual curves from Group Ten. The horizontal lines represent the percentage of correct responses. The vertical lines represent the number of series of 20 trials each. The dotted lines represent the record for correct responses and for hesitations. In lettering,—A is the end of the learning process with the light, B is the end of the simultaneous series, C is the end of the learning process with the sound.

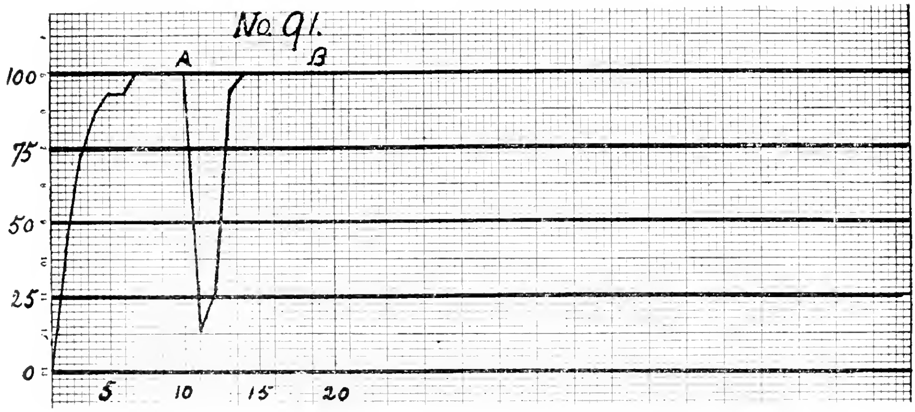


FIGURE XI

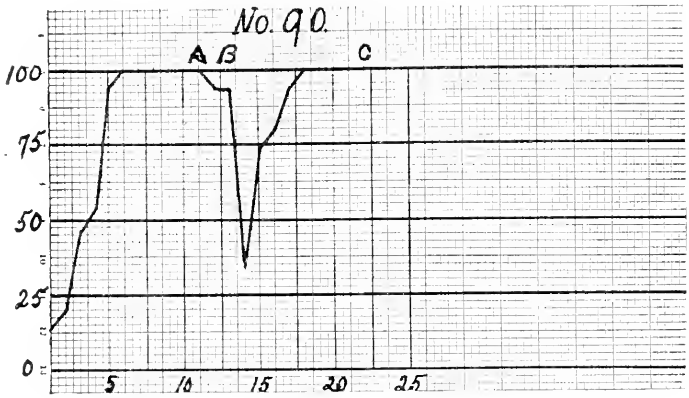


FIGURE XII

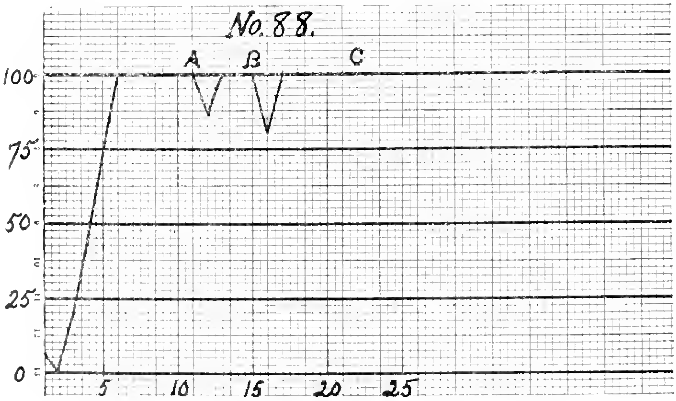


FIGURE XIII

Curve 91 (Figure XI) is an individual curve from Group Eleven. Curve 90 (Figure XII) is an individual curve from Group Twelve. Curve 88 (Figure XIII) is an individual curve from Group Thirteen. The horizontal lines represent the percentage of correct responses. The vertical lines represent the number of series of 20 trials each. In lettering,—A is the end of the learning of the light, B, in curve 91, is the end of the learning with the electric shock, and is the end of the simultaneous series in curves 90 and 88, C is the end of the learning process with the electric shock.

although short, of practically no learning, then a very sudden jump up to perfect response, rather than the sudden initial rise and the final long period of perfecting the process found in many learning curves. In the dotted curves it appears that the steepness of the ascent is modified a little in some cases. These take into account not only the correct responses but also the “hesitations” and the times of ‘slowing up’ due to the stimulus presented. But the light was learned so quickly that little difference is made by taking these additional evidences of learning into account. The few series necessary for learning the light also prevents the early period of non-learning from standing out clearly. One individual case in particular, number 84 in Group XI, illustrates the rapidity of learning very strikingly. The first series in this animal's record shows one correct response out of 15 chances, this one being the last of the fifteen. The next six series show a perfect record. This makes its record run as follows, counting in per cent of correct responses: first series, 6 2-3 per cent; second series, 100 per cent; third series, 100 per cent; fourth series, 100 per cent; fifth series, 100 per cent; sixth series, 100 per cent; seventh series, 100 per cent. One could scarcely find anywhere a record in animal learning that would equal it.

Since similar phenomena appear in learning the negative response to the other sorts of stimuli used, discussion will be reserved until those results have also been given.

(2) Sound,—Six groups of animals, XIV, XV, XVI, XVII, XVIII, XIX, were taught the negative response to the sound stimulus. The results have been summarized in the following tables:

GROUP XIV

Animal	Trials	Series
No. 59	345	23
No. 61	465	31
No. 63	540	36
No. 64	435	29
No. 65	465	31
Average	450	30

GROUP XV

Animal	Trials	Series
No. 60	450	30
No. 67	375	25
No. 68	345	23
No. 69	300	20
No. 70	345	23
No. 73	405	27
Average	370	24.66

Group XVI

Animal	Trials	Series
No. 62..........	540	36
No. 66..........	495	33
No. 72..........	285	19
No. 74..........	405	27
No. 75..........	450	30
No. 76..........	450	30
Average.........	437.5	29.16

Group XVIII

Animal	Trials	Series
No. 129.........	795	53
No. 130.........	765	51
No. 131.........	630	42
No. 132.........	795	53
No. 133.........	795	53
Average.........	756	50.4

Group XIX

Animal	Trials	Series
No. 152.......	705	47
No. 153.......	630	42
No. 154.......	645	43
No. 155.......	630	42
No. 156.......	705	47
Average.......	663	44.2

Average for 27 animals, 525.5 trials, 35.03 series.

The record for Group XVII is not included in the average with the remaining groups since we were compelled to make some changes in the method of procedure with that group. It was the first group to which we attempted to teach the negative response to sound and by means of it a final method was worked out. We gave the seven members of this group on the average fifty series of trials with the same sort of series as was used in the positive response previously reported and with the single sounder located in the food box and marked S in figure II. After the complete failure of all seven animals to show any evidences of learning in the fifty series of twenty trials each except such as could be ascribed to position habits, the three sounders along each side of the experiment box were introduced and the type of series adopted which was used in all the experiments with the negative response. After these changes were made the record for this group in learning the sound is practically the same as the normal record for sound secured later with the other groups reported.

To bring out the main features of the learning process when the sound was used, I have inserted a few typical curves. These curves show the same general features as those illustrating the learning with the light. In these curves, however, some of the distinctive features show up much more plainly than in those for the light stimulus. These curves show the complete record for the animals, both while learning the sound and when transferred to the light and pain. Figures XIV to XVII.

The significant features of the learning process with the negative response to the sound stand out most clearly in the individual curves presented. These features have already been mentioned—the rather long period of no progress at first, the sudden and rapid perfecting of the response when once learning begins, and the few errors after once the response is learned. An apparent exception is the record of number 61 of group XIV. Here the early part of the record is very irregular until the eighteenth series, when it rises regularly and rapidly to perfection at the twenty-fifth series. In contrast with this record stands that of number 67 of Group XV. No sign of learning was shown there until the thirteenth series and learning was practically complete in the twenty-first series. Or still more striking is the contrast shown by the record for number 154 of Group XIX. While there is some slight evidence from "hesitations" that the sound stimulus was being noticed from the very first, not until the thirty-fifth series did good evidence of learning appear, and learning was practically complete by the thirty-eighth series. The record for number 129 of Group XVIII shows the most gradual rise of any, although for seventeen series, there is scarcely any sign of learning. For this rat it took thirty-one series to complete the learning after it began to exhibit progress whereas the usual number with the sound is much less, one even falling as low as four. It is the only case where the learning can be truly spoken of as a gradual process.

(3) Pain,—Seven groups of animals, XX-XXVI inclusive, were taught the negative response to pain. All of these represent normal records except Group XXII. Its record, therefore, has been omitted. This group was the first to be trained with the pain stimulus and served in part to work out the particular method for giving the pain stimulus. At first the stimulus was given at one point only as the animal came down the alley, the point being

FIGURE XIV

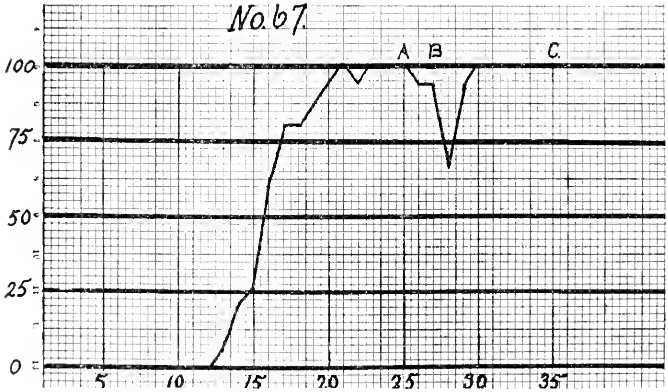


FIGURE XV

Curve 61 (Figure XIV) is an individual curve from Group Fourteen. Curve 67 (Figure XV) is an individual curve from Group Fifteen. Horizontal lines represent percentage of correct responses. Vertical lines represent the number of series of twenty trials each. In lettering,—A is the end of the learning process with the sound, B (in curve 61) is the end of the learning process with the light, B (in curve 67) is the end of the simultaneous series, C (in curve 67) is the end of the learning process with the light.

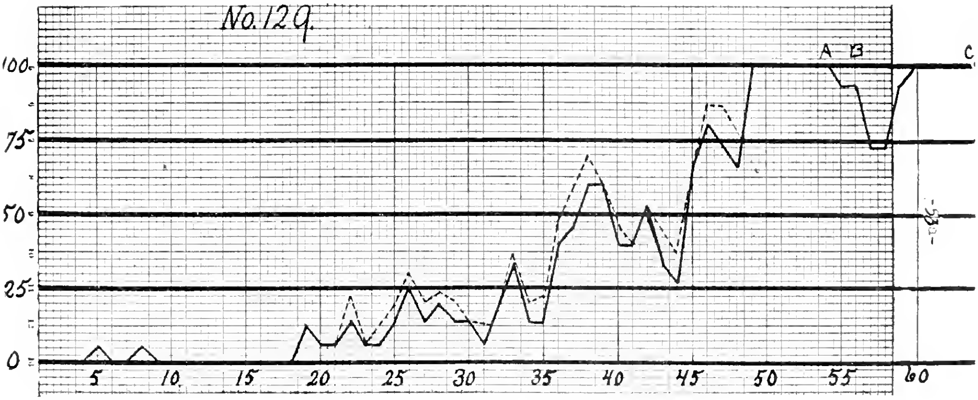


FIGURE XVI

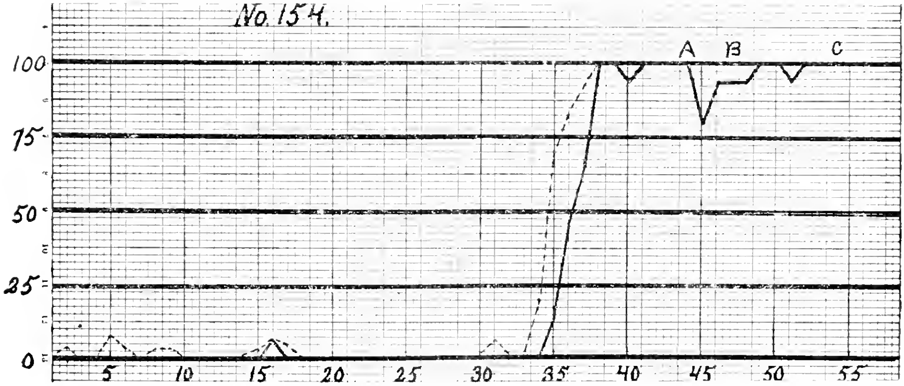


FIGURE XVII

Curve 129 (Figure XVI) is an individual curve from Group Eighteen. Curve 154 (Figure XVII) is an individual curve from Group Nineteen. Horizontal lines represent the percentage of correct responses. Vertical lines represent the number of series of twenty trials each. Dotted lines represent the record both for hesitations and for correct responses. In lettering,—A is the end of the learning process with the sound, B is the end of the simultaneous series, C is the end of the learning process with the pain.

varied from trial to trial. However, later to make the conditions more nearly uniform with the training given with other sorts of stimuli, the stimulus, when once given, was continued until the animal turned back. The following tables summarize the results:

GROUP XX

Animal	Trials	Series
No. 165.........	210	14
No. 166.	210	14
No. 167.........	195	13
No. 170.........	150	10
Average.........	191.25	12.75

GROUP XXI

Animal	Trials	Series
No. 162.........	210	14
No. 163.........	240	16
No. 164.........	165	11
No. 168.........	165	11
No. 169.........	210	14
Average.........	198	13.2

GROUP XXIII

Animal	Trials	Series
No. 157.........	195	13
No. 158.........	180	12
No. 159.........	150	10
No. 160.........	210	14
No. 161.........	240	16
Average.........	195	13

GROUP XXIV

Animal	Trials	Series
No. 146.........	195	13
No. 147.........	150	10
No. 148.........	195	13
No. 149.........	225	15
No. 150.........	150	10
No. 151.........	180	12
Average.........	182.5	12.17

GROUP XXV

Animal	Trials	Series
No. 141.........	180	12
No. 142.........	165	11
No. 143.........	150	10
No. 144.........	135	9
No. 145.........	180	12
Average.........	162	10.8

GROUP XXVI

Animal	Trials	Series
No. 134.........	270	18
No. 135.........	255	17
No. 136.........	225	15
No. 137.........	195	13
No. 138.........	180	12
No. 139.........	165	11
Average.........	215	14.3

Average for 31 animals, 191.13 trials, 12.74 series.

A number of illustrative curves are given below. These curves show the record of these animals not only while learning the response to the pain stimulus, but also when transferred to the other sorts of stimuli. They will be referred to later also when discussing transfer from pain to light and sound. See Figures XVIII to XXI inclusive.

Here again practically all curves show a period of no progress at the beginning, then a rapid rise to perfection and no long process of perfecting.

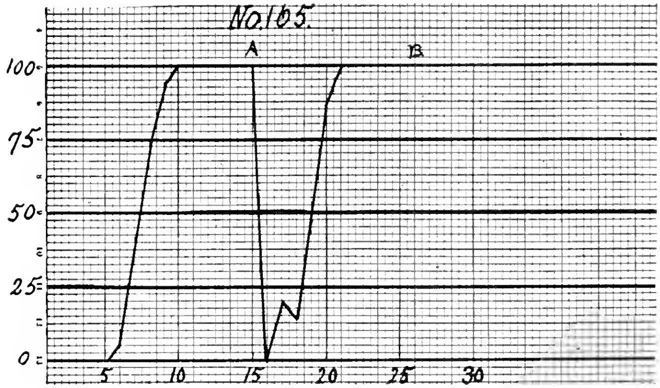


FIGURE XVIII

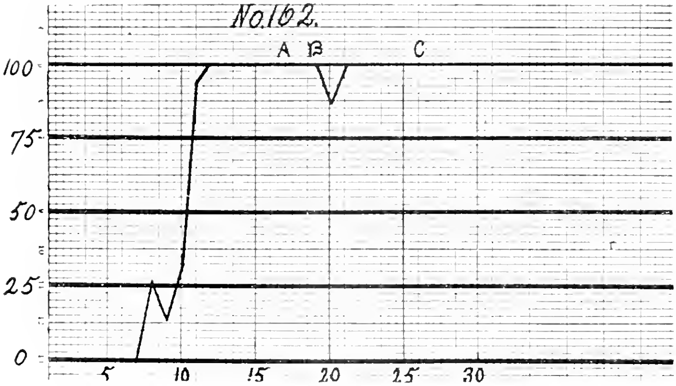


FIGURE XIX

Curve 165 (Figure XVIII) is an individual curve from Group Twenty. Curve 162 (Figure XIX) is an individual curve from Group Twenty-One. Horizontal lines represent the percentage of correct responses. Vertical lines represent the number of series of twenty trials each. In lettering,—A is the end of the learning process with the pain, B (in curve 165) is the end of the learning process with the light, B (in curve 162) is the end of the simultaneous series, C (in curve 162) is the end of the learning process with the light.

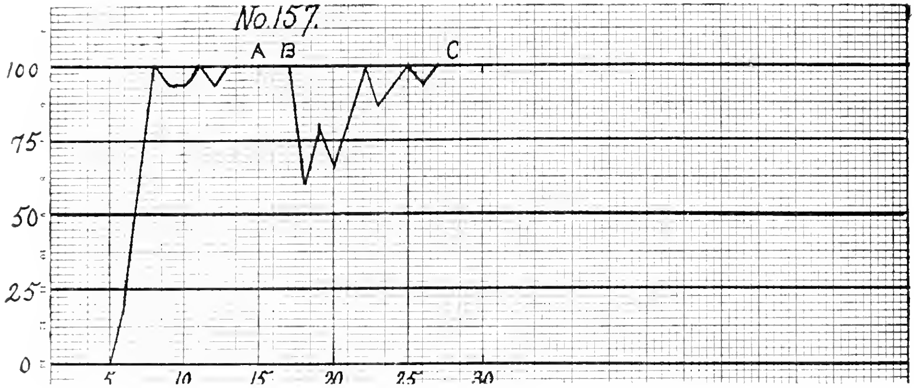


FIGURE XX

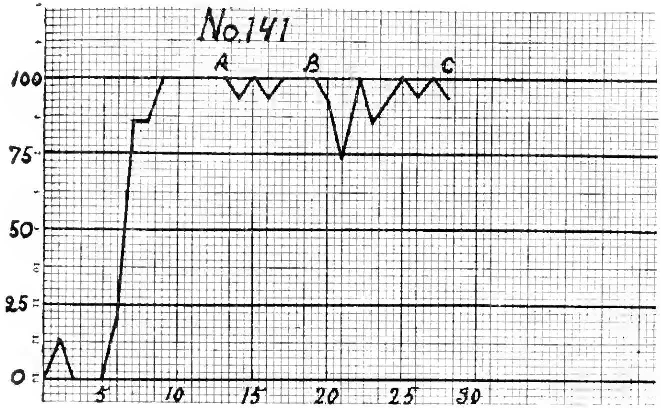


FIGURE XXI

Curve 157 (Figure XX) is an individual curve from Group Twenty-three Curve 141 (Figure XXI) is an individual curve from Group Twenty-five. Horizontal lines represent the percentage of correct responses. Vertical lines represent the number of series of twenty trials each. In lettering,—A is the end of the learning process with the pain, B is the end of the simultaneous series, C is the end of the learning process with the sound.

Another noticeable feature in these cases is the smoothness of the rising curve. It seems that when once the process is started there is practically no loss, but what is gained in one day's work is retained in the next in practically all cases. This last feature does not stand out so strikingly in the curves for learning the negative response to the light stimulus, and still less in the curves for learning the sound stimulus.

Another interesting feature closely related to the one just mentioned is the almost complete absence of marked plateaus. The curves for pain are the freest, those for light next, and those for sound least free.

This feature would indicate that for white rats working under the conditions of these experiments with negative response, the electric shock is the most uniformly effective stimulus of the three sorts of stimuli used. The light ranks second, and sound ranks third. We have every reason to believe from their general behavior in the presence of the electric shock that white rats are naturally negative to such a shock. We do not have definite proof that white rats are negative to the light and sound used. From general observation of their behavior in the presence of the light and sound stimuli used the writer is inclined to believe that they are more likely to prove to be negative to the sound than to the light. The electric shock uniformly produced the greatest emotional disturbance in the animals, calling for the greatest exercise of patience on the part of the experimenter. The electric shock most uniformly compelled the attention of the animal. The first few series of non-learning with the electric shock were not due to lack of attention to the stimulus as was quite evidently the case, in part at least, with the sound and light. Rather they were due to emotional disturbance produced by the strangeness of the stimulus. So these non-learning series might after all be counted as series indicating the acquirement of emotional adjustment to the stimulus rather than the specific response adjustment which the rise in the curve is supposed to indicate. Thus it might be claimed that, if an ideal learning process is one in which there is progress practically from the beginning and one which shows no relapses nor plateaus, the process of learning the negative response to the electric shock in these experiments represents practically an ideal case.

There is no striking difference in the lengths of the non-learning periods for the pain and for the light. Neither is there any important difference in the number of series necessary to complete the learning process with the pain and with the light. The only really significant difference is in the smoothness of the rise of the curve in each case, yet this difference is not so great as to be very striking. It is likely due, as suggested above, to the greater power of compelling attention which the electric shock seems to possess. Its unlikeness, measured from the human standpoint, to anything else falling within the normal experience of the white rat might be the secret of its power.

But we still have the question of why the light was practically just as effective as the electric shock while the sound was decidedly less effective, having both a much longer non-learning period, a much greater irregularity of effectiveness, and a much longer learning period. Why this advantage of light over sound if the animals are really negative to neither? As suggested above, the animals seemed more inclined to be negative to the sound than to the light, if we are to judge from the general disturbance in their behavior produced by the two. If this were true, one would expect the response to the sound to be learned more quickly than to the light. Just the opposite was the case. Moreover, the animals were evidently negative to the electric shock but not to the light. Yet they learned the response to the light in practically the same way and practically as quickly as to the electric shock. But it might be that the greater emotional disturbance caused by the electric shock offset its greater effectiveness due to the animal's being negative to it. Perhaps the explanation of the comparative effectiveness of the light and sound can be found partly in the general conditions under which the experiments were conducted. It was much easier to control the light from external sources than the sound. So the light when presented represented a more uniform contrast with the general environment than the sound when it was presented. This more uniform contrast might have increased the effectiveness. Then we still have the problem of the comparative intensities of stimuli from the different sense fields. For instance, how intense a sound shall be considered to be equal to a given intensity of light or of electric shock? We have no way of determining this independent of the reaction of the animals. All that can be said in the present case is that under the conditions of the experiments the electric shock was the most uniformly effective stimulus, the light next, and the sound least, in establishing the required negative response.

Another interesting thing not indicated in the curves and tables was the attempt on the part of some of the animals when learning the response to the pain to avoid the stimulus by jumping over the places where the shock had been previously received. A few typical cases are those of numbers 114, 115, 124, 128. The record of number 114 is as follows: in the fifth series of trials, it jumped twice; in the seventh series, five times; in the eighth, four times; in the ninth series, once; in the eleventh series, twice. The record for number 115 is as follows: in the fourth series, once; in the fifth series, three times; in the eleventh series, once. The record for 124 is as follows: in the twelfth series, five times; in the thirteenth series, once; in the sixteenth and seventeenth, once each. The record for 128 is: in the eighth series, eight; in the tenth series, twice. All of these "jumps" occurred before the change of procedure took place as mentioned in the description of this series of experiments. That is, they all occurred when the pain stimulus was given at but one place in the alley rather than all the way down as was the case in the method finally used. It is noteworthy, too, that these "jumps" occurred as a sort of preliminary method of responding to the pain stimulus, since after the animal had really begun to choose, or, in other words, to make the correct response no attempt at such avoidance occurred. Not all the animals in this group which learned the pain stimulus attempted to avoid it in this way. The four animals mentioned are the best examples in the records.

The fact of "jumping" indicates a more or less definite sense of location, although there is no evidence that the "jump" was even generally made at the place where the last shock had been received. Most of them occurred at the first place in the alley where it was possible to receive the shock or at the last place. Some few occurred at every place in the alley, that is, when the animal started down the alley, it jumped at *every place* where a shock was likely to be received and so completely avoided the shock. One would be led to suspect by gross observation that vision was mainly instrumental in guiding the reaction in this jumping.

(4) Summary,—If we are to gather together the results for learning the negative response we find the following facts outstanding:

(a) The electric shock is the most effective stimulus, both from the point of view of rapidity of learning and from the point of view of smoothness of the learning curve, there being practically no lapses and no plateaus.

(b) The effectiveness of the electric shock is probably due to the fact that the animals were negative to the shock. The failure to show progress from the beginning was likely due to the emotional disturbance produced by the strangeness of the stimulus.

(c) The superior effectiveness of the light over the sound was probably due in part but not wholly to the better control of general environmental conditions in the case of the light.

(d) *The inferior effectiveness of the sound in comparison with the light and pain might indicate also that the visual and tactual stimuli play a more important part in the regular life of the white rat than do auditory stimuli. While the tactual has always been assigned a prominent place, there has been a tendency to belittle the importance of the visual. Another result pointing toward and emphasizing the visual has already been mentioned in discussing the results of the experiments where pain was given as punishment for wrong choice when learning the positive response to the sound stimulus. Also, if "jumping" just reported is guided by vision, it furnishes an added case in point.

---

* Bearing on this point, see, Pearce, *Jour. Animal Behav.*, Vol. 7, No. 3, p. 177.

(e) Attention has been called to the unusual form of the majority of the individual learning curves of animals learning the negative response. Contrary to the nature of the ordinary animal learning curve the greatest improvement does not take place in the earlier trials, but there is at first a period of little or no progress and later a period of rapid improvement with a quick perfecting of the response. In form these curves come more nearly into agreement with Swift's curves on Learning to Toss and Catch Balls. *As far as the writer is able to discover, the curves given in this paper are the first animal curves of this nature to be reported. Going on the assumption that perhaps the difference was due to the fact that all the evidences of learning were not being recorded, an attempt was made to correct this failure by taking note of "hesitations" and "retardations" in the animals' actions which could evidently be assigned to the stimulus as cause. But even this did not "correct" the form of the curves. As far as could be determined "errors" or "choices," with modifications of the latter in the form of "hesitations" and "retardations of movement," were the only criteria which could be made use of in measuring the progress of learning or the relative rate of learning. Unless we posit some change in the organism which is produced by the various trials but which does not manifest itself extrinsically in measurable behavior until a certain degree of strength has been reached, and then manifests itself by taking almost complete control of the animal's activity in that situation, we are at a loss to account for the difference in the rate of progress of learning. That would mean that in certain kinds of problems we might expect learning curves of this kind or, in other words, that the shape of the learning curve depends in part upon the nature or kind of problem.

In contrast with these curves for the learning of the

---

* NOTE: Professor Yerkes, of Harvard, reports in his monograph on the Mental Life of Monkeys and Apes (Behavior Monographs, Volume 3, Number 1, 1916, page 68) that one of his animals (Julius) showed the type of learning curve found in the work reported in this paper and claims that never before has a curve of learning like this been obtained from an infra-human animal. However, his work was done subsequent to the work reported in this paper. The work reported here was done during 1913 and 1914, but, owing to unavoidable delay has not been published until now.

negative response, the curves for the learning of the positive response show none of these peculiar features. In the positive curves there is evidence of learning from the very beginning, the process is quite gradual until completed, the process is quite irregular, showing both lapses and plateaus, and when learning is once completed, the standard is not so consistently maintained as in the negative. Since the general situations in the two problems were quite alike and the specific stimuli used (light and sound) were quite alike, the different character of the responses required must be largely the cause of the striking differences in the learning curves. It is to be remembered that the negative curves for sound do not stand out in as great contrast as the other negative curves.

SECTION FOUR

FEATURES BEARING ON TRANSFER

A. *Positive*

(1) Light to Sound,—Two groups were trained with the light stimulus and transferred to sound. Group I was transferred directly from the light to the sound when once the response to the light had been learned. With Group II each animal was given ten series or two hundred trials with the light and sound stimulus presented simultaneously, after the response to the light had been learned, and then transferred to the sound.

The curves given on page 20 show the general results. These curves show the learning of the response to the light stimulus, but they also show the record for the control series in each case, the record for any simultaneous light and sound stimuli, and the record for the sound stimulus alone which was substituted for the light stimulus.

The immediate result of the transfer was that the two groups dropped in efficiency 36 per cent and 26 per cent respectively, or down to that degree of efficiency which can be accounted for by chance or position habits. Three of Group I were given 25 series or 500 trials with the sound alone and at that time showed an average efficiency of 72 per cent whereas they started the sound with an average of 67 per cent. The other two of this group were given 11 series or 220 trials with the sound, and ended with an average of 70 per cent, whereas they started the sound with an average of 55 per cent. All six members of Group II were given 20 series or 400 trials with the sound alone after the series with sound and light simultaneously and ended the 20 series with an efficiency of 67 per cent whereas they began the sound alone with an average of 73 per cent. As stated above a study of the records shows that the animals all began to depend almost wholly upon alternation when the sound was substituted for the light, although they do not maintain it with any high degree of regularity at all. Because no promise of further results was evident and because of the negative results in learning sound with groups already reported the work with these animals was discontinued.

(2) Sound to Light,—Since no animals learned the positive response to the sound we had none to transfer from sound to light. This part of our program had to be abandoned.

B. *Negative*

(1) Light to Sound,—Three groups of animals were first taught the negative response to the light and then were transferred to the sound. Group VIII was transferred directly from light to sound. Group IX was given two series of simultaneous light and sound stimuli after having learned the response to the light and then were transferred to the sound. Group X was given four series of simultaneous light and sound stimuli after having learned the light and then were transferred to the sound. The results are gathered together in the following tables. These tables indicate the number of series and trials necessary for learning the response to the sound after having learned the response to the light, and after having been trained with, the simultaneous light and sound wherever such training was given. Illustrative curves showing the complete records of certain individuals may be seen on pages 33-35.

To understand these tables one must keep in mind that only fifteen chances to respond to the stimulus were given in each series of twenty trials.

GROUP VIII

This group was transferred directly from light to sound

Animal	Trials	Series
No. 53	150	10
No. 54	150	10
No. 55	150	10
No. 56	165	11
No. 57	150	10
No. 58	150	10
Average	152.5	10.16

GROUP IX

This group was given two series of simultaneous light and sound

Animal	Trials	Series
No. 77	165	11
No. 78	150	10
No. 79	165	11
No. 80	120	8
Average	150	10

GROUP X

This group was given four simultaneous light and sound

Animal	Trials	Series
No. 93........	105	7
No. 94........	105	7
No. 95........	105	7
No. 99........	105	7
No. 102.......	105	7
Average.......	105	7

SUMMARY OF RESULTS

	Trials	Series
Average for learning sound (negative) shown in normal records..	525.5	35.03
Gained by direct transfer....................................	373	24.87
Gained by 2 simultaneous light and sound.....................	375.5	25.03
Gained by 4 simultaneous light and sound.....................	420.5	28.03

The first thing to be noticed in the results is the fact that it is a decided advantage in learning the response to the sound to learn first the response to the light and then substitute the sound stimulus for the light stimulus either directly or with a few intervening series in which the sound and light are simultaneously given. Thus it took on the average 35.03 series for the animals to learn the response to the sound when no previous training had been received in making such a response. But by training first with the light stimulus and then substituting directly the sound stimulus only, 25.96 series were needed for learning the response to both the light and the sound and only 10.16 of these series were used in learning the response to the sound. That represents a saving of 24.87 series in learning the response to the sound by first learning the response to the light. By presenting the two stimuli simultaneously for a few series after the response to the light had been learned, a greater saving was effected. Thus with two simultaneous series a saving of 25.03 series was secured, while with four simultaneous a saving of 28.03 series was secured. This means that by training with the light until the response had been learned and by giving four series with light and sound simultaneously, the animals were trained to respond to the sound alone with a sufficient degree of accuracy to enable them to meet the standard during the next seven series, the shortest possible time in which they could meet the standard set. It also means that the quickest way for the animals to learn the response to the sound is not to train them directly with the sound right from the first, but is to train them first with the light, then give a few simultaneous light and sound series, and then give the sound alone. The latter method required only 24 series altogether for the light, for the simultaneous light and sound, and for the sound, while the former method required 35.03 series all told for the sound only.

With Group VIII after learning the response to the light and the sound of the regular electric sounder, tests were also made with the sound produced by striking a three inch bell with a rubber hammer. This bell was held by the experimenter in front of the box and below the vision of the animal. After tests with the bell, tests were also made with wooden pipes of 2048, 1024, 512 and 256 vibrations. These pipes were sometimes blown by the experimenter who handled the animals during the experiments and sometimes by an assistant who stood just behind the regular experimenter. In sounding the bell or pipes no attempt was made to make a regular continuous sound. The only attempt was to keep the sound of moderate loudness whether given irregularly or continuously. After a few trials with these devices as sounders, it was the usual thing for the animal to turn back at the first sound of the bell or the first " toot " of the pipe, that is, the transfer was practically perfect.

It was in these trials with the sound after having learned the light, that there was some evidence of " position in the alley " functioning as a stimulus to cause the return of the animal, that is, the animal would turn back without any sound stimulus having been given. In cases where the animal did turn back, when no stimulus was given by the experimenter or before such stimulus had been given, and entered the other return alley the stimulus was given in the other alley and the animal required to return to the alley first chosen to get back to the food box. Such cases, however, were rare. A few typical records run as follows: Number 77 made one false return out of 540 trials; number 78 made four false returns out of 580 trials; number 79 made three false returns out of 560 trials; number 80 made three false returns out of 580 trials; number 102 made three false returns out of 500 trials. The number of false returns recorded run from none to four per animal. Since they were so few no record was kept except for a part of the animals used. It must be noted, however, that practically all of the false returns were made after the animal had learned the response to one stimulus and had been transferred to the other. Practically all of them also are to be found in the records where sound was the stimulus being used. Some of the false returns can be accounted for by extraneous sounds from outside the laboratory. These false returns, to the writer's mind, indicate that the response had become considerable of a reflex and any sort of stimulus of a kind similar to the one which had become effective was sufficient to set it off rather than a matter of habitual turning at a particular place in the alley or at a particular position in the series of trials; in other words, *the response had become generalized for stimuli of that sort.*

Another thing which indicates that the response had become generalized for all stimuli of the sound type which could be " sensed " by the animal is the record of Group VIII with the bell and the pipes. As indicated by the record no great decrease in ability to respond was produced by substituting these two sorts of stimuli for the regular sound stimulus. The tests with the bell and the pipes were given as preliminary tests of the rat's ability to hear tones. Electrically driven tuning forks were also tried, but it was found to be impossible to control the sound of the tuning forks sufficiently well to suit the type of reaction required or already learned by the animal. After being trained on the light and the regular sound, the animal's reaction took place so rapidly that a mere click of the sounder was sufficient to stop the headlong course of the animal and cause it to turn back into the other alley. After placing the animal in the main alley, so quick was the choice that time was scarcely to be had to press the key and give the sound as a warning to turn back. In the case of the tuning forks the time required to get the fork vibrating with sufficient ntensity to make a plainly audible tone (for the human ear) was so long that the animal had made the complete run up the main alley and down one of the return alleys before the fork had begun to give out such a tone. So the work was abandoned as far as making it any further test for sensitivity to tonal stimuli was concerned. Hunter's work on Tonal Sensitivity in White Rats, recently published, makes it reasonably clear that the rats, in cases where the bell and pipes were used, were really reacting to the noise element of the stimuli and not to the tonal element. So even if the tuning forks could have been properly controlled for such a response as was here required, it is extremely doubtful whether the animal's action would have been affected by it.

(2) Light to Electric Shock—Three groups of animals were first taught the negative response to the light and then were transferred to the pain. Group XI was transferred directly from light to electric shock, Group XII was given two series of simultaneous light and electric shock after having learned the response to the light and then were transferred to the electric shock, Group XIII was given four series of simultaneous light and electric shock after having learned the response to the light and then were transferred to the electric shock. The results are gathered together in the following tables. These tables indicate the number of series and trials necessary for learning the response to the electric shock after having learned the response to the light, and after having been trained with the simultaneous light and electric shock whenever such training was given. Illustrative curves showing the complete records of certain individuals may be seen on page 36.

To understand these tables one must keep in mind that only fifteen chances to respond to the stimulus were given in each series of twenty trials.

GROUP XI

Direct Transfer

Animal	Trials	Series
No. 84..........	165	11
No. 85..........	165	11
No. 86..........	165	11
No. 91..........	135	9
No. 97..........	135	9
No. 101.........	180	12
Average.........	157.5	10.5

GROUP XII

Transferred after two simultaneous series

Animal	Trials	Series
No. 81..........	150	10
No. 82..........	180	12
No. 89..........	150	10
No. 90..........	135	9
No. 98..........	165	11
Average.........	156	10.4

GROUP XIII

Transferred after four simultaneous series

Animal	Trials	Series
No. 83........	105	7
No. 87........	105	7
No. 88........	105	7
No. 92........	105	7
No. 96........	105	7
Average.......	105	7

SUMMARY OF TABLES

	Trials	Series
Average for learning electric shock, Normal Record given........	191.13	12.74
Gained by direct transfer....................................	33.63	2.24
Gained by two simultaneous.................................	35.13	2.34
Gained by four simultaneous.................................	86.13	5.74

Again in this series of experiments we find decided evidence of transfer. The question here would be whether first learning the response to the light helped or hindered the learning of the same response to the electric shock. The results show that by first learning the response to the light a saving of 2.24 series was made in learning the response to the electric shock. Whereas by introducing two series of simultaneous light and pain after learning the light a saving of 2.34 series was made in learning the pain, and by introducing four simultaneous a saving of 5.74 series was made or, in other words, by first learning the light and by making use of four series of simultaneous light and pain, the animals were able to meet the required standard for the pain in the next seven series, the smallest number possible.

One might wonder why only 2.34 series were gained by two simultaneous when 2.24 were gained by direct transfer and 5.74 series were gained by four simultaneous. The explanation may be found in the following fact. The stimulus was always more or less disturbing whenever an animal was introduced to it for the first time. This was particularly true whenever the electric stimulus was given alone. Considerable care and patience had to be exercised with the animals until they got over the emotional disturbance. This usually required about four series. The two or four simultaneous series, while not showing this disturbance in a uniformly low percentage of correct responses, yet served mainly, in the experimenter's opinion, to overcome the emotional disturbance produced by such a strange stimulus. It is very possible that if the electric stimulus had had as little disturbing effect emotionally as the light apparently had, the transfer effect would have been more in proportion to the number of simultaneous series given, and so more gradual in its increase. Practically the same type of result was found in the case of transfer from light to sound, where two simultaneous series showed very little advantage over direct transfer, while four simultaneous series produced considerable advantageous effect.

(3) Sound to Light —Three groups of animals were first taught the negative response to the sound and then were transferred to the light. Group XIV was transferred directly from sound to light. Group XV was given two simultaneous series of sound and light before being transferred to light. Group XVI was given four series of simultaneous sound and light before being transferred to light. The results are gathered together in the following tables. Illustrative curves are given on pages 40-41.

GROUP XIV

Transferred directly from Sound to Light. Summary of record for Light

Animal	Trials	Series
No. 59..........	165	11
No. 61..........	165	11
No. 63..........	150	10
No. 64..........	120	8
No. 65..........	150	10
Average.........	150	10

GROUP XV

Transferred with two simultaneous series of Sound and Light. Summary of Record for Light.

Animal	Trials	Series
No. 60..........	135	9
No. 67..........	120	8
No. 68..........	120	8
No. 69..........	120	8
No. 70..........	120	8
No. 73..........	105	7
Average.........	120	8

GROUP XVI

Transferred with four simultaneous series of Sound and Light
Summary of Record for Light

Animal	Trials	Series
No. 62........	105	7
No. 66........	105	7
No. 72........	105	7
No. 74........	105	7
No. 75........	105	7
No. 76........	105	7
Average	105	7

SUMMARY SHOWING EFFECT OF TRANSFER

	Trials	Series
Average for learning Light, Normal Record found	198.87	13.26
Gained by direct transfer from Sound	48.87	3.26
Gained by transfer with two simultaneous series	78.87	5.26
Gained by transfer with four simultaneous series	93.87	6.26

Little need be said here except to call attention to the fact that even in learning the response to the light which proved to be very easy for the animals, there was an advantage in first learning the same response to some other sort of stimulus. As in the series previously reported a few series with the two stimuli simultaneously given overcame almost entirely any loss there happened to be by passing directly from learning the one to learning the other.

(4) Sound to Pain — Three groups of animals were trained with the sound and transferred to the pain. Group XVII was transferred directly from sound to pain. Group XVIII was given two series of simultaneous sound and pain after learning the response to the sound and then were transferred to the pain. Group XIX was given four series of simultaneous sound and pain after learning the response to the sound and then were transferred to the pain. The results are gathered together in the tables given below. Illustrative curves are to be found on page 41.

GROUP XVII

Transferred directly from sound to pain

Animal	Trials	Series
No. 104	105	7
No. 105	120	8
No. 108	135	9
No. 109	120	8
No. 110	135	9
No. 111	135	9
No. 113	120	8
Average	124.3	8.3

GROUP XVIII

Transferred with two simultaneous series of sound and pain

Animal	Trials	Series
No. 129	120	8
No. 130	180	12
No. 131	120	8
No. 132	135	9
No. 133	150	10
Average	141	9.4

GROUP XIX

Transferred with four simultaneous series of sound and pain

Animal	Trials	Series
No. 152	105	7
No. 153	105	7
No. 154	105	7
No. 155	105	7
No. 156	105	7
Average	105	7

SUMMARY

	Trials	Series
Normal Record for learning pain found	191.13	12.74
Gained by direct transfer	66.83	4.44
Gained by two simultaneous	50.13	3.34
Gained by four simultaneous	86.13	5.74

Little comment is necessary here except to call attention to the fact that the direct transfer brought a greater saving in learning the pain series than the transfer with two simultaneous sound and pain series. Notice was called in the series of experiments where the animals were transferred from light to sound to a somewhat similar case where two simultaneous series produced little advantageous effect over direct transfer. In the present case the two stimuli, when simultaneously given for two series, actually proved a hindrance to transfer when compared with the direct transfer effects with the same two stimuli. *This is the only instance in all the experiments performed where simultaneous series proved less advantageous than direct transfer.*

(5) Pain to Light—Two groups of animals were transferred from pain to light. Group XX was transferred directly. Group XXI was given two series of simultaneous pain and light, after having learned the response to the pain, and then were transferred to the light. The results are given in the tables below. Illustrative curves are to be found on page 43.

GROUP XX

Transferred directly from pain to light

Animal	Trials	Series
No. 165	165	11
No. 166	165	11
No. 167	135	9
No. 170	120	8
Average	146.25	9.75

GROUP XXI

Transferred with two simultaneous series

Animal	Trials	Series
No. 162	105	7
No. 163	105	7
No. 164	105	7
No. 168	105	7
No. 169	105	7
Average	105	7

SUMMARY

	Trials	Series
Average for learning light, Normal Record found	198.87	13.26
Gained by direct transfer	52.62	3.51
Gained by two simultaneous	93.87	6.26

One interesting feature of the results is that even in the case of learning the response to the light, which was very easily acquired on the average, it turned out to be advantageous to learn first the response to the pain stimulus.

While two simultaneous pain and light series were given the second group and that number proved sufficient to perfect the transfer effect, yet it is the conviction of the writer from observation of the general behavior of the animals while the simultaneous series were being given that one series of simultaneous stimuli would have been sufficient. Since this series of experiments was the last to be performed, time prevented the testing out of that conviction.

Another interesting feature not shown in the regular tables and curves was the attempt to discover how many times an animal would turn back when the stimulus was presented, without being allowed to return to the food box after a correct response had been given. If the animal chose the right alley it was confronted with the stimulus. Then when it returned and entered the left alley it was again met by the stimulus. Then upon returning to the right alley, the stimulus was again given. This was continued until the animal disregarded the stimulus or until it refused to work longer. Number 168 turned back three times before disregarding the stimulus. Number 169 turned back seventeen times and then refused to work any longer. Number 162 turned back nine times before disregarding the stimulus; number 163 five times; number 164 eight times, and number 166 thirteen times. These figures indicate that the response had become reflex and also that it was not merely a habit of entering one alley and turning back and returning to the food box through the other. These tests were conducted with both doors to the food box open and some of the returns to the other alley were made when the animal was within sight of the open door to the food box. This test was tried only with the light stimulus and only with those animals which had been transferred from the pain to the light.

(6) Pain to Sound — Five groups were trained with the pain and transferred to the sound. Group XXII was transferred directly from pain to sound. Group XXIII was given two series of simultaneous pain and sound. Group XXIV was given four series of simultaneous pain and sound. Group XXV was given six series of simultaneous pain and sound. Group XXVI was given eight

series of simultaneous pain and sound. The following tables gather the results together. Illustrative curves will be found on page 44.

GROUP XXII

Transferred directly from pain to sound

Animal	Trials	Series
No. 114..........	420	28
No. 115..........	525	35
No. 116..........	150	10
No. 171..........	165	11
No. 117..........	150	10
No. 118..........	495	33
No. 123..........	450	30
No. 124..........	240	16
No. 125..........	495	33
No. 126..........	210	14
No. 127..........	285	19
No. 128..........	165	11
Average..........	312.5	20.83

GROUP XXIII

Transferred with two simultaneous series

Animal	Trials	Series
No. 157..........	165	11
No. 158..........	255	17
No. 159..........	135	9
No. 160..........	225	15
No. 161..........	120	8
Average..........	180	12

GROUP XXIV

Transferred with four simultaneous series

Animal	Trials	Series
No. 146..........	225	15
No. 147..........	210	14
No. 148..........	285	19
No. 149..........	180	12
No. 150..........	150	10
No. 151..........	210	14
Average..........	210	14

GROUP XXV

Transferred with six simultaneous series

Animal	Trials	Series
No. 141..........	135	9
No. 142..........	120	8
No. 143..........	105	7
No. 144..........	135	9
No. 145..........	105	7
Average..........	120	8

GROUP XXVI

Transferred with eight simultaneous series

Animal	Trials	Series
No. 134.......	105	7
No. 135.......	105	7
No. 136.......	105	7
No. 137.......	105	7
No. 138.......	105	7
No. 139.......	105	7
Average.......	105	7

SUMMARY

	Trials	Series
Average for learning sound, Normal Record found..............	525.5	35.03
Gained by direct transfer................................	213.	15.2
Gained by two simultaneous..............................	345.5	23.03
Gained by four simultaneous..............................	315.5	21.03
Gained by six simultaneous..............................	405.5	27.03
Gained by eight simultaneous..............................	420.5	28.03

It again stands out clearly that it is an advantage in learning one sort of stimuli to have first learned the same response to another sort of stimuli, and that a few simultaneous series will render the transfer effect sufficiently perfect for the animal to meet at once the standard of learning set up at the beginning. A glance at the tables will show, however, that a gradually increasing number of simultaneous series does not bring a gradually increasing advantage. Attention to this fact has been called in previous series of experiments. In the present case two simultaneous series proved more advantageous than four simultaneous series. Whether a larger number of animals would give an average which would eliminate this discrepancy and the ones previously reported is yet to be tested. Theoretically, one might expect them to be eliminated.

In order to get before us all the tabular results bearing on the problem of transfer, we have gathered them together and expressed the results, not only in the number of trials gained, but also in the percentage of gain. This will enable the reader to get at the amount and the conditions of transfer most quickly.

Light to Sound

	Trials	
Normal sound record	525.5	
Gained by direct	373	88.7%
Gained by two simul.	375	89.1%
Gained by four simul.	420.5	100%

Sound to Light

	Trials	
Normal light record	198.87	
Gained by direct	48.87	52%
Gained by two simul.	78.87	84%
Gained by four simul.	93.87	100%

Pain to Light

	Trials	
Normal light record	198.87	
Gained by direct	52.62	56%
Gained by two simul.	93.87	100%

Light to Pain

	Trials	
Normal pain record	191.13	
Gained by direct	33.63	39 %
Gained by two simul.	35.13	40.7%
Gained by four simul.	86.13	100%

Sound to Pain

	Trials	
Normal pain record	191.13	
Gained by direct	66.83	77.5%
Gained by two simul.	50.13	58.2%
Gained by four simul.	86.13	100%

Pain to Sound

	Trials	
Normal sound record	525.5	
Gained by direct	213	50.6%
Gained by two simul.	345.5	82.1%
Gained by four simul.	315.5	75 %
Gained by six simul.	405.5	96.4%
Gained by eight simul.	420.5	100%

C. *Summary of Conclusions*—The results of the experiments reported in this paper go to support the general contention that "the fact of transfer can not be doubted," that is, of positive or advantageous transfer. It has been found that in every case learning a response to one situation, having a given element or stimulus as the dominant or controlling factor, is a help in learning the same response to the same situation but having a different element or stimulus, not present before, as the dominant or controlling factor; that even in some situations the learning process for one sort of dominant stimulus is actually reduced in length so much by first introducing another dominant stimulus, that the time and effort for both is less than for the one alone. Such was actually the case in learning the negative response to the sound.

Looking at the results from the point of view of "generalized response" or "generalized habit" we can say that responses are not always *particular*, but may become truly *general*; that is, the same response may serve for situations whose dominant stimuli are as different as can be found, provided the other features of the situation are the same and remain constant. This is really the reverse of the cases usually spoken of under the term "generalized habit." In such cases the dominant features are supposed to remain the same while the minor features vary from one case to another. So "generalized habit" can not always be said to depend upon similarity of dominant features in the situations responded to, or upon lack of discrimination of such dominant features. We can secure "generalized responses" where the dominant features of the situation are actually different for the organisms making the responses, provided we can take difference of *sense channel* as the basis for discrimination of stimuli. In such cases there would be difference of neural pathways, at least in the sensory portion of such pathways. The "identical elements" in such pathways would be at least partly in the association and motor portions. Complete identity can not be said to be present.

According to the results given, one, and the only one, of the conditions of the degree of advantageous or positive transfer clearly shown is the simultaneous presentation of the two controlling or dominant stimuli after the response to one has been learned. While in practically every case considerable disturbance was produced at the introduction of the new stimulus along with the old, yet the presence of the old helped in every case in learning the new. In no case did it require more than eight series with the old and new together to perfect the response to the new. In one case, pain to light, it required only two series.

Would it be too much of a hazard to hypothesize thus.—Variations in stimuli allow of positive or advantageous transfer effects, while variations in response, an aspect of the problem which has not been tested in these experiments, produce negative effects?* Of course in any case where training of any sort has gone on in any general situation, the general familiarity with the situation will soon give a freedom from emotional disturbance and a general food seeking reaction. Both of these will tend to be carried over to later responses to the same general situation. These may offset any negative effects produced by a *simple* change in the response, unless, as was done with the experiments reported in this paper, the animals were first made perfectly familiar with the apparatus and with the ways back to the food box before any experimenting proper began, so that these factors were made as nearly equal in influence as possible.

These experiments are not to be regarded, nor are they regarded by the writer, as offering a final or complete solution of the problem of transfer. They are to be taken as offered—a very simple and humble beginning of the solution of that very complex problem. Our results have made clear that advantageous or positive transfer can and does take place in situations the most simple and have shown some of the conditions favoring such transfer. As the situations studied grow more complex it must be remembered that the conditions at work in these simpler situations *may* still account for many of the results obtained. On the other hand, however, with the introduction of many varying factors, both subjective and objective, the results for the simpler conditions may not hold at all.

---

* NOTE: Bearing on this suggestion, see
Hunter, *Jour. Animal Behav.*, Vol. 7, No. 1, pp. 49-65,
Pearce, *Jour. Animal Behav.*, Vol. 7, No. 3, pp. 169-177.

LITERATURE CITED

1. Coover. Formal Discipline from the Standpoint of Experimental Psychology. *Psych. Rev. Mon. Sup.*, Vol. 20, No. 3, pp. 224 ff.
2. Yerkes. Dancing Mouse, p. 262.
3. Thorndike. Animal Intelligence.
4. Richardson. A Study of Sensory Control in the Rat. *Psych. Rev. Mon. Sup.*, Vol. 12, No 1.
5. Yoakum. Some Experiments upon the Behavior of Squirrels. *Jour. Compar. Neurology and Psych.*, Vol. 19, p. 514.
6. Hunter. Some Labyrinth Habits of the Domestic Pigeon. *Jour. Animal Behav.*, Vol. I.
7. Bogardus and Henke. *Jour. Animal Behav.*, Vol. I, pp. 125-137.
8. Angell. *Psychology*, p. 261.
9. Gore. Article in Dewey's ' Studies in Logical Theory.'
10. Hunter. Auditory Sensitivity in the White Rat. *Jour. Animal Behav.*, Vol. V, pp. 312-330.
11. Watson. Behavior, p. 272.
12. Op. Cit. (No. 3).
13. Op. Cit. (No. 7).
14. Vincent. White Rat and Maze Problem. *Jour. Animal Behav.*, Vol. V, pp. 1-24, 140-158, 175-185.

The Behavior Monographs

Edited by JOHN B. WATSON

The Johns Hopkins University, Baltimore, Md.

VOLUME 1

No. 1 The development of certain instincts and habits in chicks. By Frederick S. Breed. Pp. iv + 78, $1.00, postpaid.

No. 2 Methods of studying vision in animals. By Robert M. Yerkes and John B. Watson. Pp. iv + 90, $1.25, postpaid.

No. 3 An experimental study on the death-feigning of Belostoma (–Zaitha Auect.) flumineum Say and Nepa apiculata Uhler. By Henry H. P. Severin and Harry C. Severin. Pp. iii + 47, $.65, postpaid.

No. 4 The biology of Physa. By Jean Dawson. Pp. iii + 120, $1.50, postpaid.

No. 5 The function of the vibrissae in the behavior of the white rat. By Stella Burnham Vincent. Pp. iv + 81, $1.15, postpaid.

VOLUME 2

No. 6 The delayed reaction in animals and children. By Walter S. Hunter. Pp. v + 86, $1.15, postpaid.

No. 7 The Canada porcupine: a study of the learning process. By Leroy Walter Sackett. Pp. iii + 84, $1.15, postpaid.

No. 8 Audition and habit formation in the dog. By Harry Miles Johnson. Pp. iv + 78, $1.00, postpaid.

No. 9 Habit formation in a strain of albino rats of less than normal brain weight. By Gardner Cheney Basset. Pp. iv + 46, $.65, postpaid.

No. 10 Distribution of effort in learning in the white rat. By John Linck Ulrich. Pp. iii + 51, $.65, postpaid.

No. 11 The effect of age on habit formation in the albino rat. By Helen B. Hubbert. Pp. v + 55, $.65, postpaid.

VOLUME 3

No. 12 The mental life of monkeys and apes: a study of ideational behavior. By Robert M. Yerkes. Pp. iv + 145, $1.50, postpaid.

No. 13 A study of perseverance reactions in primates and rodents. By G. V. Hamilton. Pp. iv + 65. $.75, postpaid.

No. 14 An analysis of the learning process in the snail, *physa gyrina* Say. By Elizabeth Lockwood Thompson Pp. iii + 97, $1.25, postpaid.

No. 15 The effect of length of blind alleys on maze learning: An experiment on twenty-four white rats. By Joseph Peterson. Pp. iii + 53, $.75, postpaid.

No. 16 An experimental study of transfer of response in the white rat. By Harry H. Wylie. Pp. iii + 66, $1.00, postpaid.

Regular subscription $5 per volume, postpaid.

THE BEHAVIOR MONOGRAPHS
Emerson Hall, Cambridge, Mass.

Directory of American Psychological Periodicals

American Journal of Psychology—Worcester, Mass.: Florence Chandler.
Subscription $5. 600 pages annually. Edited by G. Stanley Hall.
Quarterly. General and experimental psychology. Founded 1887.

Pedagogical Seminary—Worcester, Mass.: Florence Chandler.
Subscription $5. 575 pages annually. Edited by G. Stanley Hall.
Quarterly. Pedagogy and educational psychology. Founded 1891.

Psychological Review—Princeton, N. J.: Psychological Review Company.
Subscription (with Psychological Bulletin) $6. 480 pages annually.
Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin—Princeton, N. J.: Psychological Review Company.
Subscription $3.25. 480 pages annually. Psychological literature.
Monthly. Founded 1904. Edited by Shepherd I. Franz.

Psychological Monographs—Princeton, N. J.: Psychological Review Company.
Subscription $4 per vol. 500 pages. Founded 1895. Ed. by James R. Angell.
Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.: Psychological Review Company.
Subscription $1.25 200 pages. Founded 1895. Edited by Madison Bentley.
An annual bibliography of psychological literature.

Journal of Philosophy, Psychology and Scientific Methods—New York:
Sub-Station 84. Bi-weekly. 728 pages per volume. Founded 1904.
Subscription $3. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Substation 84, New York: Archives of Psychology.
Subscription $5. 600 pp. ann. Founded 1906. Edited by R. S. Woodworth.
Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology—Boston: Richard G. Badger.
Subscription $5. 480 pages annually. Edited by Morton Prince.
Bi-monthly. Founded 1906. Entire field of abnormal psychology.

Psychological Clinic—Philadelphia: Psychological Clinic Press.
Subscription $1.50. 280 pages annually. Edited by Lightner Witmer.
Monthly (9 numbers). Orthogenics, psychology, hygiene. Founded 1907.

Training School Bulletin—Vineland, N. J.: The Training School.
Subscription $1. 160 pages annually. Ed. by E. R. Johnstone. Founded 1904.
Monthly (10 numbers). Psychology and training of defectives.

Journal of Race Development—Worcester, Mass.: Louis N. Wilson.
Subscription $3. 460 pages annually. Founded 1910.
Quarterly. Edited by George H. Blakeslee and G. Stanley Hall.

Journal of Educational Psychology—Baltimore: Warwick & York.
Subscription $3. 600 pages annually. Founded 1910.
Monthly (10 numbers). Managing Editor, J. Carleton Bell.
(Educational Psychology Monographs. Edited by Guy M. Whipple.
Published separately at varying prices. Same publishers.)

Journal of Animal Behavior—Cambridge, Mass.: Emerson Hall.
Subscription $5. 450 pages annually. Founded 1911.
Bi-monthly. Robert M. Yerkes, Managing Editor.

The Behavior Monographs—Cambridge, Mass.: Emerson Hall.
Subscription $5. 450 pages per volume. Edited by John B. Watson.
Published without fixed dates, each number a single research.

Psychoanalytic Review—Washington, D. C.: 3617 Tenth St., N. W.
Subscription $5. 500 pages annually. Psychoanalysis.
Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J.: Psychological Review
Company. 480 pages annually. Experimental. Founded 1916.
Subscription $3.25 Bi-monthly. Edited by John B. Watson.

Journal of Applied Psychology—Worcester, Mass.: Florence Chandler.
Subscription $4. 400 pages annually. Quarterly. Founded 1917.
Edited by G. Stanley Hall, John Wallace Baird and L. R. Geissler.

THE BRANDOW PRINTING COMPANY
ALBANY N. Y.