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iii

CONTENTS

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A Study of Sensory Control in the Rat

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I desire to express here my obligation to Professor James R. Angell for constant assistance and encouragement. I am particularly indebted to Professor John B. Watson, under whose immediate direction the experimental work here presented was undertaken and carried out. My thanks are due also to Professor Harvey Carr for suggestions and criticisms of the manuscript, and to Miss Ethel Chamberlain, who assisted me during a portion of the experimentation.

CONTENTS.

INTRODUCTION.

PART FIRST.

I. EXPERIMENTAL RESULTS.

PART SECOND.

PART THIRD

INTRODUCTION.

a. Problem and Scope of Present Study.

The work presented here grew out of a series of tests upon the rat, begun in April, 1906. Watson,[1] in an investigation which he was carrying on at the time, had found that the only necessary sensory avenues employed by the rat, in learning the maze, were the kinæsthetic and organic, and that visual, olfactory, auditory and tactual impressions could in all probability be dispensed with.

The present problem parallels that of the above investigation and may be briefly stated as an attempt to determine the function of the different sense organs in the reactions of the rat to situations requiring various types of movement. In problems like that of the maze, the general activity of running is the one most utilized. The sensori-motor arcs need only to be integrated: Whereas the coördinations which are employed in the learning of such problems[2] as Nos. I, II, and III of the present series (such as digging, bending the back and climbing upward through holes; stepping on a plane and advancing upon it until a trap door falls; raising the head and lifting a latch with the snout, etc.) are not so habitual to the animal. The sensori-motor arcs involved in the learning of these problems must be established more or less *de novo*, and at the same time be combined into a series which can function more or less automatically.

It may be assumed that since running, which is the chief form of activity involved in the maze, is so reflex-like in character, it might well be carried out by the use of kinæsthetic sensory impressions alone. The coördination involved in problems of the manipulation type, not being so reflex in character, would, if the factors involved in the formation of human habits

[1] Watson, J. B., Psych. Rev., Mon. Supp., vol. viii, no. 2, 1907.
[2] These problem boxes are described in detail further on.

may by analogy be assumed to hold in the case of the rat, require the coöperation, at first (*i. e.*, at least during the learning process), of visual and olfactory impulses, provided such were at hand. Later on such coördinations might in their turn be controlled by kinæsthetic means.

The general type of coördination in the maze, is, as has been stated, that of running. The animal, in addition, must learn what turns to make, and where to make them. Carr and Watson's [1] later report of work with a maze in which the length of the alleys may be changed, goes to show that the knowledge of the direction of the turns and the point at which they occur, is governed by the kinæsthetic and organic character of the cues for the turns. This fact explains the success of blind and anosmic rats in learning the pathway in a maze.

In Problem I of the present series, the pathway which the animal must follow is more complex than that of a maze in that it involves climbing down from the top of a box, finding a hidden entrance to the food box, digging away an obstruction, crawling under a board and up through an opening. The solution, however, is more simple than that of any of the others in that it demands little manipulation, and on the whole approaches the labyrinth type of problem. How much the guidance from vision and olfaction may assist the functionings of kinæsthetic and organic processes in the *learning* of this type of problem is one of the chief questions to be answered by the present research.

Problem II is thought to be still more difficult for the rat. The animal must learn here to press down an inclined plane at a distance from the box, and establish the association of the falling of the plane with the opening of the door of the food box. It is possible that olfaction might render the food stimulus more intense, and thereby quicken the reactions of the animal; but the question we are more concerned with is whether the olfactory values of the different parts of the environment, such as the smell of the plane, of the door of the food box, etc., aid the animal in adjusting to such situations.

[1] Jour. Comp. Neur. and Psych., vol. xviii, no. 1, 1908, p. 27ff.

Vision might also be the means by which he attains his orientation in such an open space as surrounds the apparatus.

In Problem III, the rat must raise a latch holding a door in place in order to reach the food. The area in which the successful movement must be performed is very circumscribed, being only the immediate locality of the free end of the latch. Vision may be necessary to locate the door and the latch. Olfaction may possibly play a considerable rôle here likewise.

Problem IV necessitates the rat's jumping from one platform to another in order to obtain food. It is hardly conceivable that this coördination could be successfully executed for any considerable distance without the aid of vision.

If different types of sensory control are used by the animal in meeting such different situations, the fact should become evident in a comparison of the behavior of normal rats with that of rats deprived of the use of the important sense organs.

In carrying out this investigation groups of normal white rats, normal black-and-white rats, blind rats and anosmic rats[1] were used.

During the course of the above research the experimenter collected data bearing upon the questions of sex and individual differences, and on the possible influence of previous training. While these topics were subsidiary to the main problem, the results seem of sufficient value to justify their presentation.

b. General Method.

The experiments which are here reported were as carefully controlled as possible: the tests were made every day and at the same hour of the day. Unless otherwise specified, the rats were about 120 days old when the experimentation began. This standard of age was adhered to because the rats at this time have much of the energy of youth together with fully developed neural and physiological mechanisms. Fidelity to this requirement, as may be imagined, caused the experimenter

[1] The anosmic rats, with one exception, died during the ravages of an infection which afflicted the rat laboratory. The group had only partly completed its work. On this account, the inter-comparison of the records of normal and of defective rats is not as complete as is desired.

much difficulty. A need for a group often arose when none in the laboratory satisfied the demands. This frequently meant a considerable delay until a litter of the proper age could be found. As a rule, the rats had been bred in the laboratory and were known to be of good stock. Males and females used in the work were kept in separate cages and, for the most part, were tested on separate apparatus. This was done as a check to any possible tendency toward tracking, and to minimize the emotional disturbance of fear caused by an unusual odor. The problem boxes were carefully washed and left for a time in the open air before being assigned to a new group. On account of the fact that the rats were required to get into the problem box for food, rather than to release themselves from confinement, it was necessary to inclose the area in which they were to work. A larger cage of wire netting was placed over the problem box, which is spoken of in later descriptions as the *control cage.* When an animal is confined within his problem box, no such outer cage is necessary. But unless a rat were—in a measure—confined, his insatiable curiosity would preclude a solution of his problem within reasonable time limits.

The boxes were kept covered by these cages when not in use so that no predatory wild rats could leave an odor on them. On one occasion the laboratory boy carelessly removed the control cage, and left a problem box exposed. Wild rats had evidently been about, for the next day each rat introduced into the cage became exceedingly timid with fright, and the emotional reaction was so strong and so persistent that it necessitated the abandonment of the series. Upon another occasion, when the rats which had just finished their work for the day were eating in the problem box, the experimenter killed several wild rats which had been caught in a trap. Every effort was made to remove any trace of odor from the experimenter's hands; but when the white rats were carried back to their living cages one of them seemed much frightened. The next day he objected to being handled and when put into the test cage he crouched. When he moved about at all, he slunk along close to the floor, cringing, and became quite motionless at any loud or sharp sound. For several days his behavior suggested fear, and only after the fifth day had passed did he revert to his normal behavior. Possibly it is not necessary to go so into detail in these matters. But those who have worked with animals realize how difficult it is to maintain constant conditions. If the experimenter takes the precaution to state explicitly the methods of control in the work reported, it inspires more confidence in the minds of those who wish to utilize the results.

Milk-soaked bread, except where otherwise stated, was the food stimulus used. Hunger was relied upon as being the most constant and most natural incentive to activity. The rats were allowed to eat but sparingly of the food after the first successful efforts, but after the last trial for the day had been given they were permitted to satisfy their hunger. On the whole the method of reward was adopted as the most efficient means of controlling the reactions of these animals. The rats were never allowed to become ravenously hungry. Such a condition puts a premium upon useless and frantic movements.

The food in the problem box was always placed in the same location. The position of the food box in relation to the control cage, and of both to the points of the compass, was constant. The rat was always put into the cage at the same point and with approximately the same bodily orientation. As the entrance was at the east in the first three problems, the rat entered the control cage facing west. Particular precaution was taken in this matter, since all experiments with the rat have shown him to be very susceptible to slight changes in his environment. He has a tendency to establish a pathway from the entrance to the food box, and to follow it carefully. Unless he attains orientation quickly and pursues this pathway he becomes confused. This confusion is evidenced by the display of the same random activity present in his first trial.

Since the time records must furnish the greater part of the basis of comparison in these problems especial care was taken to maintain constant conditions. Comparisons were to be made between entire groups, and between individuals of the same or different groups. This fact, likewise, necessitated the employment of great vigilance in experimentation.

The animals were always tame when beginning the work and were accustomed to being handled. Every group was given the different tests in the same serial order. On account of the possible influence of education, groups were not set upon the second problem without the experience of the first. While at the time the difference between the work of trained and of untrained rats was only hypothetical, the results reported here (p. 103) seem to justify the precaution.

Care in maintaining these conditions—such as the age of the rat; amount of previous training; continuous daily experimentation; and an equal number of daily trials—made the work difficult. It is admitted that even with the care taken, the conditions were not ideal. In many cases a comparison is made of tables and curves formulated from the records of groups composed of unequal numbers of individuals, and of unequal sex representation. However, the writer, at least, feels that the records obtained represent very fairly the abilities of the animals experimented upon. While much in the way of accidental variation is doubtless present, the records on the whole are reliable.

PART I

I. Experimental Results.

A. TESTS ON PROBLEM BOX I.

1. Description of Apparatus and General Statement of Learning Process.

The first problem box used was a modified form of the one used by Small[1] and by Watson.[2]

In this box the rat has to dig through sawdust in order to reach the entrance of the food box. The box is 30 cm. long, 22.5 cm. wide and 17.5 cm. high, the top and the bottom being of inch boards. The box is raised by supports at the corners so that the bottom is 5 cm. above the table upon which the box rests. The sides and ends of the box are covered with wire netting. The netting on the sides extends down to the table, while that on the ends goes only to the bottom of the box, leaxing an open alley under the box between the two extended side walls. In the center of the raised floor is a rectangular opening through which the rat climbs from below into the box to obtain food. This opening is 8 cm. by 10 cm. A larger opening in the top of the box, 10 cm. by 12.5 cm., allows the experimenter both to admit the food and to remove the animal from the box (see fig. I). This opening has a thin board cover which is pivoted on a screw near one of its ends.

During experimentation the sides and ends of the box were covered with sawdust to the height of the floor of the box. This height was chosen arbitrarily to insure a practically constant amount of sawdust for the animal to remove. A wire netting control cage, 52.5 cm. long, 37.5 cm. high and 37.5 cm. wide, with a door on one side, was placed over the box.

[1] Am. Jour. Psych., vol. xi., no. 2, p. 135.

[2] *Animal Education*, p. 14.

The problem with which the rat is confronted is the necessity for removing the sawdust either at the north end or at the south end of the box. Since the east and west sides are entirely covered with wire netting, movements at these points are useless. The rat must dig away a quantity of sawdust, crouch and then crawl under the floor of the food box proper, and later climb up through the hole in the floor. At the beginning of the test the animal was always placed on the top of the box facing west. It must learn to descend to the floor of the cage, orient itself as regards the north or south end of the box, and dig underneath the floor of the food box as described above. As has been stated, the pathway which the animal

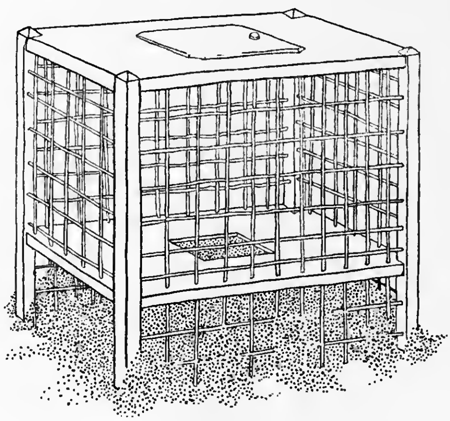


Fig. 1.

must establish is relatively simple, and, with the exception of the digging, crouching and climbing movements, the problem approaches in its simplicity that of the labyrinth type.

The above task was presented first to normal, then to defective rats in the hope of obtaining evidence for the type of sensory control utilized by the rat in forming such an association. The normal rats furnished the standard time and error record with which the records of the defective animals were compared.

The rats at work upon this problem were fed once a day for three days in the box, a handful of sawdust having previously been sprinkled over the floor to accustom them to its presence.

The time consumed during the test was taken by means of an ordinary stop-watch, which was started just after the door of the cage was closed after admitting the animal, and was stopped when the rat had all four feet in the food box.

The general description of the learning processes involved in this problem may be easily set forth by a reference to the following notes of an individual taken at random from the records of the normal rats.

Notes on the Behavior of Normal Female Rat IV in learning Problem I.

	TRIAL.	
4/20/06	1	Examined most carefully both outer cage and food box. Seemed to get odor of food and dug two-thirds length of east side. Scratched lightly at west: active but unfortunate. Began scratching at south, but gave up immediately and began at east. Dug under at west end of north side. Did not enter food box at once. Time 17.65 min.
	2	Entered at west of north, with few useless movements. Time .25 min.
	3	Entered at west of north. Time .15 min.
4/21/06	4	Scratched on east side, left, returned again to east and dug frantically, then entered from south. Time 1 .02 min.
	5	Scratched at east, entered at south. Time .15 min.
	6	Hesitated for an instant at east, but did not scratch. Time .08 min.
4/22/06	7	Entered without useless movements from the south. Time .18 min.
	8	Entered at north; dug spasmodically. Time .42 min.
	9	South. Time .17 min.
4/23/06	10	South. Time .12 min.
	11	Went to south, hesitated, seemed confused, dashed to north and in. Time .20 min.
	12	North. Time .03 min.

The above notes show many characteristic features of the learning process. The quick drop from high to low time record, which may be seen from the form of the curve, (Plate I) is typical of the first and second trials. The sudden elimination of useless movements is not always so pronounced as in the case noted above. Often errors persist through half of the series. The manner of the elimination of errors is well illustrated in the above notes. There is at first a persevering effort to dig through at the east or west side, followed by less and less persistent endeavor at these places; later there is present only a hesitancy in passing such points, and finally, as the habit progresses, no notice of them at all.

This procedure quite parallels that of the rat in eliminating his errors in the maze. He at first explores the cul-de-sac with care, then runs into it for shorter and shorter distances, hesitates at its opening, and finally disregards it utterly. Such behavior is indicative of the early random and accidental nature of the movements, and illustrates one phase of the kinæsthetic character of control.

2. *Statement of Results.*

a. On Normal White Rats.

In order to obtain normal records with which to compare the records of defective animals, a group of eight white rats was used. These rats were 122 days old, all of one litter and were healthy, active individuals. Being bred in the laboratory their previous ancestral history was known to be of the best. Four of them were males and four were females. None had been used previously in experimentation. Table I shows the average records of this group. On Plate I the graphic representation of this average is given.

The table shows also the maximum and the minimum time records for each trial, the number of rats whose records coincide with the average for the group at that trial, and the number whose records are greater, and the number whose records are less than the average. Since average alone is not always an adequate representation of the accomplishments of a group, these few supplementary facts are added to make the average of greater value.

When the records were tabulated, the rat making the maximal and the animal making the minimal record at each trial was noted. The number of maximal and of minimal records made

TABLE I.

Showing the average, the minimum, and the maximum time-records of eight normal white rats upon Problem I. The last three columns show the number of animals whose time records are (1) equal to the average, (2) below and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM	1.	2.	3.
	min.	*min.*	*min.*			
1	7.04	3.97	17.65		6	2
2	1.69	.28	7.20		6	2
3	.48	.15	.83		4	4
4	.80	.20	1.58		3	5
5	.35	.13	.75		5	3
6	.30	.15	1.07		7	1
7	.25	.10	.55		5	3
8	.23	.10	.44		5	3
9	.27	.15	.68		5	3
10	.18	.12	.50		7	1
11	.16	.07	.30		4	4
12	.13	.13	.28		2	6
13	.15	.07	.42		6	2
14	.09	.08	.18		5	3
15	.11	.10	.22		5	3
16	14	.10	.35		5	3
17	.18	.11	.48		5	3
18	.13	.10	.20		5	3
19	.19	.07	.45		5	3
20	.09	.10	.13		4	4
21	.17	.05	.60		7	1
22	.12	.10	.33		6	2
23	.11	.10	.20		4	4
24	.23	.07	.73		7	1
25	.14	.05	.42		6	2
26	.10	.07	.20		5	3
27	.08	.05	.18		4	4
28	.12	.05	.22		4	4
29	.15	.05	.45		5	3
30	.07	.05	.13	1	3	4
31	.07	.06	.13		5	3
32	.06	.05	.09		4	4
33	.12	.05	.37		6	2
34	.17	.06	.52		5	2
35	.09	.04	.18		4	4
36	.19	.06	1.03		7	1
37	.14	.07	.50		6	2
38	.09	.07	.22		5	3

TABLE I.—Continued.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
39	.09	.07	.13	1	4	3
40	.09	.07	.45		6	2
41	.24	.08	1.33		7	1
42	.15	.06	.33		7	1
43	.12	.06	.23		6	2
44	.11	.04	.33		6	2
45	.10	.07	.32		6	2
46	.06	.03	.12		5	3
47	.06	.03	.09		6	2
48	.06	.03	.10		4	4
49	.06	.03	.10		4	4
50	.23	.08	1.26		7	1

by each individual was then given its percentage value. These percentages are shown below.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL.

Minimal Records.	*per cent.*	*Maximal Records.*	*per cent*
Male I	41.6	Male I	11
Male II	10.6	Male II	8
Male III	22.0	Male III	7
Male IV	10.6	Male IV	24
Female I	1	Female I	29
Female II	6	Female II	6
Female III	3	Female III	7
Female IV	5	Female IV	8

These supplementary tables with others similar in character will be discussed in a later section (p. 117). They are inserted here to show, in a measure, how dependent the average may be upon the variations among individuals. Male IV and Female I are responsible for more than one-half of the longest records. Male I, on the other hand, though he has not the least percentage of maximal records, has made more than two-fifths of the total number of minimal records. In other words, he has almost as many minimal records per trial to his credit as have the other seven combined.

The last three columns of Table I were added in the hope of further clarifying the table and the curve. These columns show roughly to what extent the average represents the group. Often a high record of one individual will raise the average of the group, so that in the corresponding curve a high point is a result of the long time record of one rat. The rises in the curve at the twenty-first, thirty-fourth, thirty-sixth, forty-first and fiftieth trials are thus explained as due to the individual and probably accidental variation of some animal. On the other hand, on the sixth and tenth trials seven rats made records below the average without skewing the curve.

b. On Normal Black-and-White Rats.

A group of black-and-white rats was set to work on the series of problems. These rats were females, all of one litter, and were 112 days old when experimentation began. They had been much petted from infancy and were unusually tame.

Table II shows the time-records made by these rats and Plate I shows the graphical representation of these averages. This group, as may be seen by a glance at the curve solved the problem in uniformly less time than the normal white rats, not only with the time-records much lower on the whole but the first successes were accomplished after an exceedingly short interval.

The following table gives the percentage of minimal and of maximal time-records made by each rat.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Female I	51	Female I	16
Female II	13	Female II	18
Female III	27	Female III	25
Female IV	9	Female IV	41

Table II.

Showing the average, the minimum, and the maximum time-records of four black-and-white females upon Problem I. The last three columns show the number of animals whose time-records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
	min.	*min.*	*min.*			
1	.82	.25	1.53		2	2
2	.23	.67	.37		2	2
3	.23	.67	.52		3	1
4	.12	.07	.17		2	2
5	.76	.05	.11		3	1
6	.17	.06	.47		3	1
7	.08	.07	.12		2	2
8	.08	.05	.19		3	1
9	.13	.05	.22		2	2
10	.08	.06	.10		1	3
11	.07	.05	.10		3	1
12	.06	.05	.06		2	2
13	.07	.05	.10		2	2
14	.18	.05	.53		3	1
15	.06	.05	.09		2	2
16	.07	.05	.10		2	2
17	.10	.07	.14		2	2
18	.11	.07	.26		3	1
19	.05	.05	.06		3	1
20	.09	.04	.17		2	2
21	.05	.03	.07		2	2
22	.05	.04	.07		2	2
23	.04	.03	.05		2	2
24	.05	.03	.08		2	2
25	.05	.03	.06		2	2
26	.06	.03	.12		2	2
27	.11	.03	.35		3	1
28	.04	.03	.04		3	1
29	.04	.03	.04		1	3
30	.03	.03	.04		2	2
31	.05	.03	.10		3	1
32	.04	.04	.04		1	3
33	.04	.03	.04		2	2
34	.04	.03	.06		3	1
35	.04	.04	.05		2	2
36	.05	.03	.07		2	2
37	.06	.03	.12		3	1
38	.07	.04	.09		2	2

TABLE II.—Continued.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
39	.04	.03	.05		3	1
40	.06	.04	.12		3	1
41	.03	.02	.04		2	2
42	.06	.04	.10		2	2
43	.08	.03	.17		3	1
44	.18	.02	.63		3	1
45	.06	.05	.07		1	3
46	.04	.03	.05		2	2
47	.04	.03	.05		2	2
48	.04	.04	.04		2	2
49	.04	.03	.05		3	1
50	.03	.03	.04		1	3

c. On Blind Rats.

Nine blind rats of which four were males, and five were females, were trained on the problem.[1] The animals were about four months old and all were in excellent condition.

Table III gives the records made by eight animals of the group. A curve on Plate I shows the graphic representation of the averages given in this Table.[2]

TABLE III.

Showing the average, the minimum and the maximum time-records of eight blind rats upon Problem I. The last three columns show the number of animals whose records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3
	min.	*min.*	*min.*			
1	2.73	.48	6.41		5	3
2	.51	.25	1.00		5	3
3	.91	.23	1.97		4	4
4	.73	.17	2.45		6	2
5	1.52	1.20	8.06	1	6	1
6	.51	.10	1.42		5	3

[1] Cf. Watson, *ibid*, pp. 47 ff.

[2] The time-records of one male are not included in the average. They are unusually slow and are discussed in the paragraphs on individual differences.

Table III.—Continued.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
7	.23	.12	.42	1	4	3
8	.49	.07	1.93		6	2
9	.70	.04	1.92		5	3
10	.50	.17	1.63		4	4
11	.84	.07	3.60		6	2
12	.35	.06	.63		4	4
13	.28	.07	.67		4	4
14	.49	.13	1.13		5	3
15	.28	.05	.53		4	4
16	.19	.05	.48		5	3
17	.17	.04	.42		5	3
18	1.06	.05	6.13		6	2
19	.30	.05	.83		4	4
20	.21	.05	.52		5	3
21	.45	.05	1.10		5	3
22	.24	.05	.63		6	2
23	.42	.04	1.05		5	3
24	.13	.05	.33		4	4
25	.12	.05	.23		4	4
26	.37	.07	1.12		3	5
27	.18	.09	.32		4	4
28	.15	.05	.32		4	4
29	.57	.07	3.13		7	1
30	.31	.07	.45		4	4
31	.25	.08	.52		5	3
32	.18	.05	.42		6	2
33	.15	.08	.25		5	3
34	.27	.07	.75		5	3
35	.13	.04	.30		5	3
36	.16	.05	.47		5	3
37	.23	.07	.78		4	4
38	.20	.06	.42		5	3
39	.26	.37	.87		6	2
40	.26	.05	.70		4	4
41	.16	.05	.38		3	5
42	.26	.04	1.02		6	2
43	.34	.05	1.33		6	2
44	.21	.06	.45		5	2
45	.25	.07	.92		5	3
46	.22	.47	.53		6	2
47	.18	.06	.49		6	2
48	.21	.07	.42		5	3
49	.25	.04	.83		5	3
50	.13	.04	.45		6	2

The averages in the above table are low, but lack uniformity. There was wide variation among the individuals of the group, which was noticeable not only in their time records, but in their general behavior.

The following table shows the number of minimal and of maximal time-records made by each blind rat.

TABLE SHOWING PERCENTAGES OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Male II	23	Male II	4
Male III	21	Male III	6
Male IV	6	Male IV	3
Female I	1	Female I	46
Female II	22	Female II	8
Female III	22	Female III	4
Female IV	2	Female IV	25
Female V	3	Female V	4

Female IV made one-fourth, and Female I nearly one-half of the total number of maximal records. Males II and III and Females II and III, together made 88 per cent of the minimal records.

d. On Anosmic Rats.

Since in these problems the animals at all times are in relatively close proximity to the food so that odor stimuli might affect their reactions, five anosmic males were tested upon the problem. Each had had the olfactory bulbs removed as described in detail by Watson.[1] The animals were in good condition when experimentation was begun—forty days after the loss of the bulbs—and remained so throughout the test.

Table IV gives the time-records. The graphical representation made from the average is shown in Plate I.

It seems unnecessary to comment at length on the results given in the table and curve. The effect of individual varia-

[1] *Ibid.* p. 49 ff.

TABLE IV.

Showing the average, the minimum and the maximum time-records of five anosmic males upon Problem I. The last three columns show the number of animals whose records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE	MINIMUM.	MAXIMUM.	1.	2.	3.
	min.	*min.*	*min.*			
1	13.27	1.62	46.30		4	1
2	.63	.25	1.98		4	1
3	.22	.13	.40		3	2
4	.32	.14	.52		3	2
5	.28	.07	.62		3	2
6	.43	.07	2.92		4	1
7	.26	.07	.50	2	2	1
8	.57	.10	2.22		4	1
9	.32	.07	.73		3	2
10	.14	.07	.23		3	2
11	.20	.07	.43		3	2
12	.31	.07	.98		4	1
13	.30	.07	.90		4	1
14	.20	.06	.37		3	2
15	.31	.06	.67		2	3
16	.36	.13	.63		3	2
17	.22	.06	.40		2	3
18	.28	.16	.50		3	2
19	.20	.07	.43		3	2
20	.24	.08	.47		3	2
21	.16	.03	.25		3	2
22	.32	.05	.78		2	3
23	.35	.07	1.05		4	1
24	.21	.05	.42		3	2
25	.14	.08	.22		3	2
26	.23	.06	.42		2	3
27	.20	.07	.37		2	3
28	.30	.07	.67		2	3
29	.27	.06	.62		2	3
30	.16	.13	.24		3	2
31	.20	.07	.42		2	3
32	.27	.05	.48		2	3
33	.29	.05	.82		4	1
34	.17	.06	.33		4	1
35	.28	.07	.70		3	2
36	.57	.04	1.60		3	2
37	.39	.05	1.18		3	2
38	.17	.04	.32		3	2

TABLE IV.—Continued.

NO. OF TRIAL	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
39	.32	.08	.63		3	2
40	.28	.08	.53		3	2
41	.27	.07	.38		2	3
42	.29	.06	.55		3	2
43	.28	.13	.42	1	2	2
44	.40	.28	.67		3	2
45	.22	.08	.38		3	2
46	.35	.08	.75		3	2
47	.25	.05	.68		3	2
48	.29	.05	.93		4	1
49	.24	.09	.43		2	2
50	.41	.07	1.13		4	1

tions on the curve is roughly shown in the table below, which gives the percentage of minimal and of maximal records made by each rat.

TABLE SHOWING PERCENTAGES OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL RAT.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Male I[1]	0	Male I[1]	55
Male II	6	Male II	10
Male III	38	Male III	6
Male IV	56	Male IV	2
Male V	0	Male V	27

Male I, as is indicated, made more than one-half of the longest time-records: the time-records of Male V were also long. Were it not for the eccentricities of these two rats, the curve

[1] The emotional attitude of Male I, together with a tendency to gnaw at all that came in his way was responsible for his apparent slowness. He was a very active and hardy rat, but his efforts were entirely misdirected. He spent much of his time endeavoring to clamber from the top of the box, and once down, was more than likely to climb back up immediately and begin all over again his frantic attempts to get off. When he did spend any length of time on the floor of the cage, he vented his energy in gnawing at the outer cage, or at the wire netting of the food box. On later problems his propensity to gnaw became the despair of the experimenter, as it necessitated perpetual repairing of the apparatus. Rat V likewise spent much time in the endeavor to get off the box. He finally acquired the habit of getting off at a point nearest the door. Later, if he did not at once reach this position, he seemed utterly at a loss what to do.

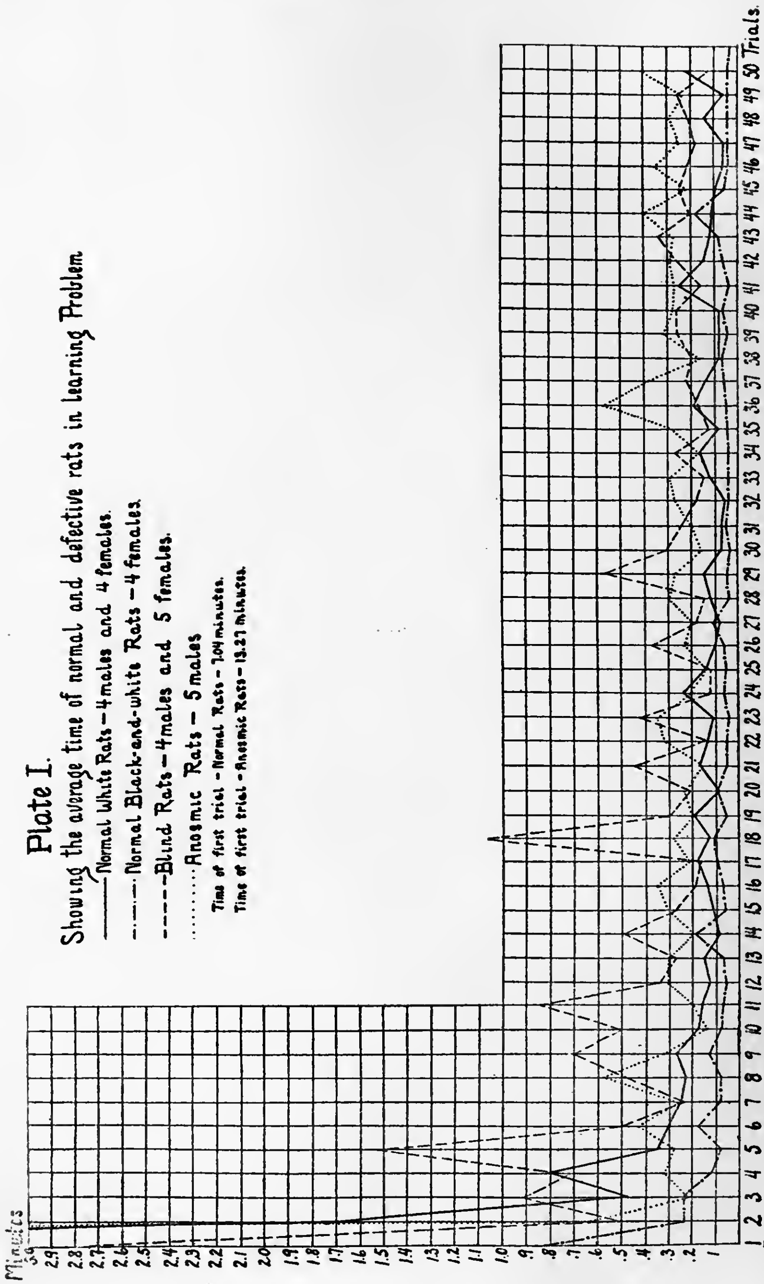


Plate I.
Showing the average time of normal and defective rats in learning Problem
——— Normal White Rats — 4 males and 4 females.
-·-·- Normal Black-and-white Rats — 4 females
- - - - Blind Rats — 4 males and 5 females.
········ Anosmic Rats — 5 males
Time of first trial - Normal Rats - 7.04 minutes.
Time of first trial - Anosmic Rats - 13.27 minutes.
Minutes
Trials

for this group would have been considerably lower than it is. The records of Male IV were exceptionally short.

3. Summary.

a. Average Time-records for the Total Series of Fifty Trials.

In attempting to summarize the results of tests with different groups of rats upon this problem, only a comparison of the records made by the normal and the defective animals will be given here. The discussion of the records and a theoretical interpretation of them together with the facts brought out in later summaries will be given in the final conclusions at the end of Part I (p. 61).

The sum of the separate time-records for the entire series of fifty trials given each rat was obtained, and it was then divided by the total number of trials given that animal. This gave an average time-record for each animal on the problem and afforded one basis for the comparison of records of individuals. From the individual averages obtained as above described a group average was made which serves as an additional means of comparing the records of groups of normal rats with those of the groups of defective animals.

TABLE SHOWING GROUP AVERAGE OF THE TOTAL TIME (50 TRIALS) CONSUMED BY NORMAL AND BY DEFECTIVE RATS IN LEARNING PROBLEM.

Average Records of Groups.

	min.		min.
Black-and-White	.09	Blind	.49
White	.34	Anosmic	.55

Averages of Records of Individuals in the Groups.

BLACK-AND-WHITE.		WHITE.		BLIND.		ANOSMIC.	
	min.		min.		min.		min.
Female I	.10	Male I	.36	Female I	.11	Male I	.65
Female II	.08	Male II	.39	Female II	.42	Male II	.25
Female III	.09	Male III	.38	Female III	.24	Male III	1.09
Female IV	.08	Male IV	.27	Female IV	.47	Male IV	.17
				Female V	.32	Male V	.60
		Female I	.41	Male I	.97		
		Female II	.20	Male II	.31		
		Female III	.27	Male III	.27		
		Female IV	.46	Male IV	.27		

As may be noted in the above table the average of the group of black-and-white rats is phenomenally low as compared with that of the other groups. The individual averages of this group are very uniform. The highest individual average, which is .10 min., is just one-half of the lowest average made by any one normal white rat, viz., .20 min. The group average of the blind rats is high, but the high variations among individuals are in part responsible for the high group average. Five individual averages among the blind are lower than the group average of the normal white rats, and the lowest average of a blind rat, .24 min., is but little above that of the lowest individual of the normal white rats, .20 min. The average of the anosmic group is very slightly lower than that of the blind group, though here, too, the individual variation is high. The average made by Male II of this group is the lowest made by any normal or defective white rat upon the problem.

b. Average Time Records by Groups of Ten Trials.

The time-records of each individual were averaged by tens. The interesting fact was brought out, that of the total of twenty-six normal and defective animals, seventeen made lower averages on the second, third or fourth ten than upon the fifth. In other words: almost two-thirds of the total number of rats reached their period of highest speed early in the series of fifty trials, and later lengthened their time-records.

The average time-records by tens of the individual animals are given below. The starred averages show those instances in which the average time-record of a series of tens is shortest before the last ten of the entire series.

TABLE SHOWING AVERAGES OF TIME-RECORDS BY GROUPS OF TEN.

Black-and-white-Rats.

Individuals.

TRIALS	FEMALE I	FEMALE II	FEMALE III	FEMALE IV	GROUP
	min.	*min.*	*min.*	*min.*	*min.*
1–10	.15	.20	.26	.21	.21
11–20	.13	.06	.07	.07	.08
21–30	.06	.04*	.07	.04	.05
31–40	.05*	.05	.03*	.03*	.04*
41–50	.11	.06	.04	.04	.06

Normal White Rats.

	MALE I	MALE II	MALE III	MALE IV	AV. MALES	GROUP
	min.	*min.*	*min.*	*min.*	*min.*	*min.*
1–10	1.45	1.36	1.08	.70	1.15	1.16
11–20	.13	.16	.11*	.22	.15	.14
21–30	.09	.12	.14	.19	.13*	12*
31–40	.09	.10*	.37	.12*	.17	.15
41–50	.06	.21	.21	.13	.15	.14
	FEMALE I	FEMALE II	FEMALE III	FEMALE IV	AV. FEMALES	
min.	*min.*	*min.*	*min.*	*min.*	*min.*	
1–10	1.08	.66	.98	2.02	1.18	
11–20	.14*	.15	.12	.07	.11	
21–30	.19	.07	.16	.04	.11	
31–40	.29	.06	.06	.11	.13	
41–50	.35	.06	.05	.04	.12	

Blind Rats.

	MALE I	MALE II	MALE III	MALE IV	AV. MALES	GROUP
	min.	*min.*	*min.*	*min.*	*min.*	*min.*
1–10	1.51	.83	.65	.58	.89	.94
11–20	.33*	.26	.21	.17	.24*	.41
21–30	.80	.28	.21	.21	.37	.35
31–40	1.25	.11	.09*	.24	.42	.43
41–50	.98	.10	.16	.15	.35	.31
	FEMALE I	FEMALE II	FEMALE III	FEMALE IV	FEMALE V	AV. FEMALE
	min.	*min.*	*min.*	*min.*	*min.*	*min.*
1–10	1.53	1.50	.45	.66	.73	.97
11–20	1.38	1.17	.20	.68	.26	.54
21–30	.81	.19	.24	.30*	.16*	.34*
31–40	1.21	.14	.21	.38	.21	.43
41–50	.62	.09	.09	.33	.24	.27

Anosmic Rats.

	MALE I	MALE II	MALE III	MALE IV	MALE V	GROUP
min.	*min.*	*min.*	*min.*	*min.*	*min.*	*min.*
1–10	1.21	.30	5.00	.41	1.43	1.67
11–20	.39*	.22	.14	.15	.46	.27
21–30	.42	.29	.10	.08*	.28*	.23*
31–40	.60	.20*	.06*	.09	.50	.29
41–50	.61	.25	.14	.11	.32	.28

The occurrence of the minimum time-records early in the series may have been due to some accidental condition, such as a variation in the state of hunger. However, the behavior of the animals as well as their time-records often indicated a falling apart of the stages of the association, suggesting rather a process of dissociation, or dissolution of the association. This might be the effect of a possible decrease in the intensity of the stimulus as the reaction became automatic. The matter is commented upon here as seeming to be a situation in which an habitual coördination tends to break down through a relatively long continuance. In order to ascertain whether the rats again would lower their records for later periods of ten trials, or whether the coördination really disintegrated, the series should have been continued indefinitely, and possibly should have been controlled by changing the kind of food used as a stimulus. Lack of time prevented the continuation of this problem.

c. Discussion of Errors in this Problem.

The computation of errors which were made by the animals has been computed not upon the basis of the total number of *useless movements*, but upon that of particular kinds of random movements, namely, those by means of which a rat attempts to enter the food box from the east or west, whereas, he can only enter from the north or south. In tabulating the results, then, the attempts to dig away the sawdust at the east or west is counted an error.

The number of errors is not alone an accurate standard in the learning process of this problem. Often an error crops out in a trial late in the series after the rat has made many perfect trials. As an instance of this may be cited particularly the case of one rat, black-and-white Female IV, which made four errors, one each at her fourth trial, twelfth, thirty-fifth and thirty-eighth trial. At the thirty-fifth trial her time was somewhat longer, though at the thirty-eighth, with an error at another position, it was at her reaction time: .05 min. The scratching in these last two trials might have been a movement which was a reversion to her two early errors, or to an accidental movement set off by contact with the sawdust at that point.

Rat IV of the anosmic group made but one error and that at the first trial. Two black-and-white females made each one error, at the sixth and ninth trials respectively.

The average number of errors for the different groups is here given.

Average Total Number of Errors.

Normal White..............	8.2	Blind........................	10.
Black-and-white............	2.7	Anosmic.....................	5.2

Average Number of Trials Characterized by Errors.

Normal White.............	7.2	Blind.......................	9.7
Black-and-white	2.5	Anosmic....	4.4

d. Comparison of the Curves of the Different Groups.

Plate I shows the curves plotted from the average time-records of the different groups. The curve representing the group of black-and-white rats is the lowest curve of the four. Even at the first trial, it does not rise above the coördinate representing one minute, and for most of the time it runs below that representing .1 min. The curve is not only low, but is remarkably uniform. Whether or not the lowness and uniformity is due to the fact that these rats possess pigmented eyes will be discussed in the conclusions (p. 61).

The curve representing the averages of the normal white rats approximates more nearly than any other the character of the curve representing the black-and-white rats. At the first trial this curve is much higher, and, until the fourteenth trial, does not reach so low a point as the curve of the black-and-white animals. Again at the twentieth, the twenty-seventh and the forty-fourth trials the curve goes below: otherwise, it is higher and more variable.

The curves representing the averages of the blind and the anosmic rats, respectively, are much alike and are slightly higher than those of the normal rats. Both curves are irregular, and both follow, in general, about the same level. The curve of the anosmic group is considerably lower at the third, fourth and fifth trials, than that of any other group of albino rats.

From the sixth trial these curves cross and re-cross each other continually.

B. TESTS UPON PROBLEM BOX II.

1. Description of Apparatus and of the Learning Process.

The apparatus used in the second test of the series consists of a box, 20 cm. by 20 cm. at the base and 15 cm. high, of wire netting of a centimeter mesh. A door, 7 cm. high by 10 cm. long, is hinged at the lower corner of one side of the box (see figure 2). A latch, on the inside is controlled by a cord passing from the latch upward through a mesh above, back over a small wooden pulley to the inclined board plane,

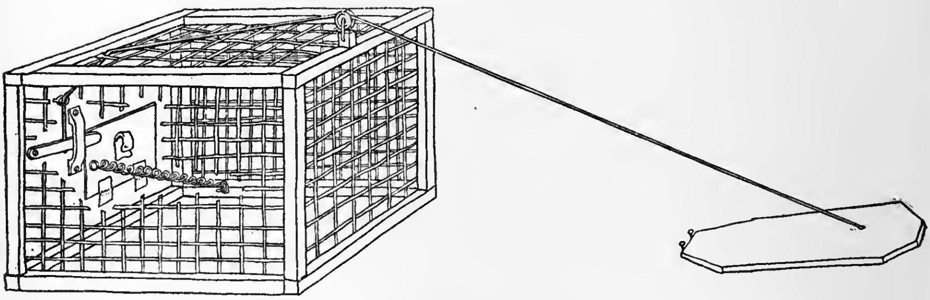


Fig. 2.

22 cm. long by 10 cm. wide, the foot of which rests at a distance of 11 cm. from the side of the box opposite the door. The angle which the plane made with the floor of the experimental cage was approximately 15°. This angle was decreased slightly when smaller animals were used.

When the rat steps upon the plane, the plane falls, and the latch is thereby pulled up allowing the door to fall inwards of its own weight. In this test there is required a series of adjustments on the part of the rat which is quite different from that demanded of him in Problem I. The question to answer is, as was the case in the learning of the problem just discussed: Does the animal use olfactory and visual impulses in the formation of these new and unusual coördinations, and if so to what extent?

The method of procedure in this test followed closely that just reported. The box was enclosed in a large wire control cage 72 cm. by 76 cm. and 37 cm. high, which could be raised when desired so as to admit the rat. The position of the box remained constant, being determined by means of several tacks in the table, which prevented the box from slipping. The plane was also kept in position by tacks at the margin nearest the box.

The animals which were used in Problem I were used in the present test. All the animals thus had had previous experience in experimentation, and this previous experience was of the same amount and kind. The results of other tests (see p. 103) show this point to be one of importance in the control of experimentation with rats. Three trials per day for ten days, and five trials per day for four days were given, making as before fifty trials in the series.

The solution of this problem is unique in that it necessitates the reaction of the rat at a distance from the food box. On this account, the learning curve from the second to about the tenth trial is much more irregular than that of Problem I. (See Plate II.) There is likely to be a second pronounced rise in the curve after the first trials. This is explained by the fact that after the first two or three accidental successes, the rat comes to associate the *position of the door with the food*, but not the *position of the plane with the food*. Consequently, the animal goes directly to the door, and finding it closed, begins to scratch and gnaw at it vigorously. This is an almost invariable procedure. The time so spent is the cause of the rise in the curve.[1]

[1] The experimenter found it necessary to secure the latch firmly during the time the rat spent tugging at the door, for when the pressure of the door was released from the latch, the weight of the plane raised the latch and allowed the rat to reach its food without performing the desired reaction. At first a hemp cord was run through a hole in the table under the box and attached to the latch. In this way the latch could be held firmly by the experimenter when necessary, and loosened at once. Later when needed a long wire slipped between the meshes of the cage and box and manipulated by the experimenter performed the same duty more surely and more easily.

An account of the procedure of one individual is given here as indicative of the learning process.

Diary Record of Black-and-White Female I on Problem Box II.

	TRIAL.	
6/5/07	1	Went at once to door of box, ran around box rapidly stopping only to sniff at food; struck the plane once, but too lightly. Gnawed industriously at door then ran about cage. Door fell at 2.33 min. without attracting her attention; she found opening at 3.07 min. and entered. Time: 3.07.
	2	Worked at door fiercely and persistently, occasionally dashed around box. Door fell at 2.00 min., in at 2.12 min.
	3	To door first, then round and round box several times. Plane fell at 1.50 min., in at 1.55 min.
6/6/07	4	Very active, but confined activity too near to box. In at once after door fell. Time: 1.78 min.
	5	Stayed too close to box. Time: 1.78 min.
	6	Had bad luck; was almost too active; jumped over plane or went around it. Door fell at 8.53 min. The rat got up on food box and spent much time in sniffing the air. Entered at 11.80 min.
6/7/07	7	Door fell at .67 min., in at .72 min.
	8	Door fell at .80 min., in at .88 min.
	9	See sixth trial. Time: 1.38 min., in at 1.50 min.
6/8/07	10	Door fell at .07 min., in at .10 min.
	11	Door fell at .03 min., in at .06 min.
	12	Rat went directly to door, then to plane, then back to door at once. Door fell at .13 min., in at .17 min.
6/11/07	18	Went as usual directly to plane and over it, but too near base, consequently door did not fall; rat then went to door, came back to plane at once, over and to the then open door. The association seems firmly fixed. Time: 18 min.

The record of this black-and-white rat was chosen because as a whole it was more typical than that of any other individual of the manner in which the association was formed, though the time records are lower than the records of an average normal rat. This rat, too, learned the problem in fewer trials than the average normal rat: indications of the *solution* were observed in the 12th trial, in which there seemed to appear the association between the door and the plane. In this trial there were no useless movements. There was little doubt that in the eighteenth trial the association had become fixed.

The above records of the individual rat serve to call attention to the nature of the learning process involved in this test. We have (1) random movements; (2) the accidental successes from which the animal at first profits little; (3) the elimination of useless movements; and (4) the completely established habit.

The elimination of useless movements in this test is accomplished in much the same general fashion as in tests of the labyrinth type. In the former, however, the rat runs about in an open space, whereas in the latter his pathway is restricted by the side walls of the galleries. Therefore the random movements in the present case survive for a longer time than in the labyrinth, because there is greater opportunity for, and a greater variety of, them. As was stated, the rat usually goes first to the door, then about the cage and to the plane and back again to the door. Ordinarily this routine continues until about the thirtieth trial, after which the movements are in a great measure automatic.

The animals, as was stated earlier, tend in their habitual reactions to go to the plane, thence to the door by a definite route, which varies with the individual. An unusual turn in leaving the plane to go to the door may so utterly confuse them, that to run back to the plane and to start over is the only apparent manner of taking up the trail. This is a very frequent occurrence, and among the last trials is almost the sole reason for a high time record.

2. *Statement of Results.*

a. On Normal White Rats.

The group of normal white rats was first tested upon the problem.[1] The averages of the group, four males and three

[1] The problem box at the beginning of this test was not of the same form as described here. The plane was set immediately at the north end of the box, instead of 11 cm. distant, and the control cage was the small one used over Box 1, instead of the larger one later used. The original conditions made the problem much easier of solution than the experimenter desired. Radical modifica-

females,[1] are given in Table V, following the form of the previous tables. The graphical representation of the averages given in Table V is shown on Plate II.

The column showing the minimal time-records at each trial indicate how quickly the problem can be solved. For the last ten trials the minimal time runs but little over .03 minutes. The curve constructed from the group averages at the successive trials shows the second rise at the third and fourth trials, which is due to the length of time spent by the rats in going directly to the door of the box and trying to push it open.

TABLE V.

Showing the average, the minimum and the maximum time-records of seven normal white rats upon Problem II. The last three columns show the number of animals whose records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
	min.	*min.*	*min.*			
1	5.88	.25	18.56		4	3
2	1.42	.04	3.49		4	3
3	2.35	.04	9.22		5	2
4	1.54	.15	5.74		5	2
5	.80	.10	3.03		6	1
6	.72	.09	2.08		5	2
7	.95	.06	4.40		6	1
8	.27	.08	.42		5	2
9	.20	.03	.47		5	2
10	.20	.07	.37		4	3
11	.49	.63	1.20		4	3

tions were made accordingly: the plane was moved out to a distance of 11 cm. and a large control cage was put over the problem box. The rats, however, had had two days experience with the original box. The above records are all taken from tests with the modified box. It is impossible to estimate what was carried over from the old situation to the new, therefore the records may not be quite fairly comparable to those of the rats which followed; yet this fact does not detract from the utility of the curve as a whole.

[1] Female I learned to open the door, not by stepping on the plane, but by tugging at the cord which attached the plane to the latch. From about the thirtieth trial she not only tugged at the cord until the door fell, but continued to tug; so that her time-records were too variable to be included in the average.

TABLE V.—Continued.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
12	.16	.67	.47		6	1
13	.30	.67	.62		3	4
14	.12	.57	.25		4	3
15	.21	.67	.52		5	2
16	.26	.08	1.03		6	1
17	.13	.05	.20		4	3
18	.07	.05	.10		5	2
19	.14	.04	.67		6	1
20	.08	.05	.33		4	3
21	.14	.05	.50		6	1
22	.09	.04	.17		4	3
23	.12	.04	.40		5	2
24	.08	.03	.18		4	3
25	.05	.03	.10		2	5
26	.22	.04	.62		4	3
27	.13	.03	.50		6	1
28	.10	.04	.27		5	2
29	.14	.03	.33		4	3
30	.12	.03	.25		4	3
31	.18	.03	.38		5	2
32	.25	.06	1.33		6	1
33	.14	.02	.52		6	1
34	.08	.05	.18		5	2
35	.11	.07	.17	1	4	2
36	.12	.03	.25		4	3
37	.08	.04	.12	2	3	2
38	.07	.03	.10		3	4
39	.07	.04	.12	1	3	3
40	.09	.04	.23	1	5	1
41	.05	.02	.08	1	3	3
42	.19	.04	.88		6	1
43	.12	.03	.17		4	3
44	.06	.03	.10		4	3
45	.10	.03	.20	1	3	3
46	.08	.03	.16	3	2	2
47	.07	.04	.22		4	3
48	.11	.03	.22		4	3
49	.08	.03	.11	1	2	4
50	.08	.06	.13	1	4	2

The following table shows the percentage of minimal and of maximal time-records made by each of the rats in this group.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY INDIVIDUALS.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Male I	12	Male I	12
Male II	40	Male II	8
Male III	9	Male III	10
Male IV	8	Male IV	12
Female II	11	Female II	8
Female III	16	Female III	31
Female IV	4	Female IV	19

b. On Normal Black-and-White Rats.

Table VI and the curve on Plate II show the records of the group of four black-and-white females. These rats were so incessantly active that it was often quite difficult to hold them in the hand. Because of this superabundance of energy their early time-records were short. They gave evidence also, as remarked above, of having acquired the association earlier in the series than the normal white rats, whose records were unfortunately rendered ambiguous for the comparison.

After the thirty-fifth trial, the rats received a fright, probably due to the odor of wild rats about the cage. They were so disturbed upon being introduced into the problem box that the final abandonment of the series was necessitated. Their reactions had been practically constant in their last fifteen trials, so that little was lost to the experiment.

The following tabulation shows the percentage of minimal and of maximal records of each animal.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL RECORDS OF EACH INDIVIDUAL.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Female I	36	Female I	26
Female II	17	Female II	21
Female III	31	Female III	25
Female IV	16	Female IV	28

The percentage of maximal records, as indicated above, is very evenly divided among the four animals. Females I and

TABLE VI.

Showing the average, the minimum and the maximum time-records of four black-and-white females upon Problem II. The last three columns show the number of animals whose records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
	min.	*min.*	*min.*			
1	1.53	.78	3.07		3	1
2	1.70	.63	2.53		2	2
3	.73	.22	1.56		2	2
4	1.02	.70	1.57		3	1
5	1.63	.47	2.78		2	2
6	4.17	.27	11.80		3	1
7	1.10	.72	1.57		3	1
8	.87	.18	1.70		2	2
9	.90	.23	1.50	1	1	2
10	.78	.10	1.62		3	1
11	.37	.06	.92		3	1
12	.48	.17	.96		2	2
13	.28	.10	.58		2	2
14	.35	.25	.58		3	1
15	.39	.05	.67		3	1
16	.22	.11	.37		3	1
17	.30	.08	.66		3	1
18	.18	.13	.25		2	2
19	.25	.14	.35		2	2
20	.16	.13	.21		3	1
21	.15	.07	.30		3	1
22	.12	.07	.25		3	1
23	.12	.07	.20		3	1
24	.11	.06	.23		3	1
25	.07	.06	.08		2	2
26	.13	.07	.18	1	2	1
27	.14	.06	.37		3	1
28	.14	.07	.28		3	1
29	.14	.07	.30		3	1
30	.16	.09	.24		2	2
31	.11	.05	.17		2	2
32	.14	.05	.30		3	1
33	.09	.05	.14	1	2	1
34	.13	.10	.16		2	2
35	.14	.08	.18		2	2

III together made two-thirds of the entire total number of minimal records.

c. On Blind Rats.

Table VII and the corresponding curve on Plate II show similarly the average records of six[1] blind rats on Problem II.

The behavior of the blind rats was characterized by a lack of eagerness, although when first introduced into the control cage they were most anxious to get into the food box; if they were not successful soon their activity abated, and random movements characterized their efforts. They were slow in forming a pathway, and in several instances no definite path was chosen.

The percentage of minimal and of maximal time-records of forty-four trials[2] for each of the blind rats is given below.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Female I	9	Female I	38
Female II	14	Female II	26
Female III	19	Female III	7
Female IV	29	Female IV	9
Female V	22	Female V	2
Male IV	2	Male IV	18

d. On Anosmic Rats.

As before stated all but one of the anosmic rats died before the experimentation had been completed.[3] The one that remained was set to work on this problem.[4] He learned the association perfectly, but invariably pressed down the plane

[1] Males I and III had died of an infection.

[2] Female II became ill, and did not work after the forty-fourth trial, consequently the percentage after this trial is not considered. Male II manifested a decided repugnance to approaching the plane, as the falling of it had apparently frightened him; therefore he did not learn the problem.

[3] The infection became apparent just after the animals had completed their work upon Problem I, and they died almost immediately after.

[4] He was the rat which gnawed at the wire in the sawdust box.

Table VII.

Showing the average, the minimum and the maximum time-records of six blind rats upon Problem II. The last three columns show the number of animals whose records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
	min.	*min.*	*min.*			
1	1.32	.52	2.08		3	3
2	1.47	.23	2.75		4	2
3	4.09	.52	12.37		4	2
4	8.37	.12	45.80		5	1
5	2.37	.43	7.57		4	2
6	2.12	.30	4.03		3	2
7	3.47	.47	8.59		4	2
8	1.68	.12	4.17		4	2
9	1.93	.07	.62		4	2
10	1.70	.10	5.19		4	2
11	1.22	.12	4.33		4	2
12	2.02	.12	8.52		5	1
13	2.08	.25	8.45		4	2
14	3.34	.55	12.03		5	1
15	2.85	.22	12.67		5	1
16	1.04	.15	2.39		4	2
17	1.71	.12	4.13		4	2
18	.83	.38	1.52		4	2
19	1.22	.28	2.62		3	3
20	1.55	.38	2.33		3	3
21	1.18	.10	4.05		4	2
22	1.04	.47	1.97		4	2
23	1.35	.26	3.50		4	2
24	1.77	.28	4.67		3	3
25	.96	.28	1.87		3	3
26	1.11	.08	2.42		3	3
27	.68	.25	1.13	2	2	2
28	.48	.13	.93		4	2
29	1.04	.15	1.18		3	3
30	1.14	.18	1.58		3	3
31	2.06	.40	2.92		3	3
32	2.80	.17	15.12		5	1
33	.70	.13	1.72		4	2
34	1.41	.17	3.57		3	3
35	.08	.27	1.85		3	3
36	1.10	.32	2.67		5	1
37	1.18	.45	3.67		5	1
38	1.48	.57	1.80		4	2

Table VII.—Continued.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
39	1.45	.18	3.67		3	3
40	.97	.25	2.88		4	2
41	.83	.28	1.03		2	4
42	.59	.17	1.12		3	3
43	.84	.25	1.10		4	2
44	.73	.25	2.53			
45	1.11	.13	2.50			
46	1.21	.17	3.17			
47	.75	.25	1.75			
48	.40	.15	.85			
49	.63	.22	1.10			
50	.70	.18	1.52			

while gnawing the string which connected the plane with the box. When he braced himself to gnaw, the pressure of his forefeet upon the plane was instrumental in pulling up the latch. He continued to gnaw until he had completely severed the string from the plane. After the thirty-third trial he invariably ran *at once* to the door when he had finished biting at the string or plane. If he did not find the door open, he went back to the place and his gnawing, and after another effort scurried to the door again. Wire was substituted for cord, whereupon his task was seemingly endless, and he varied his procedure by dragging the plane about the cage. Table VIII shows the records of this rat. The time-records of the later trials are shown as they were taken, giving the length of time consumed by the rat in different parts of the cage.

In learning the problem, this rat did not spend the greater part of the time in the early trials—as did the rats possessed of the sense of smell—in sniffing at the food through the meshes of the wire covering of the problem box. The rat did lose much time, however, in trying to get into the box by tugging at the door.

For the first six trials the time-records of this anosmic rat—with the exception of the second—are below one minute: four of them are below .50 min. His long time-records in

TABLE VIII.

Showing the time records of the anosmic rat on Problem II.

NO. OF TRIAL.	1. BEGINS GNAWING.	2. DOOR FALLS.	3. TIME FROM PLANE TO DOOR.	IN BOX.
	min.	*min.*	*min.*	*min.*
1		.23	.20	.43
2		.75	.35	1.10
3		.20	.20	.40
4		.37	.07	.44
5		.41	.05	.46
6		.33	.63	.96
7		1.42	.63	2.05
8		3.33	1.03	4.36
9		.25	.38	.63
10		2.25	1.02	3.27
11		.28	.20	.48
12		10.66	1.33	12.00
13		.08	.08	.16
14		1.00	.12	1.12
15		.23	.35	.58
16		.48	.58	1.06
17		1.65	.73	2.38
18		1.05	.45	1.50
19		2.92	.50	3.42
20		.66	.22	.88
21		2.25	.30	2.55
22		.83	.25	1.08
23		.33	.08	.41
24		.20	.17	.37
25		1.25	.13	1.38
26		1.33	.32	1.65
27		1.25	.28	1.53
28		.22	.13	.35
29		.32	.48	.80
30		1.03	.25	1.28
31		1.80	.32	2.12
32		.92	.25	1.17
33		2.12	.53	2.67
34		.83	.15	.98
35		.22	.50	.72
36		.50	.20	.70
37	.06	1.63	.03	1.66
38	.20	.85	.05	.90
39	.08	1.42	.05	1.45
40	.05	.50	.03	.53
41	.03	1.08	.03	1.11
42	.18	1.40	.08	1.48
43	.07	.67	.03	.70
44	.20	.83	.03	.85
45	.03	.63	.02	.65

the later trials were due to his gnawing propensities. The time which he spent in going from the entrance to the plane, or from the plane to the food box was not longer than that of the average normal rat. In the last nine trials the average time from entrance to plane is .10 min., and from plane to door .04 min. Disregarding the time spent in gnawing, his average time would be .14 min., for these trials; which may by a glance at the curves be seen to be but little longer than the average time-records of the normal rat, and shorter than the maximal time-records of that group.

3. Effect on Rats of Changing Position of Plane 90° to Right.

After the group of normal white rats had completed this series, the position of the plane was changed, being placed east of the box, instead of north as before. The reactions of the animals had become practically habitual before the change was made. It was thought that the change in conditions might bring out two facts regarding the behavior of the animal: (1) The nature of the sensory control in the habitual act; (2) the nature of the sensory data by means of which the modified reaction which is necessary under the changed conditions is built up.

Since the rats in this series were put always into the cage from the east, their established pathway carried them within two or three inches of the longer side of the plane. Each of the four males was tried in turn, and each went directly to the old position of the plane, then ran to the door. One of them, Rat III, seemed confused at not finding the plane in the usual position. The other three went to the food box as though not missing the link in the series. Their confusion began at the door. Each rat, after running about in a seemingly aimless manner for a few seconds, struck the plane and the door fell. Rat I could not seem to find the way to the door, even though something connected with the falling of the plane made him frantically eager to get back to the box. He finally went over the box. On the third trial he proceeded slowly and kept his nose to the floor (discovering the plane by touch ?). From the fourth to the ninth trial he passed over the plane directly on his way to the door. On both the ninth and tenth trials, he reverted to his old habit of going at once to the original position of the plane on the north. Rat II reverted to the old habit similarly in the ninth trial. Rat III went to the door first, except on the seventh and ninth trials when his route accidentally took him far enough south to touch the plane. The slight contact seemed to give him immediate orientation, for he ran across the plane and to the door at once.

Rat IV was the least confused. He ran to the old position but did not go to the door. Instead, he began wandering about and strayed across the plane in a seemingly haphazard fashion, but the instant it fell the old habit reasserted itself and he dashed off to the door. At the second trial he went directly across the plane to the door. All his other trials were almost precisely like his second. His records were quite phenomenally quick after the first trial, not going above .03 min.[1]

This test furnished some evidence upon the two facts sought for, i. e., the sensory avenues of control in the habitual act, and the sensory avenues of control in the process of readjustment. The habitual act seems to be carried out by means of the guidance of kinæsthetic impressions. The rats traversed the old pathway even when such movements did not lead to the successful solution of the problem. The old coördination broke down apparently in the case of these animals when they found the door closed; the fourth (Rat III) seemed to become confused in not getting the 'cue' at the plane itself. The question of the probable nature of the cue is discussed later in the section (p. 40).

[1] The case of Rat IV seems anomalous. The records bear evidence to the fact that the pathway of this rat in solving the original problem was variable. In seven out of the ten trials just preceding the change in the apparatus, the animal went first to the door, then to the plane. In the first trial after the change, he departed from his former custom of going first to the door, and instead went direct to the plane. His confusion at that point was no greater than he had often previously displayed in picking up the trails. On the second trial and on all thereafter, he resumed his old habit of going at once to the door; the result of which was that he threw the plane en-route as it was directly in his path from the entrance of the cage to the door of the food box. This explanation accounts for the seeming variation. It was simply a case in which the failure of the rat to acquire a stereotyped mode of response to the old situation made the adjustment to the new situation less difficult.

The new coördination necessary in the readjustment to the changed situation is built up on the sensory data by means of which the plane becomes the stimulus to the further coördination involved in running from the plane after it has fallen directly to the door of the food box. In these cases [*i. e.*, the inclined plane] the basis seems to be that afforded by touch. Contact with the plane was doubtless the evidence of its presence. Had some distance sense factor such as vision given the cue, it would seem that the animal would have had less difficulty in finding the plane in the new position which was at such a short distance from the old position. It was only when they came in contact with the plane that some sensory impulse connected with its fall set off the old association and they would dash to the door of the box. The new pathway was easily learned, though, as remarked above, not closely adhered to as three of the rats on later trials made errors in favor of the old pathway.

It had been a part of the plan of the work to further modify this test, but it was found later that the conditions of the learning process had not been sufficiently well controlled. In a test of this kind the rats should have learned the association in an environment every part of which was equally illuminated, so that a change in the apparatus would involve no change in brightness values in different portions of the field. The control cage should have been lined with canvas or other opaque material so as to preclude the possibility of orientation by means of distance sense factors. On this account a test involving such modifications as have been here made are not conclusive. The results are suggestive; and if the test were properly controlled it would be of value in isolating the different sense factors which function in forming the association.

A problem of especial interest which arose in carrying out the above work is that of the sensory avenue by means of which the rat obtains the cue for the run to the door after the plane has fallen. In the early trials of the test it seemed quite sufficient for the rat merely to run *past a certain point on the floor of the cage.* Indeed, many rats never appear to get beyond this method of reacting. But occasionally a rat hesitates at the plane apparently until he gets a cue that the door has fallen inward. In such instances the cue may be: (1) The sound of the falling door; (2) the molecular vibrations (tactual impressions) set up in the wood by the door as it falls upon the floor of the cage; (3) the jar (kinæsthetic and possibly static) of the falling plane.

Male III was an animal which did not seem to require such a cue. Several times in the last ten trials this animal would run to a position about one inch west of the plane, rear up slightly, and assume the crouching attitude which he had been accustomed to assume on the plane itself and would then dash back to the door. He had thus gone through all the movements of throwing the plane, *except that he had not performed them on the plane.* He was completely confused when he found the door closed. This rat at least, evidently oriented himself according to the relative position of the plane. The tactual element in the experience seemed of no value while the kinæsthetic experience of raising the forepart of the body and lowering it was apparently the esssential feature. This kinæsthetic series was sufficient to set off the sequent coördinations. The failure of the animal to react to the plane itself when almost touching it and when to all appearances attempting to react to it, is typical of the earlier trials of practically all of the rats. There is no evidence whatever that the rat perceived the plane or that the plane comes to be an isolable portion of the problem box situation. A rat, when attempting to get into the food box, runs around the box in varying circles. In one of these circles he runs over the plane and when he hurries back to the door, as he does after every peregrination, he finds it open. Many such trials are necessary before he establishes a pathway which includes in its course the crossing of the plane. This seems to indicate that the reaction of the animal to the plane is determined by kinæsthetic data and that the kinæsthetic experience at this point is the stimulus for the further movement, namely, that of turning to take up the pathway to the door.

On the other hand the behavior of Male II of the same group indicates a different kind of series of stimulations at the plane.

His path led him out upon the plane, rather than across it. On his forty-fourth, forty-eighth, forty-ninth and fiftieth trials he crouched near the margin of the plane nearest the cage, but as he was not far enough away from the inner margin, his weight was not sufficient to press down the plane, and consequently he could get no report. He then took another step further out and waited. Usually the door fell after his second step. It happened once on the forty-ninth trial that a third step was necessary to press down the plane. When this step had been taken, and the door had fallen, he hurried off to the food box.

It is not easy to postulate just what happened in the case of this rat. It is evident that some form of sensory data, probably auditory, combined possibly with tactual, kinæsthetic and organic, gave the cue to the succeeding coördinations.

4. Summary.

a. Average Time-records for the Total Series.

The average of the total time consumed both by the individuals and the groups is given below. The group averages are given only for the first thirty-five trials, by reason of the fact that the group of black-and-white rats were unable to finish the series of fifty trials.

TABLE SHOWING THE GROUP AVERAGE OF THE TOTAL TIME (THIRTY-FIVE TRIALS) CONSUMED BY NORMAL AND BY DEFECTIVE RATS IN LEARNING PROBLEM II.

Average by Groups

	min.		min.
Normal White	.48	Blind	1.48
Normal Black-and-White	.49		

TABLE SHOWING INDIVIDUAL AVERAGES OF THE TOTAL TIME (35 TRIALS) CONSUMED BY NORMAL AND BY DEFECTIVE RATS IN LEARNING PROBLEM II.

Normal White.	min.	*Normal Black-and-White*	min.	*Blind.*	min.
Male I	.31	Female I	.77	Female I	1.56
Male II	.84	Female II	.44	Female II	1.67
Male III	.26	Female III	.52	Female III	1.14
Male IV	.71	Female IV	.51	Female IV	1.02
				Female V	.60
Female II	.42			Male IV	.77
Female III	.43				
Female IV	.40				

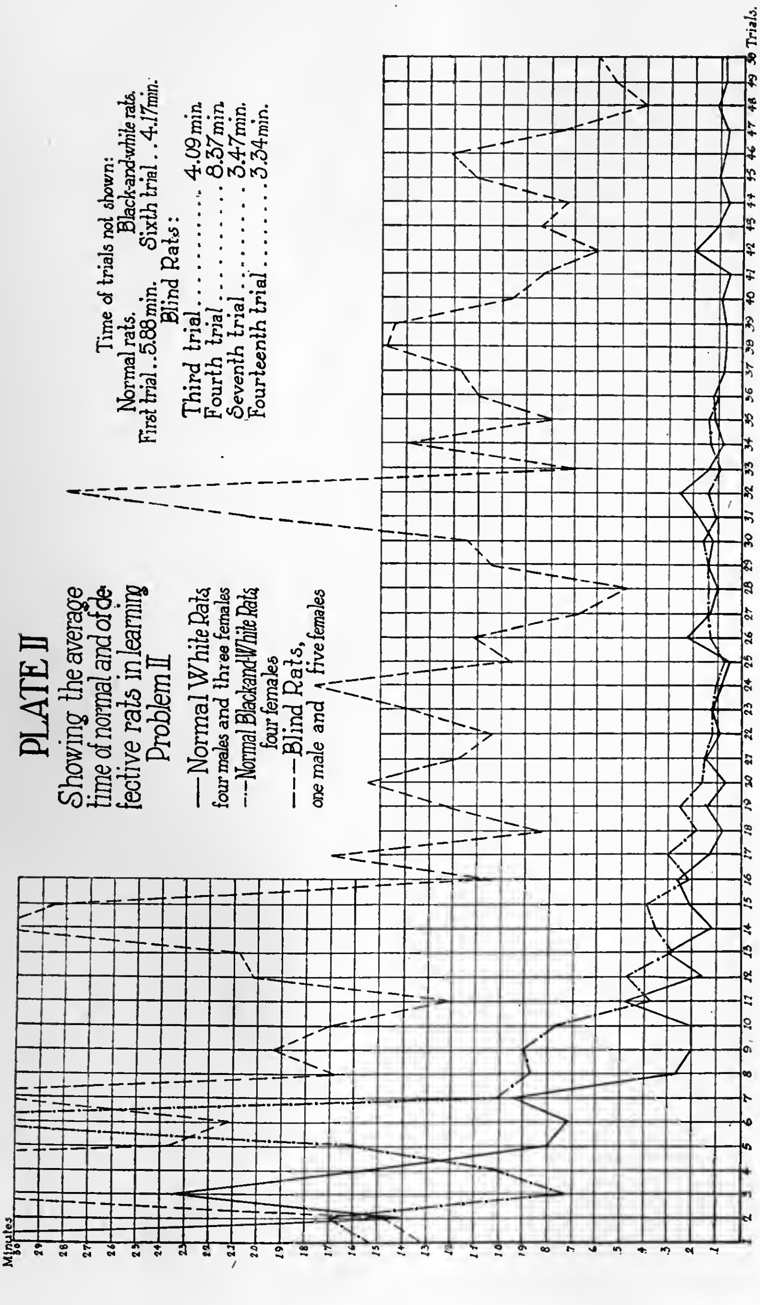


PLATE II
Showing the average time of normal and of defective rats in learning Problem II
—Normal White Rats, four males and three females
-·-Normal Black-and-White Rats, four females
- - -Blind Rats, one male and five females
Time of trials not shown:
Normal rats. Black-and-white rats.
First trial..5.88 min. Sixth trial..4.17 min.
Blind Rats:
Third trial........4.09 min.
Fourth trial........8.37 min.
Seventh trial........3.47 min.
Fourteenth trial........3.34 min.
Minutes
Trials.

There is practically no difference in the averages of the group of the white and of the black-and-white animals. The average of the group of blind rats is much greater than that of the normal rats.

The variation among the averages of the individuals is considerable. The high records of the normal white Males II and IV, were due to high time-records in the first ten trials. The averages of the normal white females are all lower than the lowest of those of the black-and-white group. Blind Females IV and V and blind Male V, made averages lower than the maximum individual records of either the normal white or the black-and-white groups. There is greater variation among individuals of this blind group than among those of any other group.

b. Average Time-Records by Groups of Ten Trials.

The following tables show the averages of the records of the series by groups of ten trials each. The averages are given for the individuals, and for the groups. The starred records show those instances in which the minimal record for a series of ten was reached before the last series of ten.

Normal White Rats.

	INDIVIDUALS.				GROUP.
TRIALS.	MALE I.	MALE II.	MALE III.	MALE IV.	
	min.	*min.*	*min.*	*min.*	
1–10	.64	2.67	.62	2.27	
11–20	.24	.17	.08*	.08	
21–30	.10*	.05*	.13*	.05*	
31–40	.11	.05	.09	.10	
41–50	.07	.08	.13	.09	
	FEMALE I.	FEMALE II.	FEMALE III.	FEMALE IV.	
					min.
1–10		1.16	.79	.96	1.44
11–20		.10	.43	.24	.19
21–30		.12	.27	.17	.13
31–40		.09*	.28	.09	.11
41–50		.17	.17	.08	.10

Black-and-White Rats.

	FEMALE I.	FEMALE II.	FEMALE III.	FEMALE IV.	
1–1	2.28	1.03	1.19	1.19	1.42
11–20	.14	.25	.47	.32	.29
21–30	.12*	.13	.08*	.18	.13
31–35	.16	.13	.09	.09	.12

Blind Rats.

	FEMALE I.	FEMALE II.	FEMALE III.	FEMALE IV.	FEMALE V.	MALE IV.	GROUP.
1–10	2.96	.51*	1.23	8.34	1.12	2.94	2.85
11–20	4.50	.74	1.23	.69	.77	2.68	1.77
21–30	1.43	2.00	1.13*	.29*	.61	1.00	1.07*
31–40	2.81	1.99	1.28	.46	.75	.84*	1.35
41–50	1.11	1.28		.40	.57	7.80	2.23

Here, as in Problem I, many rats—in this case ten out of a total of seventeen animals—made their lowest averages before the last ten trials of the series. This peculiarity is especially noticeable in the above records of blind Female II whose first group of ten averaged considerably less than any later ten.

The blind group is the only *group* whose total average shows the minimal average by ten near the middle of the series.

c. Comparison of the Different Groups.

The curve showing the average time-records of the group of normal white rats is lowest at the first trial, from the fifth to the eleventh trials, and from the seventeenth to the twenty-fifth inclusive.

The curve of the black-and-white rats has a very pronounced rise at the sixth trial; otherwise it is more regular in contour than that of the group of the normal white rats.

The curve representing the blind rats is exceedingly high and irregular though at the first trial it is lower than any other, and at the second trial is below that of the black-and-white rats and but little above that of the normal white rats. From the eighth trial this curve does not descend to the level of the curve of the normal animal.

C. TESTS ON PROBLEM BOX III.

1. Description of Apparatus, and of the Learning Process.

The third problem box submitted to these same groups of rats was the familiar one necessitating the raising of a latch. As in the two previous problems, the animal enters the box for food. The box consists of a wooden frame, 14.25 cm. in height, 20 cm. in length and 20 cm. in breadth. The frame is covered with wire netting of one centimeter mesh. The spring door, 6.25 cm. high and 10 cm. wide, is so fastened to the lower left hand corner of one side of the box, that when the latch which holds the door in place is raised, the door opens outward. (See figure 3.)

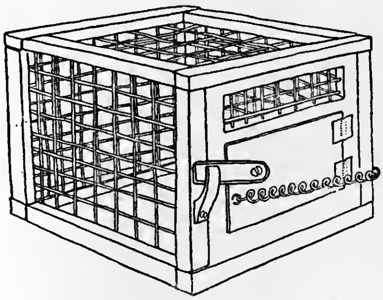


Fig. 3.

The control cage which was placed over the box is the same size as was used over Box I. A morsel of cream cheese—always of one commercial brand to insure constancy of taste and of odor—was rubbed on the back of the latch at the beginning of the series of trials. The combined taste and odor served to attract the interest of the rat, the effect of which was to lower the absolute time records of the first trials. The use of this device does not alter the general form of the learning curve, nor influence the later time records.

The unusual coördinations involved in this test are those connected with finding the door and raising the head to lift the latch. The animal may lift the latch either with its teeth, snout or claws. The rapidity of the solution depends in the first few trials largely upon the particular type of movement adopted by the rat in opening the latch. The animal which lifts the latch with its snout is likely to make the shortest time-record, in view of the fact that this movement requires fewest muscular coördinations.

The diary notes are given as the most concise and satisfactory description of the learning process.

Notes on the Behavior of Normal Male Rat III in Learning Problem III.

	TRIAL.	
6/2/06	1	The spring interests him; stands on his hind legs and pulls it continually. While working at spring, presses down inner end of latch. Crawled in over top of door. Time: 2.20 min.
	2	Same procedure as above, but animal more active. Time: .13 min.
	3	Repeated above: Time: 1.25 min.
6/3/06	4	Leaves spring to gnaw at latch; pushes down inner end of latch as before. Time: .13 min.
	5	Raises latch with teeth. Time: .25 min.
	6	As above. Goes in *over* door each time. Time: .12 min.

This particular rat crawled either over or under the door in entering the food box until the end of the series. Most of the rats discovered the easier method of entrance, and a number of them learned to raise the latch from the left, and saved themselves the annoyance of a blow from the opening door. In a number of instances the rats became wary about entering the box on account of having been struck by the door. Many long time-records near the beginning of the series are to be explained in this manner. Individual variations in the animals' methods of solving this problem are more noticeable here than in any of the other problems.

2. *Statement of Results.*

a. On Normal White Rats.

Table IX and the curve on Plate III show the averages of the time-records of the group of normal white rats upon this problem.

The group at this time consisted of four males and two females.[1]

TABLE IX.

Showing the average, the minimum and the maximum time-records of six normal white rats on Problem III. The last three columns show the number of animals whose records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
	min.	*min.*	*min.*			
1	5.72	1.87	9.48		3	3
2	.32	.08	.55		3	3
3	.33	.02	1.25		5	1
4	.17	.10	.21		2	4
5	.29	.08	.87		4	2
6	.22	.12	.37		4	2
7	.09	.04	.14		3	3
8	.09	.05	.15	1	3	2
9	.06	.03	.08		3	3
10	.06	.03	.08		4	2
11	.07	.05	.11	1	3	2
12	.05	.03	.07	2	2	2
13	.05	.02	.07		3	3
14	.05	.04	.06	2	3	1
15	.05	.03	.08		5	1

These rats did not discover the door in the first trial; but at the second they went almost immediately to it and bit and clawed at the latch and spring with great energy. By the end of the series, each rat had learned to lift the latch with its snout, and most of them raised the latch from the outer margin and were thus out of the way of the door when it flew open. The individual records are very uniform. At the fifteenth trial the coördinations were perfect and had become habitual. The experiments were therefore discontinued.

[1]This group has been reduced in numbers. Female III died, and Female I who was slow in Problem I, slower in II, and very slow in the present problem made such uniformly poor records that they were omitted in the average, as they represented a very marked variation. She made the maximum time-record in every trial. Her time-records are given and discussed in the section on individual variations.

The following table shows the percentage of maximal and of minimal time-records made by each animal.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Male I	46	Male I	0
Male II	10	Male II	0
Male III	10	Male III	13
Male IV	34	Male IV	27
Female II	0	Female II	40
Female III	0	Female III	20

Neither of the two females made a minimal record during the series, and neither Male I nor Male II made a maximal record during the series.

b. On Normal Black-and-White Rats.

This group had also been reduced in number. Females I and II died at the end of tests upon Problem Box II. Table X and the curve on Plate III show the records of the two remaining animals.

TABLE X.

Showing the Average Time-records of Two Black-and-White Rats on Problem III.

NO. OF TRIAL.	AVERAGE.	NO. OF TRIAL.	AVERAGE
	min.		*min.*
1	.99	15	.04
2	.49	16	.07
3	.12	17	.04
4	.29	18	.04
5	.09	19	.06
6	.11	20	.08
7	.07	21	.09
8	.04	22	.05
9	.08	23	.04
10	.06	24	.04
11	.14	25	.04
12	.04	26	.04
13	.05	27	.03
14	.07	28	.05

The records of these two rats are very low, and very uniform. The first successes were achieved in remarkably short time, all of the averages being below one minute. In the later trials, each rat, in its eagerness to get to the door, sometimes dashed past it, and went on around the box, thus lengthening the time-record.

The percentage of minimal and of maximal time-records made by each rat in the group is shown below.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Female III	30	Female III	70
Female IV	70	Female IV	30

c. On Blind Rats.

The behavior of the blind rats was strikingly at variance with that of the normal rat in this problem. Their time-records were long and inconstant. Practically all of the rats suffered an emotional shock from the quick opening of the door when the latch was raised. The blind animals were not alone in receiving a fright at the blow of the door. One normal rat became so cautious in his attempts to raise the latch that his efforts in going up to the latch, springing back, stepping up cautiously again, and again rebounding, came to be ludicrous in the extreme. On one occasion the unsuccessful efforts of a blind rat to raise the latch were counted. Thirty-three times she approached the latch and thirty-two times she recoiled like a tight spring! Only at the thirty-third attempt did she exert enough pressure to lift the latch, and when the door flew open she seemed paralyzed with fright for several seconds, and did not attempt to enter the box. When she finally entered she caught up a mouthful of food and ran outside to devour it. This state of high emotional tension is one cause of their poor time-records, particularly in the early trials.

The following table, Table XI, and the curve on Plate III show the time-records of the group.

TABLE XI.

Showing the average, the minimum and the maximum time-records of three blind rats on Problem III. The last three columns show the number of animals whose records are (1) equal to the average, (2) below, and (3) above the average.

NO. OF TRIAL.	AVERAGE.	MINIMUM.	MAXIMUM.	1.	2.	3.
	min.	*min.*	*min.*			
1	7.32	3.95	9.23		1	2
2	3.93	.18	9.37		2	1
3	.57	.28	.75		1	2
4	2.59	.45	6.07		2	1
5	3.79	.92	7.28		2	1
6	1.90	.22	4.95		2	1
7	1.78	.15	4.65		2	1
8	.95	.35	1.70		2	1
9	1.31	.48	2.35		2	1
10	.92	.12	1.42		1	2
11	.53	.38	.77		2	1
12	.53	.17	.80		1	2
13	.60	.20	.78		1	2
14	.30	.12	.34		1	2
15	.20	.12	.28		2	1
16	.30	.25	.35	1	1	1
17	.19	.10	.30		2	1
18	.17	.11	.23		1	2
19	.51	.20	1.13		2	1
20	.19	.07	.30		1	2
21	.28	.12	.37		1	2
22	.30	.22	.45		2	1
23	.21	.20	.35		2	1
24	23	.10	.30		1	2
25	.25	.12	.35		1	2
26	.18	.13	.25		2	1
27	.27	.23	.33		2	1
28	.14	.08	.25		2	1
29	.18	.10	.28		2	1
30	.28	.17	.38	1	1	1
31	.34	.22	.45		1	2
32	.26	.12	.35		1	2
33	.19	.08	.35		2	1
34	.36	.10	.83		2	1
35	.30	.17	.43	1	1	1
36	.29	.15	.47		2	1
37	.44	.10	.72		1	2
38	.33	.08	.55		1	2
39	.27	.05	.40		1	2
40	.23	.18	.26		1	2

The blind rats were slow in attaining their first successes, and in establishing a pathway from the entrance of the cage to the door of the food box. Several rats did not establish a definite route even after forty trials. In these cases the behavior of the animal throughout the test suggested the random activity of early trials.

The table given below shows the percentage of minimal and of maximal time-records made by each rat.

TABLE SHOWING PERCENTAGE OF MINIMAL AND OF MAXIMAL TIME-RECORDS MADE BY EACH INDIVIDUAL.

Minimal.	*per cent.*	*Maximal.*	*per cent.*
Male IV	51	Male IV	23
Female IV	34	Female IV	27
Female V	15	Female V	50

d. On Anosmic Rat.

The one remaining anosmic rat was put to work upon the problem. He learned to open the door at once, but rather than enter the food box gratified himself by gnawing away the wooden latch. He had apparently established the association by the fifth trial. His time-records are almost valueless, however, being a measure, not primarily of the length of time it took him to open the door of the food box and enter but of the time he gave himself for the demolition of the latch.

3. Effect on Rats of Changing Position of Box and Cage.

After three black-and-white males had learned this problem, the experimenter changed the position of both the control cage and the problem box. In the original experiment the entrance to the cage was on the east, and the door of the problem box upon the south. After the change, the door of the cage was to the north, and the door of the problem box was to the east. The relative positions of the entrance of the cage to the door of the problem box thus remained the same; only the absolute directions had been changed. The cage and the enclosed problem box had been rotated through an angle of 90°. The floor of the cage was of galvanized iron sheeting, and was turned with the entire apparatus.

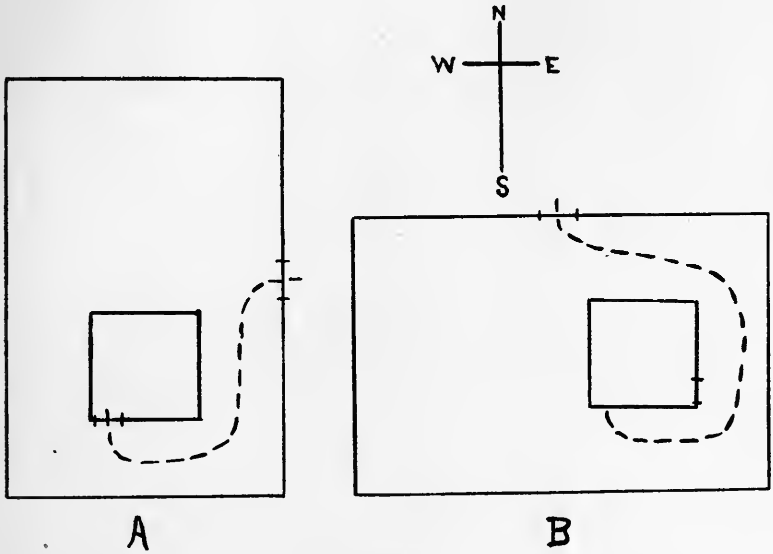


Fig. 4.

Note The dotted line shows the direct pathway from the entrance of the cage to the door of the problem box. Rats which have established a regular pathway use the one indicated in A. The dotted line in B, shows the lengthened pathway the rats followed after the problem box and cage had been turned.

Three normal black-and-white males had learned the problem. Their time-records had been reduced to .03 min., i. e., practically to the reaction time of the animal.

The appended diary notes describe the behavior of the animals very clearly.

Notes on Behavior of Rats with Turned Apparatus.

	TRIAL.	
1/7/08		Cage and box in first position, (A); door of problem box on south.
		Rat I.
	1	Direct to door. Time: .03 min.
	2	(Cage in changed position, door on east, 90° to right) Went to old position of door, 'nosed' the wire on south of box, became confused, went to west, back to south, strayed around close to side of the box; vibrissæ touched latch in passing; rat stopped, lifted latch, but did not enter at once. Time: .29 min.
	3	Struck door in hurrying past corner, hesitated, turned back, started on, turned back again and lifted latch. Time: .06 min.
	4	Repeated movements of trial 3. Time: .06 min.
		Rat II.
	1	(Cage in original position, (A). Time: .03 min.
	2	(Cage in changed position (B). Confused by entrance, went to west, paused at south, came back to door, (found it by snout ?), hesitated, then lifted latch and entered. Time: .31 min.
	3	Confused, went past door, hesitated, went to old position, came back, found door apparently by touch, opened it, but did not enter at once. Time: .15 min.
	4	Badly confused; wanders all about cage. Went to door in new position, 'nosed' it, but went away. Entered finally, more confused than ever. Time: .69 min.
		Rat III.
	1	(Cage in original position, (A). Went so fast he ran past door, and then came back. Time: .04 min.
	2	(Cage in changed position (*B*). Turned around and seemed utterly at a loss; went past door to south, "nosed," climbed on box and down on south, tried to raise wires again; strayed about until he came upon door, sniffed at it carefully and leisurely, went on, came back, evidently received stronger touch stimulation, then raised latch in his usual way. Time: .49 min.
	3	Passed door, turned back, hesitated, moving head from side to side, lifted latch and entered. Time: .09 min.
	4	Directly to south, then back to east. Time: .09 min.

The notes call attention to three characteristics of the animals' behavior under the new conditions: (1) The immediate excursion to the south side of the box; (2) confusion at failing to find the door; (3) nosing about to discover the door, and (4) the discovery of it made apparently in these cases at least, on the data afforded by either the contact of the vibrissæ, or of the snout with the latch or with the door. The door was not held firmly in place by the latch, but wavered slightly when an animal exerted pressure upon it with its snout or with its claws. This yielding of the door apparently often stimulates a general motor overflow which results in movements of vigorously biting and clawing at the door even before the animal associates the door with the process of procuring food. It is quite probable that as the rat searches for the entrance to the box, the yielding of the door or the mobility of the latch is the stimulus which releases the movements that raise the latch.

There was no indication that any of the rats located the door by means of vision, for each rat passed the door while 'searching' for it without reacting to it. Yet when the door was touched there followed the examination of the latch, and the performance of the requisite movements to open the door.

The next day after these tests and the second day following, these experiments were continued, with the same general results. When the door of the control cage was turned to the *north*, the rats went first to the *south*, then to the *east*, finally locating the door as before,—probably with the snout. *But each rat went first to the south, where the door had been two days before.* The old pathway involved a turn first to the left, then to the right, then to the right again. In the new position each rat lengthened his path and, after making the previous series of turns as before, added another turn to the right, arriving at the absolute spot to which his shorter path had hitherto brought him.

A blind black-and white rat was tested in the same manner, with the cage and box in the two positions. He exhibited the same characteristics as the normal rats, except that in his confusion he went *over* the food box, a habit that he had acquired in learning the problem. The habit had almost disappeared, but when his first attempts were unsuccessful, he reverted to his early random movements. His time-records with the box in the changed position suffered no greater increase than those of the normal rats under the same conditions.

These tests, like those involving the changed position of the plane, were not sufficiently well controlled to justify carrying them further. The animals had learned the problem with the source of light to the west. When the apparatus was changed, the brightness values of different parts of the field were also changed, whereas they should have remained constant. In addition to this, the control cage was not large enough to permit the rat to go to the entrance to the door of the problem box without brushing the corner of the box. In this way one rat accidently discovered the latch in passing. To control the conditions properly in such an experiment, a larger control cage covered with canvas, and lighted from within, would be necessary.

The above test seems to indicate the value of touch in locating the latch. The normal rats, like the blind rat, seemed to discover the latch by contact. *The functioning of anything like discriminative vision could not be detected in the behavior of any animal submitted to these tests.* If such data had been made use of, the fact should have been apparent in the method of discovering the door. A rat when 'searching' for the door often passed it, and seemed oblivious to its location although it was not more than three inches distant.

The behavior of the animals in this experiment justifies the discussion of the behavior of rats in tests in which the position of the plane was changed (Problem II, p. 38). The contention was there advanced that there was no evidence of the perception of the plane by the rat: That the plane was not isolated from the rest of the environment. In the test with Problem III, there is no evidence that the latch or the door was singled out and reacted to as an object.

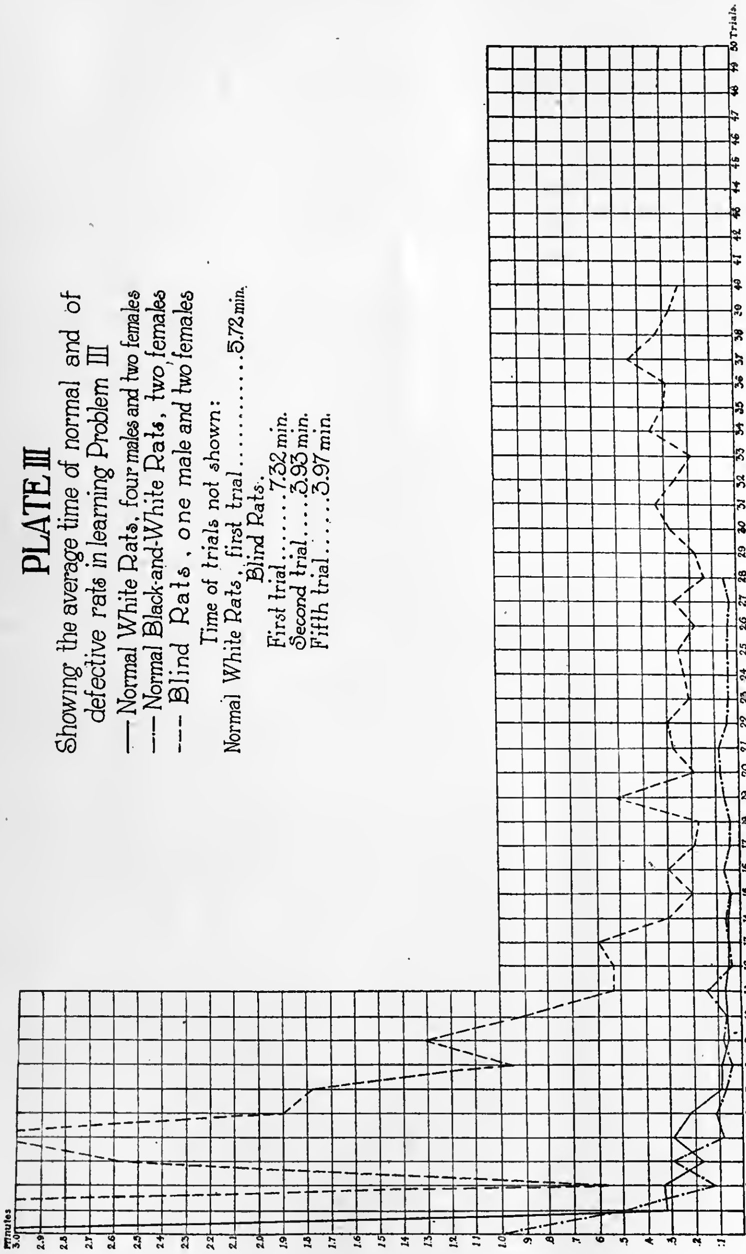


PLATE III

Showing the average time of normal and of defective rats in learning Problem III

—— Normal White Rats, four males and two females
—·— Normal Black-and-White Rats, two females
- - - Blind Rats, one male and two females

Time of trials not shown:

Normal White Rats, first trial .... 5.72 min.

Blind Rats.

First trial .... 7.32 min.
Second trial ..... 3.93 min.
Fifth trial ...... 3.97 min.

4. *Summary.*

a. Average Time-records for the Total Series of Trials.

The following table shows the average of the total time consumed for the different groups of rats for a series of 15 trials. It has been necessary to make a comparison on the basis of this number of trials since the normal white rats, which were first tested had reached a stage of proficiency in the solution at which their reactions were constant and habitual. The later records of the other groups are given in the following section.

TABLE SHOWING GROUP AVERAGE OF THE TOTAL TIME (15 TRIALS) CONSUMED BY NORMAL AND BY DEFECTIVE ANIMALS IN LEARNING PROBLEM III.

	min.		*min.*
Normal White	.50	Blind	1.81
Black-and-White	.18		

TABLE SHOWING INDIVIDUAL AVERAGES OF TOTAL TIME CONSUMED IN FIFTEEN TRIALS.

Normal White.	*min.*	*Black-and-White.*	*min.*	*Blind.*	*min.*
Male I	.36	Female III	.24	Male IV	.63
Male II	.69	Female IV	.12	Female IV	2.95
Male III	.33			Female V	1.85
Male IV	.24				
Female II	.61				
Female III	.79				

The great difference in the group averages of the normal white and the black-and-white rat is due to the very long time-record of the white rats at the first trial. The average of the blind animals is high although the average of Blind Male IV is less than that of Normal Male II and that of Normal Female III. Unfortunately the fact that there remained but two black-and-white animals renders this group practically unavailable for purposes of comparison.

b. Average Time-records by Groups of Five Trials.

Normal White.

Individuals.

TRIALS.	MALE I.	MALE II.	MALE III.	MALE IV.	FEMALE II.	FEMALE III.	GROUP.
	min.	*min.*	*min.*	*min.*	*min.*	*min.*	*min.*
1–5	.97	1.95	.83	.60	1.64	2.19	1.36
6–10	.08	.08	.09	.10	.12	.14	.10
11–15	.03	.05	.07	.04	.06	.05	.05

Black-and-White Rats.

	FEMALE III.	FEMALE IV.	GROUP.
	min.	*min.*	*min.*
1–5	.60	.22	.41
6–10	.08	.06	.07
11–15	.06	.08	.07
16–20	.07	.05	.06
21–25	.05	.05	.05

Blind Rats.

TRIALS.	MALE IV	FEMALE IV.	FEMALE V.	AVERAGE.
	min.	*min.*	*min.*	*min.*
1–5	1.36	5.49	4.08	3.64
6–10	.26	2.76	1.10	1.37
11–15	.26	.60	.42	.43
16–20	.40	.18	.24	.27
21–25	.22	.18	.36	.25
26–30	.21	.20	.22	.21
31–36	.26	.20	1.42	.29
36–40	.31	.13	.46	.30

There is not much to be added in comment on these tables. They point out the fact that there is little or no difference in the time-records of the white and the black-and-white rats, and a very considerable difference in those of the normal and the blind rats. No blind rat made an average for ten trials in later trials so low as the highest group average for the normal rats in such trials.

Probably only the blind rats were given a sufficient number of trials in this problem to render apparent a tendency towards the dissolution of the association that has been commented on in similar records for animals on Problems I and II. Three of four of the blind rats raise their averages near the end of the series.

c. Comparison of the Curves of the Different Groups.

The curve representing the average of the normal white rats is, in this problem, more uniform than in either of the preceding problems. While the first trial is high, the curve drops very rapidly, reaching and maintaining its low level on and after the sixth trial.

The curve of the black-and-white group is much lower at the first trial, and does not make such a rapid descent, though at the fifth, sixth, seventh, and eighth trials it is lower than that of the normal white rat. At the twelfth, thirteenth, fourteenth, and fifteenth trials they run no more than .01 min. apart. The third curve, that of the blind animals, is of a different contour, as it drops much more gradually. At no point in the series of 5 trials, does it reach a level near that of either of the groups of normal rats. The curve is also quite irregular.[1]

D. DISCUSSION OF CURVES SHOWING AVERAGE TIME-RECORDS OF NORMAL AND OF DEFECTIVE RATS IN LEARNING THE MAZE.

On account of the necessity of plotting the curves shown in this paper, on a much larger scale than that employed in Watson's monograph, the difference between the blind and the normal rats seems much magnified. The time-records for the later trials on the maze rarely run below .25 min., while those on Problem I of this work go as low as .02 min. A curve constructed from data given by Watson's records[2] of the normal, blind, and anosmic animals on the maze, plotted

[1] The comparison of these curves should be supplemented by an examination of the curves obtained from the records of untrained animals on this problem. These curves are given on Plates VI and VII.

[2] Watson, *ibid.*, pp.19, 59 and 62.

on the scale here employed, is shown in Plate IV. Had the curves for the records of the rats on Problem I been plotted on the scale employed by Watson, the differences in the curves would have practically disappeared.

The curve showing the records of the normal rats is made up from the averages of four normal white males of one litter about one year old. That of the blind rats is made up of the averages of four blind males thirty-four weeks old. At no point do the time-records of the normal rats, trial by trial, go lower than that of the blind. At only two points on the curve do they go as low. The fact that they were younger and probably more active may partly account for the lower records made by the blind rats. But a comparison of the average of the blind animals with that of the nineteen, whose records go to make up the norm, shows the same low record for the blind.

Watson has formulated the conclusion that rats can learn the maze without the use of vision. The present writer has the temerity to suggest in the face of some later results that vision not only adds nothing of advantage, but may quite conceivably be detrimental to the rapidity of the learning of the maze. It has been shown that the maze may be learned almost absolutely in terms of kinæsthetic and organic impulses. Since these impulses alone are sufficient, visual impulses might be conceived of as adding a distraction.

The curve from the records of the anosmic rats is shown upon the same plate. The curve from this group runs slightly below that of the normal but above that of the blind. Here, as in the case of the blind rats, it is possible that olfactory impressions may be a stimulus to movements which in this problem are detrimental to the learning process.

E. GENERAL CONCLUSION BASED UPON RESULTS OF ABOVE TESTS.

The experiment, the results of which will be reported next, is one that does not require the formation of an association such as has been required in the three foregoing experiments. Its results, therefore, will not be considered in connection with those just presented. A summary of facts will be attempted here together with a discussion of their theoretical import.

PLATE IV

Showing the average time of normal and of defective rats in learning the Maze.

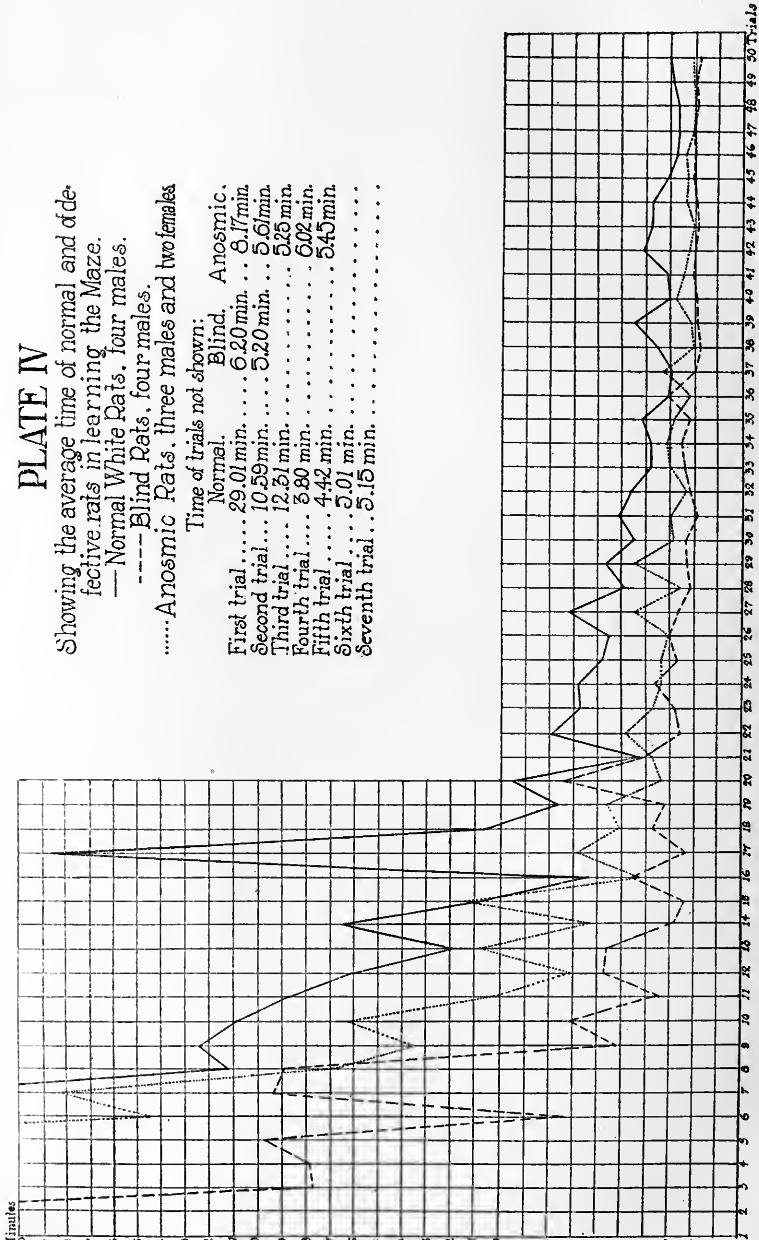
— Normal White Rats, four males.

---- Blind Rats, four males.

..... Anosmic Rats, three males and two females.

Time of trials not shown:

	Normal.	Blind.	Anosmic.
First trial	29.01 min.	6.20 min.	8.17 min.
Second trial	10.59 min.	5.20 min.	5.61 min.
Third trial	12.31 min.	....	5.25 min.
Fourth trial	3.80 min.	....	6.02 min.
Fifth trial	4.42 min.	....	5.45 min.
Sixth trial	5.01 min.	....	....
Seventh trial	5.15 min.	....	....



The aim of the investigation has been to determine upon what sensory impulses the rat mainly depends in forming the var ous associations required in these problems. The function of the different sense processes will be taken up in detail.

I *Vision.*[1]

a. Differences in Functional Value of Vision in Rats Possessing Pigmented and those Possessing Albino Eyes.

The evidence of greater importance of vision in rats possessing pigmented eyes is, upon the whole, equivocal. In Problem I (see p. 13) the black-and-white animals made phenomenally low records; not only is the average absolute time of the group much lower than that of any other group upon this problem, but the highest individual record of the black-and-white groups, is considerably lower than the lowest average record of any normal rat. The curve representing the average of the black-and-white animals is more regular, and the individual variability is less, in this group than in any other. These facts if taken alone, would seem to indicate that the black-and-white rats were at an advantage in Problem I. In the same problem, however, the two white rats, which had had previous experience in other problems, made still lower records, (p. 103).

In Problem II there was little difference in the average

[1] The term vision up to this point has been used in the most general way. At this juncture it seems necessary to qualify its significance and to indicate the different ways in which impulses from the eye may be rendered serviceable.

1. Possibly the most primitive function of such impulses is that of heightening the general tonicity of the motor area. This hypothetical tonic effect of light impulses is referred to more extensively later.

2. White light vision implies that an animal's reactions may be modified in accordance with the brightness of visual stimuli.

3. The term color-vision implies that the animal can react in a selective way to light stimuli of different wave lengths.

4. Form-and-size-vision would be said to be present if the animal were able to discriminate the form and size of the visual stimulus to which he reacts.

5. A further possibility is depth discrimination, which in the rat may or may not involve retinal factors. One would infer the presence of this form if the animal were able to react accurately to stimuli placed at varying distance intervals in the line of vision, provided that one were certain that no other form of sensory impulse were operative.

absolute time-records of these two groups (see p. 42), although five normal white individuals made average absolute time-records lower than the lowest individual of the black-and-white group. The time-records of the normal white group—as shown in series of ten trials each (p. 44)—are uniformly lower ten by ten—than the corresponding records of the black-and-white group, while the lowest individual records in such a series were made by those having albino eyes. The time-record of the white rats was lower for the first ten trials, and the difference between that of the first and second ten was in their favor. The curve is lower for the white rats to the 25th trial, and from that point there is no advantage accruing to either group.

These facts indicate that in this problem the rats with albino eyes made slightly better records than those with pigmented eyes.

In Problem III the black-and-white rats made a much better average absolute time-record, and their individual records were lower. (See p. 32.) The tables giving the average absolute time-records in groups of 5, show a much lower average for the black-and-white rats in the first and second series of five trials each, but in the third series, the white rats made not only a lower average record, but no black-and-white individual made so low a record as that of certain individual white rats.

In the results of experimentation reported later (p. 103) a comparison may be made between the time-records of four untrained white rats, and four untrained black-and-white rats on Problem III. The untrained black-and-white animals made a lower record for the first series of five, a higher rate for the second and third series, a lower for the fourth and fifth, and a higher for the sixth.[1] As has been shown in Part II,

[1]

	Untrained white rats.	*Untrained black-and-white rats.*
1– 5	11.12 min.	5.04 min.
6–10	.60	1.16
11–15	.21	.31
16–20	.18	.14
21–25	.10	.08
26–30	.06	.08
31–35		.05
36–40		.06

rats which have had experimental experience are much more apt in learning a new problem, presumably, largely because of less timidity during the experimentation. The black-and-white rats which made low records in Problem I were unusually tame at the beginning of the work, which probably accounts for their more rapid success. They were noticeably superior on Problems II and III.

It must be admitted after the consideration of the above data, that the evidence regarding the comparative functional value of vision in rats possessing pigmented and those which have albino eyes is not decisive.

b. Effect of Loss of Vision.

The following discussion must, in the nature of the case, deal with the effect of the *loss* of vision rather than with its explicit function when present in the normal animal. The entire series, as remarked above, afforded no opportunity for determining the exclusive function of vision. The change in conditions in the tests on Problems II and III illustrate this point: the animals did not seem to rely upon visual, but rather upon tactual and kinæsthetic stimuli, yet the blind rats were at a disadvantage as compared with the normal animals.

The least apparent difference between the blind and the normal white rats is in Problem I. The records of the normal rats, as a group, are better; the absolute average time for the white group is lower (see p. 21), although the difference between these averages of the two groups is less than the individual variations among the normal rats. The average of the absolute time for the poorest two normal males and two females (Males II, III and Females I and V, averaging .41 min.) compared with that of the best two blind males and two females (Males III, IV and Females III and V, averaging .27 min.) proves this statement conclusively. In fact, if the poorest two records of the nine blind rats be rejected, the average for the remaining seven animals is .33 min., which compares most favorably with the group average, .34 min., of the normal rats.[1]

[1] The extremely high records of the two blind animals, Male I and Female I, are responsible for the higher group average of the blind rats.

As regards the *rate* of learning, the normal rats were superior (see p. 21). The *first two* successes of the blind rats were accomplished more quickly (see Plate I) than those of the normal, though from the fifth trial to the end of the series the curves representing the normal rats is, for the greater portion of its length, below that of the blind animals.

In Problem II it may be questioned whether the blind rats formed the necessary association for the solution. But two animals, Females IV and V (see p. 42), so reduced their time-records that their individual curves approach the contour of a learning curve. The records, averaged in groups of ten, show that there is not a sufficient reduction in the time-records to warrant—on the basis of time consumed—the assumption that these rats were successful in this problem. The behavior of several of the animals at the end of the series warranted the statement that no definite path was chosen The greater variability of the group renders its average absolute time useless as a basis of comparison.[1]

A comparison of the graphs showing the average time-records of the various groups (see Plate II) suggests the doubtful justification of considering the curve of the blind animals as a learning curve at all. It must be recalled here, however, that two blind rats did arrive at the solution, though somewhat more slowly than any normal rat.

In Problem III, likewise, there is a very considerable difference in the records of the two groups. The blind rats learned to solve the problem, though the absolute time is higher throughout the series, and the rate of learning much slower (see p. 59). The individual variations as to time-records is not nearly so evident here as among these individuals on Problem II.

The differences in the results obtained in tests with blind animals, and with those which possess vision, vary with the nature of the experiment to which the two groups are subjected. Watson found that vision could be dispensed with in the learning of the maze without perceptible loss to the process. In Problem I of this series there was but little advantage

[1] Compare the average record of Blind Female II for the first ten trials with that of any other normal or defective rat.

accruing to the normal animals; in Problem III there was considerable, and in Problem II the blind animals were at a decided disadvantage. In the face of these facts it seems possible that the loss of vision is disadvantageous to the solution of these problems in proportion as the problems demand movement which, to be effective, must be definitely controlled as to the exact locality in which it is to be put forth. In the maze, the activity of the animal is definitely confined by the maze itself; namely, narrow alleys which the rat must traverse. Problem I approximates the labyrinth type in that the animal, during the solution of the problem, is somewhat restricted in its movements by the nature of the apparatus. Problem III demands movements performed within a definite area, movements unrestricted save by the motor tendencies of the animal. Problem II demands a similarly specialized movement with the added complication that the area in which the movement must be performed is at a distance from the food stimulus. Assuming that the above problems represent, in the order I, III and II a series of increasing specializations of adaptive reactions, it would seem that the loss of vision becomes more disadvantageous throughout the series.

The above tests are not of such a character as to afford unequivocal evidence concerning the possible function of vision.

The eye as previously stated affords impulses to the motor center which are presumably tonic in character. The motor impulses which are to result in general bodily movement are always conditioned by the sum of tendencies operative in the motor area. If the tonic condition of this area is low, as might be the case in blind animals, it might well happen that the requisite association would be slow in forming. It is possible in this way to account for the fact that although in their behavior these animals gave no evidence as to the function of vision the rats that through blindness may have had an insufficient energy surplus of the kind called for in these coördinations were slow in learning or failed to learn in so far as the problems demanded well concatenated activities.

2. *Olfaction.*

The group of anosmic rats made a higher average absolute time-record for Problem I (see p. 21), and the rate of learning was comparatively poor for the group as a whole. Two individuals, Males III and IV made records that were lower—in series of ten trials each—than the average of the normal group.[1] The average time for the first ten trials is particularly low for Males II and III.[2] The individual variation is high.

On Problem II the time-records of the one anosmic rat are valueless as a basis of comparison. He learned the problem, but did not solve it as the other animals had done (see p. 34).

Problem III was also learned in an eccentric fashion, which vitiated the time-records, although the association was well-formed and at the rate of the normal rat (see p. 52).

In the learning of the above problem it is not necessary for the rat to establish and follow a pathway on the basis of olfactory impressions. Such impressions may accelerate or retard the learning process; accelerate when the odor is a part of the stimulus connected with the problem box, e. g., when the stimulus releases movements which may result in the successful manipulation of the apparatus, such as clawing or biting at a latch; otherwise disadvantageous, resulting in the dispersal of 'attention,' as when the rat spends time in smelling the control cage; or, as in Problem II, in sniffing at the food, when his movements to be successful, must be performed elsewhere. This is the probable explanation of the fact that the time-records of the anosmic rats are frequently lower than the corresponding records of normal animals.

3. *Touch.*

The impulses furnished by the sense of touch seem to play an important part in the adaptation of the animal to these sev-

[1] Eliminating the record of the first ten trials of Male III, which was high because of a very long time consumed in his first success, the average of the group for the trials from 1–10, .84 min., is much lower than that of the normal rats.

[2] Compare these records with that of the blind anosmic rat, given on p. 106.

eral experimental situations. A very great difficulty arises when an attempt is made to separate the functions of the tactual from that of the kinæsthetic and organic. The tactual impulses alone, or in the complex, are the stimuli to the digging movements in Problem I, and in part to the movements of pressing down the plane in Problem II. In Problem III, certain familiar tactual impressions are evidently the stimuli to the discovery of the latch after the rat has arrived at the locality of the door. Contact *seems* also to be the cue to movements which result in raising of the latch, during both the learning process and the period in which the movements are habitual.

4. *Kinæsthetic and Allied Impressions.*

The rôle of kinæsthetic impulses in the early processes of learning probably varies in inverse proportion to the degree in which the movements must be adapted to a definite locality. Later in the process, as the movement becomes more or less automatic, the kinæsthetic and allied impulses seem to assume first importance as the means of control. In the maze such sensory impressions are sufficient. In Problem I no others were *indispensable* to a rapid establishment of the requisite associations. In Problem III, it was evident that vision could be profitably dispensed with in proportion as the reactions became automatic. In Problem II these impressions resulting from muscular activity were not only essential in the following of the pathway, but seemed also to be of service in giving the cue for the excursion to the door of the food box after the plane had been successfully lowered.

F. PROBLEM IV.

1. *Description of Apparatus and of Method of Teaching Rats to Jump.*

The three foregoing problems have been solved—though with varying degrees of success by the blind rats—by means of an evident reliance mainly upon kinæsthetic and tactual stimuli. The necessity remained to devise a problem in which at first sight it would seem that vision *must* be the only, or at least the esssential, means of control.

Dr. Carr, when working with rats on the maze, used one rat which jumped from his hand to the table, although the next day the rat jumped in the direction of the maze but struck the floor. Two black-and-white rats which worked upon Problem I, in their anxiety to get food, acquired the habit of jumping from the experimenter's hand into the open door of the cage, a distance often of six or eight inches. These observations suggested the construction of a piece of apparatus which would necessitate *jumping* as a means of obtaining food. It would seem that in such an activity vision would be essential for successful coördination.

It is conceivable, however, that the stimulus which leads the animals to jump from one platform to the other may come through one or more of three pathways: (1) visual, (2) olfactory, (3) tactual and kinæsthetic. Factors 1 and 2 alone would be adequate to control the direction and distance of the first jump. Factor 3 might cause jumping to occur, but only after some experience of consequence would it serve to control the distance and direction of the leap. Under the conditions of the experiments here considered, it was possible to eliminate contact as a means of sensory control by keeping the tactual conditions constant throughout all the experiments. The attempt to eliminate the kinæsthetic factor was made by varying irregularly the distance between the two platforms. Smell was eliminated by the use of anosmic animals, by keeping the apparatus clean, and by control experiments upon the normal animals in which no food was given until after the jump had been taken. It was thought that the rôle of vision, the remaining factor, could be determined by comparing the behavior of the normal animals with that of the blind. In view of the fact, however, that the normal animals were so deficient in the ability to control their movements when the distance between the platforms was altered, the experiments are not decisive as regards determining the nature of the rôle which vision plays.

The factors involved in this coördination are so delicate and so complex that the time allotted to it was not sufficient to answer all the questions which arose. Indeed, the experimenter feels that many of the questions which are here tentatively answered must be supported by a much larger group of facts before they should have anything like scientific assent.

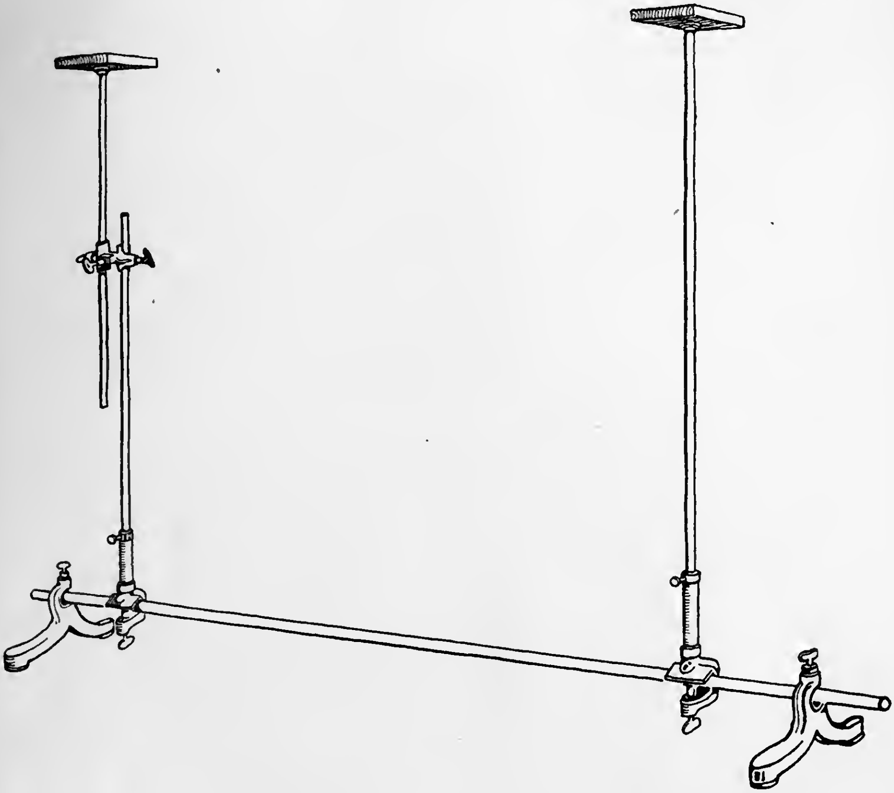


FIGURE 5. SHOWING THE MODIFIED FORM OF THE APPARATUS.

The first apparatus used in this experiment consisted of two 2 inch by 2 inch wooden uprights 20 inches high, each surmounted by a 5 inch by 5 inch platform of 1 inch board. The uprights were attached below to a 4 foot length of 2 inch by 2 inch timber, supported by wooden legs. One of the uprights was fixed, the other movable. The distance between the platforms[1] might be varied from 0 to 40 inches. The entire apparatus was painted white.

The first group of rats consisted of four normal white males. Their exact ages were unknown, but were estimated about 180 days. They had been used previously for three weeks in tests upon the maze, and were accustomed to being handled. Since time-records were not sought here, but information about the delicacy of functioning of such visual-motor adjustments as would be required in jumping coördinations, the age and the training of the rats were not matters of concern.

These animals had learned to jump from the experimenter's hand to the open door of the cage, and had accomplished these feats at varying distances up to 12 inches. When they were put to work upon the apparatus, they had acquired the coördinations for short distances. These coördinations are not common to rats held in captivity. They do make short leaps in springing to and from the wire sides of their cages, but any such long jumps as they had to accomplish in these tests are entirely foreign to their usual habits. In the majority of cases the difficulty and slowness of the training was distressing to the experimenter, though in several instances the ease with which the jumping coördinations were acquired was surprising.

Most of the animals were emotionally disturbed by the conditions of the experiments; in three cases a fall so frightened the animals that they refused for a time to react in later tests.

The method of teaching the rats to jump was ordinarily laborious. The apparatus was placed in the middle of the floor in such a position that the rats were forced to jump toward the east. The platforms were placed at a distance of four inches apart. The rats were coaxed across with a morsel of food. Platform II was within reach of the animal's nose and the step across was usually taken without hesitation. After each successful effort the animal was allowed to eat a trifling amount of food. When the rats had become accustomed to stepping across, the distance between the platforms was gradually increased one inch at a time. Up to a certain

[1] The platform upon which the food was placed and *to* which the rat jumps will be designated as Platform II: the one *from* which it jumps as Platform I.

distance the rat was able to step across with little difficulty, and contact of the snout or vibrissæ with Platform II seemed to be the essential stimulus in the majority of cases.

The difficulties began when the distance was increased until Platform II was out of the reach of the rat's snout or vibrissæ. Here a double complication arose: (1) The old contact stimulus was lacking; and (2) there was the necessity for making a springing movement, in which at one instant, all four feet are without support. Several rats had no difficulty at this point; some had great difficulty; but eventually they learned to make the muscular adjustment required for the leap. Three defective rats—after many hours of coaxing—utterly failed to make the coördination. The following notes show in detail the typical behavior of a normal rat while learning to jump.

Diary Notes on Behavior of Normal White Rat III (Female) in Learning to Jump.

	INCHES.	TRIALS.	
11/30/07			Fed on platform. Small, frail, but active rat.
12/1/07	4	5	All good.
	5	5	All good.
	6	5	All good.
12/2/07	5	5	All good.
	6	5	All good.
	7	5	On first trial she scrambled slightly;[1] other trials good.
	8	5	First trial, struck slightly toward north side of platform, second trial, on south side of platform, other trials good.
	9	5	All good.
	10	5	Scrambled on first trial, others perfect.
	11	5	Scrambled on first trial.
12/3/07	8	1	Went clear over platform.
	9	5	Perfect.
	10	5	First trial a slight scramble, others perfect.
	11	5	Similar behavior.

[1] The phrase 'scrambled slightly' is descriptive of those trials in which the rat landed with the hind feet off the upper surface and against the side of the platform. The word 'scrambled' indicates that the rat landed with the fore feet, but not the hind feet on the platform, and scrambled on. If the rat could not climb on *easily*, the result was noted as 'short.'

	INCHES.	TRIALS.	
12/4/07	9	1	Perfect.
	10	5	Perfect.
	11	5	Perfect.
	12	5	Perfect.
	13	5	First and fourth trials scrambled, three perfect.
	14	5	Scrambled on second trial, others perfect.
12/5/07	11	5	Perfect.
	12	3	First trial landed with heels on edge, but struck squarely.
	14	4	First and second trials, scrambled slightly.
	15	4	First trial short, others perfect.
12/6/07	10	1	Went clear over.
	12	1	Struck squarely but with great force.
	14	1	Perfect.
	15	1	Perfect.
	16	1	Perfect.
	18	3	First trial scrambled, others perfect.
	20	4	As above.
	22	6	Second trial, landed south of platform.
			Third trial, struck north of platform. In these long jumps she landed with such force that she was almost breathless for several seconds afterward.
12/7/07	16	1	Would have slid off east side platform if experimenter had not caught her.
	18	1	Same procedure as above.
	20	1	Again the same.

This rat was not tested at distances greater than 22 inches. Rats I and II, which were larger animals, had learned to make longer jumps. Their records for trials greater than this distance are given below.

Rat II.

	INCHES	TRIALS.	
8/20/07	22	10	First and sixth trials, a little short. On other trials he slid across platform and nearly went off.
8/21/07	24	10	First, short; third, fourth and eighth scrambled. (His foot was sore.) Second trial overshot; fourth, to north side.
8/21/07	24	10	First, short; second, overshot; third and fourth, to north side of platform; eighth, struck squarely, but had too much momentum and slid off.
	26	5	First, scrambled; second, slightly long; fifth short.
	28	5	Third, short; fifth, overshot. (Foot was sore, discontinued tests for the present.)

Rat III.

8/23/07	22	10	Third and sixth trials, scrambled.
	24	10	First, short; second, scrambled; third, low.
8/24/07	26	10	Second trial, struck north side of platform; fourth, slightly short and to south; ninth, overshot.
	28	10	Second trial, slightly long; fourth, scrambled; others perfect.
	30	10	Second, fourth and sixth trials, low; rat had begun jumping downwards. Was apparently not aiming at platform.

The notes mention several characteristic features of the learning process: The 'scramble' on the first trials for lengthened distances; the over-innervation for shortened distances; and the frequent compensations for errors, as when the rat landed on the south side of the platform on one trial, it struck upon the north side on the next trial.[1] This characteristic is referred to in the discussion of a later test.

[1] The experimenter attempted to devise some means whereby an objective measurement of the rat's coördination could be taken. If the records could have been obtained of the exact point at which the fore feet first came in contact with the plaform, a curve could be plotted showing the accuracy of the adjustment. A cloth, marked in black and white squares 1 cm. in size, was carefully tacked over the top of Platform II. The experimenter endeavored to note the lines

2. *Jumping in Constant Direction, i. e., Apparatus in East-West Position.*

1. Statement of Results.

a. On Normal White Rats.

The results of the tests on the four normal white rats has been sufficiently discussed in the description of the learning process in the foregoing paragraphs. Each rat learned to jump the distances up to and including 22 inches. One had jumped 28 inches with considerable accuracy: one other had made eight perfect coördinations out of a possible ten at this distance, and seven out of a possible ten at 30 inches.

Three other rats learned to jump. One of them, a small male, learned to jump a distance of 22 inches in eleven days, but was slow for several days thereafter. A second, the best of the entire group at the first trials, learned in two days to jump 15 inches, then began to hesitate and finally refused to take such long distances. A third rat easily attained a distance of 12 inches, after five days training, but the later learning process was retarded by emotional factors, the results evidently of a fall on the third day of the tests. Eventually after five weeks of constant training, he jumped 22 inches, but with an average of only 50 per cent of accurate adjustments.

b. On Normal Black-and-White Rats.

Three female black-and-white rats were employed in the experimentation. Two of them were animals which had been used in the series of previous problems. Each did exceptionally well, both in learning to jump, and in the accommodation to changed conditions of the experiment. The third rat was the mother of the above two, an extraordinarily energetic animal, and one without fear. Her records on this problem—the only one she attempted—are little short of phenomenal. She was placed on Platform I at a distance of five inches from the food

upon which the rat alighted, but the movements were so quick, and the rat so often slid along by reason of his momentum, that the attempt was a failure. The use of smoked paper was likewise out of the question, as the resulting imprint was only a large erasure of the lampblack.

platform. She stepped across at once. The distance was increased to 6 inches and she did not hesitate. She jumped sixty times within an hour on her first day with but one error; these trials included five at 14 inches. Her complete record is given below.

Black-and-White Rat Number III.

	INCHES.	TRIALS.	
11/14/07	5	5	First day. Had never been placed upon platform before. Stepped across immediately.
	6	10	All trials perfect; jumped across at once.
	7	10	Perfect. The most active rat we ever knew.
	8	10	Perfect.
	9	5	Struck platform squarely every trial and always jumped immediately when she was returned to Platform 1.
	10	5	All trials perfect.
	11	5	All trials perfect.
	12	5	All trials perfect.
	13	5	All trials perfect.
	14	5	First trial, struck left side platform, all others squarely. Sixty trials first day!
11/15/07	10	1	Went clear over.
	12	1	Struck and slid off east.
	10	5	First trial, overshot; second, scrambled, others perfect.
	11	5	Good; last trial, struck and slid off.
	12	5	First two, a little short, others perfect.
	13	5	As above. (Not so active. Muscular soreness from unusual exertion of yesterday?)
11/16/07	12	5	Three perfect, two scrambled.
	14	5	First trial, short and fell.
	15	5	First and second trials, a little short.
11/17/07	12	5	All good.
	14	5	First trial, a little short; second, scrambled.
	15	5	Same as above.
11/18/07	14	5	Four perfect.
	15	5	First and second a little short.
	16	5	Good.
	17	5	Perfect.
	18	5	First trial did not strike exactly in center.
	19	5	First trial, short and fell.
	20	5	A little short first trial, others perfect.
	21	5	As above.
	22	5	All perfect.
11/19/07	18	5	All good.
	20	5	Four trials perfect.
	22	5	Perfect.

Black-and-White Rat I.

	INCHES	TRIALS.	
8/26/07	8	8	Jumped almost at once but scrambled; second and third good; fourth, seventh and eighth, scrambled; all others good.
8/27/07	8	6	All good.
	9	5	Second, scrambled; all others good.
	10	10	First, scrambled; all others good.
	12	10	Second and ninth, scrambled; all others good.
	14	10	First and tenth, short and fell; third scrambled; others good.
8/28/07	8	2	Jumped entirely over both trials.
	12	10	Second and eighth scrambled; others good.
	14	6	First and third, scrambled; others good.
	10	10	Second and third, scrambled; others good.
	18	9	Third, fifth and sixth, scrambled; ninth, short and fell; others good. Was breathless and seemed tired.
8/29/07	12	2	Both jumps too long, went over platform.
	16	5	First, struck on north of platform and scrambled; distance good but direction faulty; third, scrambled; others good.
	18	10	First, too far north; second, third and sixth, scrambled slightly; ninth, toward north.
	20	10	Fifth, scrambled slightly; others good.
8/30/07	20	10	Sixth, scrambled slightly; ninth, short; tenth, good.
	22	10	First and third, short; seventh and ninth scrambled.
8/31/07	20	7	Second, scrambled; fifth and seventh, short and fell.
9/ 1/07	18	5	First and third, scrambled slightly.
	20	10	First, second, seventh and ninth, scrambled.
	22	10	First, scrambled; third, fell on south.

Black-and-White Rat II.

8/27/07	8	0	Would not jump.
	5	5	Stepped over easily.
	6	5	Hopped across.
	8	5	Hopped across.
	9	10	First, a little short; third, scrambled.
	10	10	First, scrambled.
	12	10	On seventh trial, fell off platform in preparing to jump. All other trials good.
	14	1	Short and fell. Tired.

	INCHES	TRIALS.	
8/28/07	10	0	Would not attempt it.
	6	5	*Very slow in starting.* All trials good.
	8	5	All good.
	10	5	All good.
	12	5	All good.
	14	4	First, scrambled; others good.
8/29/07	12	0	Would not attempt it. Waited 15 min.
	10	0	Would not attempt it. Waited 15 min.
	8	0	Would not attempt it. Waited 15 min.
	5	5	Seems much afraid and needs a great deal of coaxing.
	7	5	Better; all trials good.
	14	5	All good.
	16	5	All good.
	18	5	All good.
	20	2	All good.
8/30/07	16	10	*Very slow.* First trial, scrambled; tenth, short and fell.
8/31/07	16	0	Would not attempt to jump.
	14	5	Timid. First, too long and fell; others good.
	16	10	First, scrambled; fifth, fell off the south side of Platform II.
	18	5	First, second, third and fifth trials, scrambled.

Work with this rat was here suspended during an alteration of the apparatus. The remainder of the learning process was like that of the other normal rats at these distances, and is not quoted further.

c. On Blind Rats.

The experimenter attempted to train four blind rats. The animals were active, and one of them had had experience under experimental conditions. The method was the same as in the tests with the normal rats, though the procedure was *much slower*.

The rats were fed for several days upon Platform II, which was east of and 2 inches distant from Platform I. The animals were always placed on Platform I, facing the east, and after they had stepped across they were carefully lifted back and set down facing the east, upon Platform I. The normal rats acquired their own orientation relative to Platform II; the blind animals always adjusted themselves for the jump in the position in which they had been set down upon the platform. With these blind rats it was necessary to make the increase by shorter gradations, one-fourth or one-half of an inch. Two of the rats would not attempt to cross a space wider than they could reach with their vibrissæ. The notes on the behavior of one of these is given below, beginning with the distance of four inches.[1] The notes taken on less distances contribute nothing. The number of trials is not always given, as they had not been counted at such short distances.

Blind Black-and-White Rat on Problem IV.

	INCHES	TRIALS.	
10/4/07	4	10	Stepped across to food platform, always from the southeast corner.
	4½		Stepped across many times.
	5		As above.
	5¼	10	Was obliged to spring a little; always from the southeast corner.
10/5/07	5		Would not try; failure. Was obliged to lessen distance.
	4½	4	Stepped across three times after much coaxing by holding food in front of him.
10/6/07	5		After thirty minutes he stepped across on his own initative from the south-east. Could not be coaxed across. Time, forty-five minutes.
10/7/07	4¼	10	Stepped across from southeast corner.
	5		Failure. Will only reach or spring as far as vibrissæ can touch.

The procedure as noted above was repeated for several days with little variation and no satisfactory results.[2]

A blind rat which also had had previous experience was

[1] This rat had successfully solved the previous problems.

[2] This rat would not allow his fore feet to leave the platform unless his vibrissæ reported contact with some object. When the platform was beyond the reach of his vibrissæ the experimenter touched their tips with a pencil, whereupon he put out his fore feet to step over. He never raised his hind feet until his fore feet had a firm footing, but he could always be induced to make an attempt by stimulating his vibrissæ. A deodorized glass rod was used instead of the pencil and it had the same effect, showing that it was contact alone, and not olfaction that tempted him to make the effort.

labored with for many days, with not so good results.[1] He would not step across when the platform was within reach of his nose. He was a slow rat at best and achieved no credit for himself in the previous experiments.

Blind Rats III and IV (white untrained females) achieved signal success in this test. Both learned eventually to jump distances of eleven inches, and Rat III successfully cleared fifteen inches. A portion of the notes on the behavior of this rat is given here as they are of particular interest.

Blind Rat III

	INCHES	TRIALS.	
11/19/00			Began the experimentation with the platforms two inches apart. Coaxed her across with a morsel of food. She used vibrissæ to locate the platform. Distance gradually lengthened to four inches. This was the daily program for ten days.
11/29/07	4½	10	Stepped across. An active rat.
	5	5	Hopped across. (Had never been able to get a blind rat to 'hop' before.)
	5½	5	Hopped across. Never turns around. (When returning blind rats to Platform I they were always placed with head toward food platform. They rarely altered this position.)
	6	5	Struck platform squarely.
11/30/07	4½		Would not hop across; obliged to reduce distance to four inches and increased one-half inch at a time. Would not cross after five inches.
12/1/07	4½		Good.
	5	5	Stepped across at five inches. Very slow.
	5½	5	Hopped after stretching across.
	6½	5	Sprang across.
	7½	5	Good.
	8½	5	First trial, heels on angle, others perfect.
	9½	5	Same procedure as above.
	10½	5	First trial, scrambled slightly.
	11½		Getting tired and slow. Scrambled in two trials and in fourth trial did not aim right; struck wall at northeast; fell hard but it did not frighten her. Commenced eating at once when placed on food platform.

[1] This rat was Male I whose records on Problems II and III were disregarded in the average of the groups.

	INCHES	TRIALS.	
12/3/07	$5\frac{3}{4}$		Would not hop across and could not step across.
	5		Would not step across.
	4		Stepped across.
	5		As above.
	$5\frac{3}{4}$	4	Coaxed across first trial. Hopped across in other trials.
	$6\frac{3}{4}$	2	Went entirely over and struck wall.
	$7\frac{1}{2}$	4	Went over platform and fell first trial, second, the same, third, went to east side, and just saved herself from falling. Fourth trial, perfect.
	$8\frac{1}{2}$	5	All perfect. (For four days succeeding above there was the same procedure every day. At the beginning of each daily experiment, experimenter was obliged to reduce distance to four inches; the animals seemed to carry over nothing of advantage from one day's experience to the next. Each day learned anew to step across and later to jump to platform.)
12/7/07	7	2	Loitered about for a long time then jumped nearly across platform.
	9	2	First trial, perfect. Second, off at north.
	11	2	Perfect.
	13	4	First trial, scrambled up over edge. Second, fell.
	15	4	Did not strike the platform squarely. Scrambled each time on to the platform.
12/8/07	7	10	Jumped over platform to wall of canvass control cage. Does not jump to platform but jumps aimlessly. Eighth and ninth trials, struck wall at distance of twenty seven inches.
12/9/07	12		Jumped across to wall six times; distance twenty inches. Changed distance of platform but would not jump toward it. After dozens of trials the experimenter gave up in despair.

Rat IV had learned to strike the platform squarely at a distance of eleven inches. At this stage of her training she discovered that she could crawl down the standard. Sharp points were placed about the edge of the platform to prevent her descent, whereupon she jumped directly to the floor below. Further experimentation was futile.

d. On Anosmic Rats.

To determine accurately that vision and not olfaction furnished the sensory control of the adjustment, two anosmic rats were tested upon the apparatus.[1]

The method of training of this animal was the same as that with the blind rats—the distance being increased by half an inch at a time. The following are extracts from the notes taken on his behavior:

Anosmic Rat I on Problem IV.

	INCHES	
12/3/07	5	Steps across many times but awkward and afraid.
	$5\frac{1}{4}$	Has to be coaxed across; slow and evidently much disturbed by fear.
	$5\frac{1}{2}$	An entire failure after thirty minutes of coaxing.
12/13/07		After ten days of daily experimentation has made no progress. Was stiff with fright much of the time when urged to take a distance greater than he could step across. For several days he has been gnawing fiercely at the sides of Platform I and has rounded off the edges and corners.
12/14/07		Failure!

A second anosmic rat was procured for the test. He was hurried through the series with a fewer number of trials at each distance because of the experimenter's apprehension concerning the length of his tenure of life. He was in excellent physical condition but had he died there would have been no possibility of procuring another anosmic rat in time for the experiment. On the first day he succeeded in convincing the experimenter that the olfactory stimulus was not the essential factor in the jumping reaction. The notes quoted below give the details of his record.

[1] The first animal was the one which had formed the associations involved in Problems I, II and III, though his time records in the last two problems were practically of no value because of the time he wasted in gnawing the apparatus.

Anosmic Rat II on Problem IV.

	INCHES	TRIALS.	
12/6/07	5½	1	First time upon platform. Stepped across immediately.
	6	1	Jumped readily to Platform II.
	8	2	Perfect. No hesitation.
	9	3	First trial, scrambled; others perfect.
	10	3	Exact repetition of previous trial.
	12	3	Same as above.
	13	3	Scrambled, tired. (All of the above trials within ten minutes.)
12/7/07	7	2	Jumped across.
	9	2	Perfect.
	11	4	First trial, scrambled; others perfect.
	13	3	Third trial, scrambled a little.
	15	3	First trial, scrambled; others perfect. Very active.
12/9/07	7	2	Did not jump readily at first, finally coaxed across. Struck squarely.
	9	2	Perfect.
	11	3	First trial, scrambled; others good.
	13	3	Perfect.
	15		Short, and fell twice; afraid, put him up.
12/12/07	6	1	Would not jump at first.
	8	1	Same as above.
	14	4	First and second trials, scrambled; other trials perfect.
	15	5	As above. (Is lame in left hind leg.)
12/15/07	16	5	Slow. First trial, struck on south side. Second, same but nearer center.
	18	5	First and third trials, scrambled slightly.
	20	3	Second trial, struck platform, but fell off.
	22	7	Fourth trial, scrambled; sixth, little short and fell. Tired.

The behavior of this rat in the above test was in every respect like that of the normal animals. He had had previous experience on Problem I, and was apparently undisturbed emotionally by the conditions of the experiment. He learned to jump his maximum distance in a shorter time than did any other white rat, though to what extent his facility was due to fearlessness and to the fact that the experimenter lost no time in lengthening his distances cannot be estimated.

ii. Summary.

1. Five normal white rats, the three normal black-and-white rats, and one anosmic animal were able to learn to jump successfully a distance of at least 22 inches. These adjustments were acquired with comparative ease. One other normal white rat learned to jump as long a distance as 22 inches with difficulty, and another did not learn to jump more than 15 inches. No normal rat failed to learn to jump. Two of the blind rats (III and IV) achieved success in this test. Both learned to jump a distance of 11 inches. One (Rat III) learned to jump a distance of 15 inches. Here the coördination broke down apparently on account of the fact that a large percentage of her jumps were inaccurate; she had to scramble onto the platform much of the time, and she often failed utterly to strike it and consequently fell. Rat IV learned to jump a distance of 11 inches, but the coördination broke down upon her discovery that she could crawl down the standard.

2. One anosmic and two blind rats were utter failures. Two were willing to step across to the second platform, but they were either unable, or else refused, to jump. The failure of the anosmic rat was probably due to the fright occasioned by the unusualness of the conditions of the experiment and not to any lack of proper sensory control. Under any other circumstances he ran about naturally in search of food. The blind animals did not seem to be emotionally disturbed, and hence their failure was probably referable to a lack of adequate stimulus.

3. *Effect of Changing Direction in which Jump Must be Taken.*

In order to determine more accurately the sensory factors involved in the coördinaton it was decided to change the position of the apparatus and thereby the direction in which the animal has to jump. It would seem that if the rats can accommodate *at once* to changes in the direction of Platform II, some distance receptor must be operative. Such a test might also show the possible presence of some 'directional' factor which is not visual in character. Three white rats had been trained to jump distances gradually increasing from 6 to 30 inches. These longer distances, as has been noted, were too great to permit of accurate adjustment on the part of the rat, and they demanded an unnecessary expenditure of energy. Accordingly, a record of 80 per cent of perfect coördinations at 22 inches was chosen as a standard of efficiency to be attained before the animals should be tested with the apparatus turned in another direction. Three white rats had reached this degree of capability.

The apparatus was then so adjusted that the rat must jump 22 inches to the south for food. To the surprise of the observers, two of the rats continued to jump toward the east for twenty successive trials each. The third rat jumped twice toward the south, though he did not jump far enough to land on the platform; at the third trial he settled down comfortably on the starting platform and refused to jump.

Acting on the possibility that the two rats were jumping toward their cages—which were to the east—or reacting to other features in the environment of a visual or olfactory character, the conditions of the experiment were radically changed.

A cabinet 4 feet by 4 feet, by 6 feet was built. The framework was of 2 by 4 inch timber, the sides and top of white canvas. The cabinet was illuminated by a 32 c.p. electric light fastened to the center of the top of the cabinet. The visual and olfactory conditions of the environment were thus rendered subject to control. At this time the apparatus itself was improved. The connecting rod was made of 1 inch pipe, clamped in iron end supports. The uprights supporting the platforms were of ½ inch steel, clamped at right angles to the base. One of the uprights consisted of two 18 inch steel bars clamped together so that the height of platform it bore might be varied from 18 to 30 inches. The apparatus in this form was much more easily adjusted to horizontal changes in distance. It also possessed the added advantage of offering any possible adjustment in height.

While working with the wooden platform the feet of the animal became sore. In making such leaps as are necessary in these tests—covering sometimes a distance of 24 inches—the rat lands heavily upon the forefeet. This might have been the cause of the soreness. The platforms were later covered with cork matting, and this in turn with soft leather. The whole was then painted light gray. Though the paint added somewhat to the resistance of the surface the rats had little difficulty thereafter with soreness of the feet.

After the cabinet had been constructed, the rats which had learned to jump on the old apparatus were tested in the new one. There had been an interval of three weeks since their last trials and several days' training was necessary to bring them up to their former standard of accomplishment. While training the rats the experimenter remained within the cabinet to catch them when they fell and to feed them immediately after they reached the platform. After the habit had been reëstablished and it was desired to test the animal with the apparatus in a new position, the rat was observed from without through a slit in the canvas. The position of the observer outside the cage was also changed in every test, in order that the rat might not associate the sound of the experimenter's movements with the direction in which the jump must be taken.

The possibility of an olfactory stimulus was here minimized by allowing no food in the cabinet. The rat was fed from the experimenter's hand after the jump, and Platform II was kept clean, and newly covered, top and sides, with black-and-white checkered cloth—to add greater character to the visual stimulus.

Each day before the apparatus was turned the rat was given five or more tests in jumping toward the east which during the previous training had been the constant direction. If 80 per cent of the trials were perfect, the cabinet and apparatus were rotated. This change necessitated jumping to the south, the north, or the west, as the alteration might demand, in order to reach Platform II. Care was taken to place the rat on Platform I in different positions during the various trials, so that the initial position would be no cue to the essential orientation.

i. Statement of Results.

a. On Normal White Rats.

The records of the normal white rats in this test are given below.

White Female I.

	POSITION OF PLATFORM II.	
12/9/07	East 22 in.	Ten trials, 80 per cent perfect.
	South.	Perfect.
	West.	Refused to jump.
	North.	Refused to jump.
	West.	Jumped south.
	East.	Refused to jump.
12/10/07	East.	Eight trials, all good.
	North.	Went entirely over platform to north, and struck canvas.
	West.	Refused to jump.
	South.	Fell off platform in preparing to jump toward south, and was frightened. No other tests today.
12/11/07	East.	Five trials, perfect.
	West.	Perfect.
	South.	Jumped east.
	North.	Perfect.
	West.	Jumped south.
12/12/07	North.[1]	Jumped east.
	West.	Jumped south.
12/13/07	South.	Jumped east.
12/14/07	South.	Jumped east.
	North.	Slow and confused. Will not jump.
12/15/07	West.	Refused to jump, apparently much confused.
12/16/07	East.	Same as above.

[1] Platform II was not placed at the east for the first trial, as the animal exhibited a tendency to jump east habitually, and it was feared that this position might unduly emphasize the tendency.

White Male II.

	POSITION OF PLATFORM II.	
12/6/07	East 24 in.	Ten trials, 80 per cent perfect.
	South, 22 in.	One trial, immediate and perfect accommodation.
	West, 24 in.	One trial, perfect, jumped at once.
	North, 24 in.	Direction perfect, but jumped too short.
	East, 24 in.	One trial, perfect.
		Apparatus turned but not cabinet.
	North, 24 in.	One trial, perfect.
	North, 24 in.	One trial, perfect.
	East, 24 in.	One trial, went slightly to right of platform, grazing the side.
	West, 24 in.	One trial, perfect.
12/7/07	East, 24 in.	Ten trials, and but 30 per cent perfect. Jumped down toward bottom of apparatus. Did not try to jump to platform.

White Male III.

12/6/07	East, 24 in.	Five trials, scrambled slightly each time. Animal is ill and weak.
	North, 24 in.	One trial, right direction but short.
	South, 24 in.	One trial, same as above. Did not work again and died soon after.

Two of the three rats jumped to the platform in the new position at every test on the first trial. The third rat, Female I, jumped to the platform which was toward the south on the first trial, but on the second, third, and fifth trials she refused to jump. On the fourth she jumped south again when she should have jumped east. On the first trial for the second day with platform north she jumped to it at once, then refused to jump the next time. On the first trial on the third day the adjustments were perfect, though on two of the later trials she jumped in the wrong direction. On the fourth, fifth and sixth days she made no perfect coördinations, either jumping to the east, with one exception, or refusing to jump at all. The tests had to be discontinued because of her disinclination to leave Platform I. She would jump toward the east with the platform in that direction but not otherwise. In the case of Rat II, also, there was a tendency for the coördination to break down under the changed conditions, as this rat took to jumping toward the base of the opposite standard, and could not thereafter be induced to jump to the platform. This series was necessarily abbreviated on that account.

b. On Normal Black-and-White Rats.

Three black-and-white rats were given the same test. They had attained the necessary standard of efficiency, i. e., 80 per cent of perfect coördinations at 22 inches. The following are from the notes taken on this series.

Black-and-White Female I.

	POSITION OF PLATFORM II.	
10/29/07	East, 22 in.	Five trials; 100 per cent perfect.
	South.	One trial, perfect.
	West.	Jumped southeast five times in succession apparently at a shadow caused by the joining of the canvas strips.
10/30/07	East.	Six trials, 80 per cent perfect. Shows tendency to jump toward northeast.
	South.	Two trials, first, slightly to east of south. Second, perfect.
	West.	Two trials, first; slightly to south of west; second perfect.
	South.	One trial, jumped to wall on south.
	East.	Two trials, first, jumped to south; second to east.
	West.	Two trials, first, southwest; second, perfect.
	North.	Refused to jump.
11/1/07	East.	One trial, perfect.
	North.	Jumped northeast to canvas. Same relative direction.
	West.	Jumped southeast.
	North.	Jumped northeast to canvas.
11/5/07	South.	Jumped northeast to canvas. Jumped to wall repeatedly but not to platform. Tests discontinued.

Black-and-White Female II.

	POSITION OF PLATFORM II.	
11/30/07	East.	Five trials, 60 per cent perfect, others good.
	South.	One trial, perfect.
	North.	One trial, perfect.
	South.	One trial, perfect.
10/31/07	East.	One trial, perfect.
	South.	One trial, perfect.
	West.	One trial, perfect.
	North.	One trial, perfect.
	West.	One trial, perfect.
	North.	One trial, perfect.
11/1/07	East.	One trial, perfect.
	West.	One trial, perfect.
	South.	One trial, perfect.
	North.	One trial, perfect.
		TURNED APPARATUS BUT NOT CABINET.
11/2/07	South.	One trial, direction perfect but distance short.
	West.	One trial, rat confused. Put back.
11/3/07	East.	One trial, direction correct, distance short.
	North.	One trial, perfect.
	West.	One trial, perfect.
	South.	One trial, perfect.

Black-and-White Female III.

11/19/07	East, 22 in.	Ten trials, 80 per cent perfect.
	South.	One trial, good.
	West.	One trial, good.
	North.	One trial, good.
	East.	Eight trials, 75 per cent perfect.
	North.	One trial, perfect.
	South.	One trial, perfect.
	West.	One trial, jumped to floor.
11/21/07		Very active rat; gets innervation before muscular accommodation, consequently makes random leaps.
11/24/07		Will jump only to floor. Tried several devices to prevent this, but none successful. Series discontinued.

The black-and-white rats, like the white ones, reached a point in the tests where the accommodation to the distance and the direction broke down completely, though it did not break down so soon. These rats had not been at work at the test as long as the white animals which had been trained upon the old apparatus and retrained upon the new.

c. On Anosmic Rat.

The anosmic rat had just reached the necessary maximum of 22 inches before the test with apparatus rotated could be made. He had been given two trials with the apparatus changed. Through some mishap on the part of the laboratory attendant the rat gained his liberty, and was not seen thereafter. His records for the two trials follow:

Record of Anosmic Rat: Position of Apparatus Variable.

	POSITION OF PLATFORM II.	TRIALS.	
12/15/07	East	7	Fourth trial, scrambled; sixth, short and fell. Seems tired.
	South.	1	Slow, but accurate.
	West.	1	Good. Very slow.

ii. Summary.

1. Four of the six rats were able to direct their jumps equally well, regardless of the direction in which the jump must be taken. The other two animals were able to accomplish this in about 50 per cent of their trials.

2. These two other rats were by no means failures on the problem. One of them, White Female I, jumped to Platform II which was south at the first trial. She had always previously jumped to the east. After this trial she often refused to make the effort. Of the twelve trials in which she made an effort, she was five times successful in the direction of her jump, and failed seven times. Of these seven failures, four were jumps to the east and three to the south.

The remaining rat, Black-and-White Female I, did not attempt to jump toward Platform II when it was turned to the south, but jumped five times in succession to a point in the wall of the cabinet where one width of canvas overlapped another, and wriggled through, emerging on the outside of the cabinet. On the following day she jumped in the direction of Platform II, five trials, though she did not always strike it squarely on the first trial, then missed by jumping south when she should have jumped east. Soon after she failed to make any attempt to jump to Platform II, but jumped to the walls instead.

4. *Effect of Altering Distances Between Platforms.*

a. Effect of Altering Horizontal Distance.

During the training period it became evident that the rats were unable to accommodate with any degree of ease to a distance which was shorter than the one for which they had established a habit. It will be remembered that the rats had to start any given day's work with a jump which was slightly shorter than the maximum jump which they had been able to attain the day before. Under such conditions the animals would often over-innervate for the first few trials and jump entirely over Platform II.

A series of tests was undertaken to determine (1) the number of trials necessary for and (2) the sensory factors involved in a readjustment to shortened distances. The experiments are not so numerous as had been planned by reason of the fact that the coördination had broken down in many of the animals. The preceding section shows that changing the direction in which the jump must be taken tended to disintegrate the coördination with all the animals but one. If this had been predictable, the present experiments would have preceded those of the last section.

The tests here reported upon were made in the cabinet under conditions closely similar to those reported in the last section. The food, however, consisting of sunflower seed, was placed in a small receptacle which hung from the far edge of Platform II. It afforded no visual and probably little olfactory stimulation.

Before decreasing the distance between the platforms the now thoroughly experienced animals were allowed to establish a habit for the distance of twenty-two inches. The distance was then shortened and the effort of the animal to accommodate to it was recorded. The notes below show the changes made and the essential features of the animal's behavior.

i. Statement of Results.

α. On Normal White and on Normal Black-and-White Rats.

The results on the white and on the black-and-white rats are given together, since the numbers are too small to justify a separation. Only three animals could be used for the purposes of the test. The notes on the behavior of Female III are given below.

White Female III.

	INCHES.	TRIALS.	
12/31/07	22	5	First and second, to left of center of platform; third, fourth and fifth, good.
1/1/08	16	5	First, long, went over platform; second, struck but slid off far edge; third and fourth, good; fifth, perfect, struck squarely.
	8	8	First, entirely over and struck opposite wall of cabinet; second, third and fourth, shorter but entirely over; fifth, like first; sixth, seventh and eighth, entirely over.
1/2/08	22	5	First short, about one-half of distance to platform; other four trials good.
	8	5	First, second, third and fourth, long; fifth, struck platform in passing but slid off.
1/3/08	22	5	First trial, short; second, landed on the right side of the platform; fourth, scrambled; fifth, good.
	16	5	First, struck platform in passing over; second, struck squarely; third and fourth, scrambled; fifth, good.
	8	1	Went entirely over at first trial and refused to jump again.
1/4/08	22	0	Failure—refused to jump.

White Male I.

	INCHES.	TRIALS.	
8/24/07	22	4	All perfect.
	12	10	First trial, second and third, entirely over; fourth, his hind feet and tail grazed platform as he went over; fifth and sixth, over, but shorter and struck platform with his tail; seventh and eighth, grazed, platform with all fours as he went over; ninth, struck platform on far side and slipped off; tenth, landed on further side of platform but stayed on.
8/25/07	22	0	Rat refused to jump; was evidently not well. The animal had a sore foot and the tests were discontinued. It died soon after.

Black-and-White Female III was experimented with and her records follow.

Black-and-White Female III.

	INCHES.	TRIALS.	
10/14/07	22	5	First trial, scrambled; other four trials perfect.
	16	5	Jumped entirely over platform at every trial.
10/15/07	22	0	Refused to jump.
	16	0	Refused to jump.
	8	22	Went over platform at every trial; seemed to be jumping about 22 inches.
11/4/07	22	5	All good.
	8	10	All much too long.
	16	10	First trial, shorter than 22 inches but entirely over platform; second, shorter than first; third, feet grazed plane as she went over; fourth, same; fifth, struck but slid off; sixth, good; seventh and eighth, like fifth; ninth and tenth, good.

b. Effect of Altering both Horizontal and Vertical Distances.

Up to this time the platforms had been at the same height, so that the main direction of the necessary jump was horizontal. The apparatus was now adjusted so that Platform II was 6 inches higher than, and at a distance of 16 inches from Platform II. Several rats were tested under this condition, but the upward spring seemed almost impossible of acquisition and no rat was successful. The attempt to jump upward was unmistakably made, with the result that the animal sometimes struck with considerable force against the standard or the under side of the platform, or else landed on the wall opposite or upon the floor. No long continued effort was made to train the few remaining jumpers lest the repeated errors should render them unfit for further experimentation.

The apparatus was then re-adjusted so that Platform II was 10 inches below the level of and 16 inches distant horizontally from Platform I.

i. Statement of Results.

α. On Normal White, and on Normal Black-and-White Rats.

The notes on the behavior of the two remaining animals follow.

White Female III.

	INCHES.	TRIALS.	
1/6/08	22	5	All good.
	16 Horizontal, 10 Vertical	5	Was slow in preparing to jump; seemed ready to spring several times before she finally essayed it. Looked downward toward the platform. First, landed on left margin of platform; second, third, fourth and fifth, good.
1/7/08	16 H., 10 V.	5	Same slow and elaborate preparations as yesterday. First and second, perfect; third, scrambled slightly; fourth and fifth, perfect.
	8 H., 10 V.	5	Slow in starting. Jumped downward but considerably over the platform. Did not seem at any trial to shorten her jumps from those of yesterday. All trials a failure.
1/8/08	8 H., 10 V.	5	Behavior as before, only slower. No successes.
1/9/08	8 H., 10 V.	2	After long intervals she jumped twice with results as above; then seemingly discouraged she settled down for an hour and refused to make any efforts.
1/10/08	8 H., 10 V.	0	Would make no effort.

Black-and-White Female II.

	INCHES.	TRIALS.	
1/5/07	22 H.	5	All trials perfect.
	22 H. 10 V.	5	Jumped down and struck platform squarely, but angle was so great that she slipped off; second, struck squarely and slid but did not fall off; third, fourth and fifth, perfect and with less force.
	16 H. 10 V.	10	First, entirely over; second, struck platform with hind legs and tail as she passed; third, shorter but still over; fourth, fifth and sixth, landed but slid off by reason of momentum; seventh, eighth, ninth and tenth, struck squarely on platform and did not slide.
1/6/07	16 H. 10 V.	6	First, struck platform with hind feet only; others good.
	8 H. 10 V.	10	First and second, over; third, over but struck in passing; fourth, shorter; other six trials entirely over.
1/7/07	8 H. 10 V.	20	No nearer coördination than before.
1/8/07	8 H. 10 V.	8	As yesterday; all trials were failures. The platform was then moved out to the point which would intersect her leap. The distance proved to be 15 inches.
	15 H. 10 V.	6	First, struck platform with tail; second, struck platform on right side and fell off; third, struck on right side but stayed on; fourth, jump a little long but stayed on platform; fifth, struck it and fell off; sixth, good.

ii. Summary.

1. No rat was able to make the adjustment when the distance was changed from 22 inches to 8 inches in a reasonable number of trials. One rat failed after seventy trials. All animals were able to adjust without *great difficulty* to the change from 22 to 16 inches apparently by means of a trial and error method. An average of about one trial was necessary in order to effect this readjustment. When the dis-

tance was shortened the animals always jumped too far on the first trials.

2. In the few cases where the distance was suddenly lengthened the jump was usually too short on the first trials. It was impossible to compare the ease of the readjustment to the lengthened distances, with that for the shortened distances, by reason of the fact that the animals were taught to jump long distances by gradually increasing the distance between the platforms. The adjustment to an increased distance was thus more habitual than that to a shortened distance.

3. The animals were able to adjust successfully at once for the lowered position at 16 inches. They could adjust for a lowered and shortened distance more easily than for merely the shortened distance. They could not, however, adjust to the lowered and shortened position at 8 inches. It was evident that they were making the effort but they invariably jumped out too far.

5. CONCLUSIONS.

The purpose of the foregoing tests was to estimate the importance of vision in the coördination required in jumping. Three aspects of the coördination as a whole were considered: *a* Learning to jump a given distance when the direction of the jump was constant; *b* the effect, after the jump under constant conditions had become automatized, of changing the distance of the jump, the direction remaining constant as before; and *c* the effect of changing the distance and the direction of the jump in either the horizontal or vertical planes, or in both. The data gathered from the various experiments seem to justify the following general conclusions, stated in the order of the problems as indicated above.

a. Learning to Jump—Direction Constant.

The results indicate that the loss of vision in some way interferes with learning to jump long distances and greatly decreases the ease and rapidity in the acquisition of the coördination for short distances. In the case of the two blind individuals which failed, it seemed that some element was lacking which was essential to the initiation of the act. The fact that two blind rats learned to jump even the shorter distances, and that the normal animals had to accommodate by a trial and error method to sudden increases and decreases in the distance between platforms, indicates that up to a certain point, other than visual factors are concerned in these adaptations to a distant stimulus. The blind animals, unlike the normal animals, did not move about when placed facing the east on Platform I: they were given their orientation and retained it. The normal animals moved about on the platform so continuously that the experimenter made no effort to put them down in a relatively constant position. The fact that their orientation was given to the blind rats was probably the reason of their success. An attempt was to have been made to test this factor by changing the initial position, but the coördination disintegrated before the test could be made. Certainly the experiments on the process of learning to jump are not decisive in indicating what rôle vision plays in this coördination.

It has been shown that the tactual, kinæsthetic and olfactory senses are able to mediate accurate adjustments to short distances even in the absence of visual impulses. The separate rôle played by each of these senses in the case of the blind animals has not been determined. Judging from the tests upon the anosmic animals it would appear that olfactory stimuli can be dispensed with both during the acquisition of the habit and at all later times. Touch, as a partially controlling factor, does, however, enter into the early adjustments of the blind animals, since they will more readily form the habit of jumping if the snout or vibrissæ are stimulated by the platform to which the animal has to jump. This latter statement applies in some degree at least even to the animals possessing vision. Once the habit is formed, however, the initial tactual impulses can be dispensed with.

In regard to the function of kinæsthetic impulses in the case of the blind animals, it seems safe to affirm that they soon come to usurp whatever function tactual impulses from the snout and vibrissæ exert in the learning process. They soon become the only indispensable means of control in the blind animal for such short jumps as they were able to accomplish.[1]

b. Effect of Lengthening or Shortening the Jump, Direction Constant.

From the experiments on p. 93 it follows that the change in visual impulses conditioned by lengthening or shortening the distance between the platforms is not adequate to effect the change in innervation necessary for a successful coördination. Lengthening or shortening the distance between the platforms may bring about a change in accommodation and in convergence (kinæsthetic factors) and certainly occasions a change in the intensity of the visual impulses and the size of the area of the retinal elements which receive the stimulation (change in visual impulse proper). In the case of these types of animals, monkey, cat, etc., where adjustments under similar conditions are accurate, the above noted changes in the sensory complex in all probability are sufficient to bring about the proper modification in the motor discharge. In the case of the rat, however, these delicate changes in sensory stimulation are inadequate to modify the habitual motor response. The rat apparently, in order to accommodate to the changes in distance, must make trial movements, that is, *must establish a habit of jumping a given distance.* Any change in the distance calls for learning factors similar to those already discussed on p. 72. It is evident that by means of these trial jumps the animal is bringing *into play the large muscles of the body* (as contrasted with the eye muscles and the ciliary muscle) *and is thereby gaining a control over the motor area which it is perhaps impossible to obtain by the visual changes and the changes involved in accommodation and in convergence.* These facts in themselves are suggestive of the relatively secondary importance of vision in the life of this animal.

It is thus seen that the attempt to eliminate the function of kinæsthetic impulses by irregularly changing the distance has

[1]The term kinaesthetic as here employed necessarily includes whatever impulses come from the skin of the feet. These impulses are presumably fused with those from the muscles.

not been successful in isolating the rôle of vision, by reason of the fact that when the distance is altered the habit breaks down and readjustment must take place. Had the animals been able to accommodate to the changed conditions without trial movements, the conclusion that the visual complex (visual impulse, accommodation and convergence) was the essential sensory factor involved in this coördination would be justified. But since trial movements are necessary, the problem remains as to whether kinæsthetic impulses alone are responsible for it.

c. Effect of Changing Direction of Jump.

The experiments summarized on p. 92 and p. 98 were much more successful in giving evidence of the function of a distance receptor. Since the possibility of the use of olfaction as a sensory control had been practically eliminated by previous experiment by precautionary methods above described, and since audition could not have furnished such guidance, it is evident that vision or some other undetermined receptor, functioned here in such a way as successfully to control the adjustment to a distant stimulus. Assuming for argument that vision is the effective source of control, it may be maintained that the visual impulse seems to afford evidence concerning the direction of the stimulus but is apparently not alone capable of controlling the amount of innervation necessary to make the requisite adjustments. In other words, visual impulses in such a form as may be designated white light vision are operative[1] and afford a basis for controlling the direction of the adjustments, but do not operate so as to furnish information concerning the third dimension.

Four rats of the six were able to adjust accurately and immediately to any direction of Platform II, (p. 92). A fifth was successful in five out of twelve trials including the *first.* The sixth rat, (when she was not jumping through a slit in the canvas before the cabinet was lined with other material) made the adjustment correctly in five trials out of six.

[1]Watson (*Animal Education*, p. 85) remarks "other things being equal, rats show a decided preference for well-lighted rather than dark places."

In the experiment in which both the distance and direction were changed, the two rats tested made the successful coördination. In this test, it will be recalled, the rat was obliged to jump downward and outward to reach Platform II, a horizontal distance of 16 inches from, and a vertical distance of 10 inches below Platform I. The downward jump had not hitherto been required in the experiment, and the animals accommodated themselves to the change immediately. Possibly this immediate accommodation was due to the fact that Platform II, being 10 inches below the level of Platform I, afforded a visual stimulus area about four times larger than when in the horizontal plane of Platform I. The stimulus was thereby much more effective. This fact of instant adjustment to a directional change, and a trial and error method of adjustment to a merely distance change, is the basis for the assumption that vision (or some other unknown distance receptor) affords information as to the direction but not as to the distance, of the stimulus. The observation that while the animals could jump downward to Platform II at 16 inches distant horizontally from the support of Platform I and 10 inches below it, but not at 8 inches horizontal distance [and 10 inches below] confirms an earlier statement that vision is in many instances overruled by the habitual innervation tendency.

The possibility of a directional factor seems to receive some confirmation in the results of the test. Of the eight cases of miscoördination (not due to jumping toward the canvas) four were jumps toward the east, the direction in which the jump was learned, and four were toward the south, the direction of the first jump after the change. What the nature of such a factor may be the present test made no attempt to investigate. Whether it was this factor which led to the breakdown of the coördination in the case of every rat but one is a question which only further experimentation can solve.

PART SECOND

A. EFFECT OF TRAINING UPON THE RATS.

I. Experimental Results.

1. Comparison of Records of Trained and of Untrained Rats.

a. Normal White Rats on Problem I.

While training two female white rats upon Problem I for a purpose other than that of these tests, it was found that their time-records were lower than normal. They had previously learned the Hampton Court maze. Thinking that this lower time record might be the result of tuition, the records of these rats were preserved in order that they might be compared with those of *normal untrained* animals upon the same problem. In Table XII is given the averages of the trained and of the untrained groups; and on Plate V is shown the averages of both groups.

TABLE XII.

Showing (1) the average time-record of 8 untrained normal white rats, (2) the average time-record of two trained normal white rats, and (3) the time-records of a trained blind anosmic rat upon Problem I.

NO. OF TRIALS.	1.	2.	3.
	min.	*min.*	*min.*
1	7.04	5.69	.55
2	1.69	.23	.18
3	.48	.09	.30
4	.80	.43	.12
5	.35	.10	.07
6	.30	.05	.11
7	.25	.09	.10
8	.23	.11	.06
9	.27	.05	.30
10	.18	.10	.12
11	.16	.05	.05
12	.13	.14	.08

TABLE XII.—Continued.

NO. OF TRIALS.	1.	2.	3.
13	.15	.05	.17
14	.09	.05	.05
15	.11	.05	.22
16	.14	.14	.10
17	.18	.08	.12
18	.13	.04	.07
19	.19	.06	.22
20	.09	.04	.05
21	.17	.06	.10
22	.12	.06	.05
23	.11	.07	.10
24	.23	.37	.11
25	.14	.07	.05
26	.10	.12	.11
27	.08	.15	.04
28	.12	.06	.05
29	.15	.10	.04
30	.07	.09	.08
31	.07	.06	.06
32	.06	.05	.05
33	.12	.07	.06
34	.17	.05	.07
35	.09	.15	.07
36	.19	.05	.12
37	.14	.04	.07
38	.09	.05	.10
39	.09	.05	.10
40	.09	.03	.07
41	.24	.04	.08
42	.15	.06	.09
43	.12	.05	.06
44	.11	.21	.14
45	.10	04	.05
46	.06	.04	.05
47	.06	.05	.07
48	.06	.12	.06
49	.06	.05	.05
50	.23	.04	.07

The comparison shows that the averages of these trained normal animals are far below those of the untrained normal white rats. The average of their records is—in the main—below the minimal time-records of the normal untrained group.

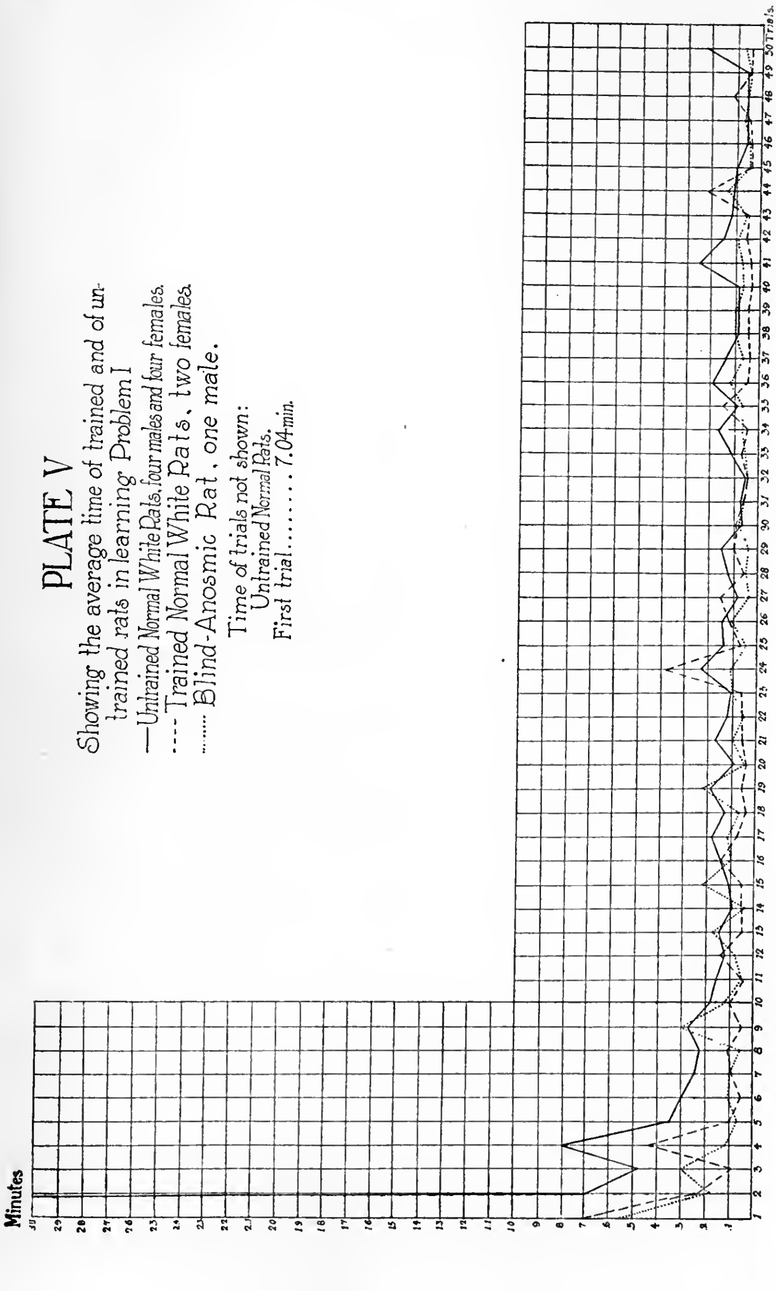


PLATE V

Showing the average time of trained and of untrained rats in learning Problem I

— Untrained Normal White Rats, four males and four females.

---- Trained Normal White Rats, two females.

...... Blind-Anosmic Rat, one male.

Time of trials not shown:
Untrained Normal Rats.
First trial........7.04 min.

The average of the lowest two records of the normal group is considerably above that of the average of these two experienced rats. No conclusions are justifiable upon the results of this comparison, since the two animals *might* have been extreme variations. The records are, therefore, tentatively put forth in connection with those of other trained and untrained groups.

The records of these two trained females averaged by tens in a series are given below. They may be compared with other records of the same kind on p. 21.

The Average Time-Records of the two Females for Entire Series.

	min.		*min.*
Female I	.19	Female II	.20

Average Time-Records by Groups of Ten.

Trials.	*Female I.* *min.*	*Female II.* *min.*
1–10	.68	.71
11–20	.08	.06
21–30	.06	.10
31–40	.07	.05
41–50	.07	.07

Female I made a total of five errors, and Female II a total of six errors in the series.

b. Blind Anosmic Rat on Problem I.

The time-records of the blind anosmic rat upon this problem are given in Table XII. He had been trained in the maze during a long series of tests. This rat was undoubtedly a very robust animal. His records on Problem I are much below those of the untrained normal rats, the blind rats, or the anosmic rats with vision. He became very active when put into the control cage and attacked the problem at once.

The Average Time-Record for Entire Series (50 Trials).

Blind Anosmic Rat	.10 min.

Average Time-Records by Groups of Ten.

	min.		*min.*
1–10	.19	31–40	.08
11–20	.11	41–50	.07
21–30	.07		

This record may be compared with those of the other rats which are given in the section on Problem I (p. 21).

Each of the two curves, representing the averages of rats which had had previous training, are lower than any one of those representing the averages of untrained white rats.[1]

c. Normal White Rats on Problem III.

Four untrained normal white rats were set to work upon the problem. Three of the animals were males about 150 days of age, and one female 128 days of age. The method of conducting the experiment was the same as in the earlier test. The rats were tame and in good physical condition.

Their method of solving the problem was the same as that of the trained animals, except that the untrained rats consumed much more time in achieving their first successes. The minimal time record for the first trial was more than 29 minutes. The rats were energetic and industrious, but they spent a great deal of time examining the control cage, though, like the trained animals they had been fed for three days in the cage to accustom them to the environment. Their average was not reduced to one minute until the fifth trial. The averages of the trained rats on the other hand did not go above one minute after the first trial. The averages of the untrained rats were reduced to .10 min. at the twenty-first trial though they were higher thereafter; while the averages of the trained animals dropped below .10 min. on the seventh trial and were later no higher.

Table XIII and Plate VI show the averages of these trained and untrained animals.

The curves show very plainly the great difference in time-records of the early successes and also show the fact that even from the thirty-fifth to the fortieth trial the records of the untrained rats were not so low nor so uniform as those of the trained rats from the tenth to the fifteenth trial.

[1] In view of a possible difference in the function of vision between white rats and those having pigmented eyes, it is no more than fair to limit the comparison here to records of albino rats.

TABLE XIII.

Showing the average time-records of trained and of untrained normal white rats on Problem III. The first column of averages represents the group of seven trained rats; the second, the third, and the fourth columns, the average, the minimum, and the maximum time-records of the untrained rats.

NO. OF TRIAL.	AVERAGE.	AVERAGE.	MINIMUM.	MAXIMUM.
	min.	*min.*	*min.*	*min.*
1	5.72	40.14	29.15	54.38
2	.32	5.97	2.76	13.17
3	.33	.80	1.28	11.27
4	.17	7.77	.62	13.87
5	.29	.92	.45	2.17
6	.22	.91	.55	1.65
7	.09	.99	.25	2.58
8	.09	.37	.07	.98
9	.06	.43	.22	.75
10	.06	.28	.20	.43
11	.07	.24	.12	.37
12	.05	.13	.08	.25
13	.05	.22	.07	.35
14	.05	.23	.14	.30
15	.05	.23	.13	.35
16		.15	.10	.25
17		.21	.10	.45
18		.19	.12	.27
19		.22	.08	.43
20		.11	.05	.18
21		.09	.03	.22
22		.08	.03	.18
23		.08	.03	.17
24		.10	.03	.28
25		.17	.02	.58
26		.08	.03	.20
27		.07	.04	.10
28		.06	.03	.10
29		.06	.03	.15
30		.04	.03	.05

d. Normal Black-and-White Rats on Problem III.

Table XIV and Plate VII show the averages of the trained and untrained groups of black-and-white rats on this problem. The difference in the two curves is rather startling and calls for some descriptive comment.

PLATE VI

Showing the average time of trained and of untrained normal white rats in learning Problem III.

— Trained Rats, four males and three females

--- Untrained Rats, three males and one female.

Time of trials not shown:

	Trained Rats.	Untrained Rats.
First trial	5.72 min.	40.14 min.
Second trial		5.97 min.
Fourth trial		7.77 min.

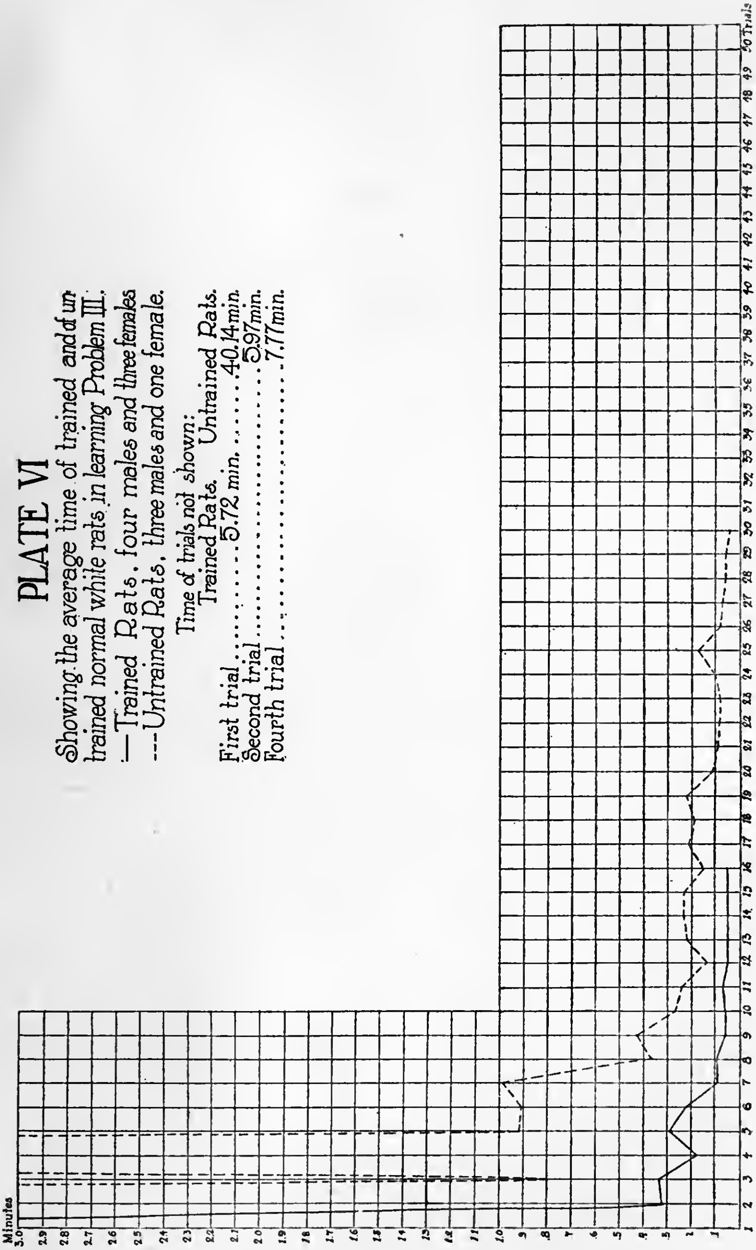


TABLE XIV.

Showing the average time-records of trained and of untrained normal black-and-white rats on Problem III. The first column gives the averages of the trained animals; the second, the third, and the fourth, give respectively the averages, the minimum, and the maximum time-records of the untrained animals.

NO. OF TRIAL.	AVERAGE.	AVERAGE.	MINIMUM.	MAXIMUM.
	min.	*min.*	*min.*	*min.*
1	.99	7.09	1.62	12.65
2	.49	4.97	3.42	6.42
3	.12	1.23	.72	1.70
4	.29	2.76	.32	4.25
5	.09	9.15	.40	25.78
6	.11	.76	.43	1.33
7	.07	2.44	.20	6.63
8	.04	.67	.41	.87
9	.08	1.35	1.00	1.95
10	.06	.57	.18	1.28
11	.14	.21	.11	.28
12	.04	.23	.18	.28
13	.05	.80	.07	1.42
14	.07	.15	.07	.27
15	.04	.18	.13	.22
16	.07	.21	.13	.32
17	.04	.12	.08	.17
18	.04	.10	.03	.17
19	.06	.14	.09	.22
20	.08	.14	.05	.28
21	.09	.08	.07	.08
22	.05	.07	.04	.18
23	.04	.10	.05	.10
24	.04	.08	.05	.07
25	.04	.05	.04	.09
26	.04	.07	.05	.08
27	.03	.09	.07	.12
28	.05	.10	.05	.15
29		.08	.07	.10
30		.06	.05	.07
31		.04	.03	.07
32		.06	.04	.08
33		.05	.04	.06
34		.04	.03	.08
35		.07	.04	.11
36		.05	.04	.06
37		.05	.07	.06
38		.07	.06	.06
39		.05	.04	.05
40		.06	.05	.07

PLATE VII

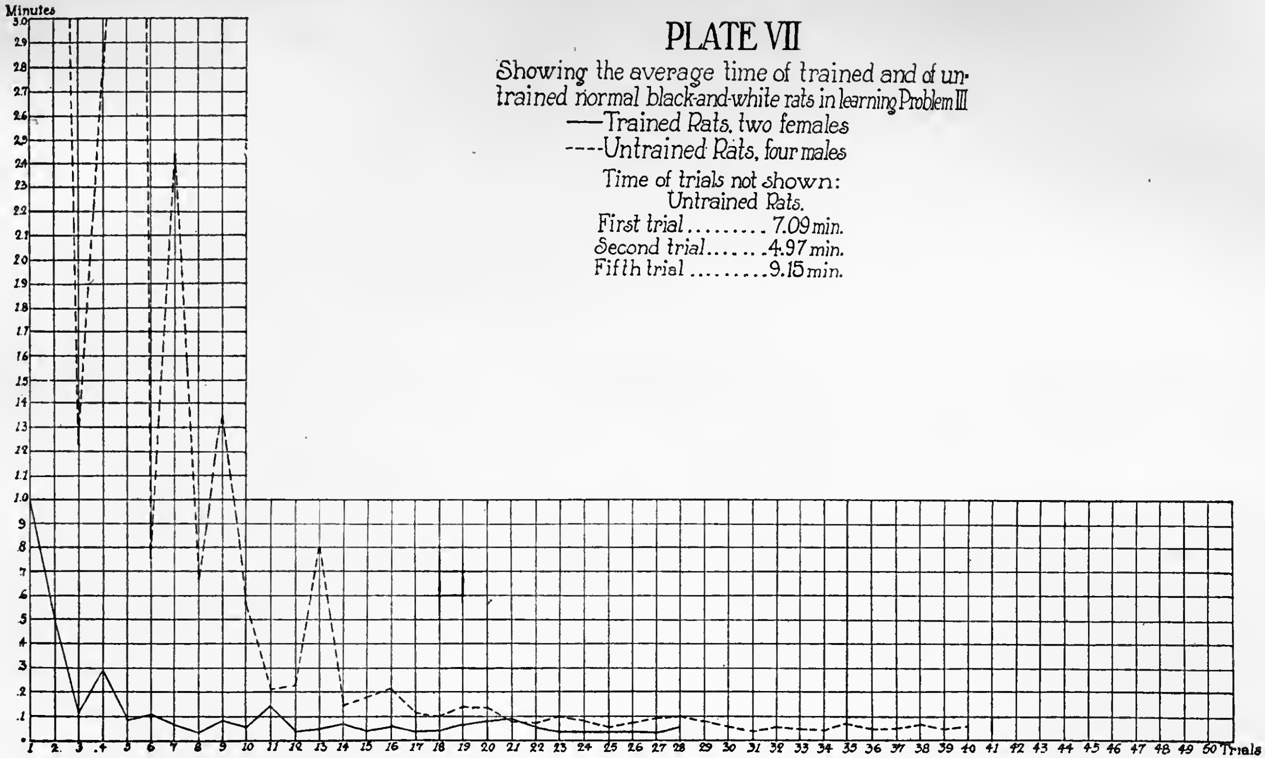
Showing the average time of trained and of untrained normal black-and-white rats in learning Problem III

—— Trained Rats, two females

- - - - Untrained Rats, four males

Time of trials not shown:
Untrained Rats.

First trial .... 7.09 min.
Second trial .... 4.97 min.
Fifth trial .... 9.15 min.



The untrained animals were tame but naturally not so tame as the trained rats when they began on this problem.[1] Two of them were frightened by the opening of the door; one rat was particularly careful not to approach the door from the front, but came up to it cautiously from the left so as not to be too near when the door should fly back. The rats did not locate the position of the door at once as the trained animals had done. This is doubtless one cause of the very high averages up to the tenth trial, the other apparent cause being the avoidance of the door.

e. Blind Rats on Problem III.

The experimenter desired to complete the comparisons with a discussion of differences as shown in the records of the behavior of experienced and inexperienced blind rats on Problem III. When the records were assembled it was found that the individual and accidental variations were so high among the defective animals that the averages of so small a group would be valueless in a comparison. Table XV shows the time records for each individual.

At the time of the experiment, but three inexperienced blind rats were available. Of these one made exceedingly poor time records for the first ten trials of the series, and his generally poor records throughout were a marked variation. Another animal made uniformly poor records as compared with the experienced blind rats, while the third rat was by far the most active of the twelve blind rats that were experimented upon. His records represent a marked variation in the other extreme. Consequently, an average of the records of these three animals would be valueless as a basis for comparison with any other group.

[1] The untrained animals were all of one litter, were 110 days old, and were of the same parentage as the litter of trained rats. The mother of the rats was the most active and energetic animal that had been tested in any of the experiments.

TABLE XV.

Showing the individual time-records of three untrained blind rats on Problem III.

NO. OF TRIAL.	MALE I.	FEMALE I.	FEMALE II.
1	4.77	6.32	6.47
2	1.45	15.72	1.30
3	.28	.35	.65
4	.28	1.87	.28
5	.18	4.17	3.50
6	.30	5.13	1.03
7	.12	6.37	.53
8	.15	5.53	.97
9	10.15	12.73	.80
10	.18	18.55	.65
11	.12	15.98	.90
12	.13	12.42	.42
13	.12	1.08	1.45
14	.13	.45	1.33
15	.12	.20	3.50
16	.08	10.98	1.10
17		1.87	1.95
18		.27	1.17
19		1.25	2.28
20		1.57	1.92
21		.25	2.58
22		.28	.88
23		1.60	.78
24		1.02	.05
25		.27	1.32
26		1.56	1.95
27		.17	1.87
28		.20	1.28
29		.37	.75
30		1.38	.68
31		.57	1.11
32		.27	.53
33		.93	.43
34		.32	.60
35		.17	.75
36		.22	
37		.37	
38		.08	
39		.45	
40		.28	

2. Summary of Facts Brought Out in Foregoing Experiments.

The comparison of the time-records and of the learning curves of each group of untrained rats with a group corresponding in age, variety (albino, or black-and-white), and condition (normal or defective) show that in every instance—with the possible exception of the case of the blind rats which cannot be cited as either confirmatory or contradictory—the trained animals made uniformly better records than the corresponding groups of untrained rats.[1]

II. CONCLUSIONS.

In view of the differences exhibited between the curves of the several groups of trained and untrained animals, it seems advisable to analyze the experience acquired in the solution of the previous problems. The first consideration is the effort to formulate a statement of: (1) What experience the animal acquires in the previous series of tests, and (2) what effects may be carried ever from one situation to another. Such a carrying over might result either in a transfer or an interference of training. The curves apparently justify the statement that, within the limits of such problems as were here employed, those rats which had had previous experimental experience were more apt in learning a new problem. Trained animals not only acquire the requisite association in a less number of trials but the early time-records are shorter. Expressed in terms of the neurological and physiological organism, the shorter time-record might be the result of a modification of either the motor or sensory system, or both. If the modification were one affecting the motor centers of the cortex and the efferent pathways, the stimulus might (1), release a greater amount of innervation, resulting in greater general activity, or (2), release movements which had become habitual in the earlier experience, and which would be advantageous in the attempts to solve the new problem, i. e., fewer random movements, and an earlier accidental success would result. If the modification

[1] Yerkes (*The Dancing Mouse*, p. 263) found that the acquisition of one labyrinth habit facilitated the acquisition of others.

were one affecting the sensory pathways and centers, the general result would be (1) an increased susceptibility to the stimulus—rendering the stimulus more intense—and (2) a decreased resistance in the connections between the sensory and motor centers, so that the indirect effect of the stimulus upon the musculature would be more immediate.

Observations of the behavior of the two groups of animals lead the writer to accept both of these possibilities as facts; that the stimulus is more intense and the activity more *immediate* and *better coördinated* in the case of the trained as against the untrained animal.

Previous to the work on Problem III, each rat had each day for thirty-four days been lifted from the door of its living cage, put through the door of the control cage upon the table and allowed to satisfy its hunger from food which had to be reached by its own exertions. It is reasonable to suppose that after such a long process of habituation to such experimental situations, the experience of being lifted from the living cage, carried to a distance and placed into another cage, might become for the rat a stimulus to activity when placed in the control cage.

In Problem II, given just previous to III, the entrance of the problem box occupied the same relative position, i. e., on the lower left-hand corner of the south side of the food box. When the trained rats were put into the cage containing Box III, they went to this position and began to 'nose' about. The contact sensations apparently released motor impulses which resulted in the scratching, biting, pulling and clawing at the spring, the latch, or the edge of the door. The first success soon followed. When the untrained rats were put into the cage, the environment was a stimulus to only the most general and uncontrolled activity. The rats examined the control cage as well as the box. They sniffed at the food and the latch, and then went on to examine other parts of the box and the cage. They often sat down to wash their faces and scratch themselves spending far more time at this procedure than did the trained animals. Motor energy in these animals, in the absence of any more specific stimulus, seemed to drain off into these reflex channels. On the other hand all problem boxes (on account of past experience) served as potent stimuli to the trained rats; the animals had satisfied their curiosity as to the surroundings during previous tests and did not lack an incentive to effort in the present environment. Therefore in the case of the trained animals the stimulus released movements which were more advantageous in the solution of the problem than it did with the untrained animals.

The effect of the emotional attitude of the rat has been disregarded up to this point. The emotional element is a most important and ever present factor in the reaction of the animals. The rats, as stated before, were tame at the beginning of the experiments. They were accustomed to being handled and when the door of the cage was being opened, they came eagerly. At the end of the series this lack of timidity had advanced to a point that might be called familiarity. Instances of this were noted on a number of occasions when rats became ill or aged and refused food in their cages, they ate quite freely from food in the hands of the experimenter.

Accompanying the change in the emotional attitude of the rats toward the experimenter is the change in the emotional attitude toward the control cage and to problem boxes in general.[1] When this part of the environment which is common to all the tests has lost much of its novelty, there is nothing to interfere with the normal discharge of the impulses which will speedily result in adaptive movements. Because the situation as a whole is novel to the inexperienced rat, there is a state of high emotional tension in which motor impulses, foreign to the problem in hand, are set up by the strange sensory

[1] The same behavior as is here commented on as characteristic of trained animals was observed in the case of two brood rats which were bought from a small boy. These rats had absolutely no fear and exhibited no signs of disturbance when placed in strange experimental situations. The reactions of one of them which learned to jump a distance of 14 in. in the first day's test are described on page 76. The other rat solved Problem III in much less time than any other untrained rat. Her records are not included in the tables or curves as she was an old rat and did not conform to the age requirement for these tests.

These instances are cited in support of the contention that the emotional tone is a great (if not the greatest) factor in the ease with which the rat adapts itself to a new situation.

impressions. In the case of experienced rats no such outburst of motor energy is incident upon their introduction to a new problem; consequently they are in a better position to begin work immediately upon the elements in the situation which are novel.

B. INDIVIDUAL AND SEX DIFFERENCES AS SHOWN BY BEHAVIOR.

1. *Sex Differences.*

All of the tables giving the percentages of minimum and of maximum time-records made by each animal (such as are shown on p. 12) have been assembled and the following table compiled from the total number. The animals are ranked 1, 2, 3, etc., according to the percentage of maximum and of minimum time-records. For instance, the rat which made the greatest number of minimal trials is ranked 1; the rat with the next greatest number is ranked 2. The rat ranked as 1 in the portion of the table showing the rank in maximum time records, is consequently the animal making the greatest number of maximum, or poor time-records. The table is shown on next page.

The tabulation is of interest, showing as it does the comparative ranks of the animal in the different problems. It is rather striking that the greatest number of minimal records made in mixed groups are, with one exception, to the credit of the males, while in such groups the greatest number of maximal records are made by the females.[1] The least number of minimal records, with one exception, were made by females and the least number of maximal records were made by males. The records are not a sufficient basis for any general statement as to sex differences.

It is often, but not always true, that an animal which makes a good record on one problem makes good records on the other two. The ranking of the group of black-and-white rats on Problem I and II are striking.

[1] This is not true in Watson's work on the maze, in which the shortest records were made by the females. Yerkes (*Dancing Mouse*, p. 276) also found that the females were superior to the males in the labyrinth test, although the males were superior in discrimination tests.

Table showing sex and comparative rank of each animal in number of minimal and of maximal time-records on Problems I, II, and II.

MINIMUM.				MAXIMUM.			
PROBLEMS.	I.	II.	III.	PROBLEMS.	I.	II.	III.
Normal White Rats.				*Normal White Rats.*			
Male I	1	3	1		3	3*	0
Male II	3*	1	3*		4	5	0
Male III	2	5	3*		5*	4	3
Male IV	3*	6	2		2	3*	2
Female I	7	0	0†		1	0†	0†
Female II	4	4	0		6	5	1
Female III	6	2	0		5*	1	3
Female IV	5	7	0¶		4	2	0¶
Normal Black-and-White Rats.				*Normal Black-and-White Rats.*			
Female I	1	1	0¶		4	2	0¶
Female II	3	3	0¶		3	4	0¶
Female III	2	3	2		2	3	1
Female IV	4	4	1		1	1	2
Blind Rats.				*Blind Rats.*			
Male I	0†	0	0‡		0†	0‡	0‡
Male II	1	0	0†		5*	0†	0†
Male III	3	0	0¶		4	0¶	0¶
Male IV	4	6	1		6	3	3
Female I	7	5	0¶		1	1	0¶
Female II	2*	4	0¶		3	2	0¶
Female III	2*	3	0¶		5*	5	0¶
Female IV	6	1	2		2	4	2
Female V	5	2	3		5*	6	1

* *Two or* more animals attained the same rank in these cases.
† Records not included in averages but given under individual variations.
‡ Would not learn problems.
¶ Died before completing tests.

2. *Individual Differences.*

The following records of individuals are those which, for the reasons given, were not included in the averages of the groups, but are appended here for the purpose of comparison with the average records of the group.[1]

PROBLEM I.

Blind Male I made 72 per cent of the total number of maximal records, although he made 10 per cent of the minimal records. After the twenty-ninth trial, this rat made every maximal record. From the twelfth to the twenty-fourth trial, his records were unusually long, but after the twenty-fourth his behavior was such as to render his records incomparable with those of the other blind rats. He was slow and made errors repeatedly. The animal seemed timid in getting down from the top of the box. He was generally disturbed by the experimentation, and would crouch and quiver when the experimenter handled him. He became more nervous and irritable as the tests proceeded.

PROBLEM II.

Normal White Female I was the slowest of the group of eight normal white rats which were tested upon Problem I. She was also the slowest of the group on Problem II, and here her time-records represented a much wider variation than in Problem I. In the second problem she was slow in her movements and did not associate the act of stepping out on the plane with the falling of the door of the food box. Between the twentieth and thirtieth trials she established the habit of biting at the string which connected the plane with the latch of the door. She did not always go at once to the string and often she made several efforts,—at one time six—before she exerted sufficient force to throw it. These two causes of her long records, as may be seen, made the results too variable to be included in the average. In Problem III, her records were even more irregular.

[1] Yerkes (*ibid.*, p. 264 ff.) found wide and important individual variations among his mice.

Blind Male I, the records of which on Problem I have already been shown separately and commented upon, would make no effort to solve Problem II, and as he seemed to suffer from great timidity, the test was abandoned after five days.

Blind Male II was an active, healthy animal, and solved the problem well in his first efforts and seemed to have established the association by the twentieth trial, when he became disturbed by the falling plane,—evidently its noise—and thereafter he avoided it. When he stepped upon it at all, he did not step heavily enough to open the door. He made many trips to the plane, then to the door; then to the plane again, and finally, when his efforts were entirely too erratic to be useful, the test was discontinued.

PROBLEM III.

Normal White Female II made the maximum time-record for the group at every trial. She was slow in her movements and in addition often opened the latch while leaning downward over the door from the top of the box. She sometimes opened the door from the floor of the cage, but never until after she had spent some time on the box. Consequently, her records are given separately. In this problem as in the one previous, she did not learn to solve the problem in the manner which was customary for the other animals.

Blind Male II, which was not entirely successful in Problem II did not in this problem reduce his records to an approximate constancy even after fifty trials. His early time-records were both exceedingly long and variable. The cause of the poor records, as before, was slowness of movement, probably the effect of his timidity. The sudden opening of the door frightened him at the first trial, and he crouched motionless for half a minute before he seemed to regain courage to move about the cage. He avoided the locality of the door quite consistently for many trials, and when he finally went to the door, his movements were so slow and cautious the time-records were incomparable with those of the other blind rats. At the end of a series of 78 trials, his time-records were still long and variable.

TABLE XVI.

Showing Individual Differences on the Various Problems.

NO. OF TRIAL.	PROBLEM I.	PROBLEM II.		PROBLEM III.	
	Blind Male I.	Normal White Female I.	Blind Male I.	Normal White Female I.	Blind Male I.
1	6.02	2.55	.45	16.05	5.33
2	1.63	.73	.19	74.25	5.37
3	1.57	.63	1.58	1.45	16.82
4	1.27	.25	.35	.92	2.58
5	1.83	.13	.02	1.00	.85
6	1.17	3.52	.58	1.45	13.42
7	.37	.75	.43	1.42	11.92
8	.08	.73	5.60	.36	10.50
9	.98	9.08	1.70	.25	5.03
10	.27	1.55	3.83	.22	11.65
11	1.02	1.53	.48	.30	7.28
12	.78	1.77	5.12	1.73	4.88
13	.11	10.62	.30	.29	6.53
14	.15	1.27	6.12	.28	1.13
15	.17	.88	3.62	.33	4.32
16	.37	.22	4.20		2.30
17	.12	.45	2.17		5.78
18	.10	.33	3.15		3.37
19	.33	.17	1.03		1.47
20	.15	.13	5.75		2.05
21	.25	.50	2.10		3.47
22	.15	.17	2.13		.75
23	.43	.04	6.28		.42
24	.18	.05	6.33		.37
25	.15	.07	2.10		1.05
26	.85	1.28	4.67		.30
27	2.62	.31	2.55		.53
28	1.45	.67	2.87		.87
29	.13	.42	5.25		1.17
30	1.79	.22	2.82		.70
31	.58	1.58	6.23		.15
32	.50	3.75	.72		.25
33	.59	.65	3.08		.20
34	1.90	1.28			.70
35	3.08	.48			.62
36	.78	.15			.45
37	.62	.33			.60
38	1.72	.40			.68
39	1:30	1.22			.53

Table XVI.—Continued.

	PROBLEM I	PROBLEM II		PROBLEM III	
	Blind Male I.	Normal White Female I.	Blind Male I.	Normal White Female I.	Blind Male I.
40	1.47	1.05			.42
41	2.68	.65			.85
42	.74	.90			1.52
43	1.16	.98			.97
44	1.13	1.35			.72
45	1.66	.52			.72
46	.65	.38			1.38
47	.67	.27			.58
48	.62	.73			1.85
49	.78	1.30			1.17
50	.37	1.73			.85

PART THIRD

GENERAL CONCLUSIONS.

The following paragraphs summarize briefly and schematically the different conclusions drawn from the results of the experiments above described:

1. No positive evidence has been revealed as to the comparative acuity of vision in animals with albino eyes and those with pigmented eyes.

2. The tests with three problem-boxes requiring manipulation for solution afforded no conclusive evidence of the function of the visual impulses in the successful activities of the rats. The lack of vision, however, was disadvantageous in proportion as the problem demanded finely coördinated and narrowly localized movements.

3. In the test which necessitated a jumping reaction on the part of the animal, the visual stimulus apparently afforded a basis for the proper control as to the *direction* in which the jump was to be taken, but failed signally to afford any adequate basis for accommodating to changes in distance only. The visual impressions were not a sufficient control when the length of the jump was changed and after a seeming struggle between visual and kinæsthetic factors, the coördination broke down completely. Blind animals learned to jump considerable distances, but they were first given their orientation.

4. Olfactory stimulations had evidently little importance in the problems here utilized. Such impressions were quite as likely to interfere with, as to guide the formation of, the requisite habit.

5. Tactual impressions, noticeably from the vibrissæ,

seemed in these instances, to furnish a stimulus for many requisite movements, but evidently did not assist in the following of a pathway. The impressions afforded by the vibrissæ were utilized in locating projecting surfaces mainly in the vertical plane.

6. The kinæsthetic and tactual impulses have been shown to be of value in the different problems here employed. These impulses were involved as essential factors in control not only in the learning processes, but also in the reactions after they had become habitual. In the tests involving jumping, in which the conditions were changed after the habit had become established, the animals were unable sufficiently to modify the *amount* of innervation required, and, as shown above, the reactions were consequently unsuccessful.

7. There were noted wide variations in the capabilities of the animals both as to amount of activity and the capacity of forming definite associations. There were wider variations among blind than among normal animals. Slight sex differences were remarked, though the range of sex variation is less wide than that of individual variation.

8. The effect of tuition among the rats employed in the series of problems is evident. The animals which had had previous experience in experimental situations were more apt in attacking and solving new problems. This was doubtless due to two causes: (1) that the situations had much in common, so that a transfer rather than an interference of training resulted and (2) that the decrease in emotional tension due to the comparative lack of novelty in the experimental routine made the problem stimulus more intense, therefore more potent, and the resulting activity less diffuse.

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On the Influence of Complexity and Dissimilarity on Memory

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PREFACE.

While the investigation contained in the following pages belongs to the field of pure psychology, the motives which prompted it were considerations rather of educational psychology, and it is believed that not the least of its significance will be found in its bearing upon certain problems in this latter field. The experiments were carried out at the University of Chicago during the year 1908–9. The subjects were eleven graduate students in the Department of Psychology, of whom seven were men and four women. Messrs. E. B. McProud, W. C. Vogt, H. Kimmel, F. A. C. Perrin, J. W. Hayes, E. H. Sutherland, and J. W. Baumgardner, and Misses Emma Felsenthal, Mary C. McIntosh, Edith Turner, and Jeanette Obenchain. Two of them, Mr. McProud and Miss Felsenthal, were obliged by pressure of other work to withdraw after the Gray and the Violet Sets and the Renaissance Set had been completed. Their places were taken by Mr. Baumgardner and Miss Obenchain. In the latter part of the year supplemental experiments, described in V–3, were carried out with Messrs. Sutherland and Hayes and Miss Turner. They did not participate in the earlier experiments.

For the splendid spirit in which all eleven carried out their part of the investigation I wish to express to them my deep appreciation and heartfelt thanks. To Professor James R. Angell, Director of the Laboratory, and to Professor Harvey Carr and Dr. Karl T. Waugh I am glad of this opportunity to express my sincere thanks for assistance in the formulation and execution of the work.

HARVEY ANDREW PETERSON.

CONTENTS

ON THE INFLUENCE OF COMPLEXITY AND DISSIMILARITY ON MEMORY

Harvey Andrew Peterson

I. HISTORICAL INTRODUCTION AND PROBLEM

The question of the relative persistence in memory of different kinds of materials has been worked out to a considerable extent. It is known that words in the form of connected passages are vastly better retained than an equal number of disconnected words or phrases; and that objects, actions and pictures are better retained than their verbal equivalents. Numbers and abstract words belong to the third stage of difficulty, while nonsense syllables are the most elusive and most quickly forgotten of all materials commonly used for experimentation. Presentation of the materials to several senses is more effective than presentation to one only. Up to a certain stage—where the limit is we do not know—complex material is retained better than that which is simpler; and recurrent similarity in the presentations of series otherwise different is disastrous. Why all this should be true is not so easy to find out. There has been little attempt to seek explanatory principles which would unify these diverse facts. The relations of complexity and similarity are both intricate and close. It is believed that a comprehension of their relations will afford a unifying principle for some of the other facts we have enumerated. These relations are apparently somewhat as follows, so far as they have been worked out. We may first take complexity.

A thing is complex when it has many parts and the parts have many interrelations, that is, the whole has a high degree of organization. A telephone exchange is complex when in operation, while a corresponding quantity of wire, colored glass and electricity is simple because unorganized. But the fewer the parts the simpler the whole because the possibilities of interrelation are less.

Complexity affects the memory through interest and attention. The simple is easily mastered. The parts and their relations are few and the content is soon exhausted. The complex, provided it is not too far beyond us, whets our curiosity. It gets the attention and the thing is in so far better remembered, because it stays in consciousness long enough to make a deep impression. But the complex has a superiority in recall as well as in presentation, because of the high degree of organization. This makes it a unity. One part brings others. A general idea of the whole is also a very effective starting point for recall. We pass to the experimental literature.

Meakin demonstrated the superior persistence of the complex over the simple in passive attention.[1] He exposed a line and an angle together for five seconds directing the subject looking at them to divide the time equally between them. As soon as the exposure was over the subject closed his eyes and remained passive for sixty seconds reporting the entrance and exit of the two images. The angles were in consciousness an average of 38 seconds; the lines, 32 seconds. In the same way he compared plain figures and identical figures with concentric lines just inside the periphery, plain and identical colored figures, and figures of less and of more complex outlines. His results were, for the plain and the marked, 24 and 37 seconds; for the plain and the colored, 31 and 38 seconds; for the simple and the complex outlines, 27 and 35 seconds.

Binet and Henri compared the memory of school children for disconnected words and for sentences, the material being read to the children.[2] Although the comparison was not carried further than lists of seven or eight words, it was found that about twenty-five times as many words were recalled from the sentences as from the lists containing equal numbers of words. The reproduction was immediate. Mere connecting words in the sentences were disregarded.

[1] *Psych. Rev.*, 1903, Mon. Sup., no. 4, p. 235.

[2] Binet and Henri: *L'Année Psych.*, 1894, I, 1–59.

Dr. Gordon introduced a novelty into memory work when she made the complex material consist of combinations wholly *unfamiliar* to the subject.[1] This property cannot be claimed *in toto* for sentences, since the past experiences of the listener render them a unity to some extent, even before they are heard. Dr. Gordon's simple material consisted of series of nonsense syllables, nine in a series, shown through a single aperture. With this she contrasted three types of complex series in the first of which each syllable had a background of a different color. In a second type the syllables were black like the simple type, but were shown through nine apertures distributed around the periphery of the screen; thus each syllable had a distinctive spatial position. In a third type the backgrounds were colored and the positions were also changed, the apertures forming a vertical column in the middle of the screen. The standard of measurement was the number of repetitions required for complete learning, and relearning after a day, and in the test the question was not how many of the combinations of color, position and syllable can the subject recall, but merely how many syllables can be recalled. The novelty of the experiment consisted in the expectation that the secondary associations of syllable with color and position would be formed *during the brief period of learning.* Unquestionably these complex series were at a disadvantage compared with the simple series because of the burden thus imposed, but on the other hand in recall they possessed the theoretical advantage of furnishing the subject with more recall cues, namely, the color and the spatial location of the syllables. Kuhlmann has shown, what was suspected before, that spatial location is one of the most easily formed and most lasting associations.[2] The principle involved has been stated by James, that other things being equal, the chances of a fact being recalled are in proportion to the number of connections between it and the rest of our experience.[3] The results of the Gordon experiment showed a decided advantage for the complex series. The fig-

[1] Gordon, K.: Meaning in Memory and Attention. *Psych. Rev.*, 1903, X, 267.

[2] Kuhlmann, F.: *Psych. Rev.*, 1906, XIII, 321.

[3] James: *Principles of Psychology*, I, 661.

ures give the average number of repetitions required for learning and relearning.

	First learning	Second learning
Plain syllables shown in one place	7.05	4.06
Plain syllables shown in nine places	6.60	3.63
Plain syllables shown in one place	6.04	3.40
Colored syllables shown in one place	5.62	2.79
Colored syllables shown in nine places	5.27	3.26

Dr. Gordon suggested that the advantage of increasing the complexity in this way would soon reach a limit, where more potential clues would be offered than the learner could use in so short a time of learning, and where distraction would begin to operate. It is the main problem of the present investigation to find these limits in a few typical cases.

If the mind loves complexity somewhat, it loves variety and change at least as much,—common sense would unhesitatingly say, more,—and here again it is through the superior hold which variety has on our attention and interest that the memory is improved. Let us first distinguish the pair, variety and sameness (or similarity), from complexity and simplicity; for though they have much in common they are not identical. Two things are varied when one is more or less different from the other, has something not contained in the other. It is true that variety presupposes complexity, for without a plurality of parts and relations some of which are now included and now left out, variety is impossible. But the converse is not true, for the same complex may recur without affording much variety. The simple usually has sameness about it, though it may have that low order of variety which comes from a mere change of parts, as when one changes from a collection of colors to one of letters. There is little complexity in either because of the relative absence of organization. *The distinction which will be significant for the present investigation is the fact that one might increase the complexity without necessarily increasing the variety.* It is the *varied* complexity which in our opinion seems fruitful for memory and which the present investigation is concerned with.

The beneficial effect of considerable variety upon memory is a commonplace. A speaker who uses only a few tones is at a disadvantage compared with one who uses intonation properly. School programs rotate their studies with an eye to variety, and not only interest but memory directly profits by the change. Much of the bad effect of prolonged sameness in work is traceable to fatigue, but not all, as we shall see. All memory is associative. There is no such thing as memorizing things without relating them. With prolonged sameness or similarity the associations derived from a presented material, quite apart from fatigue, become confused with one another, jumbling begins, and the memory weakens, unable to straighten out the tangle of similarities. On the contrary the dissimilar are much more easily kept apart, and fatigue is less.

In the experimental investigations variety is quite commonly spoken of as vividness. Miss Calkins' investigation is well known.[1] Numbers were associated with colors in couplets, twelve couplets constituting a series. To speak first only of the vividness experiments proper, all the numbers in a series except one were black two-place numbers, the one remaining was a three-place number or had red figures, or was differentiated qualitatively from the others by some other similar means. One of the ordinary numbers was associated with the same color as the vivid number. Thus the series of twelve couplets consisted of ten ordinary couplets, one critical couplet, and one so-called normal couplet which competed with the critical in recall. The ordinary and normal couplets were two different standards of comparison for the critical pairs. In the test for immediate recall the subject was shown the colors in altered order and was asked to give the numbers which had been shown with each. The repeated color was shown only once. It might recall the vivid number alone, or both the vivid and the normal number, or the normal alone. The vivid number was recalled 52 per cent of the possible number of cases, the normal 21 per cent, while the general average of recall for the ordinary combinations of all the series was 26 per cent. Thus

[1] Calkins, M. W.: Association, *Psych. Rev.*, 1896, Mon. Sup., no. 1.

we have a measure of the effectiveness of *this kind* of vividness.[1] In other experiments the critical number was the same as the rest of the series in all respects save that it was repeated three times. This measured the effectiveness of repetition. In a similar way other experiments tested the worth of the first and last positions in the series (primacy and recency). All four kinds of critical couplets were superior in recall to the ordinary and the normal combinations by varying amounts.

The analysis of the relations of sameness and variety made in the preceding pages leads us to an interpretation of this experiment which is quite different from that of Miss Calkins. Just because there was only one of the critical pairs while there were eight or ten of the ordinary pairs in a series, all four types of experiments (vividness, frequency, recency and primacy) are studies in the effect of variety. All four of the critical couplets are a change from the sameness of the other couplets of the series. The superiority of the frequent couplet for example is due in some measure to the fact of its repetition. But its superiority is due quite as much and perhaps more to the fact, already mentioned, that the other couplets of the series are all pretty much alike, while the frequent couplet affords a change, amid sameness. It is extremely probable that if the relations were reversed, the mass of the series being given the threefold repetition and the critical couplet being given only once, the critical couplet would still be better remembered, providing that the subject knew which was the critical couplet when he saw it.[2] In the experiments of Miss Calkins on frequency the second repetition of the critical couplet furnished this clue. The isolation of the frequency factor from the sameness-variety factor could be secured only by repeating *one-half* of the series a larger number of times than the *other half*.[3]

[1] The italics are ours.

[2] "It is not the mere intensity of the stimulus which is effective in attracting the attention so much as it is the change in intensity. . . . A negative change will have the same general effect." Pillsbury: *Attention*, pp. 28–29. The same is true of memory to a less extent.

[3] The question has been worked out with nonsense syllables in the form of the worth of the individual repetitions. Cf. Ebbinghaus: *Grundzüge. d. Psychol.*, I, p. 652. Zweite Auflage.

Take next the results on primacy. Suppose that the length of the series had been three couplets instead of twelve, and that the color which had stood in the first place had been repeated in the middle place with a different number: Would primacy have been worth any more than normal (here the middle)? Probably not. Then primacy is purely another form of vividness. There is no other factor involved, as there was in the frequency case. The experiments on recency involve two factors, recency and sameness-variety. The worth of the former is attested by the fact that in memory work in general the terms just preceding the last are better recalled than their immediate predecessors. But the last term is so *very much* better recalled than the term just preceding it (which is almost as recent), that the sameness-variety factor is much the larger one.[1] To summarize the foregoing criticism, frequency and recency improve the memory apart from the question of sameness and variety. Vividness and primacy are simply species of variety. But the fewness of all the critical combinations compared with the large number of ordinary combinations gives the critical ones the advantage which comes from a change from the routine, and in view of the greatness of the numerical disparity, the whole investigation is a study mainly of different forms of vividness or, in our own terms, of variety.[2]

Aside from Miss Calkins' investigation, and the one by Dr. Gordon already described (which may be viewed as an experi-

[1] Cf. Ebbinghaus: Op. cit., p. 653. Müller and Pilzecker: *Ztsch. f. Psych.*, 1900, *Ergänzungsbd. I, p.* 264.

[2] It is significant that the percentages of recall for the normal numbers were usually less than that of the ordinary combinations. The fact is another illustration of the disastrous effects of similarity. The critical color and the normal number together are similar to the same color and the unusual number taken together. If ab bc stands for these two couplets respectively, given b (the common color), a has by no means as Titchener says (*Experimental Psychology*, vol. I, pt. II, p. 407) "a chance equal to that of the other terms of the series," for it is interfered with by a competing association, bc. For the same reason the figures given for vividness, frequency, etc., *in so far* underestimate the value of the factors since interference is always mutual. Of course the factor of dissimilarity contained in them much overbalances the interference.

ment either in variety or in complexity[1]) the writer is not aware of other experiments on the positive effects of variety. There are however a number of important and extended researches on the negative side, namely, the injurious effects of increasing similarity of associations, in comparison with which the material free from the similarity is relatively varied.

Müller and Schumann found that syllables which had been used once in old series were more difficult to learn in new series than syllables which had not been used before.[2] The old associates either appeared in consciousness and hindered the formation of the new connections, or there was a purely physiological interference manifested by the poorer recall. Here the old series and the series in which they were used again constitute together the less varied material, while the series not re-used are the more varied material.

Müller and Pilzecker made an exhaustive study of interference using nonsense syllables.[3] These experimenters demonstrated that in the case of two associations of the type ab bc the interference is mutual. Their method was in principle the same as that of Müller and Schumann, except that they employed the method of successes. Each day the subject learned four antecedent series and an equal number of sequent series. The antecedent series were normally constructed eight-syllable series. They were read in trochaic rhythm, hence each series fell into four feet, a foot containing an accented and an unaccented syllable. The series were given a fixed number of repetitions and in the recall (to come later) the subject was given the accented syllables in altered order and asked to give the unaccented ones which had accompanied each. After

[1] Because the increase was in the direction of *dissimilar* complexity. The spatial positions and the colors which were added to the syllables were themselves varied.

[2] Müller and Schumann: Exp. Beiträge zur Untersuchung des Gedächtnisses. *Ztsch. f. Psych.*, 1894, 6:177 and 318. Also Müller and Pilzecker, who on p. 83 in the work referred to in the following note summarize Müller and Schumann's results.

[3] Müller and Pilzecker: Exp. Beiträge zur Lehre vom Gedächtniss. *Ztsch. f. Psych.*, 1900, Ergänzungsbd. I.

learning the four antecedent series the subject was given the four sequent series. These were formed by combining two of the accented syllables of the antecedent series with new unaccented syllables. This furnished two of the four feet, and the other two were made of new syllables. The series may be symbolized thus:

Antecedent series: *a*b cd *e*f gh.
Sequent series: ij *a*k lm *e*n.

After learning the four antecedent and first sequent series the subject was tested on the first antecedent and the first sequent series. Then followed the learning of the second sequent series and the second pair of tests, and so on till all eight series had been learned and tested. The half of the couplets represented by the letters cd, gh, ij, and lm may be called the normal couplets; the other half, ab, ef, ak, and en, the interference couplets. The former were simply a standard with which the interference in the latter might be compared. When the antecedent series was tested (by giving a, c, e, and g), k and n interfered with the recall of b and f. When the sequent series was tested (by giving i, a, l and e), b and f interfered with the recall of k and n. The following are typical results.

Percentage of Correct Recalls.
(Number of couplets 168)

	Per cent
Normal Couplets, Antecedent Series	66
Interference Couplets of same	36.5
Normal Couplets, Sequent Series	63.5
Interference Couplets of same	52.5

The next day the tests were repeated and the relations of the two interference groups to their respective standards were found to be reversed. It was now the sequent series which showed by far the greater loss. This reversal is a study in the effects of recency, into which it is beside our purpose to go.[1]

[1] While the second learnings were still fresh in mind a and e suggested their second associates without *much* difficulty. But when the tests were put off or repeated later, the first associates became the stronger.

The extent to which the interference might go may be seen in the fact that in some cases the interference recall in *both* antecedent *and* sequent series together was less than the normal recall of the antecedent series.

It is probably unnecessary to point out the relevancy of these experiments to the question of the effects of similarity and variety upon memory. The normal couplets are relatively a more varied material than the interference couplets. While the sameness in the latter is planned with malice aforethought, so to speak, the same thing happens in daily experience where the sameness is unintentional, and not so exactly measured. The experiments of Ranschburg with numbers, described below, illustrate how daily experience is full of the ab bc type of similarity.

Ranschburg has confirmed the results of Müller and Pilzecker and Müller and Schumann.[1] Most of the details are omitted here because of their similarity to the two works last described. Using the method of successes this author arranged what he calls heterogeneous and homogeneous series. The heterogeneous were simply normal eight-syllable series. Of the homogeneous the *first* series was also normal, but the rest were not, for they were all identical with the first in respect to their consonants. Only the vowels were changed in each succeeding series. These two kinds of series were learned in parallel fashion. The confusing result may be imagined, especially when an accumulation of eight series of each kind, the learning of which had extended over a number of days, was given a few renewing repetitions and then tested "run together." His experiments with words are somewhat newer. In these latter various series of word-couplets were devised in which the words in a couplet were closely germane in thought, but the couplets were drawn from quite disparate thought universes. In other series the couplets were internally as before, but several couplets were drawn from similar and in some cases practically the same thought universes. The similarity

[1] Ranschburg. P.: Ueber die Bedeutung der Aehnlichkeit beim Erlernen, Behalten, und bei der Reproduction. *J. f. Psych. u. Neur.*, 1905, 5:93.

was entirely on the side of thought. In recall the subject was very much more certain of the dissimilar series, and the percentages of recall were much higher than in the similar series. In the latter the subject was quite apt to hesitate between the correct response and a word in another couplet which was similar in meaning, but might be quite removed in the time of learning.

In another investigation on the conditions of perception of similar and dissimilar numbers Ranschburg showed that the fusion of similar things which we have seen taking place in *memory* exists even in the *perception*, when the time of exposure is short.[1] Indeed he traces the memory confusion back to perceptual confusion, in cases where the successive presentations possess considerable similarity. The experiment is for this reason relevant to our problem: it offers an explanation for a considerable part of the interference in similar memory material. It is also important for our problem because it endeavors to measure the degree of interference due to *inherent* similarity, and not to the fact that the associations were made to interfere. In experimenting upon the span and accuracy of visual perception, using six-place numbers exposed one-third of a second, Ranschburg observed that a large proportion of the errors were traceable to the inherent similarity of certain figures, for example 6, 9, and 0, 3 and 8, 9 and 2, 1, 4, and 7. He was able to determine the nature of the errors with considerable certainty because in nearly all cases they involved only one or two figures, and the figures were nearly always in the fourth or fifth positions, less often in the third, from the left. Where only one figure was wrong, it belonged in nearly every case to one of the five following types: (1) Substitution of a similar reproduced number, e. g., an 8 for a 3; (2) assimilation to a similar nearby figure, e. g., 684*2*2*3* instead of 684*2*9*3*; (3) assimilation to a dissimilar nearby figure, e. g., 162*4*4*5* instead of 162*8*4*5*; (4) change of one of two identical figures to any other figure; (5) substitution of a figure either preferred for

[1] Ranschburg: Ueber Hemmung Gleichzeitiger Reizwirkungen. *Ztsch. f. Psych.*, 1902, 30:39.

some temperamental reason or connected closely in experience with the given figure. The fifth seldom occurred. Where two figures were wrong nearly all the cases were either simple inversion (78 instead of 87) or a combination of inversion with one of the types enumerated above. The first four classes of errors are types of similarity. The fifth and inversion are not. The large proportion of similarity errors makes it highly improbable that they are due to chance.

In the light of this statistical analysis of the errors in numbers not devised to test similarity, but cut from a statistical handbook, he constructed numbers of several different kinds of similarity for the purpose of finding out whether "the threshhold of perception is higher for homogeneous than for heterogeneous material." In the first experiment he gave in the same way as before 20 six-place numbers lacking similarity internally, and 20 six-place numbers two figures of which, usually in the right part of the series, were identical, e. g., 119495, or 141993. In a second experiment the critical figures were only similar. The results confirmed his hypothesis. Where two of the figures were similar, the errors were about three times as great as in the dissimilar numbers, but where two figures were identical, the errors were four times as great as in the dissimilar numbers. The threshold of perception is therefore higher in proportion to the similarity. These figures are valuable because they are among the few, to the writer's knowledge, in which the amount of visual similarity is controlled. Whether they bear upon the effects of similarity on *memory* is another question. There is no objection to viewing the responses of the subjects as immediate recall, providing the conditions of exposure are comparable to the conditions of exposure in memory work. It may be said that in memory tests the subject is assured of a long enough interval to properly perceive the material. But it may be quite fairly said in reply that while the absolute exposure is usually much longer than was used here, *the adequacy of it to distinguish the terms from each other depends upon their similarity.* I should say that the conditions are comparable to those in memory in many cases. On the other hand the confusion in such experiments as those of Müller and Pilzecker are not confusions in perception but in memory.[1]

To summarize the various investigations of the influence of relative sameness and variety on memory: anything which serves to distinguish a thing from a group makes it better remembered. A greater length, a different color, a more frequent repetition, an unusual position spatially or temporally are some of the variations which have been shown to be effective. Where the similarity results from the triangular association of two things with a third, if the recall proceeds from the common element towards one of the extremes, the other extreme interferes and diminishes the chances of recall. The result is the same if the start be made toward the other extreme. The conflict may be purely physiological, and apparent in consciousness only as a delayed recall. The injurious effects of similarity are however not confined to artificially constructed overlapping associations, but also occur where the similarity is inherent in the nature of the material. In contrast with this, wherever the different parts of the material learned are dissimilar, the associations are more lasting and their recall proceeds more promptly, because they do not interfere with one another. It has also been shown that the threshold of perception is higher for similar material than for dissimilar. It requires a longer time to get a clear perception of a material the parts of which are similar than it does of one the parts of which are not similar. The differentiation here referred to is however purely involuntary and almost instantaneous. If the perceiving process is cut off before clearness has been reached,

[1] Other experiments on the effects of similarity on memory are those of Bergström on the interference arising from sorting the same cards into different arrangements of the same positions, in *Am. Jour. of Psych.*, 1893, V, 356, and 1894, VI, 432. Also Münsterberg, in *Beiträge zur Exp. Psych.*, Heft 4, p. 69. Both of these deal with the interference of automatic habits of movement. The latter shows how opposed habits, after being made automatic separately, may be used for alternate periods of considerable length without interference, e. g., keeping one's watch first in one pocket and then in another, the same arm being used for both movements. It certainly offers something for reconciliation with the otherwise uniform outcome of the similarity literature.

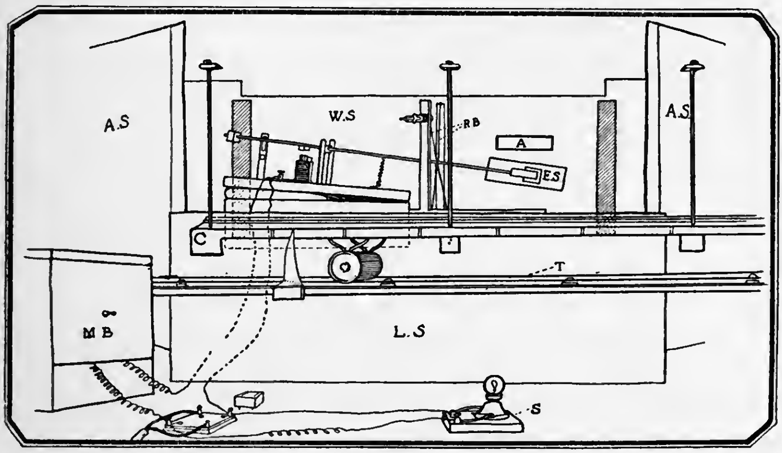
the errors in recall are largely of the type of confusion of similars.

Summarizing the influence of complexity on memory we may say: the complexity afforded the mind by sentences or connected passages is superior to the simplicity of disconnected words because of the unity of the whole passage and the resulting greater differentiation of the parts. In assimilating a simple linguistic material such as nonsense syllables, letters or even disconnected words, a person not only tries to give it a unity by connecting it with his experience, but also seeks differentials by which to distinguish the parts; and in the absence of preformed associations which may serve the purpose (for example the English words suggested by nonsense syllables), any peculiarity such as spatial location, background, etc., will be utilized. But the peculiarities thus utilized are increases in complexity. In these cases the secondary associations (location, background, etc.) are formed during the memorizing of the principal associations.

We are thus brought to the problem of the present investigation. Two factors are involved which are inversely related to each other. Increasing the complexity diminishes the similarity, and this is favorable to memorizing. On the other hand, *if the material is not a unity in advance*, complexity increases the association processes, the mental effort presumably required, and consequently the possibilities of distraction. *Under these conditions* how much will variation added to a material, highly similar in itself, shorten the learning time and improve the retention? Is there a resulting profit to the memory only when the original material is highly similar, or may it possess considerable variety? When does distraction enter? Is there a relation between the amount of complexity which a person will make use of and his retentiveness?

II. APPARATUS

With slight changes the same apparatus was used throughout the investigation. The essential part was an electro-magnetic shutter which by its movements opened and closed a small aperture in a screen. The screen was 2 m. 11 cm. long and 58 cm. high. Near the middle was an aperture 9.5 cm. wide and 2 cm. high. The subject sat in front of the screen, and when he looked at a series he brought his eyes within about 3 cm. of the aperture. Behind the screen, 74 cm. from it and parallel to it, was a track somewhat longer than the screen, supported by adjustable standards, and carrying a car 1 m. long and 11 cm. wide. On the floor of the car and running the length of it was a groove, into which any kind of series



EXPOSURE APPARATUS

The apparatus is described in the text. *A* is aperture; *E S*, electro-magnetic shutter; *T*, track; *W S*, wide screen; *C*, car; *A S*, adjustable screens; *L S*, lower screen; *S*, shunt; *M B*, metronome box; *R B*, rubber bands, upper and lower, which limit the play of the shutter.

mounted on heavy cardboard could be slid preparatory to exposing it. On the back of the car were four upright steel rods 25 cm. high on the top of which were four spring clasps. Small cardboard flaps on the back of the series when put into the spring clasps held the series rigidly in place. Two black screens 71 cm. high and 56 cm. wide, extending from the large screen on the front of the tables to the track, could be so adjusted by the operator as to confine the subject's gaze to as small a space as desired. They could be widened so as to expose a whole series at once. The car would of course then remain motionless during exposure. Or they could be narrowed to the width of a single term of a series, 15 cm. for example. For successive exposure the car would then be moved past this space by the operator during the intervals while the aperture was closed. The great advantage of the apparatus was that whenever the subject was looking the series did not move. A scale, 25 cm. in length on the front of the track, enabled the screens to be quickly adjusted to any desirable width. The operator was cut off from the subject by a strip of cardboard extending across the upper part of the space between the converging screens. The essential part of the apparatus, the shutter, remains to be described. This consisted of an iron lever 53 cm. long carrying a movable counterpoise on the short arm and the shutter on the end of the long arm. An electromagnet under the lever drew down the short arm when the circuit was closed, at which the long arm carrying the shutter moved up and closed the aperture. When the circuit was broken a coiled spring connected with the wooden base drew the arm down and thus opened the aperture. The current was led first to the commutator, then through the electromagnet and a metronome. A small rheostat connected at the commutator with the apparatus furnished a shunt to the current at the moment when it was broken by the metronome. The metronome, enclosed in a felt-lined box, regulated the movements of the shutter, while a small rod passing through the wall of the box intercepted the pendulum or set it free when desired. A year's constant use of the apparatus has led to the conclusion that it is a satisfactory exposure mechanism for memory work of this kind.

III. EXPERIMENTS WITH LANGUAGE MATERIALS

The first effort was to extend the experiments of Dr. Gordon. Series of nonsense syllables of the length used by her (nine syllables) were varied in *size, color and style of type.* The rules of Müller and Schumann were followed in constructing the series. No syllable was used twice. The variations were first tried out separately and later in combination. Variations in size were secured by using Willson Gummed Letters numbers 1, 20, 21, 23 and 25. The largest letters were 38, the smallest 7 mm. high. The letters were all of heavy type, the lines of the largest being 8 mm. thick. Thus a strong difference in sensation was secured. A medium size, No. 21, was used for the standard and colored series.[1] Color variation was obtained by painting white letters red, orange, yellow, green, blue, brown, neutral gray, and black. White furnished the ninth variation. In the series in which the size varied there were two syllables in each of four sizes and one in a fifth size, while in the color-varying series each syllable was of a different color or brightness. Five different styles of lettering were used in the form-varying series, all presenting, as nearly as could be, about the same area of stimulating surface as the standard letters. The styles of type used were the most varied that could be found and still be legible. They were (1) a heavily shaded style of print, (2) old English, (3) plain script, (4) an alphabet of closed letters similar to the old English, but having more flourishes, and (5) an alphabet of hollow letters. All of these were done in black by hand. Four series of each kind of variation, size, color, and style of type, were given to six subjects without varying the spatial position of the syllables.[2] Thus the value of each variation separately was obtained. The effects of practice were distributed by giving four series each week, one of each kind, and rotating the time situation of each kind each week. A hard rubber mouth piece held between the teeth was used to aid the subject in eliminating enunciation. It was found as shown in the table below that in immediate recall the series varying in size gave slightly better results than the standard—a gain of 9 per cent, but that color variation was of no aid, while variation in the style of print was a detriment. In recall after 24 hours, however, the result was quite different. *Size was again the*

[1] The standard series did not vary in size, color or style of type. They were the standard with which the others were compared.

[2] For further details, see p. 25.

best variation, showing a gain over standard of 58 per cent. Color was next with 32 per cent gain, while even form variation showed a gain of 19 per cent.[1]

The next step was to increase the complexity by combining the variations. In each syllable shown him the subject faced a new combination. The most complex type were series which varied in four ways: size, color, form or style of lettering and spatial position. A second type varied in two ways only, the two which in all probabliity were the most effective singly, namely, position and size. The third type was simply the standard without variation in position. Four series of each were given as before, and it was found that one had not far to go to reach the limit of effectiveness in this direction. In immediate recall the two variations showed a gain of 15 per cent, while the series containing four was no better than standard, a gain of 3 per cent. *In recall after 24 hours the two variations showed a gain over standard of only 4 per cent, while the four gave the same as standard again. The improvement shown by the subjects in the standard, 56 per cent gain over the showing made in the first four weeks, is worth noting.* It is due to practice. The unexpected fact is that the size variation with the addition of position variation has not gained proportionately. This is probably due to a concealed loss consequent upon increasing the complexity. In any case taken with the fact that four variations gave the same as none (5.21 and 5.21), it shows that the limit has been reached with this material, unless more effective variations can be found.

During the fifth and sixth of the eight weeks spent on syllables, the combination of size and position, and the combination of four variations were tried with words. It was expected that the variations would be of no positive value here because words contain so much variation in themselves, especially in meaning, as compared with syllables. The result as shown by the table, based on two series of each kind, confirmed the expectation. Interesting light is thrown by the introspections of the subjects on the cause of the failure. They made mean-

[1] The six horizontal rows of figures in the tables represent the six subjects, and give the average number of syllables or words out of nine correctly recalled.

ing-classifications such as 'farm products' or 'machinery,' which united not only two or more words in the same series, but even words from different series. With rare exceptions, position was the only mechanical aid, but even it was not needed in view of the variety afforded by meaning. In syllables they found variety in ways not provided by the operator, all of which is on the positive side of our thesis, but which cannot be reduced to tables. At the outset of the experiment the subjects were asked to lend their coöperation in shifting the emphasis of the attention to the variations introduced by the operator, and they did so. Nevertheless, resemblance to words, despite their efforts to exclude it, was one of the two main aids in recalling the syllables, the other being position. The fact that an error had been made in the case of a certain

TABLE I. *Showing averages and mean variations.*

A. NONSENSE SYLLABLES.

Immediate Recall.

EFFECT OF VARIATIONS TAKEN SINGLY.								EFFECT OF COMBINING VARIATIONS.					
Standard.		Colors.		Sizes.		Forms.		Standard.		Two Var.		Four Var.	
Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
6.25	1.25	6.84	1.01	7.92	.29	6.67	.50	7.34	1.00	8.41	.29	7.00	.50
6.67	1.17	6.75	.46	7.42	1.04	4.92	.79	6.59	.79	8.00	1.00	7.58	.80
7.33	.83	7.34	.34	8.50	.75	6.08	1.54	6.17	.83	6.83	.96	6.33	.83
5.25	1.75	4.50	1.00	6.00	.84	5.42	.92	8.33	.66	6.66	1.33	7.16	1.42
5.25	.29	5.92	.77	6.17	.59	6.09	.90	6.42	1.08	9.67	2.00	7.17	1.17
7.08	1.59	6.75	.88	5.33	.50	5.75	1.75	5.33	1.08	6.50	1.00	6.00	1.00
6.31	1.15	6.35	.74	6.89	.68	5.82	1.08	6.70	.91	7.68	1.10	6.88	.96
Recall after Twenty-four Hours.													
3.25	1.38	4.25	1.88	4.00	1.00	3.75	1.75	7.25	.88	7.25	1.25	7.25	1.75
4.25	1.19	3.25	1.38	5.50	1.25	4.50	1.50	4.75	1.88	5.75	1.38	3.50	1.25
.50	.75	3.75	1.25	3.75	.75	2.25	1.19	5.50	1.50	6.25	1.75	4.75	1.25
2.25	.38	4.00	.50	4.50	1.50	3.25	1.25	2.50	1.55	2.25	1.38	3.00	2.00
4.25	1.25	5.00	1.00	6.25	1.75	5.00	.50	5.00	1.50	5.00	1.00	5.25	.88
5.50	1.25	6.00	1.50	7.50	.50	5.00	1.00	6.25	1.75	6.00	1.00	7.50	.50
3.33	1.03	4.38	1.25	5.25	1.13	3.96	1.20	5.21	1.51	5.42	1.30	5.21	1.27

B. WORDS.

Immediate Recall			Recall after Forty-eight Hours.		
Stand.	Two Var.	Four Var.	Stand.	Two Var.	Four Var.
7.2	7.00	6.2	2.94	3.40	1.69

syllable individualized the syllable and was likely to fix it. Thus one subject said paradoxically that the syllables that he didn't get in immediate recall were the ones he got in the test for permanence.[1] For a while white type on white cardboard individualized and fixated its syllables more than colors or grays, simply because it was difficult to see. A doubt as to the identity of a letter, after being settled, was certain to fixate the syllable concerned.

If meaning is a kind of variety which persons naturally seek, as the work with nonsense syllables and words indicated, it should be susceptible to measurement. A series of twenty adjectives, and another of twenty short sentences were given to five subjects. The adjectives were:

Fat, humble, young, happy, broad, distracted, violent, privileged, sleek, reversed, cautious, immediate, ideal, polite, cold, gabled, serene, imaginative, various, trifling.

The sentences were:

Mammoth Cave is a wonderful place. Cats are not very teachable. He gave a prodigious sniff. The naval gunner is often a noble fellow. Chicago is a windy city. Genteel is a word seldom used today. Damp weather is bad for rheumatism. The need of protecting our industries is a worn argument. The mistletoe is a parasitic plant. Dogs are sociable companions for men. The theatre is potent for good as well as evil. Gambling is expensive. The steamer hit the dock a hard bump. This thread is rotten. Parks at public expense are easily justifiable. Granite is a handsome building stone. The lake is rough. The Japanese are a small race. Is abusive language ever justifiable? The owner of the horse gave a sarcastic smile, and declined the offer.

The subjects were allowed two minutes in which to study each of the series, which were given them in typewritten form.

[1] The series were shown again between immediate recall and the test for permanence See IV below.

Following directions they did not run together successive terms into larger units, and tried not to expand words into phrases or sentences. They were required to read each list through at least once. In the test for immediate recall and for retention after twenty-four hours they were required to give only the substance of the sentences, but the words verbatim. *They recalled an average of 12.8 words and 11.6 sentences in immediate recall, and 7.6 words and 7 sentences after twenty-four hours.* One of the five did worse in the sentences than in the words, the other four did as well in one as in the other. The subjects said it was difficult to keep the words from arousing mentally a thought-situation which expressed in language would have been one or more sentences. Yet it is not to be inferred that the two lists became equivalent. The *tendency* existed in that direction, and the attempt to inhibit it was partially a failure. The difficulty of controlling the material led to its discontinuance. Perhaps the significant feature, aside from the tendency toward expansion mentioned, is the fact that *the sentences are remembered as well as the words.*

Numbers are a material apparently difficult to read connected thought into, especially when given in extended series. They seem at times not very far from a dead level of monotony. Yet they may be vivified by associated material. Three series of each of the following two kinds were given: on the one hand series of two-place numbers, seven numbers in a series; on the other hand three series of similar numbers and beside each a biographical fact from the Renaissance period of Italian history. The facts were varied and some were striking. One of the series of numbers and facts was as follows:

Series III.

51 A sculptor.

93 Famous for an impromptu oration in Latin congratulating the Emperor Frederick III on his coronation.

14 A merchant of Pisa. Introduced the Arabic notation into European commercial life.

47 A monk.

26 Naples.

79 An instance of the revival of oratory. Funerals, marriages and installations of bishops were among the occasions of his efforts.

85 A bishop.

It was expected that the numbers without facts would be learned in less time than those accompanied by facts, but it was thought that the latter might be retained better. Two new series were given each week, one of each kind. Typewritten cards each containing a series were given the subject, one at a time. In the case of the numbers without the facts he memorized the list as rapidly as possible, the time record being kept by the operator with a stop watch. As soon as the subject thought he could give the list correctly he tried. If he failed, the learning process was resumed, and this continued until he gave the list correctly. The next day the series learned on the previous day was tested, and if an error was made, that series was relearned, and so on until the subject held the series correctly for 24 hours. This was the standard of learning up to which the series were brought before they were dropped. With no subsequent renewings they were tested for permanence of retention seven days and again thirteen days after the completion of learning. The series of numbers and facts were learned in the same way, except that in the tests the subject was required to give the substance of the facts with their appropriate numbers. We refer here to the learning process.

The results are given in Table 2. The first half gives the total learning times in minutes and seconds. 3:31 means 3 mins. 31 secs. In the second half, in which the recall is given, the figures indicate the number of numbers, or numbers and facts which were correctly recalled. Seven would be a perfect score for a series. No credit was allowed for partially correct numbers, or for numbers or facts apart from their correct coupling. Several cases where the recall could not be secured are marked by dashes in the table.

The numbers alone are learned in less than one-half the time taken by the numbers and facts, and are retained quite a little better.

This ended our experiments with language materials. We may summarize them as follows: Typographical variations

TABLE 2. *Renaissance Series.*

LEARNING TIMES.

Subjects.	NUMBERS.			NUMBERS AND FACTS.			AVERAGES.	
	I.	II.	III.	I.	II.	III.	Nos.	Nos. and Facts.
McP	3 : 31	1 : 50	2 : 39	9 : 15	7 : 30	6 : 26	2 : 40	7 : 44
G	: 57	1 : 43	1 : 23	10 : 15	8 : 11	9 : 37	1 : 21	9 : 21
H	7 : 17	1 : 36	3 : 28	7 : 50	4 : 41	5 : 25	4 : 07	5 : 59
J	2 : 40	3 : 27	3 : 49	10 : 28	10 : 09	12 : 24	3 : 19	11 : 00
L	6 : 18	5 : 20	3 : 42	9 : 36	6 : 21	7 : 06	5 : 07	7 : 41
F	4 : 26	3 : 51	3 : 23	8 : 00	6 : 35	5 : 05	3 : 53	6 : 33
Av	4 : 12	2 : 58	3 : 04	9 : 14	7 : 15	7 : 41	3 : 25	8 : 03

NUMBER OF TERMS RECALLED.

	AFTER SEVEN DAYS.						AFTER THIRTEEN DAYS.					
	Numbers.			Nos. and Facts.			Numbers.			Nos. and Facts.		
	I.	II.	III.	I.	II.	III.	I.	II.	III.	I.	II.	III.
McP	7	4	7	7	1	6	5	0	7	3	1	5
G	7	4	2	7	1	2	3	1	5	5	0	2
H	3	6	7	6	4	5	3	5	–	5	3	–
J	1	2	0	2	4	7	0	0	–	0	4	–
L	1	4	7	3	1	4	1	2	7	2	1	4
F	5	5	5	3	2	2	4	3	5	1	1	2
Av	4	4.2	4.7	4.7	2.2	4.3	2.7	1.8	6	2.7	1.7	2.3

AVERAGES.

	Numbers.	Numbers and Facts.
After seven days	4.3	3.7
After thirteen days	3.2	2.4

are at first a strong aid in retaining nonsense syllables, but in the course of two and a half months the advantage of series varying in this way is lost, because of improvement of the ordinary series through practice. With words the typographical variations are of no aid whatever. The meaning variation is far more important and is the kind of variation relied on. The results have an interesting bearing on the theory of advertising and printing. Our own conclusion is that if typographical variations have any value here for memory they must offer a constant novelty. *Mere variation* without a very considerable amount of newness is of no aid. If there is to be simply change from one familiar variation (or even new combination of familiar variations) to another, then the variation in the sense is the more attractive and influential, in fact the only thing that is influential. This may tax the printer rather severely, but it is some encouragement to those who depend on the content of their advertisements, rather than on the form.

When mechanical variations are left and the attempt is made to secure variety through meaning, the first part of our results are positive, the second part indecisive. Sentences are as well remembered as words, when the learning time is long enough to comprehend both. Binet and Henri and others had already shown this, but our lists of words and corresponding sentences were much longer than theirs. A still further increase in the length of the selections would without doubt show the same result. The negative results of our experiments with numbers and biographical facts are to be interpreted as a superiority of preformed associations over associations required to be formed during the learning process. In the series of numbers without facts the subjects could not prevent the numbers from suggesting associations from their past experiences. 57 became 57th St., 65 the age of a member of the family, etc. Dates, too, were suggested by some of the numbers. The impossibility of controlling the associations led to the discontinuance of the use of numbers. A similar difficulty has been mentioned already in connection with the use of lists of words.

In the next section we propose to make a brief digression to discuss a question of nonsense syllable technique. It may be omitted without detriment to the comprehension of the main problems under consideration.

IV. ON METHODS OF LEARNING AND TESTING NONSENSE SYLLABLES.

The method of learning the series of nonsense syllables in III consisted in giving the various series to be compared equal numbers of repetitions, extending the learning process over two days, and securing an immediate recall on the first day and a recall after 24 hours on the third day. On the first day the subject was given 2 repetitions of a series in practically immediate succession and then tested for immediate recall, the test being the quantity of syllables recalled and the cue, simply the direction to begin. This was followed by 5 repetitions. The next day the subject spent from 5 to 10 minutes, the exact amount determined by the operator and depending on the number of series, in recalling, as before, as many syllables as he could. Placing in the proper series was understood to be not essential. This was followed by 3 repetitions. On the third day without further seeing the series and with no other cue than an enumeration by the operator of the varieties of series which had been given, the subject again wrote as many syllables as he could recall. There was no time limit on this test. The intervals between repetitions and series were properly regulated, and kept constant. Reviewing was permitted during the progress of a repetition but not between repetitions. The rate was 50 beats of the metronome per minute with an exposure on every alternate beat. The large percentages of recall in the tables speak in favor of the workableness of the method.

The method has advantages over several of those currently in use. In the Ebbinghaus method of entire learning and relearning, if the series are short, for example nine syllables, the repetition is too gross a measure to detect small differences. The difference of 1 repetition between the 4 and the 5 (let us say) necessary for perfect immediate recall of two nine-syllable series may represent the recovery of a single consonant, or 18 seconds of hard work on the part of the subject. Our method gets over this difficulty by giving a fixed number of repetitions, the same for the various types to be compared and measures the difference by the percentages of recall. The method of successes, used by Müller and Pilzecker and many others, also has this advantage, but has one objection that in our opinion is fatal to it for many subjects, in fact most subjects. It encourages word associations. The two syllables constituting a couplet are likely to suggest to a subject single words or phrases which form a unity in his past experience and serve here to link the two syllables together. Gan muc suggests gander mud, laj gul suggests large girl. The difficulty is a well-known one and is treated by Ebbinghaus in his Grundzüge, p. 676. It is present to some extent in the non-couplet methods, but the couplet methods (Treffermethoden) greatly increase the frequency of occurrence. In an investigation made some years ago it was demonstrated that couplets in which word associations of this sort occurred were better retained than the rest of the series, and the superiority was measured.[1] Under certain conditions, which do not concern us here since they were the same for both kinds of material, couplets in which word associations did not occur had a recall of 63 per cent correct after one day, which sank to 19 per cent after fifteen days more. In contrast with this, couplets in which word associations occurred had a recall of 82 per cent correct after one day, which sank only to 64 per cent after fifteen days more. The results were based on six subjects and 64 couplets altogether, of which somewhat the larger share were couplets in which no word associations occurred. If the couplets in which word associations occurred were thrown out or separated, there would be no objection to this method, but the waste of effort is great, and it has never been done to the writer's knowledge, except in the investigation just quoted.

Our method makes an economical use of the experimenter's and the subject's time by using short series and distributing the learning over two days. By making the amount recalled, instead of the number of repetitions required to learn, the means of measurement, it measures small differences. It keeps the word association factor at a minimum. If it has an objec-

[1] Peterson, H. A. Recall of Words, Objects and Movements. Psych. Rev., 1903, Mon. Sup., iv, p. 232.

tion, it is the possibility that the recall after the last twenty-four hours may not be enough for purposes of comparison. This is doubtless true of some subjects. In the foregoing experiments, P was such a subject. This objection would not hold for the five other subjects employed by us, with all of whom the recall was sufficient for comparison.

V. EXPERIMENTS WITH PLANE AREAS

1. Adding Variations to a Material Containing Little Variation

The next material selected was both simple and highly similar internally. Different shades of gray were associated with their spatial positions on a cardboard area. Four series were given, the first of which varied in shades of gray only, the second in gray and the color of the center, the third in gray and the size of the different presentations of the series, and the fourth in gray and the shape of the presentations. The same seven grays were used in all four series. They were the Hering papers Nos. 1, 2, 5, 8, 19, 35 and 49. Each series had seven terms or presentations in it, and the shades of gray ranged from black to white with about equal differences between nearest shades. All seven terms were exposed simultaneously. The gray sizes may be given as a sample series. They were, from left to right as follows:

Oblongs, height twice the width. Dark gray 128 sq. cm.; very light gray, 40.5 sq. cm.; white, 60.5 sq. cm.; black, 2 sq. cm.; medium gray, 84.5 sq. cm.; light gray, 8 sq. cm.; very dark gray, 24.5 sq. cm.

The form-varying series is shown in the plates in the Appendix.[1] All of the series except the one which varied in size were composed of terms whose areas were 50 sq. cm., and even the size series had the same total area as the other three, viz: 350 sq. cm. The color-varying series were made by pasting small oblongs of color 1.5 cm. wide and 3 cm. high on the gray oblongs, themselves 5 cm. wide and 10 cm. high. Each of the series was mounted on a sheet of white cardboard 22 × 28 inches, with the longer side as the base.

[1] Gray Forms.

Conceivably the ease of memorizing such series would depend greatly on the order of succession of the terms. However, inasmuch as one of the purposes of the investigation was to obtain information on this point, aside from certain obviously easy arrangements, it was thought best to determine it by chance. The following cases embrace all exclusions: (1) in sizes, a continuous increase followed by a continuous decrease, or vice versa; or a continouus increase alone or decrease alone; (2) in shades of gray, or violet (the latter series to be described later) a continuous change from black to white or vice versa, or from red to purple or vice versa; (3) in grays, colors, and sizes, the same arrangement as some previous series. This rule resulted probably in less uniformity of material, but at least was free from the influence of unconscious subjective favoring.

To increase the number of series a group of four series was next made which in all respects was similar to the gray group except that shades and tints of violet were used. The colors in the series with colored centers, and the shapes in the form-varying series were different from what had been used in the gray group. The shapes are given in the plates. The same sizes were used in the size-varying series as in the grays, but to diminish interference from the gray sizes they were right triangles. The effort was made to secure violets which would be about as difficult to discriminate as the grays. The ones selected were: blue violet, violet, violet tint 1, red violet, red violet tint 1, violet red. To these purple was added. The grays were learned the first week and the violets the second week.

The method of learning the series was the same as the one used in experiments with numbers and biographical facts. We shall describe the process in its several steps. When the shutter opened the subject began to learn the series. As soon as he thought he had mastered it, so that he could give it the next day, he gave a signal, whereupon the shutter closed. The subject then turned to a small table on his right, uncovered a set of unmounted duplicates of the series, and tried to arrange them in the right order. As soon as he had arranged the series as best he could he gave a signal, whereupon the operator at once removed the duplicates. If the attempt was unsuccessful, the duplicates were returned bunched and covered, the series was again exposed and the learning process resumed, and again tested, till the series had been perfectly arranged. The lengths of exposure were kept with a stop-watch.

Where more than one exposure of the series a day was necessary, the lengths of the exposures were recorded separately, and the number of arrangements which the subject required. The next day the series given on the previous day were tested again by use of the duplicates. If the arrangement of any series contained an error, that series was again exposed for such a length of time as the subject required, and again tested. The subject could not omit the arranging if he desired. If he showed by his preliminary arrangement on the second day that he had held the series perfectly for a day, it was not again shown. Having been brought up to the standard, the series was dropped for thirteen days, when the retention test took place. The learning was usually completed by the third day. The purpose of all this was to bring the series up to a point of immediate fixation that would insure enough recall after two weeks for purposes of comparing the different types of series. The test which was given after thirteen days consisted in the subject's again arranging the series. No introspections were allowed and no comments could be given till after this test, because such introspections or comments would fixate the series, and do it unevenly. For introspections our main reliance was on series given especially for this purpose and not recorded in the tables. Reviewing after an arrangement had been made was not allowed. Having arranged the duplicates the subject who followed instructions dismissed the series from his mind. The intervals between exposures and between series were regulated.

Toward the end of the investigation a series of experiments was carried out upon the same subjects with similar material for the purpose of determining the value of the practice of allowing the subject to arrange. The details are given on pp. 65–7, which may be profitably read at this point. The method employed consisted in giving parallel series as nearly equal in difficulty as possible. In one-half of them the learning was carried on solely by looking at the series through the aperture. The other half were given the same lengths of exposure and in addition the subject was allowed the usual arrangement of duplicates. The recall after the lapse of a week gave the relative efficiency of the two methods of learning, and by subtracting the total recall of all the series in which there was no arranging from the total recall of the series in which arranging had been allowed, the value of the arrangements alone was obtained. Since a given number of arrangements produced a certain number of units of recall and a given amount of time of exposure produced a certain number of units of recall, by reducing both to the amounts of each necessary to produce one unit of recall it was possible to find for an arrangement its equivalent in minutes and seconds of learning time. With this average value, one for each subject, the arrangements were converted into learning time and added to the amounts actually consumed by a subject in looking at the series through the aperture. Thus our two time measurements were reduced to one.

This in brief was our method. In one way it lacked exactness, namely, in the distribution of the learning periods over varying numbers of days. But in another way it had exactness. By making the subject the judge of the time necessary for learning, learning times were secured which minute for minute represented equivalent degrees of effort. A comparison of the subjects' speed of learning given on page 64 will convince any one of the impossibility of getting accurate results when giving the same exposure intervals to all subjects and for all materials. The difficulty of appraising the value of the tests allowed the subjects during learning—a difficulty which has been felt by many experimenters on memory—was satisfactorily solved by the method described. Again, one of the principal advantages of our method is the opportunity it affords for studying different types of learners. The fact that one series was learned more thoroughly than another, that the subject could not judge accurately his ability to hold a series for a day, did not introduce error into the results if the assumption be granted that recall after thirteen days is in proportion to the thoroughness of the original mastery. Any one who doubts this can find evidence in our tables in the form of mean variations.

The use of duplicates as a means of testing was adopted to avoid as far as possible the inequalities which would be introduced into the material by the ready presence or absence of names. Lehmann[1] and Angell,[2] among others, have shown that colors which can be readily named are better discriminated and remembered than those which cannot be. Again, the desire of the author was to guide the whole investigation away from language and into retention and learning of the material presented. The initials of the colors could be made into a mnemonic word and thus remembered, and similarly sizes and forms. There was no desire to experiment in the linguistic field here, and the subjects were repeatedly turned in the opposite direction and, judging by their own testimony, with success. The part played by language is discussed on p. 47 in connection with methods of learning.

The results of the gray and violet sets are given in Table 3. The learning times are given in minutes and seconds and arrangements; for instance, F. learned the grays in 4 mins. 1 sec. and 2 arrangements. The abbreviation 'arrs.' means number of arrangements which the subject required in bringing the series up to standard. The recall in the second half of the table is based on a method of scoring which briefly was this. Perfect score for a series: 14. Each term allowed 1 for correct absolute position (left end, or second from there, etc.) and 1 for correct relative position (having the correct terms on both sides of it). One-half for next-to-correct absolute position, e. g., 4th or 6th from left end instead of 5th. One-half for one neighbor correct, and one-quarter for each neighbor correct but on the wrong side. Terms whose relative positions are made incorrect by the errors of the other terms lose no credit themselves.

The table shows that when the shades of gray and violet are varied in form and size, or when they are given colored centers, there is a marked reduction in the learning times and a large

[1] Phil. Studien, V, 1889, p. 96.

[2] Phil. Studien, XIX, 1902, p. 1

TABLE 3. *Areas varied in Color, Size and Form.*
A. Learning Times.

SUBJECTS.	GRAYS.		G. FORMS.		G. SIZES.		COL. GRAYS.	
	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.
F	4 : 01	2	2 : 42	2	3 : 22	2	3 : 30	2
Mc.P.	5 : 10	7	: 54	2	1 : 02	5	2 : 01	4
G	1 : 48	8	: 30	3	: 52	7	: 46	2
H	1 : 51	2	: 43	2	1 : 38	4	2 : 15	2
J	4 : 24	3	1 : 12	2	1 : 47	2	2 : 20	2
L	3 : 34	5	1 : 25	2	1 : 37	4	2 : 21	4
Av.	3 : 28	4.5	1 : 14	2.17	1 : 43	4	2 : 12	2.67

	VIOLETS.		V. FORMS.		V. SIZES.		C. VIOLETS.	
F	4 : 34	3	: 55	2	1 : 42	2	1 : 30	2
Mc.P.	8 : 00	10	: 43	2	1 : 47	6	1 : 46	4
G	1 : 45	7	: 54	2	1 : 19	4	1 : 23	4
H	2 : 51	6	: 35	2	1 : 09	2	1 : 05	2
J	4 : 41	4	: 39	4	1 : 13	2	1 : 50	2
L	: 47	2	: 29	4	1 : 25	4	2 : 22	6
Av.	3 : 46	5.33	: 43	2.67	1 : 26	3.33	1 : 39	3.33

B. Recall after Thirteen Days.

	GRAYS.	VIOLETS.	G. FORMS.	V. FORMS.	G. SIZES.	V. SIZES.	COL. GRAYS.	COL. VIOLETS.
F	6.50	4.00	6.00	14.00	4.00	5.00	11.50	14.00
Mc.P.	4.00	6.00	8.75	6.00	14.00	6.50	8.50	9.00
G	9.50	6.00	4.00	14.00	6.25	6.00	4.00	4.00
H	4.00	6.50	9.00	11.50	4.00	7.25	10.00	10.50
J	5.00	4.00	7.00	14.00	4.00	8.25	4.00	4.00
L	4.00	9.00	11.50	14.00	4.00	4.00	6.50	4.75
Av.	5.50	5.92	7.71	12.25	6.04	6.17	7.42	7.71

increase in recall. If the arrangements be converted into learning times, and an average be taken of the gray and violet sets together, *the addition of a form variation reduces the learning time 68 per cent and increases the recall 75 per cent. The addi-*

tion of a variation in size reduces the learning time about 50 per cent and increases the recall 7 per cent. The addition of a variation in color reduces the learning time 45 per cent and increases the recall 32 per cent. Form is therefore the most effective variation, color next and size the least. All six subjects benefit by all three variations. The improvement is extremely marked and very general.

The question arises whether the variations in color, form and size were not easier to associate with position than the grays and violets, and were therefore taken as the thing to be learned, the grays and violets being neglected. In other words, was the improvement due to substitution rather than to increasing complexity? Did the subjects use the shades of gray and the violet colors at all? The likelihood that this did occur is increased by the fact that interference was likely to enter from the connection of the same shades of gray with different positions in different series, and the same for violets. The opinion of the subjects on this point is of value. They said that the grays (or violets) and their added variations formed unities before the learning process was completed, but that the variations added to the grays or violets, being the more easily discriminated, were the main reliance in learning and recalling.

The experiment does not tell us whether the combination of two variations, in themselves about equal in discriminability, would be more readily associated and better retained than either alone. For example, the table shows that shades of gray and violet colors without additional variations were about equal. Would their combination result in a learning time and retention better than either alone? While the experiment was not performed, there is no doubt that at this low level of variation it would.

To summarize the present situation in the solution of our problem: in the shades of gray or shades and tints of one color, a material has been found which has a rather large amount of sameness and simplicity about it, and still is free from the preformed associations which hindered the linguistic experiments. So far as this material is concerned the experiments just completed answer the first part of our thesis. Adding variations improves both the learning times and retention of all the subjects by large amounts, the precise amount differing with different variations. The next step is to try adding variations to materials similar in their general character to grays and violets, but containing in themselves more variety and complexity.

2. *Adding Variations to More Varied Materials.*

In this set of experiments there was not only an ascent to more varied bases than grays and violets, but there was also an effort to add variations that were equal or less in discriminability than the bases to which they were added. Color, form and size were suspected of fulfilling these conditions. Twenty-four series of six different kinds were made.

i	4 series of colors	iv	4 series of colored sizes
ii	4 series of sizes	v	4 series of colored forms
iii	4 series of forms	vi	4 series of colored forms of different sizes.[1]

The twelve series on the left aim to find the value of color, size and form separately; the twelve on the right, their value in combination. Together they should answer such questions as the following: Suppose it should result that colors and sizes each by themselves are learned about equally well. Are series which vary in both ways simultaneously (colored sizes) learned more quickly, and retained better, or not? If better, it can be due only to the increase in complexity and resulting differentiation, and not to substitution. In the same way the last two groups on the right may be compared with the last on the left.

The general plan of these series differed from that of the series of grays and violets in one important respect. The colored sizes, for example, did not add a size variation to the colors of the *previous* color series, but, as far as the realm of colors

[1] It will be found helpful in the comprehension of the following general description to read for illustrative purposes the construction of a few typical series in the Appendix, pp. 74–80. The Bradley papers were used.

offered variety, combined size with colors *not heretofore used.* Similarly when in the third, fifth and sixth groups of series above enumerated, form became a base, the *same* forms were not used over again, as was the case with the shades of gray and violet, but new forms were found. This change was in the interest of a reduction of interference. The principle could be carried out most fully in the case of forms, less completely with colors, and still less with sizes. Our guiding idea was to use all the wealth of variety which each realm, color, size and form afforded. Our attitude was therefore an entirely practical one. Of the 76 different colors used in this set and in the gray and violet sets, 38 were used only once, 18 twice, 14 three times and 6 four times. Of course the subjects did not discriminate in memory nearly as many colors. Of the 78 different shapes used in the investigation with these subjects, 48 were used only once, 26 twice and 4 three times. The repetition of forms was confined entirely to two series in each of the form-varying groups, namely, Forms I and II, Colored Forms II and III, Colored Forms of different Sizes III and IV, and these six series were composed wholly of forms which had been used once before. Thus there was a restricted area within which the effect of repeating forms could be observed. It is essential to note that in respect to this feature *the three form-varying groups were on an equal footing*, for otherwise they could not be compared. Wherever a form was used again, it was altered markedly in size and color. With series of sizes the limitations of the apparatus and work-room made the variety small. In certain extra series not included in the results we tried the effect of larger sizes, but in the size series given in the tables the aggregate area of the seven terms of a series was in every case 350 sq. cm., which was the same as for all the other series. Now within this limit the numberof sizes which are favorable for inter-term discrimination is relatively very small. We found the best showing for sizes could be made by restricting the choice to about twelve sizes, which we did. The other devices used for reducing interference were to give each series which did not vary in form or color a distinctive color and form of its own; similarly each series of colors had a distinctive series-form, etc. The series were mounted on white cardboard, 14 × 28 inches, all the terms resting on a horizontal base line. A full description of all the series used, including some extra ones not reported in the tables, but used in the analysis of errors to follow later, will be found on pp. 74–80. A few further remarks here will give a sufficient account of the series for most purposes.

As it was the intention to compare the results of pure sizes and colored sizes it was necessary to keep them equally discriminable in size. This was done by using the same sizes (though not in the same shapes) in both. Thus two series of pure sizes and two of colored sizes increased in area in the geometrical ratio of 2.77 beginning with a minimum area of .5 sq. cm. The other two series of sizes varied by irregular ratios which decreased somewhat toward the largest terms, but they were repeated in the other two series of colored sizes. The order in which the terms of the series were arranged was determined by chance, excluding the exceptions made on p. 28. The twelve series varying in form are reproduced in the plates on a reduced scale and need little further description. All of the forms in the third and fifth types were of the same area, 50 sq. cm. The guiding ideas in the selection of forms were to keep within the three classes: conventional geometrical figures, very simple decorative designs, and relatively meaningless forms. The word relatively is emphasized, for those subjects who looked for meaning in forms usually found it,—meaning of some sort. The forms vary from simple to somewhat complex, but there are no intricate forms. Considerable care was taken to make the series as nearly equal in difficulty as possible, with what success will appear in the sequel.

These twenty-four series were given in the same way as the gray and violet sets. The subjects worked on three, and if necessary to learn the series four, successive days each week; on one or two occasions, five days. To distribute the effects of practice and interference, the six types were learned concurrently, although the Roman numerals I, II, III and IV do not indicate the order in which the series were learned. All the conditions not specifically mentioned were the same as in the gray and violet sets.

Table 4. *Areas varied in Color, Size and Form. Associated with Spatial Position.*

A. LEARNING TIMES.

Colors.

	I.		II.		III.		IV.		AVERAGES.	
	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.
G......	:53	4	:39	2	:35	5	:30	4	:39	3.75
H......	:55	4	:44	2	:41	2	:51	4	:48	3.00
J......	1:47	4	1:04	2	2:21	5	:43	2	1:29	3.25
L......	1:01	1	:41	4	1:25	5	1:02	3	1:02	3.25
N......	:38	4	:44	3	:15	4	:24	2	:30	3.25
R......	:24	2	:36	2	:38	5	:19	4	:29	3.25
Av.....	:56	3.17	:45	2.50	:59	4.33	:38	3.17	:50	3.29

Sizes.

	I.		II.		III.		IV.		AVERAGES.	
	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.
G......	:36	6	:33	2	:36	4	:41	5	:37	4.25
H......	:26	2	:20	2	:46	6	:33	4	:31	3.50
J......	:35	2	1:40	4	1:31	4	1:02	4	1:12	3.50
L......	:50	2	:39	2	:47	4	:43	4	:45	3.00
N......	:34	8	:14	4	:49	6	1:04	8	:40	6.50
R......	:14	2	:22	4	:31	4	:29	4	:24	3.50
Av.....	:33	3.67	:38	3	:50	4.67	:45	4.83	:42	4.04

Forms.

	I.		II.		III.		IV.		AVERAGES.	
	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.
G......	:26	2	:41	2	:21	2	:27	2	:29	2.00
H......	:38	2	:50	2.	:30	2	:23	2	:35	2.00
J......	:31	4	:35	2	:27	2	:38	2	:34	2.50
L......	:39	2	:40	2	:31	2	:41	2	:38	2.00
N......	:45	3	:21	2	:12	2	:10	2	:22	2.25
R......	:24	4	:18	4	:13	3	:18	2	:18	3.25
Av.	:34	2.83	:34	2.33	:22	2.17	:26	2	:29	2.33

Colored Sizes.

	I.		II.		III.		IV.		AVERAGES.	
	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.
G......	:46	4	:34	2	:23	4	1:05	8	:42	4.50
H......	:59	4	:40	2	:29	4	1:12	6	:50	4.00
J......	2:05	4	:39	2	:45	2	1:33	4	1:16	3.00
L......	1:23	4	:40	4	1:04	5	:51	4	:60	4.25
N......	:52	4	:27	2	:15	4.	:26	5	:30	3.75
R......	:59	4	:35	4	:17	5	:17	4	:32	4.25
Av.....	:71	4	:36	2.67	:32	4	:54	5.17	:48	3.96

Colored Forms.

	I.		II.		III.		IV.		AVERAGES.	
	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.	Time.	Arrs.
G......	:24	2	:19	2	:33	4	:25	4	:25	3.00
H......	:22	3	:36	2	:24	2	:22	4	:26	2.75
J.......	:45	2	1:15	5	:37	2	:22	2	:45	2.75
L........	:35	2	:45	2	:38	3	:38	3	:39	2.50
N......	:16	2	:13	2	:16	3	:15	2	:15	2.25
R.......	:15	3	:13	2	:11	2	:15	2	:14	2.25
Av.....	:26	2.33	:34	2.50	:27	2.67	:23	2.83	:27	2.58

Colored Forms of Different Sizes.

	I. Time.	I. Arrs.	II. Time.	II. Arrs.	III. Time.	III. Arrs.	IV. Time.	IV. Arrs.	Av. Time.	Av. Arrs.
G......	:20	2	:13	2	:41	2	:43	2	:29	2.00
H......	:48	2	:46	2	:46	2	:30	2	:43	2.00
J.......	:43	2	1:10	4	:34	2	:38	2	:46	2.50
L.....	:28	2	:52	4	:30	2	:35	2	:36	2.50
N......	:20	2	:16	2	:25	4	:15	2	:19	2.50
R.......	:16	2	:16	2	:14	2	:14	4	:15	2.50
Av.....	:29	2	:36	2.67	:32	2.33	:29	2.33	:31	2.33

B. RECALL AFTER THIRTEEN DAYS.

Colors.

	I. Recall.	II. Recall.	III. Recall.	IV. Recall.	AVERAGES. Recall.
G...........................	5.25	11.50	7.00	8.50	8.06
H...........................	7.50	8.00	4.50	7.25	6.81
J...........................	8.00	6.75	4.50	6.00	6.31
L...........................	4.50	7.00	6.75	6.00	6.06
N...........................	6.00	14.00	7.50	4.00	7.88
R...........................	5.50	8.25	8.00	6.75	7.13
Av..........................	6.13	9.25	6.38	6.42	7.05

Sizes.

	I. Recall.	II. Recall.	III. Recall.	IV. Recall.	AVERAGES. Recall.
G...........................	7.00	4.00	4.00	11.50	6.63
H...........................	14.00	6.00	14.00	5.25	9.81
J...........................	4.00	5.00	7.00	8.25	6.06
L...........................	11.50	6.50	8.00	6.50	8.13
N...........................	5.25	4.50	4.50	7.00	5.31
R...........................	5.00	4.00	4.00	4.00	4.25
Av..........................	7.79	5.00	6.92	7.08	6.70

Forms.

	I. Recall.	II. Recall.	III. Recall.	IV. Recall.	AVERAGES. Recall.
G	11.50	4.00	6.75	8.50	7.69
H	14.00	8.00	5.25	11.50	9.69
J	11.50	4.00	11.50	11.50	9.63
L	11.50	6.75	10.00	9.00	9.31
N	11.50	7.00	10.50	11.50	10.13
R	8.25	5.00	6.00	8.00	6.81
Av	11.38	5.79	8.33	10.00	8.88

Colored Sizes.

G	5.00	4.00	6.00	10.50	6.38
H	6.50	5.50	6.25	14.00	8.06
J	11.50	6.50	5.50	8.50	8.00
L	5.00	5.25	4.00	4.00	4.56
N	4.00	8.25	8.50	10.50	7.81
R	8.25	9.25	10.00	6.00	8.38
Av	6.71	6.46	6.71	8.92	7.20

Colored Forms.

G	8.00	6.50	5.00	4.75	6.06
H	14.00	14.00	6.75	10.50	11.31
J	7.50	14.00	5.50	6.25	8.31
L	4.00	6.25	5.75	7.00	5.75
N	11.50	14.00	6.00	10.50	10.50
R	14.00	5.50	9.00	14.00	10.63
Av	9.83	10.04	6.33	8.83	8.76

Colored Forms of Different Sizes.

G	9.00	4.75	10.00	4.25	7.00
H	10.50	8.50	14.00	4.00	9.25
J	8.00	14.00	10.50	6.75	9.81
L	9.00	11.50	4.00	6.25	7.69
N	9.00	5.75	10.50	10.00	8.81
R	11.50	5.00	11.50	6.50	8.63
Av	9.50	8.25	10.08	6.29	8.53

TABLE 5. *Areas varied in Color, Size and Form associated with Position. Recall per Ten Seconds.*

G.

Series.	COLORS.		SIZES.		FORMS.		COL. SIZES.		COL. FORMS.		C. F. d. S.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
I.	.719	.764	1.060	.052	3.194	1.081	.757	.258	2.353	.705	3.000	1.312
II.	2.347	.864	.930	.182	.784	1.329	.909	.106	2.241	.593	1.439	.249
III.	1.167	.316	.714	.398	2.177	.064	1.395	.380	.943	.705	1.639	.049
IV.	1.700	.217	1.742	.630	2.297	.184	1.000	.015	1.055	.593	.675	1.013
Av.	1.483	.540	1.112	.316	2.113	.665	1.015	.190	1.648	.649	1.688	.656

H.

I.	.718	.104	2.762	1.285	2.233	.564	.600	.202	2.369	.486	1.444	.115
II.	1.164	.342	1.342	.135	1.071	.598	.850	.048	2.306	.423	1.202	.127
III.	.685	.137	1.166	.311	.960	.709	.797	.005	1.386	.497	1.938	.609
IV.	.722	.100	.637	.840	2.411	.742	.959	.157	1.470	.413	.731	.598
Av.	.822	.171	1.477	.643	1.669	.653	.802	.103	1.883	.455	1.329	.362

J.

I.	.412	.065	.506	.076	.982	.146	.542	.062	.846	.036	.924	.075
II.	.625	.148	.267	.163	.509	.619	.783	.179	.760	.050	.891	.108
III.	.180	.297	.393	.037	1.620	.492	.620	.016	.682	.128	1.353	.354
IV.	.690	.213	.554	.124	1.402	.274	.472	.132	.953	.143	.827	.172
Av.	.477	.181	.430	.100	1.128	.383	.604	.097	.810	.089	.999	.177

L.

I.	.634	.029	1.643	.531	1.949	.322	.406	.057	.727	.164	1.875	.523
II.	.864	.201	1.102	.010	1.125	.502	.656	.193	.962	.071	1.597	.245
III.	.500	.163	.919	.193	1.960	.333	.351	.112	.846	.045	.800	.552
IV.	.652	.011	.783	.329	1.475	.152	.440	.023	1.029	.138	1.136	.216
Av.	.663	.101	1.112	.266	1.627	.327	.463	.096	.891	.105	1.352	.384

N.

I.	.689	.412	.394	.083	1.402	.875	.394	.652	2.825	.246	2.013	.174
II.	1.728	.627	.714	.238	1.532	.745	1.289	.243	3.713	1.134	1.413	.426
III.	1.165	.064	.366	.110	2.861	.584	1.304	.258	1.132	1.447	1.411	.428
IV.	.821	.280	.428	.047	3.314	1.037	1.197	.151	2.645	.066	2.519	.680
Av.	1.101	.346	.476	.120	2.277	.810	1.046	.326	2.579	.723	1.839	.427

R.

I.	1.250	.056	1.470	.655	1.289	.124	.833	.320	3.111	.235	3.194	.902
II.	1.473	.279	.645	.170	.862	.551	1.233	.080	1.667	1.209	1.389	.903
III.	.909	.285	.563	.252	1.395	.018	1.492	.339	2.727	.149	3.382	1.090
IV.	1.144	.050	.580	.235	2.105	.692	1.053	.100	4.000	1.124	1.204	1.088
Av.	1.194	.168	.815	.328	1.413	.346	1.153	.210	2.876	.679	2.292	.996

SUMMARY.

	COLORS.		SIZES.		FORMS.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
G.	1.483	.540	1.112	.316	2.113	.665
H.	.822	.171	1.477	.643	1.669	.653
J.	.477	.181	.430	.100	1.128	1.383
L.	.663	.101	1.112	.266	1.627	.327
N.	1.115	.347	.476	.120	2.277	.810
R.	1.194	.168	.815	.328	1.413	.344
Av.	.959	.251	.904	.296	1.705	.697

	COL. SIZES.		COL. FORMS.		C. F. d. S.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
G.	1.015	.190	1.648	.649	1.688	.625
H.	.802	.103	1.883	.455	1.329	.362
J.	.604	.097	.810	.089	.999	.177
L.	.463	.096	.891	.105	1.352	.384
N.	1.046	.326	2.579	.723	1.839	.427
R.	1.153	.210	2.876	.679	2.292	.996
Av.	.847	.170	1.781	.450	1.583	.495

The results are given in Tables 4 and 5. In part A of Table 4 the learning times are given in minutes and seconds, and arrangements; in part B the amounts recalled after thirteen days are given on the scale of 14 for perfect recall of the series.[1] In Table 5 the recall of Table 4 is recalculated by finding the equivalents, in learning times, of the arrangements, adding them to the net learning times, and then calculating the amounts recalled per ten seconds of aggregate learning time. A summary of Table 5 follows it, and a verbal summary is given last. "Rec." means recall and M. V., mean variation.

Summary of results. For reasons evident in what follows it will be necessary to consider the subjects individually. It may be well to recall the fact that we are seeking limits to the effectiveness of increasing variety, the combination being each time new, and that from the very nature of attention limits will certainly be reached. It may be well to recall also that with the less varied bases—the shades of gray and violet—all of the subjects profited very markedly by all of the added variations. But color is a more varied base than gray or violets and, to say nothing of isolated series, a dozen series of forms contains more variety than a dozen series of colors. Accordingly it may be expected that before long the subjects, according to their powers of discrimination, retentiveness, and span of consciousness and consequent ability to resist distraction, will begin to diverge. Some will reach a limit sooner than others. Under such conditions, averages of different subjects are worth little. In what follows it will be necessary to consider the memory for size and color separately as well as in combination. Two very important results shown by all the subjects in marked degree may be mentioned first, however. First, *all six subjects retain forms much better than sizes and colors.* Secondly, *all six subjects retain colored forms better than colors.* The second of these two results is comparable to the results obtained in the gray and violet set, for in both cases the variations added were much more easily discriminated than the bases to which they were added.

[1] Described on p. 31.

Subject G. Pure colors and pure sizes are retained about equally well. Colored sizes are retained at least no better than either alone, while colored forms and colored forms varying in size, in themselves about equal, are not as well retained as pure forms. G. says:[1]

Color in colored sizes is a slight aid, but less than the natural fluctuation in ease and difficulty in passing from one size series to another. In the form-varying series, in the long run color is of no value, unless there are no peculiarities in form or size sufficient to hold the series. This seldom happens. The size variation in colored forms of different sizes may be of value on occasion.

Subject H. Pure sizes are better retained than pure colors. Colored sizes show no improvement over even the poorer of the two. Colored forms are slightly better retained than pure forms, but the series with two additions (color and size) are not as well retained as pure forms. H. says: "Colored sizes are easier to *learn* than either sizes or colors, but variations added to form are a distraction in learning. However, in recall they may confirm the arrangement after it is made." H.'s learning times in Table 4 do not confirm his remarks as to the greater ease for him of learning colored sizes. They took more time.

Subject L. Sizes are much better retained than colors. Colored sizes are more poorly retained than either sizes or colors. If the four series of each kind be arranged in order of increased retention per ten seconds of total learning time, all four colored sizes are poorer than even the corresponding *color* series. Only one of the former group is as good as any of the latter. Here the combination is actually poorer than either of the elements. Colored forms and colored forms of different sizes are also not as well retained as pure forms. L. says:

Colored sizes are harder than colors and sizes because of conflicting methods. In colors I associate the end and middle colors with their positions, and link

[1] These opinions of the subjects were obtained in all cases after all experimenting had been completed. The subjects did not know what any of the results of the investigation were except what they may have inferred from observing themselves.

the intermediate ones with them or with each other, depending on the color associations they offer. In sizes I make groups of regularly increasing and decreasing sizes.[1] In colored sizes the color distracts me from using my size method and the size variations, from using the color method. The result is I use both. As a rule forms are easier than colored forms. *Occasionally* color is an aid, for example when the forms are closely similar. The reason for the difficulty is the same as in colored sizes, namely, conflict of habitual methods. Whether the addition of both color andsize to form is an aid I am not sure.

Subject N. Colors are much better retained than sizes,—the reverse of the two preceding subjects. Colored sizes are retained better than sizes and about the same as colors. Color alone added to form shows some gain over pure form, but the large mean variation makes the advantage uncertain. When both color and size variations are added, the result is no better than in pure forms. N. says:

The color in colored sizes makes the size stand out, but introduces a conflict of methods of learning, and resulting distraction. The result is a mixture of methods. I usually use size to fix the largest and smallest ones and color, and size to fix the intermediate ones. The variations added to form also produced distraction in learning, the forms affording sufficient variety in themselves. *The color and size variations added to forms just gave me more to notice.* However, sometimes color aids in recalling the position of a form.

With the four persons above limits were reached. With the next two, however, they were not.

Subject J. Colors and sizes are retained about equally well, and colored sizes are retained considerably better than either. If the series of each group are arranged in the order of increasing retention per ten seconds, all of the colored sizes except one series are better than the corresponding series of colors alone or sizes alone. In the case excepted the colored sizes are at least as good. Colored forms and colored forms of different sizes are not quite as well retained as pure forms, although the difference is quite small. J. says:

The element of color in colored sizes is an aid,—it gives each member in the series an individuality and thus helps to fix it in its absolute position; at the

[1] On the methods of learning pure sizes, see p. 48.

same time it tends to break the 'sky-line' schemes.[1] It is a larger factor than the natural fluctuations in ease and difficulty of different series of pure sizes, with a few exceptions. In general all three variations were aids, alone or in combination: size, because it makes a 'sky-line,'—to which also forms may contribute; color because it fixes the center, or a term left out of my series-scheme.

Subject R. Colors alone are better retained than sizes alone. Colored sizes are about the same as colors. Both colored forms and colored forms of different sizes are considerably better retained than pure forms. If the series of each group are arranged in order of increasing retention per ten seconds, all the colored forms and all the colored forms of different sizes exceed the corresponding series of pure forms. There seems to be no question that the variations added to forms are an aid to this subject. She says:

The presence of color in colored sizes is an aid because it makes the middle terms (middle in area) easier to discriminate. It does not prevent the use of the pure size method (the method used by all the subjects). The addition of color and size variations to form-varying series helps materially in differentiating similar forms both in learning and remembering.

Summary: To recapitulate,—*four subjects, G, H, L, and N, show practically no gain in retention in either of the two principal types of increased variation used here.* In quite a number of cases the recall is actually poorer with the combination. The subjects are either unaffected by the variations, finding sufficient variety in the base, or they are distracted by a conflict of methods of learning which they do not succeed in harmonizing, or by the presence of variations which they use only occasionally but cannot help noticing most of the time. *In contrast with them are the subjects J. and R. To them all the additional variations were an aid subjectively both in learning and recalling, and the tables quite decidedly bear out their introspections in the case of colored sizes with J. and colored forms and colored forms of varying sizes in the case of R.* That a type of variation should be an aid to J. and R. and still not

[1] On the method of learning pure sizes, see p. 48.

show a gain in their results is possible. It may be too small to show; or it may cause an unconscious diminution in effort. If a conflict of methods exists in colored sizes they have harmonized it. This is evident from the fact that they use a combination of both methods, retaining the 'sky-line' scheme,[1] and still using color for the terms intermediate in size, or for a term left out of the size scheme. They are not distracted by the multiplicity of variations in series of the most varied type. It is interesting to note that in natural retentiveness and speed of learning J. and R. represent opposite extremes.[2]

3. Methods Employed by the Subjects in Learning the Series

Between the 5th and 6th weeks of the regular work series were given for the sole purpose of studying the subjects' methods. During this week no other series were given. On the introspections of these series, and to a less extent on those given after the final tests of the regular series the following paragraphs are based. They refer to methods of learning only. Recall methods were a survival of some of the learning methods.

Fundamentally, the subjects did not differ in the methods they employed. Briefly, they all discriminated the terms of a series from each other, and partly at the same time, partly afterward, associated them with each other and with their spatial positions. The less the terms differed from one another the more necessary it was to differentiate them. In the two series which varied only in gray and violet, discrimination overshadowed association, and to the former the long learning times of those series were due. In series of pure colors and pure sizes discrimination was easier and briefer, while in the series in which form variation entered, voluntary discrimination was at a minimum. It was much more nearly instantaneous and involuntary. As one subject put it, 'discrimination was less necessary, because there was not much likelihood that the

[1] The meaning of these conflicts of method, etc., will become clear from the discussion of the next topic.

[2] See p. 63.

terms would be mistaken for one another.' It will be shown in the analysis of errors that with exceptions this remark was true. The subjects came to feel automatically on seeing a new series whether there would be in future much or little liability of mistaking one term for another, and increased or reduced the discrimination process accordingly. Except in the grays, violets, and pure sizes, *perceptual* discrimination in the simultaneous exposure was easy. It was the discrimination in *memory* which gave trouble. The other half of the learning process, the association, consisted in grouping the terms of a series. They might be grouped either because they were adjacent (contiguity), or because they were similar. Discrimination itself was likely to have impressed similarities, because it was similarity that provoked discrimination. The grounds of classification presented an ever-changing variety. A few of the most common will be mentioned in connection with the particular types of series, to which we pass after a word on the subject of language.

Language played small part in learning and retaining the series. Several subjects said that when the forms and colors were quite familiar they were apt to suggest names, and one said that sizes were thought of by the numbers 1 to 7, but that the numbers were not run together at all. This was the subject R. Not a single subject reconstructed the series from language cues, according to testimony taken at the close of the investigation.

Colors. The ends and the middle were conspicuous positions with all the subjects. They looked to see what was there, and if a striking color was in any one of them it was likely to be made the basis of a contiguity group or at least make the association of the term with its place easier. Other contiguity groups are illustrated by the introspections: 'Red and green are complementary and in 5th and 6th positions,' 'blue green is a familiar phrase and blue is on the left of green here.' Some of the similarity classifications which united non-contiguous terms were: 'The bright and the relatively colorless or dead ones,' 'the heavy and the light ones,' 'the violet pair,' the 'reddish ones,' 'colors on the border-line between the seven spectral colors.' There was no hesitation in classing a blue green or a green blue, or even the lighter tint of blue as a blue in one series and a green in another according to the exigencies of the situation. *It is significant that the recognition of several colors, or sizes, or forms for that matter, as similar was both an aid and a risk,—the latter because it opened the way to subsequent confusion of the positions of the similar terms.* Two 'blue' colors or two 'low' forms were peculiarly liable to exchange of positions. The corrective was of course further discrimination. The number of groups in a color series was necessarily relatively large, because the series were constructed so as to give as much variety as was compatible with keeping inter-series interference at a minimum. This number varied from three to five.

Sizes. When in one series the 3d and 5th in size were next to each other, in another the 3d and 7th, and in another the 1st and 7th, and since six such series had been learned before the first two came up for final test, contiguity could not hope to prove a successful method of learning sizes, and was only rarely employed. Similarity was not used extensively if we exclude such contrasts as smallest and largest), because it led to the confusion mentioned in the case of similar colors. All of the subjects employed the same method. They grouped together a number of terms, often not contiguous, which formed, abstracting from the intermediaries, a continuous increase or decrease in size. A variation of the same method consisted in remembering the length, direction and position of the lines running through the tops of the groups. These lines were called 'sky-lines.' Some of these 'systems' or schemes were simple and natural, others complicated and natural, and quite a number both complicated and forced. By forced is meant not true without exceptions so numerous that the value of the scheme must have been slight. Several illustrations of the method may be given.

Sizes B. The correct order was 3416275.[1] The subject's system was an ascending 'sky-line' from 1st to 6th places, the

[1] The numbers denote the relative size of the terms, 1 representing the smallest.

smallest and next smallest forming a second ascending series interpolated in the first at the 3d and 5th positions. The last term stood apart.

Sizes A. The correct order was 5416372. The subject's system consisted of three 'sky-lines:' 541, 167 and 132.

One additional instance is given to illustrate the fact that subjects are not likely to hit upon the same scheme, unless it is very obvious.

A series of circles varying in size and shade or tint of red and arranged in the order 3261547 was given. One subject observed that the smallest was in the middle and the largest on the right end, that there were two pairs which decreased in size to the right (32 and 54), that 4 was nearest in shade to 7, and that the next largest was on the left of the middle. The second subject agreed with the first, as to the smallest and largest, but his further grouping was 321 and 654, in which two groups the direction and rate of decrease was the same, or was taken to be. The third subject made a rightwardly ascending group out of 147, a rightwardly descending group of 32, while 6 was discriminated from, and thereby associated with, 4 which it resembled in color. The fourth subject thought this series was easy for sizes, an opinion which the other subjects did not share. For her the middle one was the smallest, while each wing consisted, in order from left to right, of a medium, a small and a big one, with the larger three on the right. 2 was discriminated from possible rivals by its color, and 6 was the one nearest 7 in color.

It becomes evident that the schemes varied greatly in effectiveness. In the third illustration the one used by the last subject was extremely good, while that used by the third subject was poor. This fluctuation in the simplicity and naturalness of the systems explains the large variations in the retention of certain series of pure sizes and colored sizes as compared with the remaining series of the same types with the same person. For example, the unusually larger retention of Sizes I with certain persons was due to the discovery of the grouping 45, 12, 67, 3.

In the colored sizes the variation in color made the size stand out but it also tended to check and break up the size schemes. The result was a mixture of the two methods, the superiority of which to either system alone the subjects were not agreed upon, according to their testimony quoted in the verbal summary of the last table.

Forms. Here again the subjects agreed in their methods, and it was substantially the same wherever form entered as a variation. If the color and size varied also, these were always subordinate means of recall. The form was the thing, and the detailed analysis of errors given in the Appendix shows conclusively that to the subject it was the same thing whether it was large or small, black or orange. Nevertheless, color and size variations were by no means ignored. They entered as secondary means of fixation, especially wherever a form was similar to another in the same series or a past series. Here the subjects differed some according to their own testimony already given.

It is surprising that only one subject in the six, R., habitually looked for resemblances to natural objects in the forms. There had been no instruction on this point.

Compared to the methods employed in the series of pure sizes and pure colors the form method was much more similar to the color method, but less strenuous. Voluntary inter-term discrimination was at a minimum because it was unnecessary. The discrimination was involuntary, immediate (the subject usually did not hunt for it) and sensorial rather than logical. Here alone was contiguity grouping a leading and effective method. The middle was frequently, though not always, fixated and one or both wings might then form units consisting for instance of a tall figure balanced by low ones on either side of it, as in Forms B, and III. The subjects repeatedly said that in form-varying series more than elsewhere they decided doubtful cases by the appearance of the series as a whole. Not that they had an image of the appearance of the series. Rather certain arrangements tentatively made did not look right. In this feeling they were more often right than wrong. Æsthetic considerations were prominent in series varying in form or color, but discord was as effective a vivifier as harmony.

4. *Analysis of Errors.*

For a detailed and probably more enlightening account of all the errors made by two subjects, N. and R., pages 80–7 should be consulted. A general summary of the errors of all the subjects will be sufficient here. Two types of errors are distinguishable: confusion and simple fading out. By confusion is meant all cases where the subject's arrangement has wrongly followed an arrangement in some other series or in another part of the present series, because of some similarity in color, size, form, or what not. The confusion need not be mental. Quite as often it is purely physiological, as will be noticed below. We shall class all errors not due to confusion as due to fading. An extreme case of fading is the following: A young man who had recently spent five years in the tropics was conscious that his mental processes had become sluggish, and his memory less impressionable. On the second morning of work in our experiment, although only one series of a kind had been learned the day before, and only three in all, he said, when the duplicates of some of the series were laid before him, that if he had been shown that series the day before, he was not aware of it! Our classification is the result, however, of the detailed analysis of errors. The two types are not meant to be mutually exclusive, but only to define predominant characteristics. It is true that confusion is apt to occur only after a certain amount of fading has taken place.

In the series of sizes, colors and colored sizes the type of error which we have called confusion is overwhelmingly the preponderant type. In the earlier series of colors the subject associates certain colors with certain positions. In later color-varying series he has to associate with the same positions colors which in memory at least he does not distinguish from the earlier ones. Interference arises, which is more often not noticeable at the time of learning, if the learnings occur on different days, but which becomes marked in recall after twenty-four hours, and still more marked after two weeks. With the accumulation of series in which the color varies, recollection in the final test becomes worse. The interference may or may not be mental. It frequently is so, but on the other hand if the subject is following an arrangement in another similar group (similar in almost any respect), he is apt to feel quite sure that he is correct. After a number of color-varying series the subject is apt to say when trying to recall their order in a final test that the colors look about as well in one place as in another. By this he means that colors which he has classed as yellow, for example, have stood in a good many different positions, and he is uncertain which one this yellowish term belongs in. Exchange of colors in the same series is not as common as confusion of different series, because in construction the colors were intentionally varied; still, it does occur, for example when two or more colors have been grouped on account of a common property not reckoned with by the operator, as dullness, brightness, complementariness, togetherness in the subject's past experience. We have had occasion before to speak of intellectual classifications as a cause of confusion of position when not followed by further discrimination.

In series of sizes and colored sizes confusion of terms within the same series is much more frequent than in colors. This means the sizes are less discriminable perceptually. Confusion of one series with another may occur within the same half hour (for example, pure sizes with colored sizes), and is more marked with longer intervals. On the second and third day of learning the subject may be aware of it and correct it partly or wholly. Or he may be unconscious of it. The different types of situations are the same as described above for colors. It has frequently happened, however, that a size-varying series has been arranged in a final test in the order of some other series with entire correctness, and still the subject was not aware of the mistake in identity! This has never happened with colors or with form-varying series, and is an evidence of the high degree of unity of the size-varying series, especially pure sizes. In this respect they are the equal of form-varying series. This is due to the 'sky-line' and mass-group systems used in such series, and described above in connection with the methods of learning. *The absence of errors of the fading* *out type in sizes is striking. Almost all of the errors made during learning were exchanges of sizes nearest each other in area, or nearest but one.* Proof of this in the form of a quantitative statement is given on pp. 61–62 in connection with some experiments similar to these, and it is also shown very conclusively in less convenient form in the detailed analysis of the errors of R. and N. in the present experiment, to be found on pp. 80–7.

In series in which form-variation enters confusion is relatively much less frequent than in colors, sizes and colored sizes, and fading out is more common, the total number of errors being also much less. *Exchange of adjacent terms, or inversion, is the most frequent error.* It is due to not noticing the individual terms closely enough (insufficient discrimination). The subject relies on his sensory, non-logical impression, at the point where the error later occurs. This is adequate for the test which follows immediately after learning, but by the next day many of these details have faded out. We agree with Ranschburg that this type of error is relatively unimportant for studying the effects of similarity, because it is not a similarity type of error. However, confusion is by no means absent from form-varying series. Confusion of terms within the same series seldom occurs. Inspection of the plates shows why. But the single re-using of an old form, or the occurrence of forms which generically are the same, for instance two vases, crosses, figures whose general contour is triangular, etc., produces interference and confusion, the more striking only because it can be identified with more certainty, on account of the individuality of forms. The poor average recall of Forms II, Col. Forms III, and C. F. d. S. IV, and the individually poor recalls of N. and R. in C. F. d. S. II are due to the re-using of old forms.[1]

[1] N. and R. took the places of F. and Mc.P. in the experiment and learned the series in a somewhat different order from the one followed by the four other subjects.

5. Other Experiments with the Same Bases

Association with Temporal Order and with Numbers. In the last set of experiments, limits to the benefit of increasing the complexity in the direction of dissimilarity were found for four subjects at the level of pure colors and pure forms. The combination of color and size, themselves equal in difficulty, was no better than either alone; nor were the additions of color and size variations, in themselves poorer than forms, when added to forms, an aid. With two other subjects, however, the combinations were better than the variations singly. With them the limits lie in some further increase in complexity. We might have experimented with these two subjects further, adding still more variations to the same bases, or using more variable bases than colors and forms. Inviting as this was, we preferred to test the generality of the conclusion that a limit is to be expected with any subject on the level of pure colors and pure forms. This we did by repeating the experiments with new subjects, successive exposure and association of the colors and forms with other associates than spatial position.

The 24 series used in the foregoing section were given to one new subject with successive instead of simultaneous exposure, and he was required to associate each term with its temporal position in the series. The cardboard screens, which stood between the aperture through which the subject looked and the carriage by which the series were moved, were narrowed so as to expose only one term at a time. The series were entirely remounted for successive exposure on cardboard sheets, 11 inches high and 42 inches or more long. Instead of having all the terms rest on a base line, the middle points in the vertical diameters of the terms were placed on a line running the length of the sheet, midway between the lower and upper margins. When a form arranged in this way appeared top-heavy, it was lowered until the unpleasant effect disappeared. No fixation point was enforced. All of the conditions under which the previous series were given as to avoiding names, reviewing, use of duplicates to show the learner's progressive mastery of the series, the requirement that the series be held perfectly for 24 hours before the learning was discontinued, were maintained here. The only changes were successive exposure, final test after 6 days instead of 13 and arrangement of the duplicates by the subject in their temporal instead of spatial order. The interval before the final test was shortened because successive exposure made the task more difficult. In the various tests the subject, as before, did not see the duplicates until the exposure was over. He then uncovered them and arranged them in their time sequence, one on top of the other, face down. Six series, one of each kind, were given each week. The subject worked on four successive days each week and a fifth if necessary to bring the series up to standard. Four new series were begun the first day and the remaining two on the second day. The rate of exposure was 50 strokes of the metronome per minute with an exposure on each alternate stroke. A term was thus exposed about 1.2 seconds. The subject regulated the number of repetitions of the series which he took, but less than a repetition of the whole series was never given. Thus the measure of the rapidity of learning was the number of repetitions and arrangements required to bring the series up to perfect retention after 24 hours. The following table gives the results. The figures on the left and right sides of the dashes give the number of repetitions and arrangements respectively required to learn the series; the figures under them give the amount recalled in the final test, 14 being a perfect score. In the lower half of the table the same recall is recalculated per repetition of learning, after reducing arrangements to their equivalents in number of repetitions. This equivalent was found by special experiments given for the purpose in the same manner as for the subjects of the preceding group of experiments.[1]

[1] See pp. 65–7.

TABLE 6. *Areas varied in Color, Size and Form associated with Time Position.*

Subject U.

COLORS.				SIZES.			
I.	II.	III.	IV.	I.	II.	III.	IV.
4–2 5.25	4–4 3.50	4–2 5.50	6–3 4.00	6–4 8.25	11–8 11.25	7–4 5.50	6–3 6.50
FORMS.				COLORED SIZES.			
3–4 8.75	4–4 5.25	3–2 11.50	2–2 10.00	6–4 5.00	4–4 5.00	6–4 6.50	4–3 4.00
COLORED FORMS.				C. F. d. S.			
6–4 11.50	3–4 14.00	2–2 6.50	3–4 14.00	3–2 11.50	2–2 9.00	3–2 7.00	4–3 8.75

Recall per Repetition.

Series.	COLORS.		SIZES.		FORMS.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
I............	.715	.176	.650	.122	.903	.046
II............	.327	.212	.472	.056	.491	.458
III............	.749	.210	.402	.126	1.811	.862
IV............	.363	.176	.589	.061	.589	.360
Av............	.539	.194	.528	.091	.949	.432

Series.	COL. SIZES.		COL. FORMS.		C. F. d. S.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
I............	.394	.060	.906	.347	1.812	.420
II............	.468	.014	1.445	.539	1.683	.291
III............	.512	.058	1.215	.038	1.103	.289
IV............	.443	.011	1.445	.539	.970	.422
Av............	.454	.034	1.253	.366	1.392	.356

A study of Table 6 shows that colors and sizes are remembered about equally well and, as usual, forms better than either. The combination of color and size is not as effective as either separately, but both colored forms and colored forms of different sizes are more effective than forms alone.

With the subject U. and two new subjects, W. and X., the same 24 series were next associated with the numbers 2 to 8. The same seven numbers were used in every series. They took the place of the seven spatial and seven temporal positions. Certain substitutions were also made in the series of pure forms, the result of which was that no form was used twice in all the form-varying series. This was effected by substituting Forms A and B for Forms III and IV, and the alternative figures called B in the plates for those of the same number, e. g., Fig. 6B was substituted for Fig. 6. Naturally the order of presenting the terms was frequently changed to prevent the entrance of position associations. The numbers were of a large, fairly heavy style, 16 mm. high. Nos. 3, 4, 6 and 7 were black and 2, 5 and 8 were red, to reduce the labor of learning,—an application of the advantage of variety to our own experiment. The series in the form in which they had been arranged for U. were used, and the numbers were pasted on the centers of the terms. Where a term had been lowered for æsthetic reasons the number was put on the horizontal axis of the series, equally distant from the right and left sides of the term. The altered orders used to prevent position association were: 1st rep., 1234567; 2nd rep., 2134567, 3d rep., 1235746, 4th rep. 1234576, then if more repetitions were necessary, the same order over again. In the tests the subject took a set of duplicate numbers mounted on circular microscope slide covers and placed them on his duplicate set of terms. All of the other conditions were the same as for the subject U., as described above. With X. the interval between the completion of learning and the final test was reduced to 3 days, because a greater interval was too long to produce sufficient recall. U. was used again in this experiment, mainly because his time was at our disposal. This was a divergence from the fundamental assumption of our problem, namely that the combination of variations must not already have formed a unity in the subject's past experience. While he had not associated numbers with the series, the combinations of color, form and size were not wholly unfamiliar to him. Nevertheless, his results present no important difference from his previous work or from the results of the other subjects, and they are consequently included.

The results of this experiment are given in Table 7. The numbers on the left and right of the dashes give the number of repetitions and arrangements respectively required to bring the series up to standard, the numbers under them, the terms correctly associated after three days for X. and six days for U. and W. There were seven couplets in a series and a score

Table 7—*A*. *Areas varied in Color, Size and Form, associated with Numbers.*

	Colors.				Sizes.			
	I.	II.	III.	IV.	I.	II.	III.	IV.
U.	6–3	8–4	5–6	3–2	6–6	9–7	3–2	6–9
	5	5	2	4	5	5	1	5
W.	8–2	5–3	5–2	4–2	5–5	7–3	7–4	9–7
	7	3	3	2	2	1	5	5
X.	10–6	17–5	10–11	22–11	9–7	8–5	10–5	7–6
	0	1	5	3	2	0	3	5
	Forms.				Colored Sizes.			
U.	6–6	9–6	2–2	4–6	6–4	5–4	4–3	3–4
	7	5	4	7	4	5	3	5
W.	4–2	5–3	5–2	6–4	4–3	9–4	6–2	5–2
	7	7	7	7	2	7	2	5
X.	6–4	4–4	6–6	7–9	9–6	14–10	5–3	10–7
	3	3	3	7	3	7	1	7
	Colored Forms.				C. F. d. S.			
U.	3–2	4–4	3–2	2–3	6–4	4–4	4–5	2–2
	7	5	7	5	7	7	5	6
W.	6–4	6–4	5–2	5–4	4–2	5–4	4–2	4–2
	7	5	4	7	3	7	7	7
X.	5–5	3–2	3–3	7–4	6–6	3–4	3–4	3–2
	4	3	7	5	3	5	7	3

TABLE 7.—*B. Areas varied in Color, Size and Form, associated with Numbers. Recall per Repetition.*

U.

	COLORS.		SIZES.		FORMS.		COL. SIZES.		COL. FORMS.		C. F. d. S.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
I.	.453	.062	.305	.060	.436	.050	.315	.083	1.103	.256	.552	.131
II.	.347	.044	.241	.006	.262	.224	.427	.029	.468	.379	.655	.028
III.	.132	.259	.156	.079	.747	.261	.332	.066	1.103	.256	.404	.279
IV.	.630	.239	.237	.002	.498	.012	.516	.118	.712	.135	1.121	.438
Av.	.391	.151	.235	.037	.486	.137	.398	.074	.847	.257	.683	.219

W.

	COLORS.		SIZES.		FORMS.		COL. SIZES.		COL. FORMS.		C. F. d. S.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
I.	.875	.231	.400	.053	1.750	.321	.500	.153	1.167	.117	.750	.663
II.	.600	.044	.143	.310	1.400	.029	.777	.134	.833	.217	1.400	.013
III.	.600	.044	.714	.261	1.400	.029	.333	.320	.800	.250	1.750	.337
IV.	.500	.144	.555	.102	1.167	.262	1.000	.347	1.400	.350	1.750	.337
Av.	.644	.116	.453	.182	1.429	.160	.653	.277	1.050	.234	1.413	.338

X.

	COLORS.		SIZES.		FORMS.		COL. SIZES.		COL. FORMS.		C. F. d. S.	
	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
I.	.000	.055	.073	.033	.182	.006	.121	.049	.221	.158	.138	.210
II.	.033	.022	.000	.106	.207	.019	.232	.062	.365	.014	.371	.023
III.	.128	.073	.129*	.023	.138	.050	.078	.092	.645	.266	.519	.171
IV.	.059	.004	.220*	.114	.226	.038	.247	.077	.286	.093	.364	.016
Av.	.055	.039	.106	.069	.188	.028	.170	.070	.379	.133	.348	.105

* *Memorized order of numbers only.*

Summary of Part B.

	COLORS.		SIZES.		FORMS.		COL. SIZES.		COL. FORMS.		C. F. d. S.	
	Rec.	M .V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.	Rec.	M. V.
U.....	.391	.151	.235	.037	.486	.137	.398	.074	.847	.321	.683	.219
W.....	.644	.116	.453	.182	1.429	.101	.653	.211	1.050	.234	1.413	.338
X.....	.055	.039	.106	.069	.188	.028	.170	.040	.379	.133	.348	.105
Av....	.363	.102	.265	.096	.701	.089	.407	.108	.759	.229	.815	.221

of 1 was allowed for each correct association, with no partial credits. Part B of the same table gives the same recall per repetition, after calculating in the equivalents of the arrangements in terms of learning time according to the method described on pp. 65–7. A numerical summary follows the table and a verbal summary is given last.

Summary. Two of the subjects do considerably better with colors than with sizes, while the third, X., does not recall enough of either for comparison. For, as indicated in a footnote to Table 7, part B, he learned only the numbers in Sizes III and IV. During learning he mentally arranged the numbers in the order which they would have if the sizes had been exposed in the order from smallest to largest. These two series should therefore perhaps not be counted. *All three subjects do markedly better with forms than with sizes and colors. With U. and W. colored sizes are not better than color alone,* but size is substantially improved by the addition of color as would be expected. *But with X. colored sizes are much better than either color or size alone.* This is confirmed unqualifiedly by the introspections of the subject. For him pure colors or sizes were almost impossible after the first series of each, partly on account of their inherent difficulty, but more because of the interference of past associations. He made in memory few if any more color discriminations than the seven of the spectrum. *With U. colored forms and colored forms of different sizes are both more effective by considerable amounts than forms alone,*—an outcome which duplicated his records in the associations with temporal order. *The subject X. also profits by the increased complexity in both cases,—colored forms and colored forms of different sizes.* His recall is practically doubled in both cases, compared with pure forms. Here again the introspections confirmed the results. *W. on the contrary is not benefited by the additions to form.* Colored forms are even poorer. It is interesting to note that the three subjects differed very widely in natural retentiveness, that the one who remembered the least profited the most from increased variety, while the one who remembered the most did not profit by the additions in any case. U. occupies a middle place in respect both to natural retentiveness and the use made of the additional variations. When asked at the close of the investigation whether the presence of color and color-size variations in the form-varying series was a help, as constrasted with pure forms, W. replied, "Only occasionally, when the forms are similar. As a rule I find sufficient variety in the form alone." And yet she did not take the forms to be copies of objects, except in the most obvious cases. They were simply colored areas. On the other hand X. looked for resemblances to objects constantly, and found them usually.

The distribution of the benefit of increased variety is the same as we have found before. The learning time is shortened and still the amount recalled after a week is greater. U. learns forms in an average of 5.25 reps. and 5 arrs., colored forms in 3 reps. and 2.75 arrs., and colored forms of different sizes in 4 reps. and 3.75 arrs. His average recalls are 5.75, 6 and 6.25 terms respectively. X. learns colors in an average of 14.75 reps. and 8.25 arrs., sizes in 8.50 and 5.75, colored sizes in 9.50 and 6.50. His average recalls are 2.25, 2.50 and 4.50, respectively. He learns forms in 5.75 reps. and 5.75 arrs., colored forms in 4.50 reps. and 3.50 arrs., colored forms of different sizes in 3.75 reps. and 4 arrs. His average recalls are 4, 4.75 and 4.50.

The results with these three subjects simply confirm and extend the conclusion reached with the six subjects. The change to successive exposure and a different association has shown no change in the conditions of learning nor in the results. After a brief treatment of two somewhat different topics in the next three sections, the results of the whole investigation will be brought together in the Conclusion.

6. *Analysis of Errors.*

In the 24 simultaneous series begun in V 2, it was found that in colors, sizes and colored sizes, confusion was quite decidedly the most frequent cause of error, and the detailed analysis of R.'s and N.'s errors would have shown that both of the possible kinds of interference, namely of terms within the same series, and of different series with each other, were equally prominent. In the present experiments successive exposure made single schemes for the whole of a series practically impossible and the influence of past series became less. Memorizing became much more a couplet affair. The interference in the series of sizes and colored sizes particularly was almost wholly traceable to terms in the same series, as shown in the following summary, in which is included every error made in all the series in which there were three errors or less. It includes U.'s temporal-order series and U.'s and X.'s number association series.

Sizes and Colored Sizes.

SUBJECT	KIND OF ASSOCIATION.	NEAREST SIZES.	NEAREST SIZES BUT ONE.	ALL OTHERS.	TOTAL.
U.......	Temporal Order..............	8	4	2	14
U.......	Number Associations..........	21	4	0	25
X.......	Number Associations..........	17	5	4	26
Total..................................		46	13	6	65

By nearest sizes is meant sizes nearest each other in area. These sizes were simply mistaken for each other. In the next summary the proof is less certain only because color similarity cannot be so objectively defined. Its definition is due to the opinion of the experimenter, aided by a Bradley Color Book. We included in our definition only colors obviously similar in perception, and therefore quite certainly took a narrower definition than the limits within which colors would seem similar in memory.

Colors.

SUBJECT	KIND OF ASSOCIATION.	SIMILAR COLORS.	NOT SIMILAR COLORS.	TOTAL.
U.......	Temporal Order......................	2	0	2
U.......	Number Associations.................	4	2	6
X.......	Number Associations.................	11	2	13
Total..................................		17	4	21

In the form-varying series of the temporal order set of U., inversion of adjacent terms was the most common error, which result agrees with those of the corresponding spatial position associations. Of the 22 errors made in all the series in which 3 errors or less occurred, 15 were exchanges of adjacents, 4 were due to similarity in form and 3 to no assignable cause. Of the form-varying series of the number associations the only statement that can be made is that confusion due to similarity was less frequent than in the series of sizes and colors. Inversion of adjacents is here not a possible type of error on account of changes of the order in learning.

VI. INDIVIDUAL DIFFERENCES IN SPEED OF MEMORIZING AND IN RETENTIVENESS

If the differences in material be disregarded, the experiments described in V 2 give results from six subjects with the same 24 series. Since all of them were required to learn the series in the same way we may compare the subjects simply as learners. The three subjects used in the experiments with numbers cannot be compared with the first six only among themselves. The following table is compiled from the averages for the different persons given in Tables 4 and 7. Part A below gives on the left the average number of seconds which each of the first six subjects required to bring the series up to the point where he had held its arrangement perfectly for a day. On the right side are given the average amounts recalled, marked on a scale of 14. In part B similar averages are calculated for the three subjects in the experiment with numbers. These figures also are based on 24 series and the amount recalled is based on a scale of 7. The subjects are arranged in order of speed of learning and amounts recalled.

TABLE 8.

Part A.

SUBJECTS.	TIME.	ARRS.	SUBJECTS.	AMOUNT RECALLED.
R.	:22	3.17	H.	9.16
N.	:26	3.42	N.	8.31
G.	:33.5	3.25	J.	8.02
H.	:38.8	2.88	R.	7.64
L.	:46.7	2.92	G.	6.97
J.	:60.2	2.92	L.	6.92

Part B.

SUBJECTS.	REPS.	ARRS.	SUBJECTS.	AMOUNT RECALLED.
W.	5.38	3.08	U.	5.04
U.	4.71	4.17	W.	4.96
X.	7.88	5.63	X.	3.75

N. learns rapidly and retains well; R. learns rapidly and retains moderately. H. is just the converse: he learns moderately, being a tie for third place, and retains well. G. learns moderately but does not retain as well. J. gets some recompense for his extra effort, while L. learns slowly and retains with difficulty.

W. and U. learn rapidly and retain well, there being very little difference between them. W. is the more retentive for longer intervals. For a week—the interval of their test—there is no difference between them, but W. retained series for weeks after U. had forgotten them. X. learns slowly and has difficulty in retaining.

Temperamental differences also come out in the willingness to take a chance. This is shown by the number of arrangements. N. and G. were speculative. They frequently terminated the exposure of the series before they had learned them sufficiently. These were the subjects who had on occasion to take five days to learn the series instead of the usual three. L. and J. are cautious. When they end the exposure it is safe to say they have the series for that day, and quite probable that they will have it the next day without re-exposure.

VII. EXPERIMENTS TO DETERMINE THE VALUE OF ARRANGING[1]

After all of the foregoing experiments had been completed, 12 series were given to the subjects, G., H., J., L., N. and R. There were four series each of colors, sizes and forms. Two of each were learned in three successive days all six being begun on the same day. The other six were given the next week. In one of the two color series of the first week arranging was required as in all the foregoing experiments, in the other it was omitted till the final test. The same is true of the two series of sizes, and the two forms learned the first week. The times of exposure were the same for every correlative pair, the one with and the one without arranging. During the first week the series with arranging preceded their mates to allow the subject to determine the length of the learning periods. However, to prevent the series without arranging from being at a disadvantage from too short exposure, certain extra exposures were given by the operator on the third day. To be

[1] This question is very important wherever it is desired to study the learning process as well as the recall. For in order to ascertain the learner's progress towards complete temporary mastery of the material it is necessary to test him from time to time. This gives two different measures, learning time and testing time or number of tests. The attempt to reproduce a material nearly always aids in memorizing it. Ebbinghaus assumed that a test or attempt to repeat a series is equivalent to a presentation of it, but this is clearly unsatisfactory. Stephan Witasek has made an exhaustive study of the relative efficiency of readings and attempts at recital in bringing series of nonsense syllables up to perfect memorization, and in producing recall after an hour. (Ueber Lesen und Rezitieren in ihren Beziehungen zum Gedächtnis. Ztsch. f. Psych., 44: 161–85 and 246–82.) He found that for immediate recall 6 readings and 5 recitations enabled the persons to recite the series in one half the time required by 11 readings alone, and required only one thirteenth as many promptings. (The Ebbinghaus prompting method is the one referred to.) The superiority of the recitation in producing recall after an hour is not as marked as in immediate recall. Thus various mixtures of readings and recitations, the readings of course always preceding, compared with an equal number of readings alone, effected an average saving of about 11 per cent in time and 16 per cent in promptings in the final test. The foregoing disregards time consumed by the two methods. A recitation aided by promptings takes more time than a reading. However, the author shows that even on a time basis a combination of readings and recitations is more economical than readings alone. From any point of view recitations are far superior to readings in impressing value. The significance of this result for pedagogy, for determining the best methods of study, is worth noting.

more explicit, on the first day the subject wholly determined the time. He took as much as he thought would enable him to arrange the *first* series of each pair correctly the next day, and the operator allowed him the same time on the second series of each pair. On the second day the procedure was the same *if* he did *not* have them correctly in the preliminary test of that day. If he *did* have them correctly, he was given a 7-second exposure of both series of the pair. The procedure was repeated on the third and last day, for sizes and colors, but forms were dropped after the second day, because they would have been over-learned by continuance. The aim of these conditions was to bring the series approximately up to our previous standard, perfect retention for a day. However, since having arranged a series was equivalent to seeing it again, and since it was desirable for exactness to spread the learning time of both types of series over the same number of days, it was necessary to give a brief exposure of *both* series (the one with and the one without arranging) whenever one was seen. This was the purpose of the 7-second exposures. Hence our procedure was slightly different from the one used in the past. In the second week the operator wholly determined the length of the learning times, and made them the same as the series of the first week for that subject. The series without arranging now preceded its mate. Finally, there was a reversal within the pairs. The series in connection with which three of the subjects arranged were the ones with which the other three subjects did not arrange. The purpose of this reversal was to test the assumed equality in difficulty of the correlative series. The results substantiated the assumption in the main.

All the other conditions were the same as in the simultaneous series of V 2. The subject was required to associate the terms with their spatial positions. The series were displayed on cardboard sheets, 14 × 22 inches, the arrangement being in three vertical columns of two terms each, except the middle column, which had three. The usual expedients to minimize interference, in particular, individual series-shape and series-color, were used. The final test of the first six series occurred in the midst of learning the second six in order to retain the normal interference factor, constantly operative in our past work.

The procedure with the three successive subjects U., W. and X. was similar. The consonants G, H, N, P, Q, W and Y were associated with 12 series, 4 each of colors, sizes and forms. Three of the letters were white, the rest black. The learning conditions were modeled after those described above, while the testing conditions were the same as in the regular work of these subjects.

For the six simultaneous subjects the value of an arrangement was determined by dividing the total number of seconds spent in learning the six series in which there was no arranging by the number of terms recalled in those series after a week, and then dividing the difference between the total recall in the six series with arranging and the six without, by the total number of arrangements used. The first operation gave the number of seconds of learning time that would produce one term in recall with this subject, while the second operation gave the number of arrangements, or the fraction of an arrangement, that would produce one term in recall. These two quantities were therefore equivalent. Precisely the same method was pursued in calculating the value of an arrangement for the successive subjects, except that the learning times were given in repetitions instead of minutes and seconds. The values thus obtained for the different subjects were as follows:

Value of an Arrangement.

SUBJECT.		SUBJECT.		SUBJECT.	
G..........	5 secs.	L........	10 secs.	U.....	1.673 reps.
H..........	12.35 secs.	N........	12.35 secs.	W.......	nothing
J...........	21.8 secs	R........	10 secs.	X........	2.617 reps.

For the two subjects who were dropped from the experiment after the series of grays and violets, McP. and F., 10 secs. was taken as the value.

VIII. CONCLUSION.

In summing up the results of the investigation, it is well to call attention to the fact that it is not primarily an inquiry into the effects of novelty on memory. Had this been the aim the same typographical variations would not have been used for eight consecutive weeks, nor the same variations in plane areas for over four months. The experiments rather contrast the effects of simplicity with dissimilar complexity, when a few types of each are used a long time.

Language, spoken and written, is full of symbols relatively simple and similar. The sameness of a page of print contrasted with the variety afforded to the eye by looking at natural objects is striking. The possibilities, however, of improving the memory for language, by introducing mechanical variations, as suggested to us by the experiments of Dr. Gordon, have proved to be decidedly limited. In the absence of variation in meaning, as is the case with nonsense syllables for example, the memory is permanently improved by the addition of a few variations, particularly position and size, but if the number of variations be increased considerably, say to three or four, distraction enters, there is no further improvement and there may be a loss. That such variations are of great aid in arresting the attention, when they have the feature of novelty, is a popular conviction which experiment would doubtless verify. If, however, the variation takes the form of experiencing fewer of one thing than of another, the memory for the few is certainly better than the memory for the many, as proved by the experiments of Prof. Calkins.

But language differs from nonsense syllables in having meaning, in itself a powerful variation, and the experimental results give no basis for the belief that the memory for language is permanently better, when the words vary in mechanical ways. Our own experiments were few in number, but convincing. When the attempt is made to vary the meaning, a difficulty in technique arises, which is by no means a small one, namely, the difficulty of keeping the simpler material simple and disconnected. Even single adjectives or two-place numbers suggest acquired connotations so readily that the material rapidly becomes complex, and has the advantage over the material called complex that it is already made up of *units*. This was the case with our experiments with words and sentences, and numbers and biographical facts. However, the results at least show that when the learning time is fixed and sufficient for comprehending the meaning of both, short sentences can be as well remembered as single words. All three of our language experiments showed conclusively that a person will seek differentials among the things to be associated, but will give the preference to ones already connected in his experience, if they can be found.

We pass next to the experiments with plane areas. We found that forms were far more associable than colors and sizes, that is to say, the associations were much more quickly established between forms and something else than between colors or sizes and something else. In this result all nine subjects agree. We know of no other experiments on this subject except those of Bigham.[1] He found that colors were slightly more associable than forms in immediate recall, but after 2 hours and 24 hours the result was the reverse by larger differences than before. The response times in immediate recall of forms were also longer than in the case of colors. His method was similar to ours in the use of duplicate series for testing, but differed in the very important respect that the same ten forms and ten colors were used over again in every series. The test was association with position as in our experiment. The re-use of the same forms would under these circumstances produce interference, the greater for forms because of their more ready associability. The better recall of forms after two and after twenty-four hours, that is the fact of reversal, cannot be understood without knowing how many series of a kind were given.

The relative associability of sizes and colors differs in our results with different subjects, with the balance in favor of colors. Four persons remember colors better; two, sizes; and

[1] Bigham, John: Memory. Psych. Rev., 1894, I, 453.

two show no marked difference. The ninth subject is disregarded because of low recall. However, if the slighter differences shown by two persons classed as equal be counted, six remember colors better than sizes, while two are the opposite, and by very large amounts in both cases.

The very great superiority of forms over colors and sizes is certainly due to the far greater variety within a limited space which the realm of forms offers. Had all the form-varying series, instead of one-half, consisted solely of forms used only once, the superiority of forms would have been still greater by a considerable amount. Yet as many forms as that could easily have been found without making the similarity great. The simple fact is that there is a large number of easily discriminable forms, while there are only a very few easily discriminable colors and sizes, so far as the memory for long intervals is concerned. On the other hand the experiment does not do justice to the possibility of color. It would be interesting to know what would be the result of combining several colors in each term. The striking color effects of practical life are oftener color contrasts than single colors. While only experimentation can decide the question, our own results lead us to expect only slight improvements for color from this source.

While our earlier experiments were relevant to the memory for language, the later ones with plane areas are like the memory for objects, and here our results were much more positive and extended. We may disregard the differences between the associations with position and those with numbers, and treat all nine subjects together. The questions raised in the original statement of our problem may be answered as follows. On the low level of variety represented by shades of gray and shades and tints of one color, the advantage of additional variations is great and is manifested by all six subjects tried. By a mental economy the additions, here more easily discriminated than the grays and violets, become the more prominent, although a *bona fide* association is made between them by the time the learning process is complete. In most cases the person could tell which gray, or shade or tint of violet, went with a form or size, if given both variations to put together. This process of remembering a less discriminable thing by a more discriminable associate is easily identified in life. Students distinguish their notebooks by the fasteners, marks of rough usage or even bits of color. Books are distinguished by their variation in color, because the shape is so much less discriminable. In education arithmetical rules are clothed in striking examples.

With the passage to the higher levels represented by different colors and different forms, when the variations added are less discriminable than the bases, five of the nine subjects no longer profit by the increase in complexity. The two other persons in the simultaneous position associations and two in the number associations with areas still profit. No limit was found for them. When, however, the variation added is more discriminable than the base, all of the subjects again profit by the addition. This is the case when in our type of series called Colored Forms a form variation is added to color. The advantage is almost as marked as on the lower level.

The question why the increases in complexity cease to be an advantage is next in order. The ability to profit by these increases is not a function of speed of learning, as would naturally be expected. Quickness in learning here represents good powers of discrimination and facile associative processes. The figures given in Table 8 show that of the four persons who profited longest by the increases in complexity, R. and J., and U. and X., two rank first and two last in rapidity of learning the series. The ranking is the same if only pure colors, sizes and forms are averaged. This restriction can be demanded with some justice, because if those who have trouble in discriminating the terms are the ones who profit longest by the added variations, their slower discrimination would show itself most before the variations were added.

On the other hand the five to whom the complex material was not the better are in agreement as to the presence of distraction in the complex series. While distraction is therefore the most probable cause at the present time, special experiments on the span of attention are necessary to decide the matter.

It is now possible to offer an explanation for some of the differences in the memory for different materials, and the same materials learned in different ways, which were spoken of in the beginning. Objects, actions and pictures are better remembered than words, because they are more extensive and varied stimulations. Both get a certain amount of variation from the ideational suggestions called forth, by the connotations, in other words. It is probable that the ideational supplementation is somewhat richer for words than for objects, actions and pictures. But with most persons imagery is feeble compared with sensory stimulations, and we are inclined to believe that the advantage which words may enjoy in this respect is relatively slight. On the other hand in the extensiveness and dissimilarity of sensory stimulations a series of words cannot compare with a series of objects or pictures. This is ludicrously brought out when one attempts to handle type. We rest our eyes from print by looking at our surroundings. Contrast the extent and variety of stimulation obtained from looking at a house, a lawn, a lamp, a knife, a piano-player and a moving train of cars with the smallness and similarity of the stimulations obtained by looking at their printed words just given, or even printed and read aloud. Corresponding to the more extensive and varied original brain excitations of things are the more easily aroused and numerous mental cues in recall, and the greater likelihood of freedom from the interference due to similarity. If the brain excitations obtained from seeing and hearing the series of things mentioned above be denoted by the letters ABCD, DEFG, GHIJ, etc., those obtained from looking at their printed names and speaking them should be represented by the letters mnop, nopq, opqr, etc., even after the differences due to suggested imagery are included.

The explanation is the same for the fact that words presented to several senses are better remembered than those presented to one only. Whitehead has shown that when things are learned visually, there is a filtration, so to speak, through from our visual to our auditory experience taking place at the time of the visual learning, so that if a week later the same thing be learned auditorily, it takes but little more time than to relearn it visually, and of course much less time than to learn a new series auditorily.[1]

[1] Whitehead, L. G.: Psych. Rev., 1896, III, p. 258.

The same thing is true if the first learning is auditory and the second visual. Here as in our own experiments the connecting process goes on during learning, but in this case the connection is with images instead of other perceptions. The same process takes place in learning through several sense organs at once. If we not only see but pronounce, we get a more varied stimulation than if we merely see the words, and if we read them aloud the stimulations are still more varied compared with the visual alone, and the liability to confusion in recall is correspondingly less, just on account of this growing variation. Doubtless the well-known summation effects of a number of weak stimuli are also responsible for the difference. We seem to get the *meaning* more completely when we read a page aloud than when we read it to ourselves. This indicates that the visual, auditory and enunciatory stimulations combined are more effective than one or two alone in arousing associations of an ideational type. Furthermore, it is not to be forgotten that we are dealing here, in the case of language at least, with complexes that are already apperceptive units, owing to early schooling. The case is somewhat different from that of our own experiment, where the combinations were constantly new.

One of the original and less common features of this investigation is the length of the interval before the memory is tested. There are few extended investigations of the memory for materials after intervals as long as one and two weeks.

Finally, to the technique of memory work we offer a contribution. The method of measuring the memory for different materials by the amount of time or repetitions required to bring them up to the same level of efficiency meets with the difficulty of evaluating the tests taken to determine progressive efficiency. We propose a solution of this difficulty, namely, a separate determination for each person, of the average worth of a test in terms of learning time or repetitions, the two measurements to be rendered equivalent through what each will produce in recall.

IX APPENDIX.

1. *Plates.*

The following plates give the shapes and *relative* sizes of the form-varying series. The actual dimensions and the colors employed are given in the 'Description of Series' on the pages immediately succeeding.

With three subjects *U*, *W*, and *X*, certain forms, here called 'Substituted Forms,' replaced some of those in the regular series in order to eliminate repetition. They are numbered to correspond with the figures which they replaced, and the distinctive letter B is added. The first three forms, 1B, 4B, and 5 B belong to Forms I; the fourth, 7B, belongs to Forms II; and the last two in the row, 1B and 6B, belong to Forms B. In the next row 1B, 2B, and 7B belong to C. F. d. S. III, and the remaining five belong to C. F. d. S. IV.

2. *Description of Series used in V 2 and 5.*

Colors. Each term contained 50 sq. cm.

Series I. Eight-point stars. Yellow orange t. 2, green, cool gray no. 1 blue sh. 2, A-yellow medium, green yellow t. 2, red orange sh. 1.

Series II. Squares. Orange red t. 2, yellow orange sh. 2, A-blue green dark, yellow, red, green t. 1, A-red light.

Series III. Oblongs. Orange yellow t. 1, red violet sh. 2, orange red t. 1, blue green sh. 1 green yellow t. 1, A-green yellow dark, A-green light.

Series IV. Round-cornered squares. Black, violet red t. 2, orange red sh. 2, cool gray no. 2, green sh. 2, A-yellow orange medium, green blue sh. 1.

Sizes. Series 1. Red oblongs, height twice the width. Irregular ratio of terms as follows: 1 : 2 = 4, 2 : 3 = 3.06, 3 : 4 = 1.24, 4 : 5 = 1.5, 5 : 6 = 1.4, 6 : 7 = 1.5.

Series II. Equilateral triangles. A-yellow orange dark. Geom. ratio, 2.77, beginning with an area of .5 sq. cm. for the smallest.

Series III. Circles. Blue violet sh. 2. Geom. ratio, 2.77. Smallest term, .5 sq. cm.

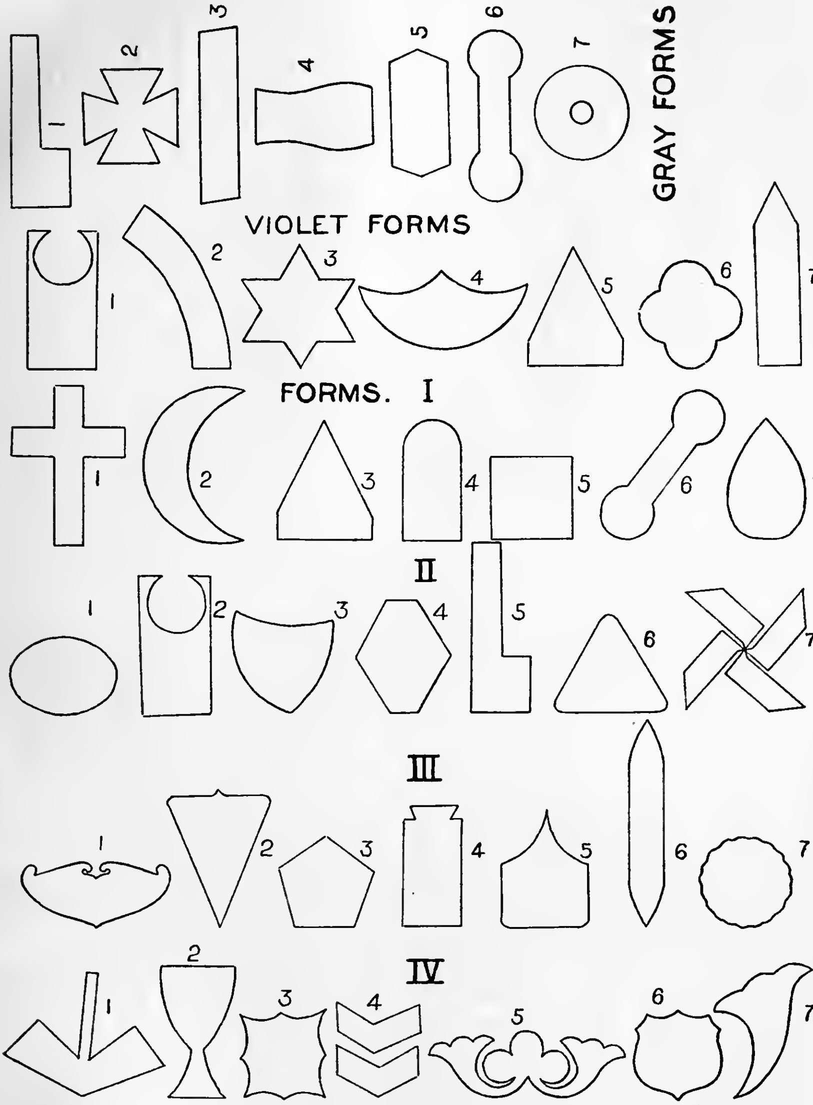
Series IV. Truncated cones, sides inclined one-tenth the width of the base on each side. Yellow orange, and irregular ratio of terms as follows: 1 : 2 = 3.9, 2 : 3 = 4.1, 3 : 4 = 2.98, 4 : 5 = 1.69, 5 : 6 = 2.1, 6 : 7 = 1.9.

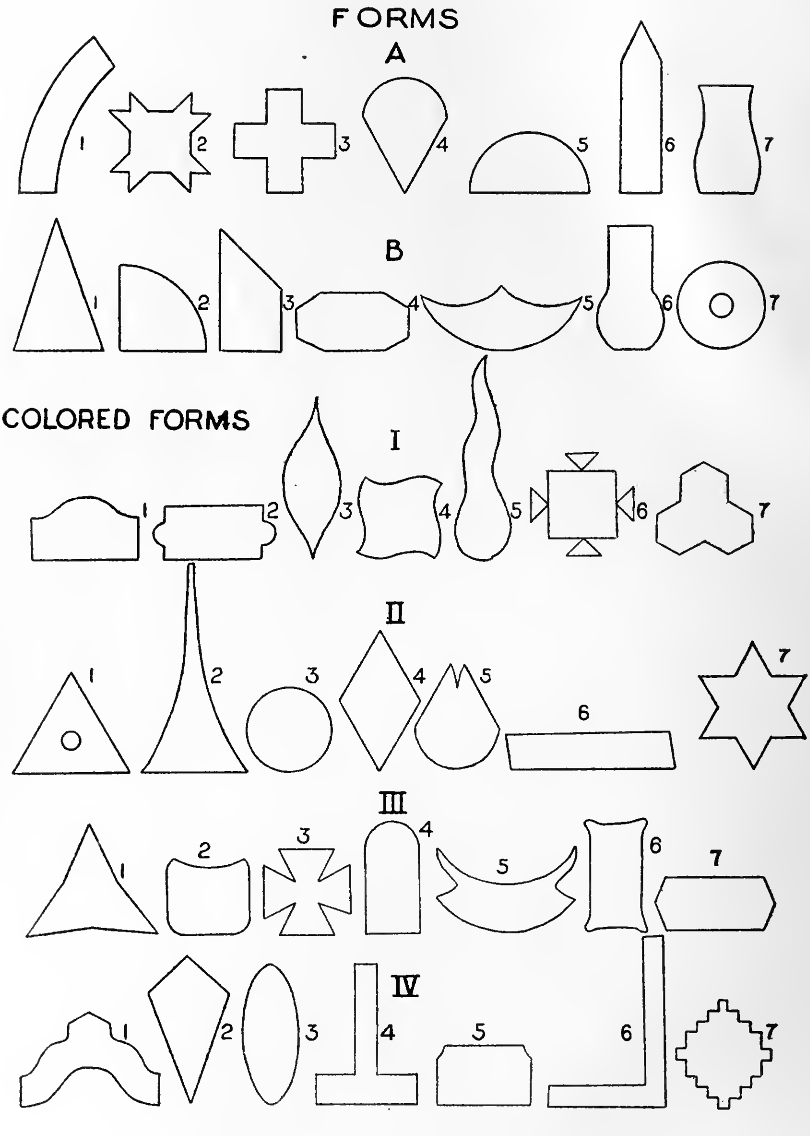
The order in which the sizes were placed in any series was determined by chance as in the gray and violet sets, excluding the cases there excluded. The arrangements employed in the above series were the following, reading the series from left to right. The figures give the areas in sq. cm.

Series I. 40.5 60.5 2 8 84.5 128 24.5.

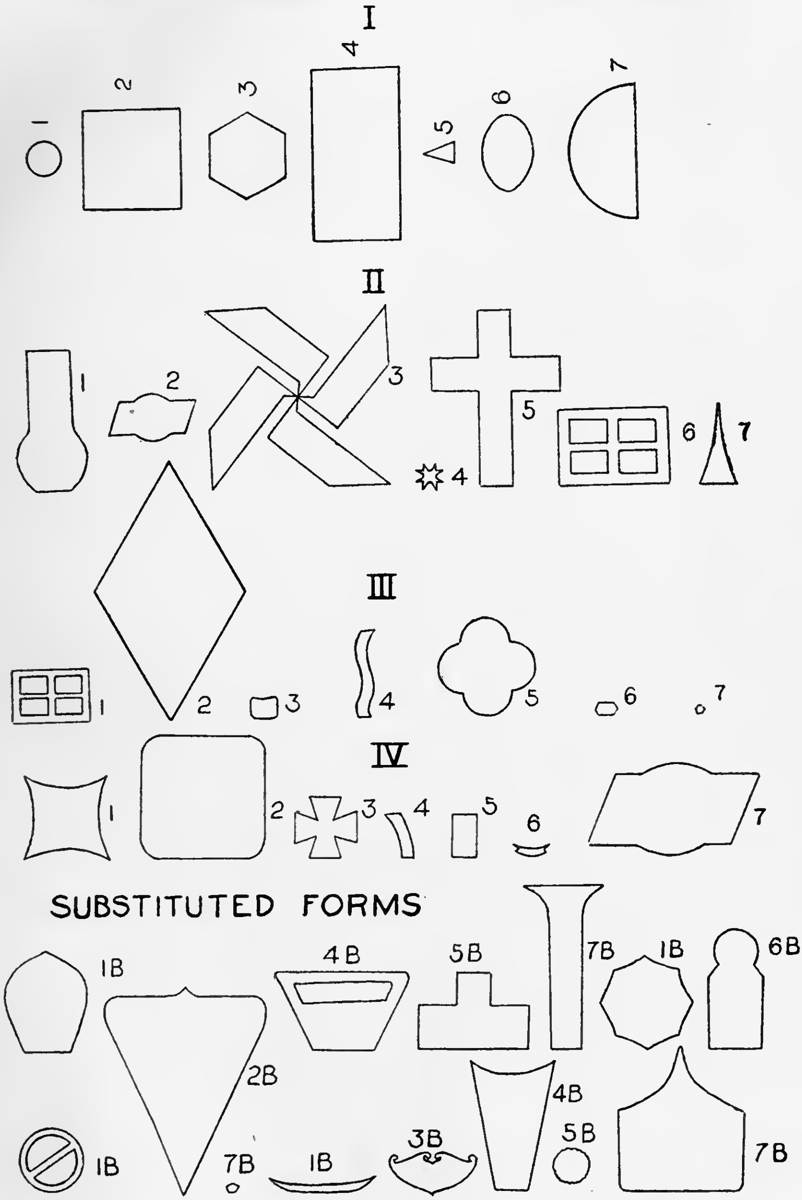
Series II. (1 is the smallest) 1625347.

Series III. 2451376.


GRAY FORMS
VIOLET FORMS
FORMS. I
II
III
IV


FORMS
A
B
COLORED FORMS
I
II
III
IV

COLORED FORMS OF DIFFERENT SIZES.



Series IV. 1.95 .50 40.6 162.2 24 8.05 84.

It will be noted that the aggregate area of each series was 350 sq. cm.

Colored Sizes. These series are the simplest advance in complexity. As the name indicates they vary in area and color. To render them comparable with series varying in size only, the term-areas of Series I repeat those of Sizes I, Series II, of Sizes IV, Series III and IV, of Sizes II and III. The series-shapes were however new. The order of occurrence of the terms may be represented as follows, the topmost one being on the left end. The figures again indicate the sizes, 1 representing the smallest, 2 the next smallest, etc. The combination of color with size was determined by chance after the seven colors for the series had once been selected. However very small terms were not given very light colors.

Series I.	*Series II.*
2 yellow green	2 blue
3 yellow orange dark	1 orange red
1 red sh. 2	5 orange sh. 1
7 orange yellow t. 1	7 yellow orange t. 1
6 red orange	4 green yellow
4 blue green sh. 2	3 blue t. 2
5 violet t. 1.	6 red violet t. 1.

Series III.	*Series IV.*
3 A-green medium	3 violet
4 blue t. 1	1 A-green yellow dark
7 A-orange red dark	6 blue green sh. 1
5 green yellow t. 2	2 A-yellow orange light
1 black	5 green blue t. 2
6 A-orange light	4 red orange sh. 2
2 A-red violet dark	7 yellow t. 2

Forms. (See plates.)

Colored Forms. These series also varied in two ways, color and form. The forms are given in the plates. Each term contained 50 sq. cm. The colors in order from left to right in the series were as follows:

Series I. Fig. 1 yellow, Fig. 2 A-green medium, Fig. 3 red violet t. 1, Fig. 4 yellow green, Fig. 5 black, Fig. 6 A-orange red medium, Fig. 7 orange t. 2.

Series II. Fig. 1 green, Fig. 2 orange, Fig. 3 orange yellow sh. 1, Fig. 4 red, Fig. 5 green t. 2, Fig. 6 yellow orange sh. 2, Fig. 7 violet.

Series III. Fig. 1 A-yellow orange dark, Fig. 2 blue sh. 2, Fig. 3 red t. 1, Fig. 4 yellow green, Fig. 5 orange yellow t. 1, Fig. 6 red violet sh. 1, Fig. 7 yellow green sh. 2.

Series IV. Fig. 1 warm gray no. 1, Fig. 2 orange yellow t. 2, Fig. 3 green yellow t. 1, Fig. 4 blue t. 1, Fig. 5 A-orange red, Fig. 6 violet red, Fig. 7 A-yellow orange medium.

Colored Forms of Different Sizes. These series varied in three ways and represented the maximum number of variations which were combined in a single series. The variations were in color, size and form. The forms are given in the plates, the sizes and colors, here. The ratios existing between each two successive terms in series I and II were the same as in Sizes I, and the actual areas were also the same as there. In series III and IV the ratios were
1 : 2 = 4, 2 : 3 = 2.25, 3 : 4 = 2, 4 : 5 = 2.17, 5 : 6 = 2.6, 6 : 7 = 3.25.
1 : 2 = 2.5, 2 : 3 = 2.5, 3 : 4 = 2, 4 : 5 = 2.5, 5 : 6 = 2.17, 6 : 7 = 1.35.

Series I.

Fig. 1	8.	sq. cm	A-yellow orange dark
" 2	84.5	"	violet red
" 3	40.5	"	A-yellow light
" 4	128.	"	yellow
" 5	2.	"	A-blue dark
" 6	24.5	"	yellow green sh. 2
" 7	60.5	"	blue

Series II.

Fig. 1	60.5	sq. cm	green t. 1
" 2	24.5	"	orange yellow sh. 1
" 3	128.	"	warm gray no. 2
" 4	2.	"	orange red
" 5	84.5	"	green blue sh. 1
" 6	40.5	"	orange
" 7	8.	"	A-red light

Series III.

Fig. 1	19.2	sq. cm	black
" 2	162.	"	violet
" 3	4.5	"	orange yellow sh. 1
" 4	9.	"	red violet t. 2
" 5	50.	"	yellow green sh. 2
" 6	2.	"	red t. 2
" 7	.5	"	blue t. 1

Series IV.

Fig. 1	43.75	sq. cm	blue violet
" 2	128.	"	green blue sh. 1
" 3	17.5	"	violet red
" 4	3.5	"	warm gray no. 2
" 5	8.75	"	green yellow sh. 2
" 6	1.4	"	red sh. 2
" 7	95.	"	A-yellow light

Each of the first two series aggregates 348 sq. cm., the third 247 and the fourth 298 sq. cm.

Extra Series. Colors A. Blue sh. 2, yellow orange sh. 2, violet, orange, green, A-violet blue light, red. Oblongs, height twice the width.

Colors B. A-yellow light, red violet t. 2, red t. 1, yellow, warm gray no. 2, blue green, yellow green t. 1. Same series-shape as in the preceding one.

Sizes A. Isosceles triangles, height twice the base. Blue t. 2, geom. ratio of 2.5 beginning with an area of .5 sq. cm. for the smallest term. Total area of series, 204 sq. cm. Order: 5416372.

Sizes B. Truncated Cones, A-yellow dark in color. The ratios were 1 : 2 = 4.5, 2 : 3 = 2.78, 3 : 4 = 2.6, 4 : 5 = 2.03, 5 : 6 = 1.3, 6 : 7 = 1.92. The actual areas in order from left to right were, in sq. cm.: 25 64 2 169 9 324 130. The total area was 723 sq. cm.

Sizes C. Quadrilaterals formed by superposing upon a square a right triangle of the same dimensions with hypotenuse to the left. Green sh. 2 in color. The ratios were, 4, 4, 3, 3.5, 1.93, 1.58. The actual areas in their order were in sq. cm.: 2 162 24 84 256 .5 8. The total area was 536.5 sq. cm.

In the next two series the areas of Sizes I were used.

Colored Sizes A.	*Colored Sizes B.*
4 blue green	5 orange red sh. 1
2 red orange t. 2	2 green sh. 2
7 warm gray no. 1	4 orange yellow t. 1
5 green t. 2	3 red t. 2
1 red orange	7 cool gray no. 1
3 blue t. 1	1 A-yellow orange dark
6 A-green yellow medium	6 blue

Forms A and B. These are sufficiently described in the plates. The results obtained from them were not used in the tables, because the forms in them had been used once before, and there was already a sufficiency of such series.

3. *Analysis of Errors in V 2.*

In the pages immediately following the errors of Tables 4 and 5 will be analyzed. The results of two subjects will be treated in detail including every series, every error in the final test and some of the errors made in learning. Some of the explanations are conjectural, and of questionable worth; many others are beyond question the true causes of the errors. The account is to a very large extent a story of interference due to similarity. It will be recalled that introspections were not allowed before the final test, so that when they were given, most of them were not of value. The conclusions given below are therefore mainly the result of a study of the arrangements of duplicate series handed in by the subject. Where introspections were used it is made evident in the text. The extra series denoted by the letters A, B, etc., are included. It is essential to remember that in the interval of 13 days between the learning and the testing of a series two more rounds of similar series were learned. When the particular test is not mentioned it is always the *final* one that is meant. By 'wing' is meant the three terms on either side of the middle term.[1]

Subject R.

Col. Sizes I and II, and Cols. A. In learning Col. Sizes I the only error was an exchange of 3 yellow orange and 4 blue green belonging in 2d and 6th places respectively. The error was probably due to their similarity in size. It occurred three times on successive days. When the next week Col. Sizes II was learned the influence of I was shown in erroneously moving green *yellow* from 5th to 1st place where a yellow *green* had stood the previous week. The error occurred twice, with no others. When in the third week Col. Sizes I received its final test, aside from an exchange of two small adjacents, 3 yellow orange and 1 red, the only error was a removal of yellow *green* to 5th place. Here the corrections which the subject got in II worked to the undoing of I. When the following week II is tested the only error is moving green yellow to the left end, this time to second place instead of to first. The original error has recurred. Cols. A, which was learned with Col. Sizes I, shows the influence of this struggle. Its yellow orange is moved from 2d to 7th place, its green from 5th to 3d place. This is very much like the first-mentioned error in Col. Sizes I. The only other errors were an exchange of adjacents, violet blue and red.

C. F. d. S. III. Exchange of adjacent terms in final test, one pair only.

Forms I. In the final test aside from one exchange of adjacents, the errors are caused by moving Fig. 7 to 3d place. No explanation ascertainable.

Sizes B and C. The kind of schemes or systems most frequently made use of by all the subjects when the exposure was simultaneous is well illustrated by the one used by R in Sizes B. The correct order was 3416275, 1 representing the smallest, 2 the next smallest term, etc. Her system according to her own testimony was an ascending 'sky-line' from 1st to 6th places, 3467, the smallest and next smallest forming a second ascending series interpolated in the first at the 3d and 5th positions. The last term stood apart. In the final test the general nature of the scheme remains, but the details have become

[1] To understand these analyses of errors the Description of Series on pp. 74–80 doubtless will have to be frequently consulted. The prefixed figures 2, 3, etc., mean the second size, third size, etc, counting from the smallest.

confused, as shown by her arrangement 3451726. The next week, when Sizes C came up for final test the system which the subject had employed in learning B was introduced into C with entire correctness. The subject is of course unaware that it had belonged to another series. This is only one of the many striking illustrations in our experiment of the law discovered by Müller and Pilzecker, that of two similar and mutually interfering associations the earlier formed one becomes relatively stronger, the greater the lapse of time since the later one was learned.[1] In our own illustration the system used in B was imperfectly remembered when still in the shadow of learning C, but after the effects of learning C had had a week to die down, the B system recovered, unfortunately for the subject, in the wrong series.

Forms II. This series has three tall figures, distributed near the middle and on or near the ends. In learning, the series was balanced about them and on one occasion Fig. 5 was put in the middle place. After two weeks the subject has forgotten the exceptions to the balanced scheme, as shown by her arrangement, in which the three tall figures of the series are placed at the ends and in the middle. Fig. 2 is on the left, 5 in the middle and 7 on the right end. The low intermediate figures are not well remembered.

C. F. d. S. IV. Figs. 5 and 6 are exchanged. Possibly their similarity in size was a partial cause. The other error was moving Fig. 1 to the right of Fig. 7, of which no explanation other than simply memory-fading is offered. Both errors in the final test only

Cols. II. Many errors in final test. No explanation.

Cols. I. Final test. Again the arrangement is chaotic. Bright colors (red and green) are erroneously put in the middle where they were in the three other color series previously learned.

Col. Sizes A. The final test is badly mixed. The subject places only one term correctly,—2 red orange. 4 blue green and 3 blue are exchanged. They are next to each other in size and similar in color.

C. F. d. S. I. A single exchange of adjacents, Figs. 2 and 3.

C. F. d. S. II. Two exchanges of adjacents, Figs. 2 and 3, and 4 and 5. Shows interference of other series: Fig 6 (window)[2] is put in 1st place, occupied in C. F. d. S. III (which preceded the present series with this subject) by a figure of the same shape, but different color and size. Fig. 1 (slender vase) is put in 6th place as in Forms B, seen 1 day before.

Col. Sizes B. Three errors. Ends, blue and orange red, are exchanged, due, the subject says, to Cols. A, where a similar blue and red were on the ends. Latter series not seen for 30 days. Orange yellow moved to the right of the term belonging in 6th place. Reason not known.

[1] Müller and Pilzecker.: Ztsch. f. Psych., Ergänzungsbd. 1, pp. 124 and 138.
[2] These names are inventions of the operator.

Cols. B. Exchange of the two terms considered colorless or 'dead' by the subject, A-yellow light and warm gray no. 2.

Col. Forms I. All correct.

Forms B. An exchange of two figures adjacent and relatively very similar in shape, Figs. 4 and 5 (irregular octagon and double crescent).

Sizes A. Another typical illustration of the schemes used by all the subjects in the sizes of the simultaneous set. The correct order was 5416372. Her system consisted, she says, of three 'sky-lines:' the first a descending one formed by the terms 541, the second an ascending one formed by the terms 167, the third an ascending and descending one formed by the three smallest terms. Despite the small total area of the series and consequently greater difficulty in discriminating the terms the system served its purpose pretty well. After 13 days and many series learned in the interval the subject got the whole series correct except a single exchange, the smallest and third smallest. She remembered and used the system in reconstruction, but forgot the third 'sky-line.'

Col. Forms III. Influence of an introspection series given two and one-half months before causes the subject to put Fig. 5 (inverted hat) in 1st place, where a figure of the same shape and size, but red instead of yellow had stood. The other errors are displacements caused by this change.

Col. Forms. IV. All correct.

Col. Forms II. Poorly recalled and no special reason evident.

Sizes I. Same result as in Col. Forms II.

Forms A. Its mediocre retention is explicable on the basis of its extremely short learning time, 10 secs. The equal-armed cross, Fig. 3, is put in 5th place, where the larger long blue cross was in C. F. d. S. II, 20 days before. The other errors are exchanges of adjacents.

Forms III. In learning, a similarity between Figs. 6 and 7 and the number 10 was noticed. In final test Fig. 4 took the place of Fig. 6 in this idea. This brought Figs. 3 and 5 together forming an unnatural looking low 'sky-line' at this point, which R broke by putting Fig. 2 between Figs. 3 and 5. The two latter were also transposed. Note their similarity.

Col. Sizes III. In learning, the only error was an exchange of 6 and 7, which are not only similar to each other in size (and in color to this subject), but are also similar to the two largest areas of Sizes II (learned at the same time) in respect to the positions occupied. The exchange was a copy of the positions of 6 and 7 in the other series. Same error repeated in the final test, and also made by the subject next to be discussed.

Sizes II. Confused with the series just mentioned. In final test 6 and 7 were placed as they should have been in that series. Further errors in this series were moving 4 to left end and 1 to a place in the middle, both in imitation of Col. Sizes III. The remaining errors cannot be traced.

Cols. III. Her learning scheme was: 'group of the left three were pronounced colors, yellow first, purple and red next, the last two a displeasing combination. Group of the right three consists of a pale one (green tint) followed by two neutral colors. Blue associated with middle, a conspicuous position.' In final test the left three were correctly recalled, but green gray was put in the middle, with which place a gray had been connected in the week intervening between the learning and final test of this series (viz: in Cols. IV). The cue for the right three was forgotten.

Sizes III. Too poorly recalled to analyze. The left three have the regularly increasing size arrangement of the original, but do not begin with the exactly correct size.

Col. Sizes IV. An almost complete copy of her last week's arrangement of Col. Sizes III, which she had recalled uncommonly well. The order there was 3465172 instead of 3475162. The order here was 3465271. The only difference is an exchange of the two smallest. This is very different from what the series should be, viz: 3162547.

Sizes IV. Too poorly recalled to analyze. Her arrangement is similar in a general way to the original in the fact that each wing consists of a large one flanked by smaller ones, but the wings are exchanged.

Forms IV. In final test exchange of Figs. 5 and 6, due to the identity of Fig. 7 with a part of Fig. 5. The confusion of similars most frequently shows itself by an exchange, partial or complete, but it seems not unlikely that at other times it results in bringing together the terms confused. This is very plainly the case here and in Cols. IV with the next subject, N. The remaining error in Forms IV was an exchange of Figs. 1 and 3.

Cols. IV. In learning, red and green in 3d and 5th places, with gray between them, formed a group. The complementary character of red and green, noted by the subject, is both a help and a risk. They were exchanged once in learning, and in final test green is again put in place of red, the latter being displaced to 2d place. The other error consists in putting blue tint in 6th place, where R had wrongly put a similar color last week. (Green blue in Cols. III.)

Subject N.

Cols. A. Influenced by Cols. II learned the week before, three colors being placed as were similar colors in that series. Blue is moved from 1st to 3d place, red from 7th to 5th and green from 5th to 6th. The other errors result from these displacements.

Forms I. Exchange of adjacents, Figs. 4 and 5.

In discussing four of the series immediately following this chronological table will be of service.

7th week. Learned Sizes B and Col. Sizes I.
8th week. Learned Sizes C and Col. Sizes II.
9th week. Final test of Sizes B and Col. Sizes I.
10th week. Final test of Sizes C and Col. Sizes II.

Sizes B. The correct order is 3416275. In final test the subject gave 3451726,—an exchange of sizes 6 and 7, and a removal of 5 to 3d place. The latter error gives a longer 'sky-line' in the first three terms, a peculiarity which may have been due to Sizes C learned the preceding week. Both the peculiar shape of the terms in this latter series and their order (26, 457, 13) emphasized lines ascending to the right. All of the subjects spoke of this and considered it rendered the series easier.

Col. Sizes I. Same erroneous idea of pronounced upward slope in the final test of this series. Instead of the correct order, 2317645, 3457126 is given. From the point of view of size this cannot be anything else than two upward slopes.

C. F. d. S. III. Exchange of Figs. 5 and 7. No similarity.

Forms II. Two exchanges of similar forms, Figs. 2 and 5, and 3 and 6.

C. F. d. S. IV. Fig. 8, a small inverted hat, moved to the left of Figs. 4 and 5. No reason apparent.

Col. Sizes II. Final test was the same as in Sizes B, with a slight change. Here it is 2351746. There it was 3451726.

Sizes C. In the final test the subject's arrangement was 3451726, the correct order being 2645713. This is exactly the same mixture of B and C which the subject employed in the final test of B, a week before.

Col. Forms IV. One displacement, Fig. 6 moved to the left of Fig. 4.

Cols. II. All correct,—very unusual. His scheme was: 'The heaviest (red) with the two lightest (yellow and green tint 1) on either side, were next to the right end (4th to 6th places). Of the remainder a light one (orange red tint 2) was on the left end followed by two heavy ones. The one in 7th place was correctly placed by being left over after the others had been arranged.'

Cols. I and B. I preceded B a week in learning. In final test I shows plainly the influence of subsequently learning B. The only error is a removal of green yellow and red orange shade from 6th and 7th places to 2d and 3d, and a consequent rightward displacement of 2–5. This is approximately the position of a bright red and yellow in B, namely, 3d and 4th positions. B, however, is also affected in its final test the next week. The interference is mutual. Aside from an exchange of adjacents (5th and 6th) the only error is a removal of green from 7th to 2d place where a bright green stood in I.

Col. Sizes A. The errors are due to a confusion of terms within the series, similar to each other in size and color. All of the subjects experienced this difficulty. The similarities in color constituted a defect in the series, and were one of the reasons why it was rejected from the regular series. In the case of this subject the errors were an exchange of 3 blue and 4 blue green, and of 5 green tint and 6 A-green yellow.

C. F. d. S. I. Two exchanges of adjacents.

C. F. d. S. II. This series shows interference of other series. Only the 1st and 4th are correctly placed. Fig. 7 (spire) is moved to second place occupied by a figure of the same shape but different color and larger in Col. Forms II seen 5 days before . Fig. 5 (tall cross) is put in 3d place occupied by a maltese cross of a very different color and size in C. F. d. S. IV. Fig. 2 (trademark) is put in 6th place. The same figure but very much larger and of slightly different color had been in 7th place in C. F. d. S. IV. Fig. 3 (pin-wheel) is put in 7th place where a figure of the same shape but different color and size had been in Forms II. C. F. d. S. IV. and Forms II. were last seen 31 days before.

Col. Sizes B. Recall too poor to analyze.

Forms B. Fig. 4 moved to 1st and Fig. 7 to 4th place. No explanation.

Sizes A. Exchange of sizes 2 and 3, and 4 and 5.

Col. Forms I. Exchange of adjacent Figs. 4 and 5.

Col. Forms II. All correct. The subject said: "Figs. 3, 4, and 5 were recalled by the appearance of the three as a group. Figs. 1 and 7 were associated with their positions, and Figs. 2 and 6 were contrasted with each other and associated with their positions.

Forms A. Interference of other series. Fig. 7 (wide vase) put in first place where a slender vase of different color had been in C. F. d. S. II 12 days before. Exchange of Figs. 5 and 6. Fig. 3 (equal-armed cross) put in 7th place for no apparent reason.

Sizes I. Exactly the same arrangement as was given for Sizes C 43 days before!

Sizes II. In learning the only error was arranging the series once in the exact arrangement of Col. Sizes III. Despite the correction which it had received, the same mistake was made in final test, the only alteration being an inversion of the last two terms.

Col. Sizes III. An illustration of the fact that the interference is always mutual. This series and Sizes II, learned at the same time, were confused with each other. The other series fared the worst, but in the final test of this series, 6 and 7 were arranged as they were in that series. There was also an inversion of the last two terms, as in Sizes II.

Col. Forms III. Fig. 5 (inverted hat) put in 1st place, occupied by a figure of the same shape and size, but different color, three months before in an introspection series. The only other error, aside from the rightward displacement of Figs. 1, 2 and 3, was an exchange of the similar figures, 4 and 6 (tombstone and knobbed oblong).

Forms III. One error in final test, an exchange of the obviously similar figures, 3 and 5 (pentagon and pentagon with curved upper sides).

Colors III. The errors were two exchanges: one of green yellow and orange yellow, similar colors, the other of colors not similar. The association of a smaller figure of the same color as one of them with the place where one of these two is put, may have been the cause. The interval was a week.

Sizes III. In final test the arrangement is quite similar to the correct order in a general way, but not in detail. Instead of 2451376 is given 3561274. This series and the next two are good illustrations of interference within a series due to poor perceptual discrimination, the kind which Ranschburg found in his six-place numbers. The very fact that the arrangement is similar only in its general contour as a whole shows incomplete perceptual discrimination of similar things, of the kind that he found.

Sizes IV. Confused with Sizes III, learned at the same time. Once in learning it was given as 3461275, the correct order of Sizes III being 2451376. The only other error made in learning was a removal of 3 from one end to the other. In the final test there was a general resemblance to the correct order. 3741265 is given in place of 4761253. It is true that the correct orders of III and IV somewhat resemble each other, but in the former the two largest are on the right, while in the latter they are on the left end. In the final arrangement of IV, the largest is back in place.

Col. Sizes IV. 1 and 4 exchanged in final test, due to an exchange of 3 and 4 in learning. Cf. their positions in the correct order, 3162547.

Forms IV. Adjacent Figs. 2 and 3 exchanged in final test.

Col. Forms IV. Figs. 5 and 7 exchanged. No similarity.

Cols. IV. Too poorly recalled to analyze. Black, 1st place, and gray, 4th place, moved to 3d and 2d places respectively,—an instance of similars brought together.

There were also 6 exchanges of numbers belonging to sizes nearest each other in area, which occurred during the learning of some of the above series, and which have not been heretofore mentioned.

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PREFACE.

In the first portion of this monograph are presented the results of investigations made in the psychological laboratory of the University of Chicago during the years 1905-07. The experiments which form the basis of the remainder of the work were carried on during the year 1907-08 in the Harvard psychological laboratory.

To the directors of these two laboratories, Professor James Rowland Angell and Professor Hugo Münsterberg, the writer desires to express his gratitude for patient counsel and stimulating criticism. He wishes also to acknowledge his obligation to the fellow-students of experimental psychology, who, in the capacity of observers, made possible the prosecution of these studies.

To the investigations of Professor R. H. Stetson in the field of rhythm the writer owes the method of attack employed in studying the relationships of muscular movement to the melody experience; and the outline of a motor theory of melody with which the present study is brought to a close is obviously the outgrowth of suggestions from Professor Stetson's important publications. Indebtedness to Professor Max Meyer is likewise evident, and nowhere more plainly than in those passages which express disagreement with his views.

My controversy with Professor Meyer is in part made necessary because of what seems to me to be an equivocal use of the term 'tonal relationship' on his part; and lest a similar ambiguity creep in to vitiate the discussions of the following pages, I have taken pains in each instance to specify in which of its two common meanings the term "relationship" is used. Musicians speak of two tones as directly "related" when the ratios of their vibration-rates are so simple that one tone is found among the first five partials of the other, or, what amounts to the same thing, when the two tones belong to a major triad, the 'chord of nature.' The "feeling of relationship" is the experience of coherence, of 'belonging-togetherness,' which characterizes the hearing of two successive tones of the sort described. The question as to what pairs of tones arouse this feeling of "relationship" must of course be answered not by an arbitrary definition but by reference to the facts of experience.

Now it is perfectly evident that this particular kind of tonal "relationship," arising out of certain acoustical properties of the sounds, is not the sole kind of relationship which may bind tones together in our experience. Two tones may come to be felt as related, in a way, merely because they have often been heard together. Moreover any two tones whatsoever, be their ratios simple or complex, are felt to be related to each other as higher and lower. Here the term relationship is used in its ordinary broad, untechnical sense.

Whenever, in the following pages, the terms "relationship" and "related" are employed in the technical sense, they are enclosed in quotation marks; and where these marks are not used, the reader is to understand that the broader, untechnical connotation is indicated.

What the musician designates as tone-color or timbre, I have called by the usual psychological terms, clang-color, or briefly, color.

CONTENTS

PART I.

THE MELODY PROBLEM.

§1. Neither musicians nor psychologists are agreed as to the meaning of the term melody. Divergent usage, leading to misunderstanding and dispute, has arisen because within the range of melody experience there exist several distinguishable mental phenomena, each of which has in turn been construed as the essential mark of a melody. Weinmann,[1] following Lipps,[2] says that a melody is a *unity*, a whole, no mere succession of tones. It is, further, an esthetic unity in which the constituent tonal elements are subordinated to a single dominating element, the tonic. This definition operates to limit the scope of his study to such melody phenomena as those exhibited in modern European diatonic music, since it *a priori* excludes the possibility of melodies which lack tonality.

The doctrine of Lipps and his followers that esthetic unity always involves the *subordination* of the separate elements of a manifold to a single chief element is opposed by Meyer[3]. In his view, the statement that a melody is a unity means merely that we experience *relationship* between the tones. Indeed Meyer defines melody in terms of relationship.[4] To say that two tones are related and to say that they form a melody is the same thing. Such a definition avoids a narrow conception of melody. The scope of the term becomes much contracted, however, by the technical meaning which Meyer attaches to the term relationship. The essence of melody consists, for Meyer, not in the experience of any kind of relationship whatever between the successive tones, but in the experience of a very

[1] Fritz Weinmann: "Zur Struktur der Melodie" *Zeits. f. Psychol.* 1904, *35*, 340.

[2] Th. Lipps: "Zur Theorie der Melodie," *Zeits. f. Psychol.* 1902, *27*, 237. See also his *Psychologische Studien*, 2te Aufl. 1905, 193 ff.

[3] M. Meyer. "Unscientific Methods in Musical Esthetics." *Jour. of Phil. Psy., and S. M.* 1904, *1*, 711.

[4] Elements of a Psychological Theory of Melody. *Psych. Rev*, 1900, *7*, 246.

special and limited kind of relationship, namely that to which the technical musical term "relationship" has come to be applied. This narrowing of the meaning of the term operates to exclude from the realm of melody those songs of primitive peoples in which vague and indefinite pitch intervals appear, as well as the so-called melodies of speech.

Can we assent to Meyer's contention against Weinmann that melodic unity means nothing more than relationship between the parts? The esthetic unity which characterizes a melody does indeed involve experience of relationship among the several tones; but this is not all. For example, it involves also the experience of completeness. If the feeling of completeness is destroyed, the 'unity' is shattered. Not merely tonal relationship, but 'form' is necessary to constitute the esthetic unity of a melody. Meyer's deed here is better than his word: for throughout his investigations he searches for something more than mere "relationship" in his melodies, namely, for an *organization* of relationships, a combination of related tones ordered in one way rather than another,—arranged, indeed, so that they generate not a mere consciousness that the elements are related, but a perception that they are so related as to form a complete structure, a whole.

There are then, three clearly distinguishable phenomena, each one of which has been put forward as the peculiar differentia of melody: (*a*) "relationship" between the constituent tones; (*b*) esthetic unity or wholeness, such as distinguishes a definite melodic phrase when contrasted with a mere fragment of melody, or which characterizes even more clearly a complete melody that is brought into comparison with any portion of itself; (*c*) tonality, or the dominance of the entire sequence by a single tone, the tonic. Weinmann's definition stresses the third of these phenomena: if there exists a song of some alien people in which the preponderance of one tone over the others fails to appear, such a song must be called by some other name than melody. Meyer at the opposite extreme, emphasizes only the phenomenon of "relationship." Wherever "relationship" between successive tones is felt, a melody exists, even though the succession be fragmentary and the hearer be left in suspense, unsatisfied.

For the purpose of the present exposition, it has seemed best in defining what shall be meant by a melody, to place emphasis upon the second of these three phenomena,—upon the esthetic unity, the wholeness, which characterizes the completed experience. This usage of the term is adopted with full realization that it is not wholly unobjectionable. After such a definition, how shall one speak of Wagner's 'endless melodies?' By what name shall one describe the effect when in a Brahms chorus, one of the middle voices for a few brief measures stands prominently forth only to be lost to the ear again in a maze of counterpoint? Is not this tonal group without distinguishable beginning or end a most delightful melody? It would certainly be called a melody if, with Meyer, we had chosen to make "relationship" the sole essential; but in the terminology we have chosen, it must be called a melodic fragment, and not, strictly, a melody.

The matter of prime importance is, of course, to realize that by whatever names they may be called, we are confronted with three different phenomena—"relationship," phrase- or period-unity, tonality—which, no matter how intimately they may prove to be bound up together, are nevertheless in introspection clearly distinguishable, and must not be confused.

§2. At the risk of incurring the charge of prolixity from readers who are most at home in this field, I shall venture to develop somewhat more fully what I mean by a melody, before attempting to formulate explicitly the melody problem.

Let the reader ask himself in what way his experience of a melody differs from his experience of a mere succession of musical sounds of varying pitch. Possibly he will reply that the group of sounds that he calls a melody is more pleasing. But this agreeableness he will admit is not the essential character. One may, for example, upon hearing a flageolet of obnoxious tone quality find the whole experience disagreeable and yet recognize that what he is hearing is a melody; or on the other hand one may take delight in a perfectly random series of sounds drawn from a beautifully voiced instrument. Something other than the pleasurable affective aspect of the

The difference which the actual clang-color makes is of course at the basis of artistic orchestration of melodies and of organ-registration. When a theme given out by the oboe is repeated by the violins we say it is the same melody, and yet it is not wholly the same.

Fourthly, the dynamic factor, the actual loudness or softness of the melody as a whole, remains to be mentioned as one of the contributors to the nature of the melody.

§5. These four factors taken in their actual or 'absolute' aspects are, however, of very secondary significance as compared with these same factors operating within the melody itself to contrast and to bind together the separate tonal elements. With reference to the *relative* duration, pitch, etc., of the individual tones, it will be convenient to treat of (*i*) the relation of each tone to its immediate associates, and (*ii*) the relation of the tone to the whole melody. (*Cf.* accompanying outline).

ELEMENTS OF MELODIC STRUCTURE CLASSIFIED ACCORDING TO THE FACTORS OF

I. DURATION

a) Actual	*b*) Relative
(*Tempo*)	i. Measure pattern
	Rhythmical figuration
	ii. Accel., Rit., etc.

II. INTENSITY

a) Actual	*b*) Relative
	i. Accent, stress, etc.
	ii. Cresc., decresc., etc.

III. COLOR

a) Actual	*b*) Relative
(*Orchestration*; *Registration*)	i.
	ii.

IV. PITCH

a) Actual	*b*) Relative
(*Absolute pitch*)	i. Interval relationships
	ii. Tonality relationships

Relations of duration of the first sort are at the basis of the measure-form and rhythmical figures, while accelerando and ritardando illustrate the relations to a more inclusive group. Rhythm is usually a result of the combination of intensity and duration relations, although this is not always the case. Thus a melody played on the organ or on a mechanical piano player lacks variations of intensity of the separate tones.

In the case of the loudness factor, the former type of relation determines the effects of accent, of stress; while the latter gives dynamic form to the whole group, the crescendo-decrescendo effects, etc.

The relative color of the separate tones has, in the enumeration of the factors of melodic structure, usually been neglected. But *a priori*, one would expect this attribute of tone-sensation, as well as the others, to be of significance; and *a posteriori*, color is found to be of vastly greater importance to melody than one might suppose who had never given the matter careful thought. The reason why this factor has been overlooked is that it usually remains constant throughout the melody. Its presence as a unifying factor first comes into evidence when an unwonted change of color enters and makes itself felt as a disturbing element: as when a singer is not skillful in passing from one register of the voice to another, or a clarinetist meets a similar difficulty in making the transition from the lower to the middle register of his instrument. The changes in color which are thus unwittingly or unavoidably introduced have their disintegrating effect, be it never so slight, upon the melody. Among violinists this is a well known fact, a commonplace. Even so slight a change of color as is involved in the passage from one string to another is recognized as of importance in artistic phrasing, and the resources of technical proficiency are sometimes taxed in the effort to meet the requirements which this principle imposes. Such a principle raises a prohibition against careless shifts of color, and at the same time offers a positive aid to artistic phrasing,—it enables the violinist to give to a group of tones a peculiar unity of its own not otherwise obtainable. Surely such a factor in the determination of melodic form as clang-color,—a factor which has a recognized place in musical practice,—does not deserve to be entirely neglected. A careful experimental study of the effects and of the possible extent of alterations of color within the melody is a psychological desideratum.

§6. All of the factors which have been discussed, the relative clang-color, loudness and duration of the sounds, have been shown to contribute to the structural unity of a melody. But not all of these taken together are sufficient to make a melody. The essential factor is still lacking, namely the pitch relations. A sequence of tones of the proper relative loudness and duration to constitute a vigorous rhythm would not be called a melody if the pitch of the tones were either uniform or random.

The pitch, too, of each tone bears certain relations to the group of tones as a whole. This makes possible such phenomena as tonality, of which it will be necessary to treat in due time. At present let us focus attention upon the relations which may exist between individual tones.

These relations between tone and tone are of several distinct types. That type which has received fullest treatment at the hands of the musical theorist is the one which has appropriated to itself as a technical term the word "relationship." Two consecutive tones were said by Helmholtz[1] to be "directly related" if they form a perfectly consonant interval, in which case one of the clearly perceptible upper partials of the first is identical with one of the second; while to be "indirectly related" the two tones must each stand in some such direct "relationship" to a common third tone. This theory of "relationship" was used by him to account for the melodic intervals of the diatonic scale. To account for the appearance of chromatic intervals, 'accidentals', in melodies, Helmholtz further recognized a "relationship by propinquity"; the 'accidental,' he said, is 'related' to its neighbor by the mere fact of nearness. The fundamentally important type of "relationship" was, however, of the other sort; and since it had a basis in the physical laws of vibrating bodies, it naturally was described in terms of ratios of vibration rates. Like the phenomenon of consonance with which it is closely allied, direct "relationship" seemed to be dependent upon the partial identity of overtones which exists among "related" tones.

What now is the psychological phenomenon of which these physical facts seem to be the origin? In what way does one's

[1] H. Helmholtz, *Sensations of Tone*, tr. by Ellis, 1895, 256 and 350.

experience of a pair of "related" tones differ from that of a pair of "unrelated" tones? The difference is easily felt, but difficult to put into words. I shall here merely quote some more or less descriptive phrases from the records of my observers. When two "related" tones are heard in succession they are felt to 'cohere', to 'belong together', to 'articulate', to 'form parts of a larger whole.' "Unrelated" tones do not so behave. Rather they are felt to 'fall apart', to 'be unrelated'; 'they do not seem to belong to the same melody.' Tones at an interval of a major third exhibit a strong melodic "relationship." If the interval is increased by a quarter of a tone the "relationship" disappears. This phenomenon of "relationship" is not to be confused with that of consonance. The dissonant major second, for instance, is an interval whose tones exhibit melodic "relationship." What the significant connection is which exists between melodic "relationship" and consonance will be pointed out later.

Another type of relation which exists between the successive tones of a melodic interval may be called the relation of pitch distance. As regards their pitch all tones range themselves in a one-dimensional series, as higher or lower; and the relative position of two tones in this series finds its conscious representative in this feeling of pitch distance. Thus, the tone g' is felt to be at a certain pitch distance from c'; while its distance from d' is felt to be not so great. It is at once perceived that one's consciousness of the distance-relation between two tones is clearly distinguishable from one's consciousness of their consonance or of their "relationship."

It will be found useful to distinguish 'definite' from what may be called 'indefinite' pitch relations. The former are characteristic of all melodies which employ the definite intervals of a fixed scale. Some kind of 'indefinite' pitch relation must be experienced by that peculiar type of unmusical person who has no exact sense for intervals, but who enjoys hearing himself sing, and who can sing simple melodies in perfect time, and with so much sense for pitch relations as is shown in ascending when the melody should ascend, and then descending when the course of the melody takes a downward turn. The pitch-outline or melodic curve of his song corresponds in a vague, general way with the pitch-outline of the melody imitated, and in-so-far it betrays some kind of a sense for pitch relationship. These 'indefinite' pitch relations are characteristic of certain primitive melodies.[1] They also are of vast importance in the so-called melodies of speech. Indeed, the infinite variety of delicately expressive inflections which enrich our spoken intercourse must be recognized as based upon pitch relations of this 'indefinite' kind. The gross difference between the rising interrogative inflection and the falling assertatory is the most obvious example of this type of melodic relationship. The mental effects produced by mere rise in pitch have been described by Meyer in terms of effects upon the attention.

> A rise in pitch causes the hearer's attention to become strained, and the more so, the steeper the ascent, if I may use this expression. A fall in pitch, on the other hand, causes a relaxation of attention, a cessation of mental activity. . . . . The same strain and relaxation of attention is to be found in music. The normal end of a mental process is, of course, characterized not by strained, but by relaxed attention; for strained attention means continued mental activity. It is natural therefore that a melody ends with a falling inflection. . . . [2]

We shall have occasion frequently to refer to the significance for the melody problem of this "phenomenon of the falling inflection."

§7. If one carefully examines different melodic intervals to discover whether there may not be still other types of relation, he will probably disclose to himself a phenomenon which has received much attention at the hands of certain writers. He will notice that many melodic intervals exhibit a peculiar character which shows itself as a tendency for us to prefer one of the two tones as an end tone. The interval of the minor third, whose tones have the vibration ratio of 5:6, possesses no such attribute: one acquiesces indifferently in either the upper or the lower as a final tone. Neither tone has any very positive characteristics of finality about it. Not so, however, with the perfect fifth (2:3). If one hears it as an ascending interval, he is dissatis-

[1] *Cf.*, B. I. Gilman, "Hopi Songs," Jour. of Am. Ethnol. and Archeol. 1908, *5*, 14 and 224.

[2] *Am. Jour. Psych.*, 1903, *14*, 456.

fied, uneasy, and under more or less tension until he hears the first tone over again. But if it is a descending fifth which he hears there is acquiescence, satisfaction, repose, and no desire to hear the first tone a second time. One may say that one of these tones stands to the other in the relationship of 'tonic', or end-tone. This aspect of musical intervals will be called by the present writer their *melodic trend.*

Observation of this phenomenon as it shows itself in intervals of relatively simple vibration ratio has led some theorists, notably Lipps and his followers, to attach great importance to the 2 ratio. They find, for example, that the trend of the fourth (3:4) is very decidedly toward its upper tone as a final tone; of the major third (4:5), toward the lower; while the minor third (5:6) exhibits no noticeable trend whatever. The trend of the major second (8:9) is toward the lower, and of the minor second (15:16) toward the higher tone. Among the wider intervals, where the reader may perhaps feel that the phenomenon is not always so distinctly and unambiguously manifest, it is nevertheless held that the minor sixth (5:8) and the minor seventh (9:16) trend upward and the major seventh (8:15) downward, while the major sixth (3:5) shows no trend toward either upper or lower tone.[1]

It will be seen that in the case of every one of these 'pure' intervals the trend is toward that tone whose rate is a pure power of 2; 2 always becomes the tonic. Where neither rate is a pure power of 2, no trend is discovered. These phenomena have been grouped by Lipps under what he calls the 'law of the number 2.'

Kürzer gesagt:—Treffen Töne zusammen, die sich zueinander verhalten wie 2^n: 3, 5, 7 usw., so besteht eine natürliche Tendenz der letzteren zu den ersteren hin; es besteht eine Tendenz der inneren Bewegung, in den ersteren zur Ruhe zu kommen. Jene "suchen" diese als ihre natürliche Basis, als ihren natürlichen Schwerpunkt, als ihr natürliches Gravitationszentrum.

Dies ist naturgemäss um so mehr der Fall, je kleiner das (n) ist.

[1] These statements of typical trends are not completely in harmony with the results of the experiments described below. Differences are most in evidence in the case of the major and minor sevenths. See p. 25 *ff.*

(n) ist aber am kleinsten, wenn es gleich o ist. Und 2^0 ist gleich 1. D.h. die vollkommenste Ruhelage und das letzte Gravitationszentrum solcher Töne bleibt immer der absolute Grundrhythmus.[1]

Upon this law of the compelling, dominating character of the 2 ratio, together with the principle that melodic "relationship" is closer the simpler the ratios, Lipps grounds his theory that a melody is a structure which gains its esthetic unity through the subordination of all its elements to one over-mastering ground-ratio, the tonic. This theory has been elaborated, in its application to modern European music, in admirable detail by Weinmann,[2] and defended vigorously by the author himself.[3]

In undertaking to explain *why* this phenomenon of melodic trend toward the power of 2 should manifest itself, Lipps makes one fundamental assumption, the assumption

> that to the rhythm of the physical vibrations which generate a tone there corresponds an analogous rhythm in the accompanying processes of tone-sensation, or in the accompanying change of psychic or central conditions; that thus the psychic or central process of tone sensation is separated into a succession of elements or elementary partial processes analogous to the succession of physical partial processes, *i. e.*, to the single sound waves.[4]

Such a correspondence between the nature of central processes and the physical processes which arouse them, Lipps has found it necessary to postulate not merely in the realm of audition, but throughout the range of sensory experience. Esthetic pleasure results from inner harmony of our mental (or central) energies. A color-contrast is beautiful if there is a subconscious apprehension of the simplicity of the combination of the ether vibrations.

In the present state of total ignorance with reference to the intimate nature of central processes no attempt can be made

[1] Lipps. *Psychologische Studien*, 2 Aufl., 1905, 195. An identical formulation is given in his *Grundlegung der Aesthetik*, 1903, 465.

[2] F. Weinmann, "Zur Structur der Melodie. *Zeits. f. Psychol.*, 1904, *35*, 340–379 and 401–453.

[3] *Cf.*, especially, *Psychologische Studien*, 193 *ff*.

[4] *Zeits. f. Psychol.*, 1902, *27*, 228.

either to establish or to disprove such an assumption. By those who cannot follow Lipps in his bold hypothesis, his theory of the number 2 must be viewed merely as a description, not an explanation, of the facts.

Weinmann undertakes to buttress this theory of the basic nature of 'duality' in vibration-ratios by reminding the reader that 'double rhythm' is the original rhythm, the simplest, the most natural, etc.[1] But this is an argument from sheer analogy; for the experience of *rhythm* in the ordinary sense of the word has nothing whatever in common with the unperceived micro-rhythm of Lipps' assumption. One is a phenomenon open to introspection, observation and experimental study: the other is hidden, unknown, hypothetical.

Even though one may not relish such a theory as that of Lipps and Weinmann, and though one may be inclined to doubt the adequacy of their formulation of the facts by means of the law of the number 2, nevertheless the phenomena of melodic trend remain and must be reckoned with. Why is it that some melodic intervals seem to end better on the upper tone and others on the lower, while with still others it is a matter of indifference which of the two tones comes last? Why is a rising fourth more 'complete' than a rising fifth? Why does an ascending major second create a demand to hear the first tone over again, while an ascending minor second does not?

§8. No further attempt will here be made to enumerate with greater completeness the various mental phenomena which flow from the facts of pitch relationship. Only those have been mentioned which are of especial significance for these studies: pitch distance, definite melodic "relationship," indefinite pitch relations, consonance, melodic trend, the phenomenon of the falling inflection. We shall later have occasion to ask which of these phenomena are primary and which secondary or derived.

Our survey of the factors—of pitch, duration, clang-color and intensity relations—which contribute to the structure of a melody, makes possible a more definite formulation of the limited purpose of these studies.

[1] *Op. c.*, 342.

How the *pitch* relations of a series of discrete musical sounds may operate to weld these sounds into the organic whole which we perceive as a melody,—this is the core of the melody problem, and to this primary phase of the subject our present investigation will be strictly limited. To this end we shall consider pitch alone, and abstract as far as possible from all considerations of rhythmic figuration, accent, force, tempo, tone quality, etc., although these various factors would all demand attention in any account of the melody problem which aimed at completeness.

PART II.

THE PHENOMENA OF MELODIC "RELATIONSHIP" AND OF MELODIC TREND.

§9. The reports of previous experimentation specifically directed toward the melody problem are few in number.

One of the most original and suggestive workers has been Professor Meyer, and a survey of his contributions will serve to bring our own problem more clearly to view.

The first of Meyer's experimental investigations[1] led him to reject the theory of the diatonic scale, and to develop a new theory of melody. He used a reed organ specially constructed so that in playing a melody the performer was enabled, for each note of the printed score, to select any one of two or three tones of nearly the same pitch. Thus after repeated trials he could determine precisely what intonation of any particular melody was most satisfactory.[2]

Meyer published his analysis of some thirteen melodies, giving the intonation of each which seemed to him to be the best. These include melodies of folk songs and chorals as well as melodies from well known classical compositions. The reader is not surprised to find that the preferred intonation does not coincide with that of "equal temperament;" but neither does Meyer find that the melodies are most satisfactory when played in the justly intoned diatonic scale familiar to musical theorists. To be sure, in the simpler melodies, most of the pitches in the preferred intonation correspond exactly with the pitches when the melody is played in accordance with the diatonic scale. Some marked exceptions appear, however. Meyer finds, for instance, that

[1] M. Meyer: "Elements of a Psychological Theory of Melody." *Psych. Rev.*, 1900, *7*, 241–273. Reprinted with revisions and additions in "Contributions to a Psychological Theory of Music," *Univ. of Missouri Studies*, 1901, *1*, 1–80.

[2] A description of the instrument, with diagram of arrangement of keys on the manual is found in the *Zeits. f. Psychol.* 1903, *33*, 292.

the 'fourth' is preferred flatter and the 'sixth' sharper than diatonic intonation demands. To render the nature of these differences more clear, reference may be made to the accompanying table.

TABLE NO 1.

	c	*d*	*e*	*f*	*g*	*a*	*b*	*c*
Ratios of pitches in diatonic scale	1	9/8	5/4	4/3	3/2	5/3	15/8	2
		9/8	10/9	16/15	9/8	10/9	9/8	16/15
	24	27	30	32	36	40	45	48
Some corresponding pitches from Meyer's Complete Scale.	1	9/8	5/4	**21/16**	3/2	**27/16**	15/8	2
		9/8	10/9	21/20	8/7	9/8	10/9	16/15
	16	18	20	21	24	27	30	32
Diatonic scale	48	54	60	64	72	80	90	96
Meyer's	48	54	60	**63**	72	**81**	90	96

The first line of fractions shows the ratio between the vibration rate of each note of the diatonic scale and the vibration rate of the key note. Reducing these fractions to a common denominator, we obtain as the resulting numerators the numbers in the third row of the table. These are the numbers usually employed to express the relative pitch of the notes in the diatonic scale. (The ratio between the vibration rate of each note and that of the next note in the scale is given in the second line of fractions).

For comparison with these, I have selected from Meyer's 'Complete Scale' those notes which are used in the simpler melodies (see lines 4, 5 and 6 of the table).

It is to be noted, first, that the ratios in the diatonic scale involve no prime number but 2, 3, and 5, whereas the other scale employs the number 7 in its fourth. Thus, to tune *f* in the key of *c* one would not tune it a perfect fourth above *c*, but would tune it at an interval of an harmonic or sub-minor seventh (7:4) above the *g* below. Moreover the denominators of all eight ratios from the newer scale are pure powers of 2 whereas this is not the case with the fourth and sixth of the diatonic scale. The amount of difference in pitch which is involved is shown in the last two lines of the table where the ratios of the two scales are reduced to a common denominator for comparison.

To understand the significance which attaches to these differences, and other more marked differences in intonation which come to light in the more complex melodies, it is necessary to examine two "laws of melody" which, if one follows Meyer, lie at the basis of musical theory.

§10. The first of Meyer's laws of melody may be called the law of *melodic "relationship*:" Only tones which are "related," directly or indirectly, can belong to the same melody. The second, a law of melodic trend, is similar to Lipps' law of the number 2.

We will give Meyer's own formulation of what he means by the term "relationship."

When we hear successively two tones, the vibration rates of which are to each other as 2:3, or briefly speaking, the tones 2 and 3, we notice something not describable, which I shall call the *relationship* of these tones. To understand what is meant hereby, the reader may listen to the successive tones 7 and 11 or 11 and 10, in which cases he will notice that the two tones have no relation at all to each other.[1]

It is a fundamental contention with Meyer,—a contention that will demand our critical scrutiny,—that this psychological quality called "relationship" attaches only to pairs of tones whose ratios are expressible in simple fractions involving no prime number above 7.

That no relationship at all is to be observed with tones represented by the prime numbers 11, 13, 17, 19, etc., leads to the conclusion that only tones represented by the prime numbers 1, 2, 3, 5, 7, and their composites possess that psychological property.[2]

This leads to the theory of what Meyer names 'the complete scale.' Since none but related tones can belong to the same melody, and since "relationship" seems to exist only between tones represented by products of 2, 3, 5, and 7, the complete musical scale, or the series of all the tones which may occur in a single melody, is represented by the infinite series of all products of the powers of 2, 3, 5, and 7 (p. 249). The beginnings of such a scale, containing so many of these related products as were found

[1] Meyer: *Psych. Rev.*, 1900, 246.

[2] *Op. c.*, 247.

to be needed in the analysis of the melodies he studied, are given by Meyer in tabular form.

In maintaining that the 7 ratio exhibits the fundamental melodic qualities and must not be excluded from musical theory, Meyer takes sharp issue with traditional treatments of the subject. Lipps and his followers who have done more than anyone else to place the theory of melody on a basis of exact descriptive formulation find no need of ratios involving prime factors larger than 5. Other writers, as Helmholtz, Gurney and Stumpf have also been content with the theory of the diatonic scale, a scale whose ratios employ the numbers 2, 3, and 5, but not the number 7. Against these, Meyer brings the charge that they have been influenced primarily by considerations involving the phenomena of harmony, and have failed to point out what facts observable in *melody* justified them in excluding the number 7. The facts as he finds them are that such melodic intervals as the sub-minor seventh (4:7) the sub-minor fifth (5:7) the septimal second (7:8), etc., do possess the pyschological quality of "relationship;" and what is of more weight, he finds that melodies played in his so-called complete scale, which admits the 7 ratio, are preferred to the same melodies played according to the diatonic scale.

Meyer has been subjected to criticism for publishing his experiments and basing an elaborate theory upon them, when the judgments of preference recorded are apparently those of a single observer, namely, the author himself. Meyer admits the force of these criticisms, but insists that even so much of induction and carefully systematized observation as this report of his studies embodied, has more claim upon the attention of a scientific reader than all the great mass of writing upon musical theory which has no scientific, inductive basis whatever.

How does Meyer account for the phenomena of melodic "relationship?" How does he explain the fact that we feel the tones 2 and 3 to be "related" and the tones 11 and 10 "unrelated?" In contrast to Lipps he does not attempt to account for the facts. On the other hand he frankly admits that he is not offering an explanation of the melody phenomena: for this, as well as for an explanation of the facts of consonance we must await further light upon the nature of neural activity and the action of the sense organs. All that Meyer is attempting, then, is to comprehensively *describe* the facts.

His first step toward this descriptive formulation has already been mentioned. As a result of his examination of the phenomenon of melodic "relationship" he decided that all cases of "relationship" are capable of being expressed in relatively simple fractions involving no prime factors except 2, 3, 5, and 7; and consequently the 'complete scale' is limited to tones expressed in these numbers and their compounds. The second step is the formulation of a law of melodic trend similar to, but not identical with, that of Lipps:

> When one of two related tones is a pure power of 2, we wish to have this tone at the end of our succession of related tones, our melody.[1]

Expanded to cover melodies of more than two tones, the law assumes the following form:

> No hearer is satisfied if after having heard once or more often the tonic 2 he does not find 2 finally at the end of the melody.[2]

In the elaboration of his theory Meyer utilizes two additional principles. One of these is that among "related" tones there exist different degrees of "relationship." The other principle is that of all those intervals which possess a certain "relationship" we have a decided preference for the smallest. The detailed development of the theory based upon these principles we shall not here undertake to summarize, but its foundations we must pause to examine more closely. It is obvious that there is need of conclusive evidence supporting the basic proposition upon which the theory is erected, the proposition that tones representable by the prime numbers up to and including 7 alone exhibit "relationship."

As evidence Meyer presents, as we have seen, two groups of facts, one derived from an examination of separate intervals and one from observation of the use in actual melodies of the 7 ratio. In both cases, as Wead[3] has pointed out in his penetrat-

[1] *University of Missouri Studies*, *1*, 9.

[2] *L. c.*, 24.

[3] C. K. Wead, *Psychological Review*, 1900, *7*, 400.

ing review of Meyer's work, the judgments recorded are apparently those of a single observer, and he a man of harmonic training. What indication is there that one who had never become familiar with anything comparable with our European harmonic musical system would experience these elementary "relationships?" "Nothing," says Wead, "can be more certain historically than that these relationships have been unrecognized by most of the men throughout the ages who have concerned themselves about music." One cannot avoid asking the question whether Meyer's deductions necessarily hold for hearers of melody other than those who, like himself, have long experienced the associations of modern European music.

A somewhat similar question arises regarding the effects of practice in detecting these melodic "relationships."

Meyer leads us to understand that only after long and careful observation did he decide that 5:7 and 7:8 exhibit "relationship." In another connection he proves[1] that "relationships" not detected at first come later to be felt, upon greater familiarity. This seems to place him in a dilemma: May it not be that the familiarity breeds the "relationship?"

It would not be rash to hazard that if Meyer had chanced to spend his early years in the Scottish Highlands it would never have occurred to him to exclude 11 while admitting 7 among the prime factors of his 'complete scale;' for in listening to the bagpipe he would have become accustomed to the interval 11:12,[2] would have learned to recognize it accurately, and to feel "relationship" between 11 and 12 as truly as between 15 and 16, or 7 and 8.

As long as the question remains unsettled regarding the inclusion or exclusion of 7, 11, or any other ratio in making up the list of elementary "relationships," a certain doubt will remain regarding the validity of Meyer's experiments on the intonation of actual melodies; for, in selecting the preferred pitches the observer's choice of alternatives for each note, it will be remembered, was limited to the two or three tones

[1] See below, p. 40.

[2] *Cf.*, A. J. Ellis: "On the Musical Scales of Various Nations," *Journal of the Society of Arts*, London, 1885, *33*, 499.

available from the scale constructed out of products of 2, 3, 5, and 7.

Instead of attempting here to settle this issue, let us ask some further questions with reference to Meyer's two main contentions. Is it true that only intervals the ratios of whose vibration rates are expressible in small prime numbers manifest the psychological quality of "relationship?" Is it a fact that of two "related" tones whose ratio can be thus expressed, the hearer always prefers as an end-tone that one which is a pure power of 2?

§11. First let us consider the fact of melodic "relationship." The major third is an interval which exhibits the character of "relationship" very unambiguously. This is an interval whose tones have the vibration ratio of 4:5. Now, what is the effect when we listen to an interval just barely wider than this, say the interval 400:501? It so happens that this interval exhibits the "relationship" more clearly, if anything, than 4:5 did,[1] although it is so nearly the same interval that those without special training cannot tell the two apart. Suppose this interval to be made a trifle larger yet, so that it has the ratio 400:504. Do the tones suddenly lose their character of "relationship?" One would hardly expect them to do so. Precisely what does occur is, that as the width of the interval is gradually increased it begins to change somewhat in character; but it remains a major third,—not a satisfactory third to be sure, but nevertheless a third with the *characteristic "relational" attributes* of that interval,—until it reaches nearly to the middle of the zone which divides the major third from a perfect fourth.

The experimental evidence, if any is required, in support of these statements, is easily obtained. The procedure adopted by the writer was to determine the effect produced upon the feeling of "relationship" by gradual but supra-liminal variations in the size of a melodic interval. Between the *b* and *c'* of a harmonium six reeds were interpolated, giving seven intervals, each of a magnitude of about 16 cents (*i. e.*, hundredths of

[1] Stumpf and Meyer found that all of the consonant intervals larger than a minor third are preferred too large. C. Stumpf and M. Meyer, "Maassbestimungen über die Reinheit consonanter Intervalle." *Zeits. f. Psychol.*, 1898, *18*, 321.

an equally tempered semi-tone). Such an interval in this region of the scale means a difference in pitch of scarcely more than two vibrations. It was thus possible to play any desired diatonic interval and also any one of half a dozen intervals intermediate in magnitude between it and the next larger interval. Only the major third and the fourth were tested. The method was without knowledge. The twelve observers were already familiar with the phenomena of "relationship" and finality in two-tone combinations. They were ignorant of the nature and purpose of the experiment. The observer was asked whether or not the two tones played were "related,"and if the response was in the affirmative the further question was put, regarding the completeness or incompleteness of the two-tone group.

It was found, with each of the twelve observers, that the characteristic feeling of "relationship" was nearly always still present when the interval had been increased (or diminished) 32 cents, (a third of an equally tempered semi-tone). The characteristic feeling for the upper or the lower as an end-tone also remained. An alteration, however, of 48 cents (roughly a quarter of a tone) destroyed the feeling of "relationship" in 74 per cent of the 96 judgments.

In general, when a pure interval is gradually modified its characteristic melodic qualities remain long after the interval has lost the characteristic qualities, *e. g.* of consonance, which it manifests when its two tones are heard simultaneously instead of in succession. This fact ought to be of weight for any theory of melody which lays emphasis upon the psychological quality of felt "relationship." Since the ratio 3:4 has no monopoly upon the characteristic "relational" qualities of the fourth, but is rather only a modal ratio about which cluster an immense number of larger and smaller ratios manifesting in some measure identically the same psychological qualities, the use, without qualification, of the symbol 3:4 to represent that particular kind of "relationship" is misleading.

What is true in this respect regarding the facts of "relationship" is of course equally true regarding the facts of finality or melodic trend.

It may be urged that we are here confronted simply with the common characteristic of perception, the modification of sensory data by central processes so that these data may be apperceived to the nearest available norm. Such tests as the above then would merely measure the tendency of the listener to hear different nearly equal intervals as the same pure interval, and do not prove that the "relationship" of the fourth inheres in any other ratio than 3:4.

But such a view neglects the fact that when we are listening to an interval slightly larger than 3:4, we may recognize it as larger and still at the same time experience the feeling of "relationship" characteristic of the fourth. The "relationship," in other words, inheres not merely in the interval 3:4, but also in intervals recognizably larger or smaller than the justly intoned perfect fourth.

§12. We shall not, however, press this consideration. Instead we shall leave in abeyance the question regarding the range of applicability of the pure powers of 2 formula, and shall seek, in the results of the experiments now to be described, the answers to certain questions with reference to the melodic trend in intervals with the simplest arithmetical ratios,—the intervals in which we are led to expect that the phenomena will be most in evidence. Does experiment establish the proposition that when one of two related tones is a pure power of 2, we wish to have this tone at the end, and that when neither of the related tones is a pure power of 2, no preference is felt for either as an end-tone? What is the relative strength of the trend in different two-tone combinations? Do the simplest ratios exhibit it most definitely? Do all observers feel it alike?

The method of the experiment was to present two tones in succession, and ask, "Can you make this second tone a final tone? Does this melody end?"[1]

The following series of ratios was used: 2:3, 5:6, 3:5, 15:16, 45:64, 4:5, 9:16, 32:45, 8:9, 8:15, 5:8, 3:4. This series was given in the 'double fatigue order,' both ascending and descending. Ten of the twelve ratios are relatively simple. Two, the aug-

[1] At the time when these experiments were planned, the experimenter was using the term 'melody' in the sense in which Meyer uses it. When the word implies nothing except "relationship," it is entirely appropriate to speak of melodies of only two tones

mented fourth and diminished fifth (32:45 and 45:64), involve pure powers of 2 but are not simple, and were included for purposes of comparison. Heavy Koenig forks mounted on resonance boxes and actuated by a rubber mallet were used as the source of sound. Each tone was sounded for five seconds. The range of pitch was limited to the once and twice accented octaves, the lowest fork being the middle c' of 256 d. v. and the highest the g'' of 768 d. v. In arranging the series *care was taken that neither of the tones of any pair belonged to a tonality which might have been suggested by the interval preceding.*

Eight persons served as observers in this series. None of them would be classed as totally unmusical, and none of them are "musicians," yet they represent, between these extremes, a wide range of musical ability. All are familar with musical notation and sing or play some from note. With at least two of the observers, there is a lack of interest in music, their skill at the piano being a mechanical acquisition. Three of the observers confessed to an acquaintance with the elements of harmony and musical theory, but it was evident upon trial that their theoretical knowledge was not concrete enough to exert any influence upon their immediate judgments of musical intervals. It may be remarked here that throughout these and also the later experiments the observers gave unreasoned judgments, the introspective records on this point confirming the opinion of the writer based upon the manner of their replies. All the observers had had training in experimental psychology.

The accompanying table gives the affirmative, doubtful and negative judgments of each of the eight observers with respect to each of the melodic intervals used.

TABLE NO. 2

Two Tones Heard in Succession. "Is the second tone a final tone?"

INTERVAL	OBSERVERS	An.	Td.	Bl.	Wl.	Rn.	Dg.	Mc.	Yo.	TOTAL
Minor Second, Ascending (15:16)	Affirmative	2	1	2	3	4	2	1	2	17
	Doubtful	1	0	1	0	0	0	1	1	4
	Negative	1	3	1	1	0	2	2	1	11
Descending	Affirmative	1	0	0	0	0	2	1	0	4
	Doubtful	0	0	2	0	0	0	1	1	4
	Negative	3	4	2	4	4	2	2	3	24
Major Second, Ascending (8:9)	Affirmative	1	0	2	0	1	0	2	0	6
	Doubtful	2	0	0	0	1	0	1	1	5
	Negative	1	4	2	4	2	4	1	3	21
Descending	Affirmative	2	2	2	3	2	3	2	4	20
	Doubtful	1	1	1	0	1	0	1	0	5
	Negative	1	1	1	1	1	1	1	0	7
Minor Third, Ascending (5:6)	Affirmative	2	0	2	0	1	0	2	0	7
	Doubtful	1	0	2	0	1	0	0	1	5
	Negative	1	4	0	4	2	4	2	3	20
Descending	Affirmative	3	0	2	1	0	1	1	2	10
	Doubtful	1	0	1	0	1	0	1	0	4
	Negative	0	4	1	3	3	3	2	2	18
Major Third, Ascending (4:5)	Affirmative	3	1	0	0	0	0	0	1	5
	Doubtful	0	1	3	0	0	0	1	2	7
	Negative	1	2	1	4	4	4	3	1	20
Descending	Affirmative	3	3	4	4	4	4	4	2	28
	Doubtful	1	1	0	0	0	0	0	1	3
	Negative	0	0	0	0	0	0	0	1	1
Perfect Fourth Ascending (3:4)	Affirmative	4	2	4	3	3	4	0	1	21
	Doubtful	0	1	0	1	1	0	1	2	6
	Negative	0	1	0	0	0	0	3	1	5
Descending	Affirmative	0	0	2	0	2	0	1	3	8
	Doubtful	1	2	2	2	1	1	2	0	11
	Negative	3	2	0	2	1	3	1	1	13
Augmented Fourth, Ascending (32:45)	Affirmative	0	2	2	1	1	0	0	2	8
	Doubtful	0	1	2	0	0	0	0	0	3
	Negative	4	1	0	3	3	4	4	2	21
Descending	Affirmative	0	0	2	0	0	1	2	1	6
	Doubtful	0	1	2	0	0	1	2	1	7
	Negative	4	3	0	4	4	2	0	2	19

Cont. of TABLE No. 2

INTERVAL	OBSERVERS	An.	Td.	Bl.	Wl.	Rn.	Dg.	Mc.	Yo.	TOTAL
Diminished Fifth, Ascending (45:64)	Affirmative	0	1	1	0	0	0	1	0	3
	Doubtful	0	2	2	0	0	0	0	1	5
	Negative	4	1	1	4	4	4	3	3	24
Descending	Affirmative	0	1	1	0	0	0	1	0	3
	Doubtful	0	1	2	0	1	0	1	0	5
	Negative	4	2	1	4	3	4	2	4	24
Perfect Fifth, Ascending (2:3)	Affirmative	1	0	2	0	2	1	1	3	10
	Doubtful	2	2	1	0	1	1	1	0	8
	Negative	1	2	1	4	1	2	2	1	14
Descending	Affirmative	4	4	4	4	4	1	1	4	26
	Doubtful	0	0	0	0	0	2	1	0	3
	Negative	0	0	0	0	0	1	2	0	3
Minor Sixth, Ascending (5:8)	Affirmative	1	4	4	2	1	0	1	1	14
	Doubtful	3	0	0	0	2	1	0	0	6
	Negative	0	0	0	2	1	3	3	3	12
Descending	Affirmative	0	3	2	0	1	0	0	2	8
	Doubtful	4	0	1	0	1	0	1	1	8
	Negative	0	1	1	4	2	4	3	1	16
Major Sixth, Ascending (3:5)	Affirmative	1	2	3	2	1	1	1	0	11
	Doubtful	1	0	0	1	2	0	1	0	5
	Negative	2	2	1	1	1	3	2	4	16
Descending	Affirmative	1	2	3	1	0	1	0	0	8
	Doubtful	2	0	1	1	3	0	2	0	9
	Negative	1	2	0	2	1	3	2	4	15
Minor Seventh, Ascending (9:16)	Affirmative	0	1	3	0	0	0	1	2	7
	Doubtful	1	3	1	0	0	0	1	0	6
	Negative	3	0	0	4	4	4	2	2	19
Descending	Affirmative	0	2	2	0	3	0	2	0	9
	Doubtful	2	0	2	0	0	0	1	0	5
	Negative	2	2	0	4	1	4	1	4	18
Major Seventh, Ascending (8:15)	Affirmative	0	2	2	1	0	0	2	2	9
	Doubtful	1	1	2	2	0	0	1	0	7
	Negative	3	1	0	1	4	4	1	2	16
Descending	Affirmative	1	0	2	2	1	0	2	0	8
	Doubtful	0	2	1	0	1	0	0	0	4
	Negative	3	2	1	2	2	4	2	4	20

§13. These results indicate that the descending major third (4:5) and the descending perfect fifth (2:3) exhibit more of the quality of finality than any of the other two-tone combinations. The one was judged definitely to end 28 times, and the other 26 times, out of a possible 32.

The other intervals showing more affirmative than negative judgments are the ascending perfect fourth (3:4) with 21 affirmative judgments; the descending major second (8:9) with 20; the ascending minor second (15:16) with 17; and the ascending minor sixth (5:8) with 14.

The diminished fifth (45:64)—both ascending and descending—and the descending minor second (15:16) each have the highest number of negative judgments—24. These are the intervals that most clearly lack finality. The ascending major second is next with 21 negative judgments, followed closely by the ascending and descending augmented fourth, minor third, minor seventh and major seventh, and the ascending major third. The percentage of negative judgments of the ascending perfect fifth and the descending perfect fourth is the smallest of any of the intervals judged not to end.

The ascending minor seventh (9:16) and the descending major seventh (8:15) are both judged to lack finality, contrary to the law of the number 2, although their inversions, the major and minor second, conform to the law. The ascending minor seventh has only 7 affirmative judgments as compared with 19 negative; and the descending major seventh has 8 affirmative and 20 negative judgments.

What is the reason for the large number of negative judgments on these larger intervals? One answer is, that the tones of these wider intervals sometimes failed to arouse any feeling of "relationship." "Those two tones do not belong in the same melody." "That second tone cannot be a final tone because it has no connection whatever with the first." "No! The tones aren't related." Such introspections were frequently given when the wider intervals were used. These not highly musical observers experienced a sufficiently strong and definite feeling of "relationship" in the case of such a small interval as 8:9, but found all "relationship" lacking in the inversion of that same interval, 9:16.

This means that in formulating the facts of *their* musical experience it would not be permissible to do as Meyer has done, and "omit the number 2 as a factor," or in other words to treat the trend and the "relationship" in any interval as identical with that of its inversion.

§14. Three-fourths of the 24 combinations are judged *not* to end more often than to end. The total number of judgments is distributed as follows:

		PER CENT
Affirmative	256	33
Doubtful	135	18
Negative	377	49

If we leave out of consideration the more complex intervals, the augmented fourth and the diminished fifth, the totals stand as follows:

		PER CENT
Affirmative	236	37
Doubtful	115	18
Negative	289	45

From these facts it would seem that in general it is somewhat harder to accept the second tone of a two-tone sequence as final than it is to judge it to be lacking in finality.

§15. Do the results of these experiments indicate that descending intervals as such tend to cause the feeling of finality? To answer this question the data of Table 2 may be redistributed so that the totals for ascending and descending intervals may be compared. Following are the totals for all the intervals represented by simple ratios involving a power of 2, then for the more complex intervals (augmented fourth and diminished fifth) and the intervals whose ratios though simple involve no power of 2, and finally for all twelve intervals combined.

Simple Ratios Involving a Power of 2:

	ASCENDING		DESCENDING	
	TOTAL	PER CENT	TOTAL	PER CENT
Affirmative	89	35	111	43
Doubtful	49	19	43	17
Negative	118	46	102	40

Complex Ratios, and Simple Ratios without a Power of 2:

	ASCENDING		DESCENDING	
	TOTAL	PER CENT	TOTAL	PER CENT
Affirmative..........	29	23	27	21
Doubtful............	18	14	25	20
Negative............	81	63	76	59

Totals for all Twelve Intervals:

	ASCENDING		DESCENDING	
	TOTAL	PER CENT	TOTAL	PER CENT
Affirmative..........	118	31	138	36
Doubtful............	67	17	68	18
Negative............	199	52	178	46

In each group, tones which are powers of 2 had the position of first tone exactly as many times as they had the position of final tone; consequently it will not be far wrong to assume that any effects due to the operation of the law of the powers of 2 are cancelled.

There is found, especially in the first of these three summaries, some preponderance in favor of the descending intervals as more definitely final and of the ascending intervals as lacking in finality.

This effect of the falling inflection has been made the object of experimental determination by Meyer.[1]

Three tones of a reed organ were played a few times in irregular succession, ending on one of them. Then they were played in a similar way, ending on another one; and lastly, ending on the third tone. This was repeated until each subject had made up his mind and written down which of these three endings was the most satisfactory to him. . . . .

Two classes of experiments must be distinguished: one in which there was no tonic effect among the three tones; and one in which there were tonic effects. In the former case the three tones were represented by the symbols 3, 5, and 7; in the latter, by 2, 3, and 9. [The tones *e*, *g*, and $7b^b$ stand in the ratio of 3:5:7; *c*, *g* and *d* would be represented in Meyer's symbolism by 2, 3, and 9.] . . . . The three tones of one experiment were always within a single octave. Each of the three tones, however, had an equal chance of exerting its influence, *i. e.*, of being the lowest of the three. (P. 458.)

Where there was no tonic effect, the lower tone, whichever

[1] *Amer. Jour. Psych.*, 1903, *14*, 456.

it happened to be, was preferred as an end tone, the totals being 5 choices for the higher, 8 for the middle, and 17, or 57 per cent of the total, for the lower tone. In the other series, one of the tones was a 'tonic.' When this tone was also the lowest tone it was preferred as the end-tone in 86 per cent of the judgments. When it was the middle tone it received 70 per cent of the choices; and when it was the upper tone only 7 per cent.

These are striking results and one wishes that these experiments had been carried farther. Brief as they are, however, they serve to emphasize that the effect of finality at the close of a melody may be due in part to the operation of other causes than the powers of 2 phenomenon.

It thus is obviously desirable, in discussing the meaning of our own results, to separate as far as this is possible the finality effect produced by the falling inflection from that which is due to the more definite pitch relations of the tones.

§16. We shall first bring together the totals for those simple intervals (Group S) whose ratios do not include a pure power of 2, *i. e.*, the minor third (5:6) and the major sixth (3:5). The second summary will include the complex intervals (Group C) involving powers of 2, *i. e.* the augmented fourth (32:45) and the diminished fifth (45:64). Then will come the eight remaining intervals, all expressible in simple ratios one of whose members is a pure power of 2. These latter it will be convenient to separate into those intervals in which the 2 tone is the higher (Group H), and those in which it is the lower (Group L).

Group S. Simple Ratios without a Power of 2:

INTERVAL	*5 : 6*	*3 : 5*	TOTAL	PER CENT
Ascending				
Affirmative	7	11	18	28
Doubtful	5	5	10	16
Negative	20	16	36	56
Descending				
Affirmative	10	8	18	28
Doubtful	4	9	13	20
Negative	18	15	33	52

Group C. Complex Ratios Involving a Power of 2:

INTERVAL	32 : 45	45 : 64	TOTAL	PER CENT
Ascending				
Affirmative	8	3	11	17
Doubtful	3	5	8	13
Negative	21	24	45	70
Descending				
Affirmative	6	3	9	14
Doubtful	7	5	12	19
Negative	19	24	43	67

Simple Ratios Involving a Power of 2:
Group H. (Higher tone a Power of 2.)

INTERVAL	15 : 16	3 : 4	5 : 8	9 : 16	TOTAL	PER CENT
Ascending						
Affirmative	17	21	14	7	59	46
Doubtful	4	6	6	6	22	17
Negative	11	5	12	19	47	37
Descending						
Affirmative	4	8	8	9	29	23
Doubtful	4	11	8	5	28	22
Negative	24	13	16	18	71	55

Group L. (Lower tone a Power of 2.)

INTERVAL	8 : 15	2 : 3	4 : 5	8 : 9	TOTAL	PER CENT
Ascending						
Affirmative	9	10	5	6	30	23
Doubtful	7	8	7	5	27	21
Negative	16	14	20	21	71	55
Descending						
Affirmative	8	26	28	20	82	64
Doubtful	4	3	3	5	15	12
Negative	20	3	1	7	31	24

According to the Lipps-Meyer formula, intervals of Group H should end better on the higher tone, and intervals of Group L on the lower. Consequently in Group H the finality effect due to the 2 ratio is opposed by the rising-inflection phenomenon, but in Group L the two forces work together.

Comparing the totals for all the intervals which according to the law of 2 should end, *i. e.*, the ascending intervals of Group H and the descending intervals of Group L, we find 59 affirmative and 47 negative judgments in the first case, as contrasted with 82 affirmative and 31 negative judgments when the effects of the two forces are cumulative. The influence of the falling inflection increases the proportion of affirmative judgments very noticeably. Preference for the descending intervals as more definitely final does not, however, come to light in comparing the descending intervals of Group H with the ascending intervals of Group L—intervals which according to the Lipps-Meyer law lack finality. In both cases the negative judgments are more than double the affirmative in number, and the totals are almost exactly the same in the two groups.

It is instructive to combine the totals for the ascending intervals of Group H and the descending intervals of Group L, obtaining in this manner the totals for all judgments upon intervals which according to the formula of Lipps and Meyer ought to be judged to end. These may be compared with the judgments upon the same intervals played in the opposite direction, which according to this law are characterized by lack of finality:

End Tone a Power of 2:

	TOTAL	PER CENT
Affirmative	141	55
Doubtful	37	14
Negative	78	31

First Tone a Power of 2:

	TOTAL	PER CENT
Affirmative	59	23
Doubtful	55	22
Negative	142	55

§17. This last summary presents strong evidence of the operation of some such tendency as that to which the Lipps-Meyer law refers. When 2 is the end tone, the two-tone group is said by these observers to end in 55 per cent of the instances, and not to end in 31 per cent, the remaining 14 per cent being 'doubtful.' When 2 is the first tone of the pair, the proportions are reversed. Only 23 per cent are judged to end, while 55 per cent are judged to be lacking in finality.

In attempting to account for the judgments which do not conform to the law, it is to be remembered that in exactly one half of the instances in each group the effect of the rising or falling inflection was acting in opposition to the phenomenon under discussion. Hence a certain ambiguity and uncertainty is sometimes inevitable. But the inadequacy of this explanation to account for all of the facts becomes manifest, when we examine afresh the separate data from which these totals are compiled (p. 25). Why does the same observer declare at one time that the ascending minor third, for instance, ends, while at another time he declares with no less positiveness that it does not end? The fact that some of the observers were but slightly musical accounts for part of these anomalies,[1] but some contradictory judgments occur in all the records including those of the most musical observers. How can the latter be explained?

The suggestion was made that the fork tones were so nearly pure that the feelings of "relationship" were weak and consequently the reactions produced were not normal. But the real difficulty did not consist in any *lack* of feelings of "relationship" and of finality, but rather in the fact that these feelings were apparently often *misplaced*. Moreover, control tests with harmonium and piano tones rich in upper partials failed to decrease the proportion of contradictory judgments.

§18. To gather further data another series was arranged containing, besides the twelve of the original series, five additional intervals: 24:25, 9:10, 27:32, 20:27 and 27:40. Five quite musical observers served, including the two most musical of those who had assisted in the previous experiment. The procedure was varied by putting the question differently: "Do you feel any desire to return to the first tone?"

With the attention thus directed, it is not surprising that some of the observers reported with certain intervals that they desired to hear the first tone again, whichever way the melodies were played, ascending or descending. Thus was forced into notice what has been called the *law of the Return*, the law that, *other things being equal, it is better to return to any*

[1] For example, when observer Bl. reported that an augmented fourth ended satisfactorily on the upper tone, he was asked to hum the interval upon which he had passed judgment, and sang a perfect fourth. The same thing occurred in the case of Td, who, however, discovered after he had sung the interval that it was not the same as the one he had originally heard, and wanted to change his judgment upon it.

starting point whatsoever than not to return—a simple, fundamental principle of musical form, of art form of any kind, indeed.

Another law to which the introspections pointed is not so simply formulated. It was brought to attention by three observers who persistently found an additional alternative in the case of certain intervals: the melody lacked finality, there was no desire to return, neither tone would serve as an end-tone but some *third* tone was demanded. Here was a melodic trend, definite, positive, insistent; a property of a single pair of successive tones, but leading beyond them to something further.[1]

It was plain that the facts of elementary melodic "relationship" and the law of finality of two-tone melodies did not tell the whole story. The phenomenon of melodic trend seemed to be of a more complex sort, even in two-tone groups, than is implied by any statement of a tendency to return or not to return. Even with these simple two-tone sequences it was necessary to recognize the operation of some such law as the following: *Two melodically "related" tones tend to establish a tonality*, and the melody is judged to end only when the final tone is one of the members of the tonic triad—preferably the tonic itself.

This law is not asserted to be a universal law. Indeed it is doubtless limited in its application to the experience of those reared in a harmonic musical atmosphere. In so far as it is found to be valid, it indicates the probability that the phenomena of melodic trend are not primary, but are derived from our experience of consonance.

These experiments were supplemented by briefer and less systematic tests upon a number of observers, unpracticed in psychological observation. The results were in general confirmatory, although not as strikingly uniform as those we have already given. Mention will be made only of four of the observers whose records are exceptional. Two of these exhibited a persistent preference for endings that suggested

[1] These introspections complicated the records so much that it is not deemed advisable to reproduce them here in full.

the minor mode. Tested upon the interval of the minor third (5:6)—no tonality having been previously supplied—these observers uniformly judged the ending on the lower tone, (5), to be satisfactory, while the ascending interval was judged to be lacking in finality. One of these observers is a very musical Welshman, and it is to be recalled that much of the characteristic Welsh music is in the minor or as they call it, the "la" mode. Tests were made upon two Japanese young men who had recently arrived in this country and who professed to have had but little opportunity to hear European music. Both were singers and one was a performer upon the Japanese flute. The tests, repeated, gave very conflicting results, and it became evident that either the interpreter had failed to make clear to them precisely what the phenomenon was upon which they were to pass judgment, or else their experience of melodic trends differs essentially from ours. Unfortunately it was not possible to carry out an extensive series of tests with these observers.

§19. For purposes of comparison, a third set of experiments was undertaken in which the tonality feeling was not left to be contributed by the hearer, but was definitely suggested to him. In the previous experiments, the utmost pains had been taken to exclude the operation of tonality by arranging that neither of the tones of a given group should belong to any tonality which might have been suggested by the immediately preceding experiment. If any tonality was present, it had a subjective origin. We have seen that many apparently contradictory judgments were given, as for instance when a minor second was judged to end, now on the higher and at another time on the lower tone, both judgments being positive and emphatic.

In the experiments now under discussion, on the other hand, the device was used of controlling the tonality, imposing it from without and testing after the judgment had been made to see whether or not the objectively given tonality had been retained. To facilitate this procedure, a piano tuned in equal temperament was used instead of the forks.

These experiments were carried out upon five musical observers, practiced in psychological observation. Three of these were quite naïve as to the nature or course of the experiment.

All the intervals of the tempered scale exclusive of the octave were employed. Each interval was used, beginning at every possible position in the scale: thus the ascending fourth was heard, beginning on 1, 2, 3, 5, 6 and 7 of the scale. The series was given in double fatigue order. The experimenter noted down the observer's introspections regarding the trend of the interval, or trends, for several optional directions of melodic movement were often detected. In these instances where more than one leading presented itself to the observer, an effort was made to determine the relative strength of each.

The result suggested by the previous experiments came clearly to view: so long as the given tonality was maintained, the trend of any interval, ascending or descending, was toward some member of the tonic chord, preferably the tonic itself. Individual differences showed themselves as stronger or weaker demands for the tonic as the end-tone, as over against the third or fifth when the latter were nearer than the tonic. For example, in the key of *c*, observer Rn felt that the sequence *g′ f′* demanded *c′* as its third tone, whereas the other four observers found the trend to *e′* stronger. The uniform tendency for all five observers, however, with all the intervals, was to rest in one of the tones of the tonic chord.

Our contention is that in the previous experiments with no objectively supplied tonality, the anomalous results and contradictions above mentioned are explicable on the hypothesis that tonalities, now one and now another, arose in the mind of the observer. The minor second *e′*-*f′* would at one time chance to suggest the tonality of *f* and end satisfactorily on the upper of the two tones; while at another time the tonality of *c* would arise, entailing quite different demands.

§20. We have too long neglected to specify what is implied, psychologically, in the term tonality. By a tonality is meant a group of mutually related tones, organized about a single tone, the tonic, as the center of relations. Subjectively a tonality is a set of expectations, a group of melodic possibilities within which the course of the successive tones must find its way, or suffer the penalty of not meeting these expectations or demands of the hearer and so of being rejected as no melody. Of these different demands, that for an end on a certain tone is the strongest and most characteristic.

It is not meant to imply that this tonality, this system of related pitches with a common center of reference, is present in consciousness as a group of auditory images. Often there is only a single simple auditory or vocal-motor image or percept to be detected. The tonality consists in the *attitude* of which the image is merely the superficial manifestation or sensory core. One can image the tone of 320 d.v. as a tonic in the key of *e* or as a median in the key of *c*, and the auditory image will be identical in the two cases, but not the total psychosis. There will be an entirely different organization of expectations, an entirely different attitude, an entirely different set of anticipations and demands, a preparedness for one set of experiences, but not for another.

So much an impartial introspection cannot fail to disclose. The position here advanced is that these same "attitudes" are constituted in large part of kinæsthetic elements—reports of processes of *motor adjustment.*

Suggestions toward such an interpretation of the tonality phenomenon were abundant enough from some of the observers. When Ha. felt a melodic trend unrealized, he often described it as a *vocal tension,* due to a tendency to sing the desired pitch. An. reported kinæsthetic sensations from the throat as accompanying the feeling of expectation. He also mentioned sensations of strain and tension in other regions, notably the diaphragm, these general tensions being especially prominent at the instant when he was attempting to retain an elusive tonality against an auditory distraction (as when, for instance, given the tonality of c, he was asked to listen to the interval c-f.) Do. found that "the effort to hold a tonality involves general organic tensions. Any lapse of attention or shifting of muscular tensions precipitates a shift of tonality. Changes of breathing will do this,'' etc., etc.

Considerations such as these pointed toward the value of an approach to the problems of the melody experience from the side of its motor accompaniments, and resulted in the experiments reported in Part III upon the motor effects of simple melodic stimuli.

Whatever the nature of a tonality 'attitude,' whatever its relations to sensations of strain and muscular movement—it is at least a phenomenon which widely pervades the musical experience of hearers who are familiar with European music. The question now arises whether either the tonality experience or the experience of finality in two-tone sequences is primary, original, fundamental: Does the law of 2 describe a primitive, natural tendency or preference, which has operated in the course of historical development to mould our musical system, or does it describe certain secondary, derived phenomena which would not be discoverable in an experience wholly uninfluenced by association? Proofs of the former alternative the writer has been unable to discover. Moreover, the history of our musical system points toward a gradual evolutionary process in which the *primary phenomena of consonance* have been efficacious factors. Hearers whose minds have been influenced by association with such a musical system, when listening to certain two-tone sequences cannot avoid feeling a preference for one of the tones as an end-tone. Some of these preferences lend themselves to formulation in terms of the Lipps-Meyer law of the number 2; but this law is only a *special case* of the more general law that every melodic interval trends toward one of the tones of the tonic chord of the tonality which it arouses. The law is based upon the tendency of every interval, yes, of even a single musical sound, to establish a tonality attitude. The manner in which the law operates will be evident from one or two simple illustrations.

What shall be said, for instance, of those curious, sometimes baffling experiences, in which a second tone is at first unwelcome, and then quickly makes itself at home and usurps the place of what had before been anticipated as the final tone? In certain instances nothing is more natural or inevitable. The first tone arouses a slight tonality feeling, making itself the tonic, so that if we call this tone *c*, we shall have an 'attitude' in which any of the tones *c*, *e*, and *g* of the tonic chord (but especially *c* itself), would be welcomed as possessing something of the quality of finality. Suppose now we hear the rising fourth *c-f*. When *f* first enters, as a final tone it is not welcomed: it does not meet the requirements of those expectations aroused by the first tone. But *c-f* is a harmonious interval: it immediately tends to shift the organization of the tonality feeling to something which will *include both c and f* in one common tonic chord. This is, of course, the chord *f-a-c-f'*, of which *f* is the fundamental. If this transition is successfully made,— and the chances are that such a transition can be avoided only with conscious effort,—then *f* becomes a final tone, and the interval which at first felt incomplete and unsatisfactory comes to a definite close.

Why does the descending fifth end while the rising fifth does not? When one hears a tone *c* and then its fifth, both fit without readjustment into the *c-e-g* tonality suggested by the first tone, and for complete finality one wishes to hear again the tonic *c*. But if, instead of ascending from the tone *c*, we hear a descending fifth from the same starting point the situation is altered. The chord which includes the original *c* and this new tone *F* is the chord *F-A-c*. Our demand is, accordingly, to hear as a final tone the tonic of this chord, which is *F*. A similar treatment applies to every instance of "direct relationship" in which the law of 2 was found to hold good. This law of the powers of 2 is no primitive universal law: the phenomenon it describes is peculiar to those minds habituated to a musical system whose scale has a basis in the laws of consonance and dissonance.

§21. The overshadowing rôle played by habit or association in the drama of our esthetic experience is not always recognized. The effect of habituation in rendering disagreeable sequences tolerable or pleasant and in changing unrelated into related tones, has been shown by Emerson[1] and also by Meyer[2], although the latter finds in his results substantiation for a very different contention, namely, the universal applicability of the "complete scale."

Emerson worked with extremely small melodic intervals and found that after much experience with these small intervals his observers developed preferences for certain sequences, showing that a melody can be constructed of tones all of which are within the compass of a semi-tone.

Meyer constructed some 'quarter-tone melodies' from the intervals of his complete scale. At the initial performance, the effect was judged by most of his observers to be disagreeable, but on repetition this judgment was modified, and two weeks later, at still another hearing, some of them came to appreciate and enjoy the music which had before been strange and incomprehensible. What an excellent illustration of the law that we do not accept as melodically good that which we cannot in some measure anticipate!

Subjected to careful introspective analysis, the feeling of finality attaching to the second tone in the interval 3:4 differs in no essential from the feeling of finality attaching to the last tone of a purely arbitrary tone combination with which one has grown familiar. In each instance the sense of finality consists of the same kinaesthetic sensations in throat and diaphragm, the same feelings of relaxation, the same repose, the same slight retardation in the rate of mental flow.

This effect of habituation is a familiar fact in the musical experience of everyone. Tonal sequences at first bizarre, strange, unmusical, later come to be appreciated, understood and enjoyed. Some degree of habituation to any succession of intervals whatsoever makes possible the act of recognition, of acknowledgment, of 'welcoming' the successive tones, to use Professor Royce's apt phrase. Habituation, then, is

[1] L. E. Emerson, "The Feeling Value of Unmusical Tone Intervals," *Harvard Psychological Studies*. 1906, *2*, 269.

[2] M. Meyer, "Experimental Studies in the Psychology of Music," *Am. J. Psy.*, 1903, *14*, 456.

sometimes a powerful factor in making possible that active participation which seems to be demanded of the hearer before the succession of musical sounds can for him be unified into the organic whole we call a melody.

§22. *Summary.* These studies began with a definition of melody which laid stress upon the feeling of unity. When the separate tones of a series are felt to be related to each other in such a manner that each tone forms part of a coherent whole, the succession of tones, we said, is felt to be a melody, and the melody problem was stated to be the problem of explaining how this feeling of melodic unity arises. An analysis of the psychological elements of melodic structure revealed many and varied sources contributing to the generation of this unity. One group of factors, however, stood out as of unique importance, namely those due to the relative pitch of the constituent tones; and to the consideration of problems in pitch relationships the scope of the present investigation was limited.

A survey of the efforts that have been made to reduce the facts of melodic "relationship" and of melodic trend to simple mathematical formulation was followed by an account of three sets of experiments upon the phenomena of melodic trend in two-tone groups. These trends, with which the feelings of finality or of lack of finality are closely bound up, were found to be due to (*a*) preference for the lower tone as such as an end tone (phenomenon of the falling inflection), (*b*) preference for a return to the first tone as an end tone, (*c*) preference for the expected ending (if one knows that a given tone is to be the last, its arrival may be sufficient to arouse the feeling of finality quite apart from the operation of any other factors), and, finally, (*d*) preference for an end on one of the tones of the tonic chord—and especially the tonic itself—of the suggested tonality.

This formulation, contrasted with the formulation in terms of 'the law of the number 2,' has the advantage of covering more of the observed facts[1] and the disadvantage, as some will consider it, of conceding that the phenomenon described is

[1] For example, the numerous instances in which 8 : 9 and 15 : 16 are judged to end better on the tone which is not a power of 2.

probably not elemental, primitive, but rather a resultant, traceable to the laws of habit and the harmonic structure of the music with which the observers were acquainted. According to this view, the laws of consonance are primary, not the laws of melodic "relationship."

This latter view finds confirmation in the instances cited where the feelings of "relationship" and of trend were clearly the outgrowth of habituation, of repetition, of custom, of association, of mere expectation.

Mention was made of the high importance which seemed to attach, in the introspections of certain of the observers, to kinaesthetic factors present in their experiences of tonality, "relationship" and trend. These facts, together with the fact that the phenomena of "relationship" are exhibited by pairs of tones which vary so widely from the simple ratios, suggest that it is not the sensory but the motor phase of the circuit which contributes the unity,—that it is not the relatively economical activity of the sensory nerves, but the relatively unified response of the motor mechanism which gives rise to the feeling of "relationship."

Our problem, then, shapes itself as the task of studying the motor responses which melodic stimuli elicit, to discover whether here is to be found any further clue to the explanation of melodic unity.

PART III

EFFECTS OF MELODIC STIMULI UPON MUSCULAR MOVEMENT

§23 To gather definite data regarding the relation of movement to the melody experience, the following experiments were undertaken, designed to test the effects of simple melodic stimuli upon on-going motor processes, voluntary and involuntary.

The voluntary process studied was the tapping movement of the index finger of the right hand. This movement was chosen because of its simplicity and naturalness, and because after a little practice it tends toward automatism, leaving the attention free to be focussed upon the stimulus. Such devices as the Jastrow automatograph and the Delabarre muscle-recorder were rejected in favor of the means here described, because it seemed highly probable that changes in innervation would become most readily manifest as alterations of a motor process already going forward. Other factors remaining constant, it is to be expected that a neural current will tend, at least in part, to find its way out of the central system along that motor channel which is already in use. Moreover the investigations of Stetson[1] and others upon complex or "combined" rhythms have made it certain that a concurrent movement coming into coördination will affect an accompanying uniform movement.

The form of apparatus used is an adaptation of the simple device employed by Stetson for recording rhythmical movements. The hand and forearm rested naturally upon the arm-rest leaving the index finger free to move throughout its entire range of flexion and extension without contact. (See accompanying figure). This free, unrestricted movement was chosen because it was found that when the finger taps against a hard surface the contact sensations serve as a

[1] R. H. Stetson: "A Motor Theory of Rhythm and Discrete Succession." *Psych. Rev.* 1905, *12*, 250.

sensory control which regulates and steadies the movement. As our purpose was to detect any slight variations which the melodic stimuli might produce in this motor process, it was obviously better to avoid as many of these controls as possible.

The periodic movement of the finger was recorded in all its details as far as changes in rate, form, and amplitude of movement in a vertical direction are concerned, by means of the recording device above mentioned. From the leather finger-cot a silk thread ran over a tiny pulley and through

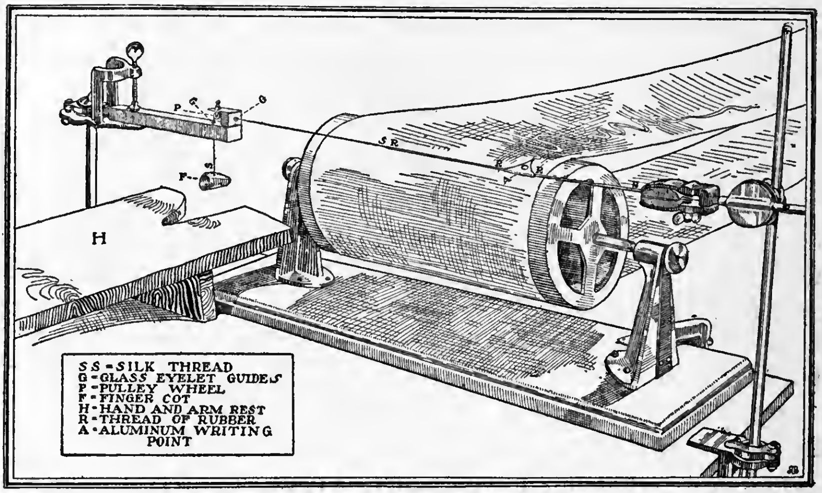

FIGURE No. 1

glass guides which prevented any loose motion. This thread led to a rubber thread, in the middle of which was an aluminum writing point, which traced a record of the finger movement upon the belt of smoked paper. A slight torsion of the rubber served to keep the writing point against the surface of the kymographic belt. By varying the length of the rubber on either side of the writing point the relative amplitude of the curve could be made as small as desired. Most of the records, however, were taken with all of the rubber upon one side of the writing point so that the curve was equal not only in form but also in amplitude to the vertical component of the finger movement. The tension of this delicate rubber was so slight that it was barely perceptible to the observer, and did not interfere with the freedom and naturalness of the movement. Indeed, the superiority of this recording device over that of a tambour lever lies in the perfect freedom of lateral motion allowed; because there is no restraint upon the finger movement, there are no sensory controls other than those cutaneous and kinæsthetic sensations due to the movement itself.

The belt of smoked paper ran between two cylinders placed about two meters apart. It was driven by an alternating current, constant-speed motor whose only variations were due to fluctuations in the rate of the generator of the Cambridge lighting plant. Tests with vibrating forks of 50 d. v. and 500 d. v. showed that the maximum variations in the rate of the belt of smoked paper were less than one and one-half per cent. As a precautionary measure, however, a time line was made a feature of all the records, interruptions at periods of one second being furnished by means of a Lough self-actuating pendulum, placed in a distant room.[1] Precaution was taken to banish all sound which might arise from the recording apparatus, such as the ticking of the electric markers. The driving mechanism was placed outside of the experimenting room, as otherwise a low hum from the motor could be heard even when it was encased in a "sound-proof" box.

One electric marker, as has been said, furnished the time line. This line also served as base line for measuring amplitudes. Another marker was in circuit with the keyboard of the harmonium which was used for giving the melodic stimuli, and furnished the record of the course of the experiment.[2] A silent pendulum was used to aid the experimenter in controlling the length of the sounds. The smoked record was

[1] Only alternate taps of the time-marker, i. e., one every two seconds, are visible in the sample records reproduced on p. 51.

[2] It is the opinion of the experimenter that a simple pneumatic attachment to the keyboard of an organ or piano with tambour recorder would on the whole prove more satisfactory than an electrical attachment.

made permanent by being sprayed with a ten per cent solution of gum sandarac in alcohol.[1]

Pneumographs of the Sumner pattern were employed to record the abdominal and thoracic breathing. The degree of sensitivity of pneumographs and tambours is shown by the clearness with which the pulse-beat appears on the pneumographic tracings, quite plainly enough indeed, especially on the curve of the abdominal breathing, to permit the computation of the pulse rate if desired.

Nothing of significance for the present investigation appeared, however, in these pneumographic curves. The reason doubtless is found not in the fact that melodic stimuli do not produce important modifications in the breathing, but rather in the fact that the duration of the stimulus used was too brief to permit the characteristic alterations to appear. In this respect the conditions were quite the reverse of those in the experiments of Foster and Gamble.[2] These experimenters using musical selections of various kinds as stimuli found that listening always tends to shorten the expiratory pause and to make the breathing faster and shallower, but not steadier; but no remarkable differences were found in the effects of loud and soft or major and minor music. One is not surprised to learn that characteristic breathing phenomena could not be isolated when use was made of such highly complex stimuli as actual musical selections.

§24 When the subject had taken his place and the pneumographs and finger apparatus had been adjusted, the nature of the particular experiment to be performed was explained. The number of tones which were to be used was told, but nothing further was said regarding the nature of the melodic intervals. The subject then closed his eyes and the experimenter started the kymograph, so that a brief record of the breathing was obtained before the finger movement began.

[1] The double-glazed paper used was too thick to be fixed by the usual device of painting on the wrong side. The use of a spray proved to be convenient and expeditious. A "fixative spray," to be had for ten cents at any art store, when fitted to a foot-power bellows, proves very satisfactory.

[2] Eugenia Foster and E. A. McC. Gamble: "The Effect of Music on Thoracic Breathing." *Amer. Jour. Psych.*, 1906. *17*, 406.

At a word from the experimenter the subject began the tapping movement taking whatever rate was most natural to him. After the tapping had continued for twelve seconds or longer the melodic stimulus was given. The tones were played upon a reed organ the mechanism of which was in electrical connection with a marker which recorded the instant of depressing and raising the keys. The general plan was for the experimenter to sound each tone for a period of three seconds. It may be thought that this period was unnecessarily long, but the observers did not find it objectionable and it has two very obvious advantages. In the first place a period as long as three seconds is sufficient to permit any motor changes which the stimulus may produce to become evident in the record of the finger movement. And in the second place the use of the three-second period minimized, if it did not indeed entirely rule out, the factor of rhythm. Stimuli whose rate is as slow as one in three seconds do not tend to become rhythmized.

After the melodic stimulus the tapping was continued for ten seconds or longer. The observer was then called upon to give his introspection. Aside from a general introspective record of the course of the experiment, the naturalness of the tapping, effect of external disturbances, and the like, the points toward which inquiry was especially directed were two: first, does this melody end? Has it the characteristic of finality, or is it unfinished? Does it leave you in suspense? Does it demand something further? Secondly, the question was raised as to whether or not the melody was pleasing. In many cases but not in all, these two aspects, the affective and the aspect of completeness, seemed to be felt as identical; that is to say, a melody was judged to be agreeable because it came to a good ending, or to be unsatisfactory because incomplete. Not infrequently, however, one met with introspective reports like the following: "That is good; I like that but it is not finished," or, "That isn't particularly pleasant, but it ends very emphatically."

A word ought to be said about the way in which the observers were first brought to an understanding of the phenomenon which was under investigation. They were not told what the phenomenon was, and then asked if they could observe it. On the contrary, the plan employed was to play an interval of an ascending fifth and then to play the same interval descending and then ask for a full introspective account. Some observers would quickly detect the feeling of relaxation, of repose, of completeness which accompanied the perceptiom of the descending fifth and which was lacking when the ascending fifth was heard. Lest they should immediately form the opinion that this characteristic of finality always accompanied a descending interval, the perfect fourth was next played. This interval they soon discovered makes a better ending upon the upper tone than upon the lower. Only after the observers had become thoroughly familiar with the phenomenon were they asked to serve as reagents in the main experiments. With two of the observers not a little persistence together with many repetitions of the intervals was required before they discovered the phenomenon, but in every case it was a genuine discovery of their own, and was not suggested to them.

§25 The observers were research students or instructors in the Harvard Psychological Laboratory, with the exception of Po., who had, however, had training as an observer elsewhere. All with the exception of Da. and Pu. were men.

It will be convenient to divide the observers into three groups according to musical ability. This classification is based upon tests in recognition and vocal reproduction of melodic intervals, immediate memory for intervals and for short melodies, and recognition of the fundamental note of a chord.[1] The method employed in this last test was as follows: a three-clang chord was played, and after it a single low clang, with the question, "Is this the fundamental basic tone of this chord? Does it, in a way, represent the whole chord? If you had to supply a bass to this chord, is this the tone you would use?" Twenty four chords were given, eight in the first position, and eight each in the first and

[1] The writer acknowledges indebtedness to Professor Meyer for the suggestion of this test of musical ability.

second inversions. The low tone which followed was always a lower octave of one of the tones of the chord, and in one half of the instances it was the fundamental. The number of right judgments for each observer is given in the second column of the accompanying Table 3. The percentages in

TABLE NO. 3

Tests of musical ability.

	RECOGNITION OF FUNDAMENTAL OF CHORD				VOCAL REPRODUCTION OF FUNDAMENTAL TONE			
Observers	Right	Wrong	Doubtful	Per cent	Right	Wrong	Doubtful	Per cent
Po........	24	0	0	100	24	0	0	100
Rk........	24	0	0	100	23	1	0	97
Rg........	22	2	0	92	22	2	0	92
Da........	20	2	2	88	23	1	0	97
Ho........	20	2	2	88	17	5	2	75
Fr........	16	4	4	75	18	5	1	77
Ta........	12	8	4	58	12	12	0	50
Mc........	8	5	11	56	10	10	4	50
Pu........	4	0	20	58	—	—	–	—

(In computing percentages, doubtful cases are distributed equally between right and wrong cases.)

the last column represent the success of the subjects in humming the fundamental tone after hearing the chord, the series of chords used being similar to the one employed in the previous test. Errors were most frequently made when the low note was not the fundamental, but was a lower octave of the highest note in the chord. It was found after the series was ended that fewer errors of this kind are made if the observer is instructed not to give his judgment immediately, but first to image the three tones of the chord separately, choose the fundamental, and then make the comparison with the low tone. On repetition of the test, this precaution served to eliminate all errors from the judgments of Rk., Ho., and Da., but did not operate so successfully with those observers whose auditory imagination is less facile.

The results of these tests when combined with the other observations on musical ability and with the results of an inquiry into the observers' musical interests, their early training and later musical experience, made it evident that the first three observers on the list had a fair order of musical capacity, although Po. was the only one whose abilities had been much developed by training. The last three observers form a distinct group, since they all fall much below the others in the tests reported in Table 3, and also in accuracy of recognition and reproduction of melodic intervals. Pu. could not even be induced to attempt vocal reproduction. The remaining three observers form an intermediate group. None of the nine were entirely lacking in musical interest, although the range represented was a very wide one. An accurate test of ability in pitch discrimination was not carried through to completion because it became evident that accuracy in the discrimination of small differences of pitch is no indication of musical ability. Po. and Rg. did not serve during the preliminary experiments. Da. and Mc. did not serve during the second half year, and their records are included only in the first of the tables presented here. Each observer served for a period of three quarters of an hour once a week.

The observers it will be recalled were directed to take whatever rate of finger movement seemed most natural to them. The individual differences, and also the individual variations from time to time, proved to be extremely wide. Early in the practice experiments, the tapping of Rk., Da., Ta., and Mc. was much slower than it became later on, and nearly all of the observers showed some tendency to increase the natural rate with practice. Within a series of experiments at a single sitting, Rg., Rk., and Mc. were apt to choose a much more rapid rate for the later experiments, unless they happened to select an unusually rapid rate to begin with. This they were apt to do if they had been walking rapidly or otherwise exercising shortly before, or if they had been under any slight excitement.

Not only do the records show great individual differences in the rate of finger movement, but also in the amplitude and

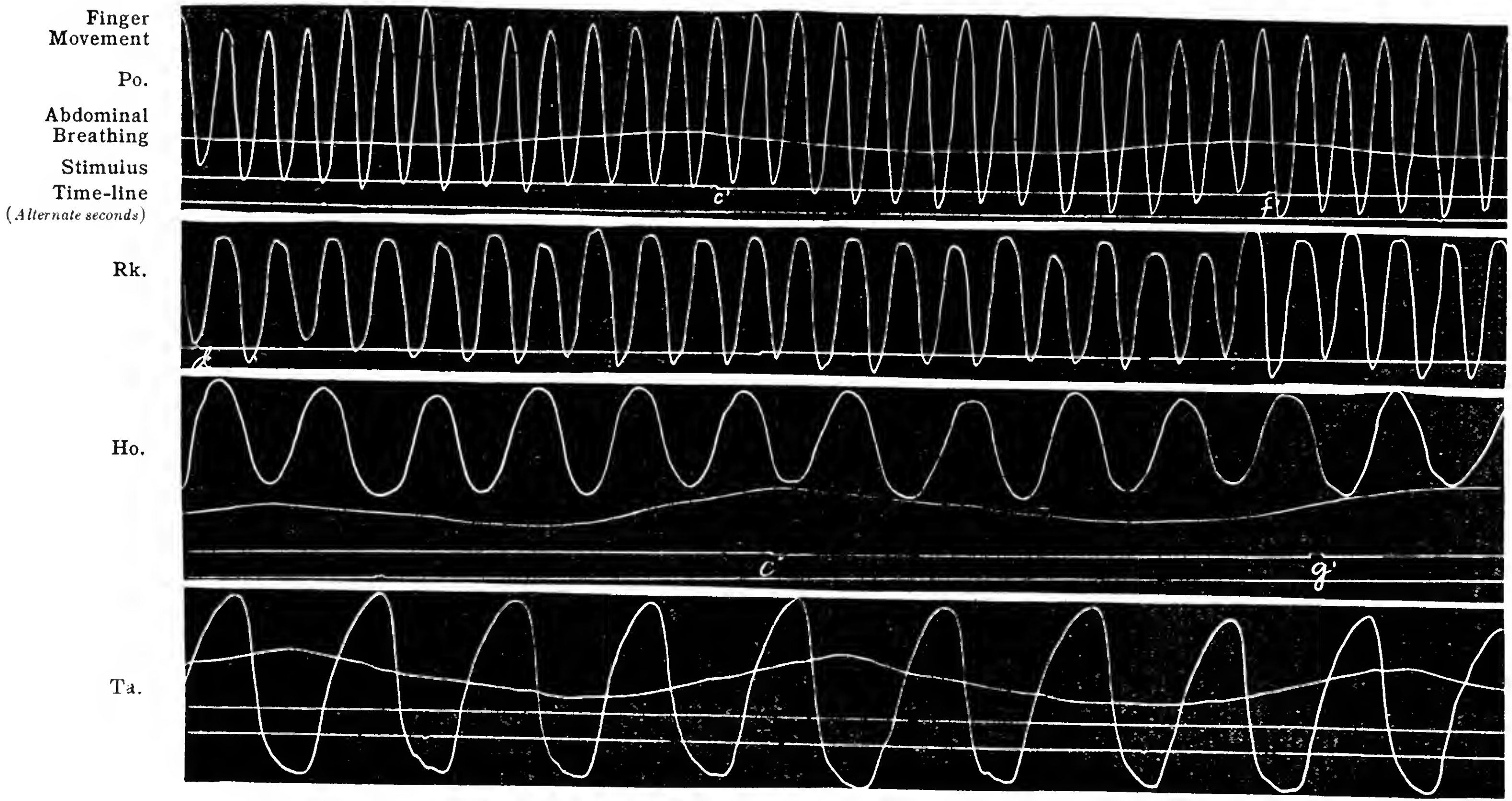

FIG. 2. Reduced one-half.

the general form. Ta.'s record is characteristically slow, wide, and extremely regular. The back stroke is similar to the beat stroke in every respect, and the transitions from the ballistic part of the movement to the controlled portion are smooth and even. The tapping of Da. and Mc. is also slow and wide, but very different from that of Ta. because the ballistic strokes are made with a jerky movement, and the portions of the curve between the ballistic strokes are very irregular. The muscular coördination is much less accurate. Ho. and Pu. also use a characteristically slow rate, but the amplitude of movement is small. One finds very considerable variations in amplitude in the records of both these observers. There is also an irregularity of line due to the fact that the ballistic portion of the movement seems to be almost wholly lacking, even from the beat strokes, (*i. e.*, the finger seems to be almost continuously under control of extensor and flexor muscle sets combined.) The maximum velocity of the beat stroke is much less with these observers than with any of the others. Rk. and Rg. are the two who show the widest variations of natural rate from time to time and also the greatest changes in the form of the finger movement. Both of them use a medium amplitude, but this amplitude varies widely. On the whole, their records show that a much greater prominence is given to the ballistic phase in both beat stroke and back stroke. In Po.'s records, which exhibit the most rapid rates of any of the observers, there is very little in the curve other than the ballistic phase: there is almost no pause between strokes. In the records of Fr., on the other hand, there is always between the vigorous ballistic strokes a relatively long relaxation phase during which the movement is extremely irregular: during these periods the finger seems to be not under the control of either the extensors or the flexors.

With reference to the amplitude of finger movement, it may be noted that with the exception of Ta., those who used a wide amplitude were those who had had some practice at the piano.

TABLE NO. 4

Normal record of rate of finger movement, and fatigue record. Rate of tapping during successive periods of three seconds each. Read from left to right. The slowest rates are printed in bold faced type. Fastest rates in italics.

OBSERVERS	NORMAL RECORD									(One minute of unrecorded tapping elapses between normal record and fatigue record.)	FATIGUE RECORD		
Po.....	252	256	256	252	254	**249**	252	260	*263*		**232**	239	235
Rg.....	*107*	104	106	99	101	99	**92**	100	99		106	104	*108*
Rk ...	130	*133*	133	130	133	132	131	**126**	130		124	124	**121**
Da ...	**97**	*115*	101	105	105	100	103	105	104		99	97	98
Ho ...	*96*	93	90	91	90	91	92	94	**89**		**86**	88	88
Fr.....	208	208	**206**	208	207	210	*220*	217	214		221	*225*	224
Ta	78	78	**76**	77	78	80	79	79	*81*		77	77	78
Mc....	84	85	83	86	87	**82**	*88*	85	86		93	91	*96*
Pu.....	112	**102**	104	107	104	104	109	110	*113*		114	*118*	115

§26 In the accompanying Table 4 are given the measurements of a set of records taken without distraction or stimulus of any kind, for purposes of comparison with records in which melodic stimuli were used. Each number gives the rate of finger movement during a period of three seconds. The rate is expressed in beats per minute, which is the same as the method employed in music for designating rates. The numbers, then, represent the metronome rates at which the observer was tapping during successive periods of three seconds each. To facilitate the reading of the table, the rate of the period of slowest tapping within the record of each observer is printed in bold-faced type, and the fastest rate is printed in italics. A glance at the table will show the extremes between which the rate of tapping varied within the course of the period of twenty-seven seconds covered by the record. It will be seen that four of the nine observers exhibit a tendency toward an increase in rate during this time, while an opposite tendency appears in the records of two observers.

The question naturally arises whether the factor of fatigue may not enter in to modify the nature of the tapping movement as the experiment proceeds. This does not seem to be the case when an experiment does not continue for more than thirty seconds, as was the case with nearly all of those to be described below. For purposes of comparison, however, there is given in connection with the normal record of the accompanying table what may be called a fatigue record. This is really a continuation of the normal records, one minute of unrecorded tapping having been permitted to elapse between the close of the normal record and the beginning of the fatigue record. During this interval the rate of tapping of four of the observers showed a diminution. With four of the others an increase in rate is seen. The record of Fr. showed the greatest variability and irregularity during this closing period. Only two observers, Da. and Mc., reported any feeling of fatigue after this experiment.

Fatigue makes its appearance very quickly if a rate more rapid than the natural rate of tapping is employed. When the reagent taps as rapidly as possible the entrance of fatigue brings with it a slowing of the rate and an increase in irregularity of rate and of amplitude.

§27 Tables 5 and 6 exhibit the effect of auditory stimuli upon the rate of tapping. These tables are prepared in a manner similar to the table of normal tapping; each number represents the rate of tapping during a three-second period. Measurements of the first few taps of each record were not made because they are certain to be more or less irregular. Measurements of the rate of tapping are given for three periods of three seconds each before the incoming of the stimulus. The stimulus consisted of the tone *a* sounded for six seconds on the harmonium. Then after an interval of three seconds, this tone was sounded again, this time for only three seconds, but it was immediately repeated and sustained for three seconds longer.

A study of this table should disclose the effects which are produced upon the rate of tapping by a musical sound and also by the repetition of a musical sound. It will be noticed that in the records of four of the seven observers there is a marked diminution of rate following the entrance of the first stimulus. The record of one observer shows a marked increase of rate at this point. In all cases there appears to be a tendency

TABLE NO. 5

Effect of a single tone, and of that tone repeated, on rate of finger-movement. The rate during each three-second period of the experiment is given. Read from left to right. Numbers showing decrease in rate at critical points in the record are printed in bold face type; increases in rate are printed in italics.

				a (6 sec)			a (3 sec)	a (3 sec)		
Rk........	136	140	141	**123**	*130*	132	132	*135*	132	138
Da........	101	109	104	**82**	*96*	80	78	**73**	(97)*	90
Ho........	79	81	80	79	78	**75**	**72**	72	71	69
Fr.........	159	157	157	**155**	155	**153**	**150**	151	150	152
Mc........	122	116	118	*128*	125	**118**	121	*127*	129	130
Ta.........	77	77	76	**69**	*75*	77	76	76	77	80
Pu.........	60	62	69	66	65	65	66	67	*72*	72

*Stopped tapping for 1.2 sec. when tone stopped; and then began at rate of 97.

TABLE NO. 6

Effect of sudden noise on rate of finger-movement. Entrance of stimulus at beginning of fifth three-second period of the record.

					NOISE			
Po..............	216	216	214	218	*226*	216	214	210
Rk..............	150	154	153	152	152	153	160	159
Ho..............	116	116	114	113	**104**	119	114	116
Fr...............	202	187	185	190	*198*	194	194	197
Ta...............	69	72	71	72	**67**	69	70	70

to return to the original rate while the tone is still sounding. The records of three observers show another diminution in rate immediately following the cessation of the stimulus, but no decided change occurs in the other four records at this point. With the entrance of the stimulus the second time a retardation occurs in three records, but this time it is not nearly as large as in the first instance. The repetition of this stimulus is accompanied by an increase in the rate of one observer and a decrease in the rate of another, the rates of the other five observers not changing materially at this point in the records. The cessation of the stimulus, however, is accompanied by an increase in the rate with two observers, and a decrease in a single instance. One observer stopped tapping entirely for a brief time when the stimulus stopped and then began again at a rapid rate.

It thus becomes evident that under the conditions of this experiment the entrance of an auditory stimulus introduces a disturbance in the process of tapping which shows itself as a change in rate, usually of the nature of a retardation. The nature of the disturbance to the tapping is made very evident by direct inspection of the kymographic records. The next tap after the one during which the stimulus enters is frequently the slowest and also has the greatest amplitude of excursion of any tap on the record. The entrance of the stimulus a second time, after a pause, produces similar but much less marked effects; and when no time interval elapses between the clang stimulus and its repetition no effect whatever is apparent.

The effects of a momentary noise as a distraction are illustrated in the experiments summarized in Table 6. Here, too, a marked change of rate appears in nearly every instance. The solitary exception is Rk., and a closer examination of his record than the table permits shows clearly that here too the the stimulus had its effect. The tap immediately following the one in which the stimulus entered is the slowest tap of the record, but in this instance it is followed immediately by taps of a more rapid rate which bring the rate for the entire three seconds up to the figure given.

It seems to be a general tendency, then, for alterations in the natural tapping rate of the finger to occur upon the entrance into consciousness of an auditory sensation. This very natural phenomenon does not call for an elaborate explanation. It may be dismissed by referring it to that large group of experiences which have as their most prominent feature the characteristic of "shock," of sudden disturbance of equilibrium demanding an adjusting act of attention, and which consequently interfere more or less with pre-existing adjustments and on-going activities. Stated in strictly neural terms, the phenomenon is reducible to an instance of the general law of diffusion, the auditory stimulus introducing a shift of neural tensions throughout the cortex, and more particularly affecting those localities in the Rolandic region which are active at the time.

The modification of rate shows itself most frequently as a retardation probably because new activities of adjustment result in inhibition of the finger movement through drainage of the neural energies elsewhere. To explain those relatively infrequent instances (15 per cent of the total number) where acceleration follows the entrance of the auditory stimulus, one might assume that the stimulus operates to produce a greater alertness, or heightened general activity in which the tapping movement shares. To explain why the very first tap following the onset of the stimulus is sometimes unusually wide and of long duration, but occasionally the reverse, recourse may be had to the facts brought out by Hofbauer[1] and Cleghorn[2] that an auditory stimulus occurring at the beginning of the contraction phase of a movement augments the movement and this reinforcement makes the total duration of the contraction-relaxation process greater; but if the stimulus enters at the beginning of the relaxation phase of the cycle, the process of relaxation is hastened and the total period is diminished.

§28 We may now turn to the experiments in which melodic stimuli were employed, asking what significant changes of rate appear, to what extent these variations are the same for the different observers under identical conditions, and especially, what relations exist between changes of rate and the typical phenomena of melody. Do characteristic changes accompany the perception of a melodic interval which is felt to lack finality? How do these changes differ from those produced by an interval which "ends?" Does a succession of two tones which lack melodic "relationship" have a peculiar effect? What of the "return?" What of disappointed expectation? What of the passage to a tone which necessitates a shift of tonality?

Tables 7 and 8 show the changes in rate of tapping which accompany the hearing of the melodic interval of the fourth, *i.e.*, of two tones whose vibration rates are in the ratio 3:4.

[1] L. Hofbauer, *Arch. f. d. ges. Physiol.* (Pflüger's) 1897, *68*, 546.

[2] Allen Cleghorn, "The Reinforcement of Voluntary Muscular Contraction." *Am. Jour. Physiol.* 1898, *1*, 338.

This is one of the most interesting of any of the melodic intervals from a psychological point of view because of the strong sense of finality which it gives when the higher tone is the last. When heard as a descending interval, it lacks this finality, and yet does not leave one wholly in suspense, for it has those elements of finality which are the property of any descending interval as such, and also those which belong to every tone in the tonic chord. Because of this complexity, judgments regarding the finality of the descending fourth are often uncertain and variable. As an ascending interval, however, there is seldom any doubt in the mind of the observer that the group is a completed whole, emphatically coming to an end. It is indeed the only ascending interval of which so broad and positive an assertion can be made. The minor second and minor sixth are the only other intervals at all comparable with it in these respects.

The tables are made up, as were the previous ones, of numbers representing the metronome rate of the tapping movement during successive periods each three seconds in length. The two tones were each sounded for three seconds, and the numbers immediately under the letters which represent the tones consequently express the rate of tapping during the course of the melodic stimulus. To call attention to changes of rate at critical points in the course of the record, use is made of bold faced type where retardations occur, while accelerations are indicated by italics. In deciding whether or not a change of rate accompanying the entrance of a stimulus was sufficient in amount to be of any significance, the writer has taken into account the degree of regularity shown in the tapping of the six seconds preceding, but has neglected the period before that, which was often so near the beginning of the tapping record that the reagent had not as yet found his pace.

Examining Table 7 with reference to the distribution of retardations and accelerations during and immediately following the melodic stimulus, one notices at once that the retardations all occur during the sounding of the tones (six during the first tone and two during the second) whereas all the accelerations are found within the period of the last tone and the period immediately after it (two during, and six after, the last tone).

In contrast with this table of the ascending fourth, the table of the descending fourth exhibits much less uniformity in the distribution of accelerations and retardations. The most striking feature is the large proportion of retardations which occur during or immediately after the sounding of the second tone.

TABLE NO. 7

Perfect Fourth, ascending. Rate of tapping during successive periods of three seconds each. Read from left to right. Bold faced type indicates retardation and italics acceleration, at critical points.

				c'	*f'*			
Po	207	208	212	212	*222*	225	223	220
Rg	94	95	94	**91**	92	*96*	96	93
Rk	101	106	104	**102**	**99**	*101*	96	99
Ho	105	103	103	**92**	93	*101*	102	100
Fr	190	192	186	**180**	**172**	*185*	179	182
Ta	76	75	86	**73**	72	*87*	78	78
Pu	118	117	120	**112**	*118*	*122**		

*Stopped tapping

TABLE NO. 8

Perfect Fourth, Descending.

				f'	*c'*			
Po	248	255	258	**253**	258	**250**	256	258
Rg	91	93	95	96	97	96	95	94
Rk	104	101	98	*103*	103	**101**	101	103
Ho	95	97	99	99	*103*	104	100	101
Fr	220	214	219	**213**	**210**	*218*	213	218
Ta	82	84	85	**80**	**74**	73	77	80
Pu	100	106	105	105	**101**	*116*	114	116

§29 The significance of these facts appears when they are brought into comparison with the results of the previous group of experiments. There it was found that a repetition of a musical sound following shortly after the cessation of the original stimulus produces effects similar to those of the first sound, but much less marked. And when one musical sound is immediately followed by another which does not differ from it in pitch or intensity there is no apparent effect upon the on-going activity, the only changes observable being in the direction of a return to the natural rate.

When successive tonal stimuli differing in pitch are used—in this instance two tones at an interval of a fourth—the characteristic variations of rate, most of them retardations, follow the entrance of the first tone; but when this is succeeded by the second tone, one does not find the same absence of further variations which marked the appearance of a second tone identical in pitch with the first. Instead one finds fresh changes of rate; and upon comparing the ascending fourth with the descending fourth one is impressed with the fact that the accelerations belong mainly to the rising interval, while most of the new retardations accompany the hearing of the descending fourth. This, it will be born in mind, is an interval that "ends" better on the higher tone.

An hypothesis with reference to the significance of these motor phenomena may here be briefly outlined, as follows: (*a*) Attention is an activity which involves both special and general motor adjustments. (*b*) The general aspects of attentive activity are of such a nature as to affect general bodily conditions; and, specifically, (*c*) the rate of a circular motor process (such as the finger-movement) which is going forward semi-automatically, will be affected by these activities, a decrease in rate signifying inhibition, due to increased activity elsewhere, and an acceleration signifying that the task of attention in organizing these activities is being successfully carried out. Retardation or inhibition, it is to be expected, will enter with the appearance of the stimulus demanding attention. Continued slow rate of movement will result if the organizing activities of the attentive process continue to meet with difficulties, while the rate will be augmented as the new adjustments come to be efficiently established.

In terms of this hypothesis, the above facts with reference to the hearing of the rising fourth would be described as follows: Sudden rise in the level of attention at entrance of stimulus, continued attentive activity during the sounding of the tones, and finally, subsidence of attentive activity with the satisfactory completion of its task; or, stated differently, presentation of a problem of adjustment as stimulus enters, continuance of the process of establishing coördination during the sounding of tones, and then increase of rate signifying the efficient accomplishment of this act.

It is this acceleration accompanying the sense of finality which seems to be of particular significance.

§30 In testing the hypothesis, the introspections of the observers must be taken into consideration, for not always is a melodic interval heard in the same way. What an interval is to the observer depends as much upon the "attitude" with which it is received as it does upon the ratios of the physical vibration rates.[1]

The order of arrangement of the observers in all the tables, it will be recalled, is that determined by the tests of musical ability. Po., the most musical, reported that the ascending fourth, while it has the attribute of finality, is less final than some, *e.g.*, the descending fifth.

"The pitch of the second tone came as a surprise. The feeling of satisfaction came only toward the end of the second sound, after I had got it placed with reference to the first. The instant of entrance of the sense of satisfaction was very marked."

(The rate for the first four taps of this period was 210, for the next four it was 228 and for the remaining three, 232.) This experience might be described as the final acceptance of a second tone as a tonic which when first heard was not so construed. If, during the hearing of the first tone, a tonality feeling gets established with this tone as a tonic—as is very frequently the case—the transition to a tone of different pitch presents three possibilities. (*a*) It may be an "unrelated" tone, foreign not only to the tonality already in mind but also to any other tonality within which the first tone would find a place. In such an instance there can be no melody feeling,[2] for there is no coherence or relevance between the tones; they do not tend to insti-

[1] *Cf. supra*, p. 32 *ff*.

[2] Here, and throughout the discussion of the experiments, it will be understood that these statements are made solely with reference to the experience of observers who are familiar with a harmonic musical system.

tute a common set of expectations; they do not belong to the same whole. (*b*) The second of the two tones may be "related" to the first as to a tonic. It belongs to the tonality already in mind, and consequently it is welcomed, as partially satisfying the expectations of the hearer; but it does not wholly satisfy them. Instead, it only makes more definite and insistent the demand that the first tone shall be heard again, at the end of the melody; it intensifies the original tonality feeling. If the sequence of tones ends here, one experiences the feeling of unrest and dissatisfaction which accompanies disappointed expectation or thwarted intent. (*c*) The second tone may be capable of entering into tonality relations with the first, but not into the tonality of which that tone is the tonic. This necessitates a shift of tonality. In place of the organized set of expectations already present, a different set appears. The extreme instance of this peculiarly subtile and elusive process occurs when the second tone becomes itself the tonic of a new tonality, usurping the power and function originally held by its predecessor, and organizing a new set of expectations. Such an instance is found in the interval of the ascending fourth.

Po. was probably not the only observer who experienced this peculiar shift of tonality upon hearing the interval of the ascending fourth; but he is the only one who detected and described the feeling of transition and the satisfaction which followed. Rg. reported that the interval seemed to him to be rather indifferent, but after hearing *f′c′* he said that *c′f′* had more finality about it than he had thought at first. Rk. reports, "That sounds like '*sol do*'; there is no need of a third tone." Ho. "That ends! It is very agreeable." Fr. "That's all right." Ta. found it difficult to give an introspective report. The interval he said was elusive, and it was hard to say just what the effect was. Pu. reported no definite effect of any sort. It must be noted that even in the case of these last two observers an acceleration of rate occurred immediately after the close of the tone.

With the descending fourth we find much less uniformity in the distribution of accelerations and retardations, and also a greater diversity in the introspective reports. The most striking and important feature is the large proportion of retardations which occur during or immediately after the sounding of the second tone. Po. reports that the interval was pleasing, but not wholly satisfactory because it lacked finality. During the sounding of the second tone his rate recovered from the slowing-up produced by the first tone but after the melody ended there was a retardation. For Rg. the interval lacked finality but as to agreeableness it was indifferent. Rk.'s introspections were interesting. "That is all right, but I can't help thinking in three's." That is to say, he gave an intellectual judgment that the interval was complete but really felt a need for something further. (Note the retardation in rate.) Ho. says, "I should like to add a third note but it is not bad." Fr., "Unfinished, but pleasant as far as it goes." Ta. "I cannot decide. I keep changing my mind. It is a puzzling interval." Pu., "Very definitely complete and pleasant."

If one examines the table in the light of these introspective comments, it is found that five of the seven records support our hypothesis with reference to the motor effect of the finality experience.

With all of the remaining tables the introspections are presented in very brief summary. The observer's own words are used, as far as the necessities of condensation allow.

§31 Tables 9 and 10 should be examined together. They show the effects produced by the melodic interval of the perfect fifth, ascending and descending. With regard to the aspect of finality, all the observers with the exception of the two least musical ones are agreed that the ascending fifth is lacking in completeness. In spite of this fact, the proportion of retardations and accelerations during the period while the second tone was sounding and immediately after, do not show a balance in favor of the retardations. The lack of finality in this interval is not sufficiently marked to produce the vivid experiences of tension which characterize the perception of some melodic intervals. A more significant reason why one should not expect a larger proportion of retardations here, will become evident shortly.

TABLE NO. 9

Perfect Fifth, Ascending. Rate of tapping during successive periods of three seconds each. Read from left to right. Numbers showing decrease in rate at critical points in the record are printed in bold face type. Increases in rate are printed in italics.

				c′	g′			
Po	225	224	225	224	*228*	*236*	236	230
Rg	129	130	127	126	125	*132*	124	128
Rk	117	117	118	119	**116**	115	114	118
Ho	102	110	111	**104**	**96**	*102*	105	104
Fr	234	233	235	*237*	**231**	232	226	230
Ta	73	74	76	**71**	*77*	*80*	81	83
Pu	102	109	108	**105**	*111*	**104**	103	

Introspections.

Po. A sense of finality, but not completely final. Pleasant.
Rg. A beginning, not an end. Wanted to go on.
Rk. Want to hear first again.
Ho. Needs third tone. Not extremely bad.
Fr. Unfinished. Pleasant.
Ta. That is finished! Felt so the instant it sounded.
Pu. Fairly complete. Agreeable ending, but I do not like so wide an interval.

TABLE NO. 10

Perfect Fifth, descending.

				g′	c′			
Po	197	204	208	**204**	*208*	*214*	219	220
Rg	129	125	135	132	**125**	*134*	133	143
Rk	106	108	108	**102**	101	*105*	103	105
Ho	109	113	111	**107**	106	*109*	*	
Fr	234	225	220	221	220	220	222	229
Ta	78	78	78	78	78	*83*	83	82
Pu	103	106	113	**101**	**97**	*112*	104	101

*Stopped Tapping

Introspections.

Po. No suggestion of further movement. Satisfactory.
Rg. Left no impression.
Rk. Doesn't need a third. Pleasant.
Ho. Can't say as to finality. Fairly agreeable.
Fr. Incoherent. Unfinished. Unpleasant.
Ta. (Introspection uncertain.)
Pu. Did not demand third note.

Table 10, the descending fifth, presents a much more uniform appearance. Accelerations following the close of the melody occur in every record except that of Fr., which shows no change in rate at this point. The introspections, however, are not as definite, three observers failing to report anything positive regarding the finished, self-complete character of the melody. The only one, however, who found the melody incomplete was Fr., the observer whose rate is the only one to show no increase at this point.

TABLE NO. 11

Perfect Fifth, descending. Three tones expected. Average rate of tapping by three-second periods. Read from left to right.

				g'	c'			
Po	284	284	275	277	275	**267**	269	269
Rk	205	202	206	**202**	204	**194**	197	223
Rg	112	117	117	**113**	112	*128*	118	127
Ho	108	110	111	111	**105**	**101**	99	100
Ta	76	76	77	**68**	*70*	*73*	75	73
Pu	104	108	108	**105**	*108*	**100**	101	104

Introspections.

Po. Amusing. Incomplete.
Rg. A feeling of incompleteness.
Rk. Disappointing.
Ho. Unfinished, because of expectancy of another tone.
Ta. Incomplete. Thought you were trying to fool me.
Pu. Surprised that there were not three. Incomplete.

The records from which Table 11 were prepared were taken at the end of the year's experimenting because it was desired to avoid the suspicious attitude which it might possibly have induced in some observers. One of the details of method, it will be recalled, was to let the observer know beforehand how many tones were to be expected, in order to keep the conditions in this respect as constant as possible. In this final experiment, however, the observer was led to expect three tones, but only two were given, the same two used in the experiment just described. (Table 10). Any changes in rate of tapping produced by *unfulfilled expectation* ought then to become evident by a comparison of these two tables, 9 and 10, and indeed the difference is sufficiently striking. Instead of uniform accelerations following the tones one finds retardations in nearly every instance.

This, then, may aid us in understanding the accelerations so frequently found where introspection reports that the interval lacks finality. As a melodic interval it is left unfinished, but in so far as the hearer was expecting a certain number of tones and that expectation was fulfilled, the experience as a whole gets a certain completeness and unity. Part, at least, of the adjustments of attention have functioned as intended, and only so much of the total motor attitude as was immediately concerned with the tonality experience as such has to be re-adjusted when the melody comes to an end on what is not a final tone.

The diminished fifth (45:64) was selected as an example of a group of two "unrelated" tones. The testimony of the observers is nearly unanimous that the interval lacks completeness and is disagreeable to hear both ascending and descending. (Tables 12 and 13.) Nevertheless there are a larger number of accelerations than of retardations. A comparison of the "exceptions" with those in the introspective table clears up the difficulty somewhat, but even then it must be said that this pair of tables tells against our hypothesis. The only recourse

TABLE NO. 12

Diminished Fifth, ascending. Average rate by three-second periods. Read across.

				b	*f′*			
Po.	265	274	277	**270**	267	270	270	265
Rg.	122	114	118	*122*	*126*	**117**	116	115
Fr.	247	232	232	*242*	**233**	*247*	239	240
Ta.	76	80	78	**72**	73	*76*	78	77
Pu.	74	73	75	**65**	*76*	*79*	79	84

Introspections.

Po. A raw rough interval. Associations with Wagner made it less disagreeable. Incomplete.

Rg. Disagreeable because incomplete.

Fr. Not finished but good as far as it went.

Ta. Unfinished but a pleasant interval.

Pu. Very disagreeable. Felt at entrance of second tone.

TABLE NO. 13

Diminished Fifth, descending. Average rate by three-second periods. Read across.

				f′	*b*			
Po	263	276	265	263	267	*274*	270	267
Rg	116	118	117	115	*131*	130	131	119
Fr	192	200	219	207	**198**	**192**	202	211
Ta	76	78	76	**70**	71	*76*	74	76
Pu	80	79	77	79	**76**	*82*	85	77

Introspections

Po. Incomplete, but not seriously so.

Rg. One more tone (he hummed *c*) would make a great difference.

Fr. Very unpleasant. It seemed complete because you told me there would be but two.

Ta. Finished. A pleasant interval.

Pu. Didn't think about completeness. At first thought it disagreeable, then not sure.

is to the principle that the tapping tends to become rapid whenever attention is freed from the stimulus, irrespective of what the stimulus may be.

The descending major third is an emphatically final melody (although Fr. and Pu. did not so describe it), and the table (No. 14) shows the expected accelerations. The most interesting feature is, however, the marked retardation in the record of Rk. The last tone was a final tone, he said, but he wanted a third tone in *between* the first and second, and tried to figure out what tone that should be. The retardation occurs in the portion of the record where this was being done.

In this and several of the following tables are given the measurements of a single record in which the rate of each separate tap is determined. Samples of the tapping of each of the different observers are thus made available for detailed inspection. It is interesting that the rate for individual taps can fluctuate as widely as it does without greater variability in the rate as measured for periods of three seconds.

The minor sixth (5 : 8) was, somewhat to the surprise of the experimenter, judged to be an incomplete and disappointing melody, ascending as well as descending. It has the

TABLE NO. 14

Major Third, descending. Metronome rate of each separate tap. Read down.

				e′	c′			
Rk.	177	187	202	198	218	178	148	172
	181	163	206	191	202	149	148	182
	168	182	202	185	153	148	246	185
	177	160	179	191	171	159	198	188
	181	164	185	211	176	162	182	171
	176	182	160	209	182	148	153	169
	182	153	190	271	163	159	191	158
	177	171	197	202	132	148	185	172
	182	182	166	226	183		183	148

Average rate for each three-second period. Read across.

Rk.	178	171	185	*207*	**188**	**162**	176	171
Po.	225	222	225	227	229	*237*	230	237
Ho.	100	103	104	**98**	97	*106*	106	102
Fr.	193	195	209	205	206	*213*	195	214
Pu.	80	81	85	**77**	80	*88*	84	86

Introspections.

Rk. Wanted a third tone *between*. Tried to decide what it should be.
Po. Surprising, but very satisfying. Final.
Ho. It became satisfactorily complete after I had thought about it.
Fr. Coherent, but suggested something further.
Pu. Needed a third tone to complete it;

TABLE NO. 15

Minor Sixth, descending. Metronome rate of each separate tap. Read down.

				g′	b			
Ho.	106	103	113	119	109	105	92	111
	117	106	115	104	89	100	103	119
	82	96	105	101	110	102	114	106
	110	117	123	106	128	101	97	102
	111	110	111	110	111	89	106	89

Average rate by three-second periods. Read across.

				g′	b			
Po.	225	220	224	**204**	*222*	222	225	227
Rk.	137	128	135	139	*145*	**130**	130	137
Ho.	104	106	113	108	108	**99**	101	105
Fr.	172	158	181	175	183	183	184	182
Ta.	106	101	105	105	107	**100**	103	105
Pu.	112	111	112	114	*127*	**118**	122	125

Introspections

Po. Surprise and disappointment on second tone. Unsatisfactory.
Rk. Does not end.
Ho. Very noticeably lacked finality.
Fr. Quite unrelated.
Ta. Tone pleasant but melody does not end.
Pu. Unsatisfactory. Incomplete.

TABLE NO. 16

Minor Sixth, ascending. Metronome rate of each separate tap. Read down

				b	g′			
Ta.......	80	86	75	81	77	79	79	81
	86	81	83	72	82	77	79	79
	82	81	83	72	79	81	81	82
	81	82	82	73	71	86	81	75

Average rate by three-second periods. Read across.

				b	g′			
Rg.......	99	101	104	**98**	101	**97**	99	100
Ho.......	102	108	110	**102**	**98**	98	98	100
Fr.......	208	207	215	*223*	**215**	**208**	201	213
Ta.......	82	82	81	**74**	77	*81*	80	79
Pu.......	97	97	97	**95**	95	*100*	102	107

Introspections

Rg. No melody; no finality.
Ho. Seemed bad at first but changes to a final interval.
Fr. Unconnected and therefore unpleasant.
Ta. Incomplete.
Pu. Unrelated. The second note seemed to change in character.

character of incompleteness very strongly as a descending interval, but when heard in the opposite direction it is possible so to reconstruct the tonality as to make the higher tone a tonic. This, the observers, with a single exception, failed to do.

Consequently Tables 15 and 16 may both be taken as showing the effects of a melody that lacks finality. The unusually large number of retardations strikes the eye at a glance.

§32 Turning now to some examples of three-tone groups (tables 17 and 18), we are confronted at the outset with the difficulty that it is usually quite possible to interpret any group of three related tones in a variety of ways, and we are thrown back upon the introspections of the observers for a starting point in our interpretation of the results. This method has its obvious disadvantages, notably those resulting from the probably imperfect reports which the average observer can give about so complex an experience as the course of a three-tone melody.

TABLE NO. 17

Three-tone groups. Average rate for each three-second period. Read across.

				g	e'	c'			
Rk........	140	143	141	142	141	*146*	*153*	147	142
Ho........	122	118	116	**109**	*117*	118	116	118	112
Ta........	71	74	71	70	*76*	*87*	88	75	72
Pu........	123	120	135	**114**	*128*	127	127	121	138

Introspections

Rk. Finished. Very good melody.
Ho. Complete, satisfactory.
Ta. Incomplete.
Pu. Uncertain.

TABLE NO. 18

				g	$e\flat'$	$b\flat$			
Rk........	130	131	126	*132*	*139*	*151*	*170*	139	136
Ho........	110	118	113	112	*115*	**111**	*118*	112	118
Ta........	70	70	68	**63**	*67*	*76*	**66**	66	66
Pu........	145	144	154	**138**	142	*148*	**141**	146	151

Introspections

Rk. Leaves me in suspense.
Ho. Unfinished. Don't like it.
Ta. Second note did not fit in at all. Very disconnected.
Pu. Fairly good ending, but the intervals are too wide.

The two melodies placed together here for comparison are very similar in form, and both are made up of wide, consonant intervals, but one of them, the first, seemed to the experimenter to have a more positive finality. The more musical observers agree with him in this. All of the retardations (neglecting of course those which accompany the entrance of the first tone) occur at the end of the less final of the two melodies.

On the whole these tables are not very illuminating.

TABLE NO. 19

Three-tone groups. Average rate for each three-second period. Read across.

				c′	*a*	*b*			
Rk........	154	152	160	**154**	*159*	*174*	**166**	179	160
Ho........	115	107	109	*112*	**105**	**86**	*96*	110	104
Ta........	69	71	71	70	71	72	70	70	71
Pu........	103	105	105	**96**	*102*	101	*109*	114	114

Rk. Unsatisfactory. *Must* go back to first tone.
Ho. Perfectly horrid! Due to the last tone.
Ta. Could give no introspection. (Note regularity of rate.)
Pu. Indifferent.

TABLE NO. 20

				c′	*a*	*b♭*			
1 Rk......	164	160	159	*173*	**168**	*180*	**170**	177	168
2 Rk......	159	156	157	**149**	145	146	*155*	171	168
3 Rk......	178	180	184	**179**	180	*187*	188	181	181
Ho........	97	102	106	104	**99**	*103*	101	100	104
Ta........	77	80	77	**73**	75	*87*	85	78	85
Pu........	97	99	98	*100*	**97**	*111*	**106**	99	98

1. Rk. Wrong, but not very bad. Second note spoiled it.
2. Rk and 3 Rk. (repetitions at a later date of same tones.) Both satisfactory and complete, the latter reassuringly so.

Ho. Last note predominates and becomes satisfactory ending.
Ta. Indifferent ending. Last note a disappointment.
Pu. Tones seemed disconnected.

Table 20 is of interest mainly because it shows the different reactions which the same melody elicited from one of the subjects at different times. The group of intervals, c′-a-b♭, is one which demands a shift of tonality, but which then ends, satisfactorily. When it was first given, Rk. did not so hear the melody: the tonality did not become readjusted. Two weeks later the experiment was repeated and this time the tones were heard as a complete melody. It was immediately given again, with similar but more positive introspective reports as the result. The three records show the expected differences in the tapping.

A striking record is that of Ta. (Table 19). He tapped throughout the course of the experiment almost with the regularity of a ruling engine. When asked for an introspective report, he could find nothing to say! The tones had had no effect whatever.

Every retardation shown in these tables finds its explanation in the introspective records. Not quite as much can be said for all of the accelerations.

With table 21 we take up the study of the "Return." The interval here used is the major second (8: 9). This is a very satisfactory melodic figure when the lower tone is the start-

TABLE NO. 21

Three-tone group. Major second. Average rate for each three-second period. Read across.

				d′	*c′*	*d′*			
Po........	248	244	249	**242**	**218**	*229*	*244*	253	251
Rk........	192	195	191	**171**	170	.170	*176*	183	187
Ho........	109	101	102	**93**	*96*	**92**	*98*	98	102
Ta........	100	107	112	**103**	**98**	*102*	**99**	101	101
Pu........	95	94	98	**89**	91	101	*105*	101	103

Po. Second tone very unpleasant. Third reinstated calm and repose of the first. At loose ends on second. The return changed all this.
Rk. Very unsatisfactory as a whole but had a certain unity about it.
Ho. I think that ended nicely. It is curious that I can not recall the middle tone.
Ta. The lower would have been a better ending.
Pu. Second note not right. Return to first gave feeling of finality.

TABLE NO. 22

				c′	*d′*	*c′*			
Po........	259	265	265	**259**	**254**	*261*	**256**	265	252
Rk........	162	188	181	**164**	**158**	*171*	*178*	192	181
Ho........	114	105	110	**97**	*100*	**93**	*100*	93	
Ta........	114	106	101	*107*	**95**	*102*	*102*	121	115
Pu........	106	118	102	*111*	**96**	95	*108*	100	102

Po. Third tone a pleasant relief from suspense.
Rk. It was all right at the time.
Ho. Very pleasant and complete.
Ta. Positively finished.
Pu. Pleasant and complete.

ing point and the end, and one is not surprised to find a large proportion of accelerations at the close. (See table 22, *c′-d′-c′*.) The record fits well with the introspections.

When the upper tone is made the point of departure and return, the melody tends to fall apart. The middle tone positively will not fit into any tonality suggested by the first. This appears very prominently in the introspective records. Another feature is that *without exception* the observers felt that the return from this lower tone to the upper was very satisfactory. "The third reinstated the calm and repose of the first," etc. The entire set of introspections accompanying this table is recommended for careful perusal as clearly setting forth the result of a return from a tone felt to be foreign to the first. The experience acquires a unity which is most certainly *not* contributed by any interval "relationship."

TABLE NO. 23

Three-tone groups. "The Return." Average rate for each three-second period. Read across.

				c′	*f′*	*c′*			
Po........	285	260	254	*259*	**244**	**238**	*248*	256	258
Rk........	154	162	164	*168*	**160**	*171*	*178*	176	167
Ta........	82	83	81	**76**	*79*	78	**74**	76	84
Pu........	99	105	110	**96**	*110*	**99**	*113*	107	108

Introspections.

Po. Much less complete than if upper tone were last.
Rk. Satisfactory ending, but not so good as *f-c-f* (hummed).
Ta. Finished.
Pu. Incomplete. Second tone unrelated to others.

TABLE NO. 24

				f′	*c′*	*f′*			
Po........	249	249	249	**236**	234	*254*	*259*	261	262
Rg........	119	126	126	**123**	*127*	**123**	*126*	128	126
Rk........	171	180	183	**172**	170	*176*	176	181	
Ta........	79	76	75	**72**	70	*73*	72	75	76
Pu........	81	86	87	*90*	**86**	*90*	*101*	97	96

Po. Emphatically final.
Rg. O. K. Finished.
Rk. Fairly satisfactory. More so than *c-f-c* (hummed).
Ta. Fairly complete.
Pu. Complete.

TABLE NO. 25

Three-tone groups. "The Return."
Rate for each separate tap. Read down.

				c'	g'	c'			
Po........	243	274	242	240	267	236	248	236	252
	285	267	246	253	236	254	246	229	267
	262	258	226	260	240	244	276	204	267
	265	260	226	247	256	252	265	223	246
	258	222	239	252	276	254	263	213	242
	260	232	221	262	262	269	236	229	254
	272	252	224	248	267	278	247	256	254
	265	254	253	233	260	250	260	265	243
	260	236	236	233	231	250	252	260	267
	276	221	236	252	269	248	233	256	240
	272	224	258	276	277	224	270	250	272
	269	232	242	272	256	228	260	258	253
	272	226	240	272	258	240	234	246	254

Three-tone groups. "The Return." Average rate by three-second periods. Read across.

				c'	g'	c'			
Po........	268	238	238	254	258	248	251	240	254
Rk........	118	118	119	**116**	118	*128*	**115**	117	
Ho........	79	80	79	78	80	*82*	*93*	95	95
Fr........	208	203	200	199	196	*206*	207	200	198

Introspections.

Po. More or less complete. Not very good.
Rk. O. K. Finished.
Ho. Complete. Very pleasant.
Fr. Complete, but not wholly satisfactory.

A study of the table of rates itself is equally illuminating. In number and distribution of accelerations, it is almost identical with the companion table, where the return was from a tone felt to be quite coherent with the first tone of the melody.

Tables 23–26 also show the effects of the return to the starting point. The intervals used differ from the preceding in that they are wider, and consonant intervals. The fourth (tables 23 and 24) ends more emphatically upon the upper note, the fifth (tables 25 and 26) on the lower. This was the judgment of the observers. The small sprinkling of retardations at the close of these melodies would indicate that this difference in finality is unable to maintain itself, as against the two factors that tend to exert an opposing influence upon the tapping, the factors, namely, of the return, and of the fact that the expected number of tones was heard and nothing further anticipated.

TABLE NO. 26

				g′	*c′*	*g′*			
Po.........	230	230	237	**227**	**208**	*239*	*246*	251	
Rk.........	138	138	140	**131**	*140*	139	**136**	128	
Ho.........	77	78	80	79	**77**	*80*	*86*	84	
Fr.........	204	197	196	197	**193**	195	193	196	203
Ta.........	74	74	80	74	73	*76*	*81*	75	81
Pu.........	106	105	107	**103**	102	*116*	*119*	110	108

Po. No feeling of finality; therefore unpleasant. No tendency to go elsewhere.
Rk. Not as complete as *c′-g′-c′* (hummed), but one isn't left in suspense.
Ho. Can't say as to completeness. Unpleasant.
Fr. Incomplete.
Ta. Better to end on second note.
Pu. Not emphatic finality; only such as any 'return' gives.

What of the octave? Meyer was unable to detect any stronger "trend" to the lower than to the upper tone, and consequently put himself on record as opposed to Lipps and the other writers who assert that the lower tone possesses the stronger finality.[1]

The question was put to each of my observers. They were asked to judge with reference to the finality of ascending octaves, descending octaves, and also groups of three tones, involving the return. Intervals in the middle region of the scale and also in the great octave were used. The results were strongly against Meyer's view. Pu., the least musical of the observers, could detect no difference in finality between the end on the upper and the end on the lower of two tones an octave apart. All others found that a stronger feeling of finality attached to the end on the lower tone. This dif-

[1] *Psych. Rev.* 1900, *7*, 248. In the light of his more recent studies on the effect of the falling inflection (see above, p. 28) we suspect that Meyer would today formulate somewhat more guardedly his statements regarding the psychological effect of the close on "1" and on "2."

ference of preference does not make itself evident, however, in the tapping records of tables 27 and 28 (the octave). At the close of the melody there is found almost exactly the same preponderance of accelerations over retardations in each of the two tables. Although one ending is better, both are good.

TABLE NO. 27

The Octave. Rate of each separate tap. Read down.

				c'	c''	c'			
Rg........	90	93	96	88	83	70	81	85	86
	81	83	93	85	93	83	92	93	90
	88	88	95	93	81	88	97	83	83
	93	88	88	81	79	81	86	90	85

Average rate for each three-second period. Read across.

				c'	c''	c'			
Rg........	88	88	93	**86**	**83**	85	*88*	88	86
Po........	258	252	254	**245**	**238**	239	*257*	256	284
Rk........	238	232	236	**211**	**204**	205	*210*	218	231
Ho........	120	120	119	**104**	*108*	109	**96**	107	108
Fr.........	186	199	199	*205*	*216*	**207**	206	218	210
Ta........	78	81	78	**72**	71	73	*77*	81	79
Pu........	96	93	98	*109*	112	**101**	*112*	120	113

TABLE NO. 28

The Octave. Rate of each separate tap. Read down.

				c''	c'	c''			
Rg........	93	96	91	76	95	90	94	88	98
	95	91	93	93	85	99	99	102	92
	90	90	93	89	88	88	90	92	93
	96	99	94	88	89	85	86	91	90

Average rate for each three-second period. Read across.

				c''	c'	c''			
Rg........	93	94	93	**86**	*90*	90	*92*	93	93
Po........	272	261	277	**245**	**232**	*262*	**254**	256	252
Rk........	251	250	259	256	*262*	260	259	268	266
Ho........		107	108	**102**	**97**	*104*	*106*	111	106
Fr.........	198	199	192	*215*	216	**207**	205	206	212
Ta........	81	81	81	**78**	*87*	82	80	81	82
Pu	130	138	116	112	114	114	*124*	120	147

§33 In the last two tables to be presented, Nos. 29 and 30, are shown the rates of tapping during the hearing of a longer group of tones. Here the exact number of tones was not told in advance, the observers being informed merely that they might expect several more than the usual number. The two "melodies" are alike in that they both start and end with "c," and both use the same intermediate tones; but they differ in the order of these tones. The first group moves slowly but naturally forward, and at length comes inevitably to rest on the last of the seven tones. The second moves as slowly and as regularly, and reaches the same goal,—and yet the goal is not the same. Subjectively it is no goal at all. None of the observers knew when it had been reached until the tones abruptly ceased, whereas with the previous group, all but one reported that they knew the last tone was the last as soon as it began to sound. The first sequence, then, is a genuine melody; the second is not.

One or two typical introspections may be quoted as representative of the sort of experience which was more or less common to all of the observers. Rk. (first seven-tone group.)

During the first three notes I did not know what was the melodic meaning or general direction, but on the fourth note it took shape and I anticipated what the next would be, and so on to the last. The last was definitely final. It didn't occur to me that there might have been more tones until you suggested the possibility of it.

(Second group.) The third note was not what I expected. The sixth would possibly have made a good ending. The last note was a disappointment; it wasn't offensive, but obviously was not the best possible.

None were satisfied with the ending of the second group of tones; all thought it more or less incoherent throughout and hard to grasp. But with the first group every observer with one exception was sure, when the last tone had been reached, that that was to be the final tone. The one exception, Pu., could not give a definite answer to the question whether the ending were a surprise or not, whether or not anything further was anticipated.

TABLE NO. 29

Group of tones judged to be a melody. Rate of each separate tap. Read down.

				c'	e'	g'	e'	f'	d'	c'			
Po.....	252	238	236	238	252	228	221	238	258	228	250	248	236
	246	228	235	240	228	220	222	222	256	204	256	236	238
	250	237	236	237	208	220	236	240	240	222	260	256	254
	230	236	237	246	208	233	218	238	251	186	233	246	238
	250	238	238	220	205	211	220	222	246	219	218	256	220
	228	236	232	221	229	220	252	261	236	254	241	254	236
	236	250	256	220	206	233	233	228	246	212	218	236	236
	237	246	258	211	254	220	231	257	228	234	244	236	238
	246	237	252	205	237	224	237	237	245	238	256	254	238
	227	254	257	217	220	217	220	254	256	226	242	234	236
	245	250	252	232	212	203	220	238	238	246	240	234	236
	236	238	257	226	210		220	222	220	236	246	236	244

Average rate for each three-second period. Read across.

				c'	e'	g'	e'	f'	d'	c'			
Po.....	241	244	247	**227**	**221**	219	*227*	*241*	242	**227**	*251*	244	239
Rg.....	98	98	92	95	**90**	89	*97*	**91**	**88**	*93*	*95*	96	94
Rk.....	200	190	196	195	**181**	180	*190*	191	*198*	*207*	205	208	215
Ho.....	87	80	87	80	78	83	82	82	79	*88*	*90*	92	88
Fr.....	205	229	228	222	223	216	**202**	*206*	*212*	212	*216*	206	216
Ta.....	69	70	68	67	**63**	*74*	**68**	66	58	*67*	68	66	69
Pu.....	102	98	105	**97**	*101*	*110*	*114*	**107**	108	**102**	*106*	109	112

TABLE NO. 30

Seven-tone group judged not to be a melody. Average rate by three-second periods. Read across.

				c'	f'	d'	g'	e'	f'	c'			
Po.....	260	254	261	**255**	*276*	**253**	*280*	**238**	*264*	**247**	*260*	253	265
Rg.....		104	108	**103**	*109*	*112*	112	*140*	**114**	**102**	101	105	108
Rk.....	152	153	162	*169*	*172*	168	168	**163**	*169*	**157**	**152**	153	171
Ho.....	102	112	111	**100**	100	102	**99**	101	*106*	*112*	**105**	110	102
Pu.....	117	105	122	117	**110**	*122*	121	**113**	115	*118*	118	125	122

In the tables the changes of rate are shown throughout the course of the melody, but the ones which are of special significance for our purposes are of course those accompanying the strongly contrasted feelings at the end of the tonal sequences. At the close of the first, every record reveals an acceleration in the rate of tapping. In marked contrast are the retardations found at the close of the other sequence. (See accompanying graph, Fig. 3.)

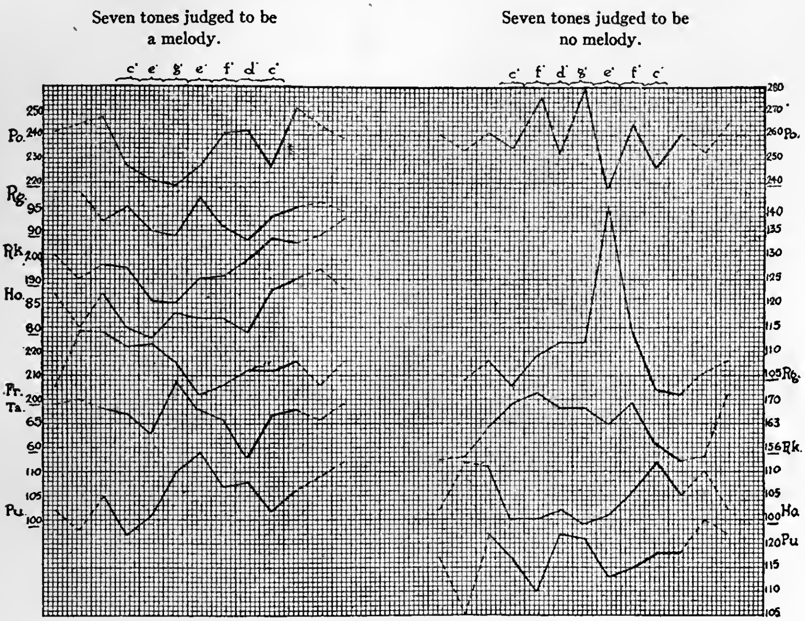

FIGURE NO. 3. EFFECTS OF A MELODY AND A NON-MELODY CONTRASTED. Each tone sounded for three seconds. Graphs represent rate of tapping during each of these three-second periods. Note general tendency toward increase in rate at close of melody, and absence of such acceleration at close of non-melodic sequence.

§34 It remains to summarize and evaluate the foregoing experimental data.

The facts which stand out with most prominence are, first the correlation between the beginning of a tonal sequence and a drop in rate of tapping; second, the correlation, nearly as close, between the conclusion of a tonal sequence and an increase in rate in case the observer knows in advance how many tones are to be expected; third, the retardation of rate at the end of a two-tone sequence when the observer has been led to expect three tones, the sequence being one which under the usual conditions produced acceleration instead of retardation of rate; fourth, retardations at the close are much more frequently encountered among those two-tone intervals which are judged to be "unrelated", incoherent or decidedly "incomplete," than among intervals judged to be melodious, coherent or characterized by finality; (*vid.*, especially, descending *vs.* ascending fourth, ascending *vs.* descending fifth, minor sixth *vs.* major third); fifth, the return to a first tone is felt as giving unity to a three-tone group, and retardations at the close are not often met with, no matter how unrelated and foreign the middle tone may have been; sixth, longer sequences of tones, the pitch relations of whose elements give to them opposite characters as regards internal coherence and finality, produce opposite effects upon the rate of tapping.

In an examination of our data, these six points come to view. The attempts to apply our hypothesis in detail to some of the results must be considered, however, simply as indications toward a possible development of the method into an analytic tool of much usefulness, rather than as bringing forward further positive evidence on the question of the motor aspects of the perception of a melody.

PART IV.

SUGGESTIONS TOWARD A MOTOR THEORY OF MELODY.

Such evidence of the interconnection between muscular activity and melody experience as has been here adduced is too slender to serve as the support of an elaborate and detailed theory. But the broad lines along which a motor theory of melody must some day be worked out may be with propriety suggested here, as harmonizing with the experimental facts in so far as they are available.

§35. Every melody, like every other experience which is a 'whole,' must have, in Aristotelian phrase, "a beginning, a middle and an end." A motor theory of melody finds the 'beginning' in the upsetting of established muscular tensions which the onset of the tonal sequence involves.

The 'middle' includes the taking of the proper 'attitude,' the organization of a set of incipient responses, and then as the tonal sequence proceeds, the making of these responses explicit and overt in the acts of responding to the successive tones. Each tone demands a specific act of adjustment for which a general and also a more or less specific preparation has already been made, and each contributes in turn to the further more definite organization of the total attitude. If a tone appears which is of such a pitch that an entirely new adjustment is necessary, that tone is unrelated: unity is destroyed; the succession of tones is not a melody. But if the new tone is so related to its predecessors that it institutes a response which is in part a continuation of the act already in progress, the unity is preserved.

The 'end' comes only with the arrival of a phase of the complex ongoing activities in which the balanced tensions can merge into each other and harmoniously resolve their opposing strains. This becomes possible when a sufficiently definite set of expectations has been aroused and then satisfied. Here we find a reason why a close on the tonic has to be 'prepared for,' in musical phraseology, by a 'leading tone' not in the tonic chord. The expectations, the muscular strains and tensions, must be developed to a certain degree of definiteness of organization before a return to the tonic can serve as the cue for a general 'resolution.' '*Lösung*' describes the close of the motor process somewhat better than its English equivalent, relaxation. A single muscle can relax. But this process of muscular *Lösung* which marks the end of a melodic phrase, a spoken sentence, or a rhythmical period, is more than mere relaxation; it is an organized, balanced muscular "resolution," to borrow a very apt technical term from the musicians.

Of some such 'beginning' and of some such 'end,' even so crude and apparently remote a line of experimental attack as the one we have used, has furnished an indication. In order to learn about the nature of the 'middle' muscular processes a more refined way of approach to the delicately complex mechanism of the melody experience must be devised. One would like best of all to record the tensions of the laryngeal muscles when no sound is being emitted. Here doubtless is one of the centers, with many persons at least, of those activities by means of which a series of separate musical sounds is bound together into the unified experience we call a melody. Already some few significant facts have been accumulated regarding vocal tensions during auditory stimulation. Seashore and Cameron have independently demonstrated that a vocal tone sung against an auditory distraction tends to vary toward a pitch which is consonant with the distracting tone.[1]

Is this muscular process whose arousal and subsidence give shape and unity to a melody, a rhythm? It certainly has many of the earmarks of a rhythm,—its motor mechanism, its relaxation following tension, its conscious aspect describable as a satisfaction of expectation—all these would lead

[1] E. H. Cameron. "Tonal Reactions." *Psych. Rev. Mono. Supplements.* 1907, *8*, 287.

one to call it a sort of macro-rhythm, a giant process similar in its essential nature to a rhythm in the usual sense. But there are fundamental objections to such an identification, chief of which are (1) that a rhythm involves *repeatedly recurrent* stresses, with recognition of similarities, as this 'ground-swell' muscular process does not, and (2) that a certain regularity, with possible variations between well-defined limits only, is essential to rhythms. The two phenomena, although both motor at basis, must not be confused.

The experimental study of rhythm has, however, disclosed a motor phenomenon essentially like the large, basic motor activity underlying a melodic unity. I refer to the particular sort of muscular tension-relaxation process which Stetson[1] found to be essential to the unity of a group of rhythmic elements felt to constitute a verse, or a rhythmic phrase.

Using a modification of the principle of the phonographic recorder, Stetson made records of spoken verse, and measured with microscope and micrometer the duration and the relative intensity of the separate syllables.

In unrhymed stanzas the duration of the verse pause was found to vary widely, but it was invariably longer than the foot pause. The typical dynamic shading of the verse was found to be of the crescendo- diminuendo form. The introduction of rhyme often shifted the climax of the crescendo to the final foot by increasing the intensity of the rhymed syllable. Although as great a verse pause was found to be possible with rhyme as without it, the presence of rhyme tended to shorten the verse pause, to bring the verse to a close more rapidly.

Within the verse the general form of the syllable as it appears in the mass of closely written vibrations often varies, but nearly always shows a square end. Several very common shapes are noticed and appear in the record as 'truncated cones,' 'boxes' and 'truncated spindles.' . . ..One syllable form has an especial interest, because of its bearing on the problem of 'finality' feeling at the close of the verse. At the close of each verse, whether with or without rhyme, the syllable

[1]R. H. Stetson. Rhythm and Rhyme. *Harvard Psych. Studies*, Vol. I. *Psych. Rev. Mono. Suppl.* 1902, *4*, 413.

form is always a 'cone.' Of about 600 verses measured not more than 15 are exceptions to this rule..............................
The form very rarely occurs within the verse, and when it does it is usually before some caesura, or under unusual conditions.

This 'cone' form of the closing syllable of the verse indicates a falling of the intensity of the voice. It is often, though not always, associated with a fall in the pitch, showing relaxation of the vocal cords. It seems to be an indication of the dying out of the intensity factor, a sinking of the tension, at the close of the verse. In the case of unrhymed verses, with long verse pause, the cone is often very much elongated, and it is quite impossible to say where the sound ceases.[1]

It will not be necessary to treat here of those portions of the motor theory of rhythm which explain, as the central, or "mental activity" theories have failed to do, the peculiar nature of the various sorts of unit groups.[2] We shall briefly sketch only so much of the theory as is requisite to explain the larger groupings such as the phrase, the verse, the period.

Stetson's theory of rhythm assumes a movement cycle involving the activity of two opposing sets of muscles. The varying tension between these muscle sets as beat follows beat never entirely disappears until the close is reached.

The continuity of the rhythmic series, whereby all the beats of a period seem to belong to a single whole, is due to the continuity of the muscle sensations involved and the continuous feeling of slight tension between the positive and negative muscle sets; nowhere within the period does the feeling of strain die out.

But at the close of the period we have a pause which is demonstrably not a function of any of the intervals of the period. During this pause the tension between the two sets 'dies out,' and we have a feeling of finality. This gradual dying out of the tension is clearly seen in the constant appearance of the cone-shaped final syllable at the end of each nonsense verse.

The period composed of a number of unit groups (the verse, in nonsense syllables) has a general form which suggests strongly that it has

[1] *L. c.*, 447.

[2] For a determination and explanation of these peculiarities, such as the closer proximity of the unaccented to the accented beat in the iambic as contrasted with the trochaic foot, etc., *cf.*, Stetson, "A Motor Theory of Rhythm and Discrete Succession," *Psych. Rev.* 1905, *12*, 293 ff.

the unity of a single coördinated movement. There is no more reason for assuming a transcendental mental activity in the case of a rhythmic period than in the case of a single act which appears in consciousness as a unity.................................At some point in the period there is a definite climax, a chief accent; the movement 'rises' to that point and then falls off. This is strikingly seen in nonsense verses spoken with a heavy accent within the verse. The accent does not stand out from a dead level, but the verse culminates at that point.[1]

As a result of his previous study of perceived as opposed to produced rhythms and especially the effects of rhyme and of wide variations of tempo,—'lags,'—introduced into different portions of the verse and of the stanza, Stetson was led to the conclusion that

there is some definite process at the end of the verse which marks the close of the verse and which takes more time in the case of blank verse than in the case of rhymed verse. If we conceive the end of the verse as a point where a dying out of the tension occurs, we may imagine that the rhyme brings an emphasis, and becomes a qualitative signal for this release. The slight increase of intensity on the rhyme contributes to the breaking up of the coördination, and at the same time exhausts and satisfies the feeling of tension which the verse embodies........... A qualitative change may be supposed to produce the effect more rapidly than the simple dying out of the tensions, which occurs in blank verse without a differentiated end accent.[2]

This finality effect which rhyme augments is entirely analogous with the finality phenomenon in melody. We have seen that in three-tone sequences mere return to the original pitch may furnish the qualitative signal for the muscular 'resolution.' If the final tone is not merely a repetition of the initial tone, but has also the characteristics of a 'tonic,' the completion of the finality process is much more definitely assured. A third cause which sometimes operates to produce the same effect is the mere satisfaction of expectation. If one hears a certain irregular series of pitches, "related" or "unrelated," often enough so that the final tone can be recognized as such, one comes to feel that the group has a certain sort of

[1] *Rhythm and Rhyme*, 455.
[2] *L. c.* 425.

unity even though there is neither a return to a starting point nor an end on the tonic. The same holds true, to a certain extent, with reference to an unfamiliar succession of tones whose number is known in advance. If the observer is told to expect four tones, a motor disposition or attitude is established which constitutes a preparedness to react to four tones, and if only three tones are heard, the finality effect may fail to appear, although the third and final tone is at once a tonic and a return to the pitch of the initial tone of the sequence.

In each of these types of melodic finality, the closing tone institutes a response which is not wholly a new reaction but which is, on the contrary, the completion of an act already in progress. The feeling of finality arises only when the completion of the act issues in a muscular relaxation which is a dying out of balanced tensions. The facts regarding those finality effects which are due to the falling inflection also coincide with such a view. Rise in pitch is not merely a result of increased tension of the vocal apparatus: it likewise produces increased muscular tension in the hearer. A falling inflection at the close consequently serves to hasten the relaxation process which marks the completion of the melody.

Finally, a motor theory of melody makes possible an unambiguous statement of the nature of melodic "relationship." Two or more tones are felt to be "related" when there is community of organized response. "Unrelated" pitches fall apart because each demands its own separate attentive act of adjustment; but with "related" tones the attitude which appears as a response to the first is a preparation for the response to the second and is completed, not destroyed, by that response. The feeling of "relationship" is the feeling that arises when the tones elicit reactions which are in some measure common. When, on the other hand, the first tone calls up one set of associates and establishes a certain attitude or organization of incipient tendencies, while the second tone tends to call up a set of associates and establish an attitude which is at variance with the first, there can be no adequacy of coördinated response and the feeling of "relationship" is prevented from arising.

The origin of these well-articulated responses which generate the feelings of "relationship" is not to be sought in a single source. The operation of two main forces must be distinguished—one of them sensory, the other associative. The first of these, the phenomenon of consonance, is native and doubtless has its basis in the relatively simple action of the sensory apparatus in responding to auditory stimuli which are more or less similar—are, indeed, in a measure identical. But although the basis for consonance inheres in the inborn structure of the nervous system and the acoustical properties of vibrating bodies, nevertheless it is a commonplace of musical history and observation that these same native tendencies are subject to tremendous modification in the course of experience. One race, one age hears as consonant intervals which another age or race has never learned to tolerate; and within the history of individuals it is easily observable that consonance and dissonance are merely relative terms whose denotation shifts with growing experience. Moreover the whole complex group of phenomena we call tonality bears witness to the power of association to amplify and organize these native feelings.

But the associative factor or the factor of experience is directly efficient in determining what tones shall be felt as "related," quite apart from any effects which it has upon judgments of consonance. Mere custom, mere habituation to a certain succession of pitches results in a facility of recognition and response which is capable of generating these feelings of "relationship." The same kind of coördinated reaction is instituted and this makes possible the same resultant feeling as that brought about by response to two successive consonant tones. The "relationship" is in both instances traceable to the motor phase of the process.

The unity, then, which marks the difference between a mere succession of discrete tonal stimuli and a melody, arises not from the tones themselves: it is contributed by act of the listener. When tone follows tone in such a manner that the hearer can react adequately to each, when the response to the successive members of the series is not a series of separate or conflicting acts but rather in each instance only a continuation or further elaboration of an act already going forward, then the tones are not felt as discrete, separate, independent, but as "related" to each other. And when, finally, the series of tones comes to such a close that what has been a continuous act of response is also brought to definite completion, the balanced muscular "resolution" gives rise to the feeling of finality, and the series is recognized as a unity, a whole, a melody.

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Report of the Committee of the American Psychological Association on the Teaching of Psychology.

Presented to the Association December 29, 1909.

Committee:

Carl E. Seashore, *Chairman*
James R. Angell
Mary Whiton Calkins
Edmund C. Sanford
Guy Montrose Whipple

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REPORT OF THE COMMITTEE ON THE TEACHING OF PSYCHOLOGY.

To the Members of the American Psychological Association:

Your committee appointed a year ago to investigate and report upon the teaching of psychology respectfully submits its report herewith.

The committee at once limited its investigation to the teaching of the *first year course in psychology*; and, to secure uniformity, this course was defined as a sophomore course running three hours a week throughout the year, or five hours a week for one semester, whether this time is given to a single course or to two.

The committee then resolved itself into three subcommittees for the division of labor as follows:

1. The Normal Schools, Guy Montrose Whipple.
2. The Colleges without laboratories, Mary Whiton Calkins.
3. The Colleges and Universities with laboratories, Edmund C. Sanford, James R. Angell.

Each of these subcommittees has circulated a questionary, worked over the returns, and made certain recommendations on the basis of these returns. Each of these four reports is herewith presented entire and independent. The arrangement represents the division of labor in the committee. No effort has been made to reduce these individual contributions to any formal committee report having the stamp of authority either from the committee as a whole or from the Association. Our aim has been to present a survey of existing conditions, to bring together some of the most helpful hints for improvement, and to stimulate interest and effective coöperation for the advancement of the teaching of the first-year course in psychology.

The committee tenders its most sincere thanks to all the psychologists who have given their generous coöperation in answering our inquiries.

THE TEACHING OF PSYCHOLOGY IN NORMAL SCHOOLS.

By Guy Montrose Whipple.

Cornell University.

This report is based upon 100 replies (84 from public, 16 from private normal schools), which were received in response to a printed questionary, mailed to the 259 institutions (189 public, 70 private) listed as normal schools in the Report of the United States Commissioner of Education for 1907.

The following states are not represented in my replies:—Arkansas, Georgia, Indiana, Kentucky, Louisiana, Missouri, New Hampshire, Oregon, Rhode Island, Tennessee, and Texas. In the consideration of the items which follow, therefore, it should be borne in mind that 11 state systems are unrepresented, and that somewhat less than half of the existing institutions have contributed data. At the same time, it is safe, I believe, to regard this report as a fair representation of the present status of psychology in our normal schools.

The printed questionary (with the omission of spaces left for replies) is as follows:

QUESTIONS ON THE TEACHING OF PSYCHOLOGY IN NORMAL SCHOOLS.

Please read all the questions before beginning to answer. When the space allowed is insufficient for your reply, please attach extra sheets, but number the replies thereon to correspond with the numbering of the questions. If you can do so, mail me a catalog of your institution, and mark the courses in psychology.

Name and location of institution.
Own name and title.
Address to which you wish printed report sent.

A. GENERAL AIM OF WORK IN PSYCHOLOGY: ITS PLACE IN THE CURRICULUM.

1. State concisely the general aim of the work in psychology, *i. e.*, tell what it is designed to do for the students.

2. Do you give separate courses in general (or elementary) psychology and in educational psychology, or are these combined in one course?

3. Do you offer any courses in experimental or laboratory psychology in addition to those in general or educational psychology?

4. (*a*) Have you a psychological or psycho-educational laboratory?
   (*b*) If not, have you any collection of psychological apparatus?
   (*c*) What is the approximate value of this equipment?
   (*d*) When was the laboratory established?
   (*e*) Is it supported by a yearly appropriation? (*f*) How much?
   (*g*) Has it separate rooms or is it united with other laboratories?

(*N. B.* In what follows, consider all courses in psychology.)

5. What is the length of the course or courses, *i. e.*, number of exercises per week, length of each exercise, number of weeks given?

6. Is the course elective (freely), or is it required? If the latter, of what group of students (what year, course, etc.)?

7. Is the course a prerequisite for other courses, *e. g.*, psychology, principles of education, methods, practise-teaching, etc.? (Please submit a brief outline showing these interrelations.)

8. What is the average number of students registered in the several courses in psychology each year?

B. METHOD OF CONDUCTING THE WORK.

9. Do you use a regular text-book or books? If so, what?

10. Does the work in psychology center chiefly about the text-book (recitations, discussions, etc.)?

11. If you use any of the following devices, indicate approximately the relative amount of time devoted to their use:
   (*a*) Lectures by the teacher.
   (*b*) Demonstrations by the teacher.
   (*c*) Experiments by students (give examples).
   (*d*) Class discussion of text or lectures.
   (*e*) Dictation by the teacher, to be taken down verbatim by students.
   (*f*) Written exercises of various sorts.
   (*g*) Exercises demanding introspection by students.
   (*h*) Exercises demanding observation of others.
   (*i*) Outside reading by students in books other than the text-book. (Required of all students or assigned individually for report? If confined to a few books, please name these.)
   (*j*) Personal appointments with individual students to discuss difficulties, etc.
   (*k*) Printed or mimeographed lists of questions or outlines for use by students to check up their knowledge of the course. (If feasible, send a copy, or state where it may be bought.)
   (*l*) Any other devices.

12. Of the devices just mentioned, which is most valuable in aiding the students to assimilate the work?

13. If you give any demonstrations in class, submit a list of these.

14. Mention any pieces of apparatus that you consider especially useful for illustration or demonstration-work.

15. Is the class period ever used merely as a study-period, or is all study done outside of the classroom?

16. If you give lectures, do these merely illustrate and explain the text-book, or do they develop topics not mentioned in the text?

17. Upon what is the final grade of the student determined,—recitations, exercises, examination on the text-book, or what? (Please enclose a sample set of examination questions.)

18. Does the class always meet as a whole, or at times in sections? Is it ever quizzed by assistants or by anyone other than the regular teacher?

19. Do you have lectures on psychological topics given at your institution by persons not connected with your faculty? Is there any regular system of such lectures?

C. CONTENTS OF COURSES.

(*Note. If you follow exactly the topics and contents of a text-book, it may not be necessary to answer questions in this Section in detail.*)

20. Submit an outline of the topics given or discussed, lecture by lecture, *e. g.*, (1) nature of psychology, (2) nervous system, etc.

21. Please indicate as nearly as possible the absolute (or relative) time that is devoted to the following topics:

(*a*) Development of notions of scientific method (meaning of experiment, introspection, etc.).
(*b*) Quasi-philosophical problems, such as the relation of mind and body, nature of mind, biological significance of consciousness, etc.
(*c*) Anatomy and physiology of the central nervous system.
(*d*) Psychology of sensation (including structure of the sense-organs).
(*e*) Attention (including interest).
(*f*) Affective processes, feeling, and emotion.
(*g*) Reflexes and instincts.
(*h*) Psychology of learning, habit, educative processes.
(*i*) Complex forms of action, *e. g.*, impulsive, selective, volitional, etc.
(*j*) Association.
(*k*) Perception (including observation).
(*l*) Memory.
(*m*) Imagination.
(*n*) Conception, judging, and reasoning.
(*o*) Problems of mental development, such as inheritance, contents of children's minds, adolescence, and other child-study topics.
(*p*) Individual differences of all kinds.
(*q*) Animal psychology.

(*r*) Borderline phenomena, such as hypnotism, sleep, dreams, psychotherapy, etc.
(*s*) Any other topics, such as hygiene of the sense-organs, fatigue, psychology of primitive peoples, etc.

D. THE TEACHER.

22. Do you give instruction in other courses than psychology? If so, in what?
23. Is there more than one teacher directly or indirectly engaged in giving instruction in psychology?
24. Do you attempt to do any original work in psychology? If so, are you aided by students?
25. Enumerate any studies (magazine articles, books, etc.) on psychological topics published by you or by your pupils in the institution.
26. Where did you receive your training in psychology?
27. What degrees do you hold, or what diplomas or certificates have you received?
28. How long have you taught psychology?

E. RESULTS, DIFFICULTIES, POSSIBLE IMPROVEMENTS.

29. Do the students display distinct interest in psychology? In what way?
30. Enumerate the ways in which you think the work in psychology helps the students in their work as teachers.
31. In presenting the subject, do you encounter any difficulties that the text-book does not resolve? What are they?
32. If you had a free hand, would you make any changes in your present methods of teaching psychology, and if so, what changes?
33. In what ways could this Committee be of assistance to you in your work?

F. REMARKS.

Kindly add any remarks that would assist us in understanding the status of the teaching of psychology in your institution.

Before listing the replies to the questionary in detail, a bird's-eye view of the teaching of psychology in the normal school may be of value. Such a view may be secured by describing, on the basis of the data submitted, the condition of psychology in the typical (or average) institution.

In this typical institution, then, the teacher has received his training from an American college, and holds a bachelor's, and possibly also an advanced degree. He has taught psychology for 8.5 years, has sole charge of the courses therein, and, in addition, gives instruction in certain courses in education.

His class of 107 pupils demands much time and energy, and other circumstances, such as scanty library and laboratory facilities and the immaturity of his pupils, conspire to render it difficult or impossible for him to prosecute original work, or to keep pace with the contributions of others.

In teaching psychology, he encounters difficulties, due in part possibly to his own lack of preparation, in part to the immaturity of his pupils, in part to the inherent complexity of the subject-matter, but more especially (at least in his opinion) to the inadequacy of the available text-books.

He is, of course, anxious to improve his instruction: he would would like to know what the instructors in other schools are doing in psychology, and would be glad to receive helpful suggestions concerning the arrangements and conduct of his course. If he could, he would extend the time given to psychology, and would work out more satisfactory correlations between the work in psychology and that in biology and in education.

The course he gives in psychology is a combination of general and educational psychology: it is required of all students as a prerequisite for subsequent or concurrent work in education,—especially in the study of methods and in practise-teaching. The class meets in one section for 45-minute periods. In all, 90 (actual) hours are devoted to the subject.

The typical teacher aims to give his pupils general familiarity with the laws and operations of mental life, with the particular idea of rendering this knowledge practical and useful in the life work of the teacher, and he firmly believes that this result is attained,—that those who have had psychology have gained a sympathetic knowledge of the child's mental life, that they appreciate the rationale of methods, and teach more skillfully. Whether the students are at the time convinced of the serviceability of their study of psychology is, perhaps, an open question. Probably about half the class exhibit interest: the other half take the course because they must, and in so far, the teacher is always conscious of the necessity of striving to make psychology at once attractive and practical.

The content of the course is determined almost entirely by the text-book (probably some one of the volumes of Angell, Halleck, James, Thorndike or Titchener). This text may or may not distribute the emphasis upon those topics that are most significant for education. The chances are that too much time is paid to the nervous system and not enough to habit, the inheritance and acquisition of capacities, and other features of the educative process. The chances are, however, that the distribution of time in any single institution does not coincide with that in any other institution.

In presenting psychology, the teacher relies mainly on the text-book: he lectures only occasionally, and in an informal manner, either upon topics discussed or topics not discussed by the text.

All studying is done outside the classroom.

The very few demonstrations that are given are confined to the nervous system, central or peripheral.

The student performs few or no experiments for himself, and is not properly trained in introspection; he may submit occasional reports of observation upon the children or the teaching in the practise school.

Discussion in the class is a feature of the course and there is a reasonable amount of assigned outside reading, both of which devices are regarded as valuable adjuncts of the course. These exercises are supplemented by fairly frequent written exercises,—themes, tests, etc.

The chances are about even that the teacher has occasional personal appointments, especially with those students whose work is below par. The chances are that he does not use formal dictation, or printed outlines, or quiz-lists.

Finally, in our 'typical' normal school there is no psychological or educational laboratory, and probably not even a collection of apparatus. If such a collection exists, it is probably confined to a few models, charts, or lantern slides of the nervous system, or to a few 'show-pieces' like the ergograph, the color-mixer, or a simple reaction-time apparatus.

With this glance at the condition of psychology in an average normal school, we may now consider the statistical results of the inquiry as exhibited by the returns from the 100 institutions.

For convenience of discussion, the order of presentation will depart from that embodied in the questionary. Attention is asked (A) to the teachers (their training, opportunities, difficulties and desires), (B) to the place of psychology in the curriculum, (C) to the aim and success attained in teaching psychology, (D) to the content of the course, (E) to the method of conducting it, (F) to the use of experimental or laboratory work, (G) to a number of conclusions and suggestions.

A. THE TEACHERS

1. *Their academic status*

Question 27. *Degrees.* Of the 100 teachers 81 reported collegiate degrees. These include 64 Bachelor's, 47 Master's, and 32 Doctor's degrees, besides 2 Medical and 3 Honorary degrees. Five hold only normal-school diplomas, or state or other certificates.

Question 26. *Institutions at which training in psychology was received.* Aside from 20 normal schools, training in psychology is reported as follows; at Chicago, 19; Columbia, 14; Clark, 13; Harvard, 11; Michigan, 8; Indiana, 5; Pennsylvania, 4; 3 each at California, Jena, Leipzig, New York University, Iowa, Wisconsin, and Yale; 2 each at Berlin, Cornell, Göttingen, Illinois, Minnesota, Stanford, Zürich; 1 each at Adelbert, Albion, Amherst, Bryn Mawr, Columbian, Cumberland, Edinburg, Erskine, Halle, Hamline, Hillsdale, Hobart, Howard, Illinois College, Kansas, Lafayette, Lebanon, Nebraska, Northern University, Oberlin, Pennsylvania State College, Smith, Toronto, Tennessee, Tufts, University of the City of New York, University of Nashville, University of North Carolina, Wellesley, and Yankton College. In many cases the teacher had attended two or more institutions of collegiate rank.

Question 28. *Length of time the teacher has taught psychology.* The replies to the question: "How long have you taught psychology?" are summarized in Table 1, whence it is evident that teaching-experience ranges from one term to 26 years. If we disregard the tendency to give the nearest 5-year multiple,—a tendency which has obviously distorted the frequencies at 5, 10, 20, and possibly at 15 years,—we may determine the average teaching-experience at 8.5 years.

TABLE 1.

Teaching–Experience, in Years, of Normal School Teachers of Psychology.

Years...	1	1	2	3	4	5	6	7	8	9	10	11	12	15	17	18	20	25	26
Number..	3	5	6	5	7	10	3	6	7	1	14	6	7	2	2	4	5	1	1

Question 23. *Number teaching psychology in each school.* But 43 replies were made to this query. Since, presumably, lack of reply indicates that but one teacher is engaged in psychology, we may, by adding 9 replies of 'one only,' conclude that in 65 of the 100 schools, the work in psychology is given by a single teacher. In 18 schools there are 2 teachers of psychology, in 7 schools 3 teachers, in 1 school 4 teachers in 6 there is more than one teacher, but the number is not stated.

Question 22. *Number giving other instruction than in psychology.* In the typical normal school, not only is there but one teacher of psychology, but this teacher is also in charge of instruction in other, often quite unrelated subjects. To be specific, 78 of the 100 teachers give instruction in courses other than psychology. This additional instruction is commonly concerned with some phase of pedagogy or with logic and ethics, as the following figures indicate:

21 History of education.
20 Pedagogy or 'education.'
14 Methods.
12 School administration or school management.
11 Ethics.
10 Logic.
8 Principles of education.
6 Literature.
6 History.
4 English.
4 School law.
2 Child-study.
2 Biology.
2 Physics.
2 Latin.
2 Economics.
2 Philosophy of education.
2 Algebra.
1 Arithmetic.
1 Astronomy.
1 Botany.
1 Chemistry.
1 Critic teaching.

4 Civics.
3 Philosophy.
3 Physiology.
3 Geography.
1 Kindergarten methods.
1 Manual training.
1 School hygiene.
1 Secondary education.
1 Zoölogy.

Question 24. *Original work attempted.* Of the 100 teachers who reported, 22 attempt to do some form of research work, in which 7 of them are aided by seniors or selected pupils. But most of these 22 state that in the normal-school environment it is hard to get the time, energy, or incentive to do such work.[1]

Question 25. *Publications by teacher or pupils.* Twenty-seven normal-school teachers have published articles or books upon psychological topics. In several instances, however, the titles mentioned are those of doctorate theses or other studies prepared by the teacher in the university or at least outside of the normal school. It appears again, therefore, that only in a few exceptional institutions are the conditions (whether of teaching staff or institutional routine) such as to favor creative work.

2. Their difficulties and desires.

Question 31. *Difficulties encountered in teaching psychology.* Difficulties are attributed (*a*) to the pupil. (*b*) to the teacher himself, (*c*) to the inherent complexity of certain topics, and (*d*) to the text-book.

[1]The writer is impressed with the fact that the opportunity apparently open to the normal-school teacher of psychology to conduct experiments on a large scale in regard to the application in classroom teaching of psychological principles, *i. e.*, the opportunity to indulge in experimental pedagogy–is not, for one reason or another, adequately utilized. The model, practice, and observation classes attached to the normal schools would seem to offer a rich field for experimental work of this kind. The most obvious difficulty is, presumably, the lack of properly trained students, and the short time available in the average normal-school course, which puts pressure on the teacher to concentrate his attention upon the work of teaching his pupils the elementary facts of psychology. But there are a limited number of normal schools that aspire to the dignity of "Normal Colleges" and that even grant an A.B. degree. Here, with high-school preparation of all students and a four-year professional course, there should be a place for valuable experimental work.

(*a*) The pupil is said to be too immature (2)[1] or disinclined to think, to be willing to master the text, but not the subject.

(*b*) Only two teachers attribute their difficulties in some measure to themselves. One teacher does not feel sure what ought or what could be taught in psychology in the normal school: another regrets his inability to keep up with the literature of the subject.

(*c*) Special difficulty is reported with heredity, the theory of evolution, the psychology of hearing, the affective processes, the "physiological side," the nervous system, the history of psychology, volition, the hygiene of study, the functional point of view, the tendency to confuse the mental and the physical. Five teachers say that the principal difficulty is that of so adapting psychology that it shall bear on the daily life of the pupils and particularly upon the work of teaching. Two believe that the absence of a laboratory equipment makes their work more difficult.

(*d*) But the text-book is evidently the *bête-noire* of the normal-school teacher, although at the same time, as will be seen later, it is a main reliance in his teaching. The text-book is said to have too few historical sketches, to be too vague, too abstract (5), too difficult (6), too technical in terminology (5). It fails to show the bearing of psychology upon conduct, and fails to make proper use of the student as his own laboratory (3). "The scientific texts are too hard: the easy texts are not scientific." Those at present on the market are declared to be "the pedagogical blunders of great scientists who know enough psychology to write a book, but not enough to understand the mind of the high-school graduate. Again, it is remarked that "the text-book doesn't resolve any difficulties: it makes them."

Question 33. *How assistance could be rendered.* The normal-school teachers think that they could be best helped in their work (*a*) by the preparation and circulation of reports such

[1]Numbers in parentheses following classificatory data in this and other paragraphs refer to the number of reports in which the data appear, but where an item is mentioned but once the number has been frequently omitted.

as the present one, because they "want to know what others are doing" (incidentally, a few desire that this report be sent to their state superintendent of public instruction or to those that prepare the state examinations in psychology), (*b*) by extending suggestions as to the arrangement and conduct of courses in psychology, (*d*) by giving attention to the text-book evil, and (*e*) in miscellaneous ways.

(*b*) The desire for help in the conduct of courses may be evidenced by the following quotations: "Outline for us a laboratory course, giving a list of apparatus, maker, cost, etc." "Outline a course in educational psychology, with experiments and apparatus" (2). "Outline a two-year course in normal-school psychology, recommending texts, and supplying a bibliography." "Outline a 20-weeks course in psychology." "Show us how to teach psychology to 16-year old pupils." "Set standards for this kind of work. Show us the essentials" (3). "Suggest books on the psychology of method," etc.

(*d*) Five teachers beg the Committee to write "a decent text-book." The characteristics of such a text are also specified by several correspondents: it should be "a living, working psychology," "one that is free from scientific slang,"(!) "one that treats of psychology as personal experience," and "a sensible one for beginners."

(*e*) Other suggestions of ways of assistance are: "Develop coöperation between university and normal-school teachers." "Find out from teachers who have graduated from normal schools what work in psychology was actually helpful to them." Some teachers ask for specific information, *e.g.*, "Give me some scientific information about the relation of blood to brain activity." "Tell me if there are any psychological differences between the whites and the colored race."

Question 32. *Changes desired.* The changes that would be made by the teachers of psychology, if they had a free hand, relate in the main either (*a*) to the place of psychology in the curriculum and its relation to other branches, or (*b*) to the content or method of the course.

(*a*) The crying need is that of more time (11). It is also desired to defer psychology until the last year (3), or to divide the work into two courses, an elementary and an advanced (2). The wish is expressed, also, for better articulation with biology (3), physiology (3), pedagogy, school hygiene, methods, and practise teaching.

(*b*) So far as alteration of content or method of presentation is concerned, it is significant that 19 wish to introduce laboratory or experimental work. Seven wish to give more exercises in the observation of children, 4 to "emphasize the genetic side," *e.g.*, by adding a course in child-study, 4 to give more demonstrations in class, 3 to work more by the inductive method. Other desired changes are to emphasize social psychology, philosophy, religion, and mental hygiene, to provide more reference books, to institute more group work, more conferences with students, to give intensive work on a few important topics, to increase the teaching staff, and to provide a vacant period after each recitation for use in discussion, conferences, experiments, etc.

B. THE PLACE OF PSYCHOLOGY IN THE CURRICULUM.

Question 8. *Number of students.* Restricting consideration where more than one course is offered to the introductory or first general course in psychology, we find that 90 institutions report 9,669 students in psychology, an average per class of 107.3 (m.v. 68.6). The largest number is 500, at Ypsilanti, Michigan; the smallest 5, at the J. K. Birch [Private] School, Enfield, N. C.

Question 18. *Meeting in sections or as a whole?* In 71 schools the psychology class meets as a whole; in 14 it meets in 2, 3, 6, 8 or 9 sections. In 6 institutions the class meets occasionally in sections, *e.g.*, for laboratory, or some form of advanced work.

Question 2. *Elementary and educational psychology combined or separate?* The 97 replies show that these courses are combined in 67, separated in 30 institutions.

Question 6. *Elective or required course?* Only 6 schools report elective courses. These courses may be taken after the introductory course in general psychology (including usually educational psychology), and are devoted to such phases as physiological, genetic, or abnormal psychology, or 'mental hygiene.'

Question 6 (*continued*). *Required of what group of students?* It is impossible from the data at hand to draw reliable conclusions upon this point, both because practise evidently differs widely in different institutions, and because the normal-school course varies from 1 to 4 years in length, and the descriptive terms "2d year," "Junior," "Sophomores," etc., have no meaning unless a catalogue of every school is at hand to discover their significance. From appended remarks, however, it is clear that two factors are at work, (1) a desire to postpone psychology as late as possible in order that students may be more mature and experienced, and (2) a desire to introduce it earlier so that it may serve as a basis for the study of method, and for observation-work and practise-teaching. Even when psychology is deferred to the final year of the normal-school course, it precedes or accompanies the courses and work just mentioned. In a few schools a very elementary course is given in the first and a more systematic course in the last year,—a procedure which evidently aims at the accomplishment of both of the desires cited.

Question 7. *Psychology a prerequisite for what courses?* As has been noted, psychology in the normal schools is a required subject and must commonly precede certain other courses. In the 68 replies received to Question 7, these sequent courses are specified as follows: practise-teaching (48), general or special method (29), pedagogy in general (17), history of education (13), principles of education (12), observation work (9), supervision or school management (9), philosophy of education (6), advanced psychology (3), child-study (2), moral philosophy (1).

Question 5. *Length of the course.* The length of each daily exercise in psychology was reported by 83 institutions. From the accompanying distribution (Table 2) it is clear that 40–45 minutes is the typical class period in the normal school. The few cases of 90-minute exercises presumably refer to laboratory or demonstration periods or to exercises given over partly to observations, discussions, etc.

TABLE 2.

Number of Minutes Devoted to Class Periods in Psychology.

Minutes	30	35	40	45	50	55	60	90
Number Institutions	2	1	22	42	7	1	5	3

In 81 reports, the data were sufficient to enable the computation (with a possible error of some 5 per cent) of the total time (in 60-minute hours) given to psychology. The results are: average time, 90 hours (m.v. 34); maximum, 270 hours (3 years, 120 weeks, at the State Normal School, Albany, N. Y.); minimum, 22.5 hours, (10 weeks, three 45-minute periods each, at the State Normal School, Danbury, Conn.). These extremes and the large mean variation of the average indicate that there exists little agreement in normal schools with regard to the time to be devoted to psychology.

C. THE AIM IN TEACHING PSYCHOLOGY AND ITS REALIZATION.

Question 1. *General aim of the course.* The usual answers are: the aim of psychology is to give a general familiarity with the operations and the laws of mental activity (28) in such a manner as to afford a satisfactory basis for the art of teaching (75). Less frequently is the aim considered to be the supplying of general culture (19). The introductory course is often conducted primarily as the basis for further study of advanced psychology (22). In a few instances (3), the aim is frankly stated to be the satisfaction of legal requirements.

Question 29. *Interest displayed by students.* Of 54 replies, distinct interest is reported in 50, interest on the part of a few students in 4 cases. It may be fair to assume that in the 46 institutions that fail to reply, little interest is shown in psychology. When it is displayed, interest is said to be manifested by questions, by participation in discussions and debates (21), by the voluntary assumption of study and reading beyond the class assignments (14), by the application of psychological principles in practise-teaching (9),by general good work and zeal in the classroom (5), by the election of non-required advanced courses (3), by continuing to study psychology after graduation (2), by acknowledging help from psychology in other studies (2), by acknowledging a changed attitude toward books, people, and life in general (2).

Question 30. *How psychology helps students in their work as teachers.* As already noted, a primary aim in normal-school psychology is to afford a basis for the art of teaching. It is evident that as a rule, the teachers of psychology, believe that this aim is attained, though not to the degree that might be wished. We have, unfortunately, no information from the graduates of the schools themselves as to the value they have gained from psychology: according, however, to the school authorities, psychology gives teachers greater sympathy, tolerance, and patience (10), enables them to teach more intelligently, scientifically and skillfully (8), especially because they know how to organize or plan their work, (8) and see the reasons for the devices and rules of method (6), which now become guides instead of rules, so that teaching becomes a profession instead of a trade (2). Psychology develops thoughtfulness (2), an investigating spirit (2), and common-sense (1); it prevents sentimentalism, makes students more critical (1), in that they search for causes and know what proof is (1); it makes them also more observant (2), more confident (1), more tactful (1), more progressive (1). It "gives a safe attitude toward mental processes" (1), and makes a saner disciplinarian (1).[1]

Psychology is calculated also to improve the relation of the teacher to the child, since the teacher who has studied psychology has a better understanding of the child's nature (7), knows what to expect and what not to expect of children (6), and appreciates individual differences in children (6).

[1]In the words of one enthusiastic teacher: "If the great truths presented therein are comprehended, the teacher is enabled to see life as a compact whole, with all its members bound together by a common Creator, common temptations, and a common destiny. He is able to rise above the petty vexatious things of every-day life to the serene heights attained only by those who can see from the beginning to the end the plan of an all-wise Creator working itself out to a glorious triumphant ending!"

Again, psychology renders assistance because it makes clear the real nature of the educative process (2), particularly of the process of learning (1), and shows the teacher how to develop concepts (1), how to develop ideals and motives (1), how to secure independent work by pupils (1), and how to utilize play, imitation, suggestion, and other instinctive tendencies.

By showing the physical conditions of mental life, psychology enables teachers to understand mental hygiene (3). It is said both to "get students away from books," and to "enable them to understand pedagogical literature."

"It induces organization with reference to development of children, school hygiene-nature and value of subject-matter, means of communication, personal and social control "(1).

D. THE CONTENT OF THE COURSE IN PSYCHOLOGY.

Question 20. *Outline of topics presented.* Only 8 outlines were submitted in response to this query. These have been subsumed under Question 21. One institution follows a state syllabus: several institutions shift the contents of the course from year to year: 27 follow a prescribed text-book for the content, and of the 60-odd institutions that remain, the majority may be assumed to follow a text-book, so that the content of the course is best indicated in the list of texts that follows.

Question 9. *Text-book used? If so, what?* (*a*) No text is used in 5 institutions, a single text-book in 34, two texts in 26, three texts in 15, four or more texts in 12 institutions. Many teachers assert emphatically that no one should attempt to teach psychology with but a single text-book, and it is probable that, of the 34 that use only a single text, there is more or less use of other books for collateral reading.

(*b*) *Classification of texts used in normal schools.*

If we divide the texts, as reported, into three groups, we obtain the following distribution: (1) extensively used,—Angell, Halleck, James, Thorndike, Titchener; (2) less extensively used,—Betts, Dexter and Garlick, Gordy, Kirkpatrick Salisbury, Witmer, (3) used only occasionally,—Baker, Baldwin, Binet, Buell, Davis, Deatrick, Dewey, Hall, Hannahs, Harris, Hill, Horne, Huey, Judd, Ladd, Maher, McLellan, Morgan, Putnam, Ribot, Roark, Sanford, Seashore, Stout, Sully, Tracy, White.

It is to be understood, of course, that many institutions that use one of these books for a text make more or less use of others for outside reading, especially of texts on experimental work, such as Judd, Seashore, Sanford, Titchener, Witmer. Use is made also of Baldwin's *Dictionary of Psychology* and of texts in neurology and physiology.

Question 21. *Distribution of time to the several topics within the course.* Because the majority of teachers follow the distribution found in the text-book at hand it is well-nigh impossible to present a satisfactory picture of the distribution of time in normal schools to the several topics that comprise the usual course in psychology. Unless these text-books are collected and examined, the status of the courses in which they are used cannot be reduced to quantitative terms, and the difficulty of this task has seemed out of proportion to the probable value of the results that would be obtained. In some 35 cases, the classification embodied in the questionary has been utilized, and these data have been summarized in Table 3.[1]

Perhaps the most interesting feature of this Table is the divergence that it reveals in the extent of time that different institutions devote to the several topics. This divergence is, of course, due in large part to the divergence already noted in the total amount of time devoted to psychology, yet it seems worth while to accentuate the variability in the allotment of

[1]In explanation of this Table, it should be said that the data were submitted in absolute, in relative, and in descriptive terms. Where the distribution of time was indicated as a fraction of the total time, this has been recalculated by reference to the data supplied under Question 5 so that it might be stated in absolute units. The descriptive terms have been reduced to three groups, (1) "incidental," "brief," or "very little," (2) "some," "occasional," (3) "much," "emphasized," or "special attention."

time by bringing together in another place (Table 4) the minimal and the maximal time devoted to the several topics, in so far as this is revealed by the data of Table 3.[1]

TABLE 3.

Distribution of Time to Various Topics in Psychology, by Number of Institutions.

		Notions of Scientific Method.	Quasi-Philosophical Problems.	Central Nervous System.	Sensation and the Sense-organs.	Attention and Interest.	Affection, Emotion.	Reflexes, Instincts.	Learning, Habit, Educative Processes.	Complex Action.	Association.	Perception, Observation.	Memory.	Imagination.	Conception, Reasoning.	Mental Development, Child-study, etc.	Individual Differences.	Animal Psychology.	Sleep, Dreams, etc.	Hygiene of the Sense-organs, Fatigue.
		a	b	c	d	e	f	g	h	i	j	k	l	m	n	o	p	q	r	s
Lessons	1	3	1	2	1								1				1	3	–1	
	2	6	9	2	1	2		2	2	2	3	2	1	4	2		4	3	4	2
	3	5	5	2	3	3	1	6	1	2	2	3	2	3		3	2	2	3	
	4	2	1	1		1	1	1	2			1	3	3	1				1	1
Weeks	1		2	6	7	7	6	7	4	6	12	9	10	7	2	2	1		4	3
	2		3	8	8	8	13	8	9	9	5	5	7	9	13	1	1	1	1	2
	3		1	1		4	3	1	2		1	3	2		5	4	1	1	1	2
	4			3					3	1		1			3	2				1
	5						2										1	1		
	6					1	2				1									
	9					2	1													
	10															1				
	13															1				
	24															1				
Very little		5	8	4	1	1	3	3		5	3	1	1	1	1	4	4	10	6	
Some		4	4														3	2		6
Much		1	1	3		9	5	3	7	1	4	5	5	2	3	6	6		1	1

[1]This divergence in emphasis of different phases of psychology in the normal school may be further illustrated by the following examples of questions selected from numerous examination papers submitted with the replies to the questionary. An inspection of these questions will convince the reader that the instruction in the schools concerned is sometimes too technical, sometimes too diffuse, and that it often exacts on the part of the pupils ability to answer questions that scarcely conform to a scientific standard.

1. On what does the value of a school exercise depend?
2. Why is he who is no longer a student unfit to lead others?
3. Name 10 standpoints from which one may study the stream of thought.

TABLE 4.

Minimal and Maximal Amounts of Time Devoted to Important Topics in the Course in Psychology.

Topic.	Minimal Time.	Maximal Time.
(a) Scientific methods	1 Lesson	4 Periods
(b) Quasi-philosophical problems	1 Lesson	3 Weeks
(c) Central nervous system	1 Lesson	9 Weeks
(d) Sensation and the sense-organs	1 Lesson	9 Weeks
(e) Attention and interest	2 Lessons	3 Weeks
(f) Affection, feeling, and emotion	3 Lessons	3 Weeks
(g) Reflexes and instincts	2 Lessons	3 Weeks
(h) Learning, habit, educative processes	2 Lessons	6 Weeks
(i) Action and volition	2 Lessons	4 Weeks
(j) Association	2 Lessons	3 Weeks
(k) Perception and observation	2 Lessons	4 Weeks
(l) Memory	1 Lesson	3 Weeks
(m) Imagination	2 Lessons	2 Weeks
(n) Conception, judging, reasoning	2 Lessons	4 Weeks
(o) Mental development, heredity, child-study and adolescence	3 Lessons	24 Weeks
(p) Individual differences	1 Lesson	5 Weeks
(q) Animal psychology	1 Lesson	5 Weeks
(r) Hypnotism, sleep, dreams, etc.	½ Lesson	3 Weeks
(s) Hygiene of the senses, fatigue, etc.	2 Lessons	4 Weeks

4. What must precede the sensation of the color of your paper? The same of the odor?

5. Analyze water in terms of the Atomic theory.

6. Distinguish between sensations and associations.

7. Name 10 differences in pitch.

8. Is an educated man always a good man?

9. Outline imagination as in class.

10. Recall the weight of your books on your arm: contrast and compare the weight of the inner order with that of the outer order.

11. Give in full the principles of a system of education that is the logical outcome of psychology.

12. Name 50 instincts, localizing each loosely in its period.

13. What is right, good, wrong, evil, and what drives us to do right?

14. Define materialism, personal identity, intuition, synthesis, enthymeme.

15. Name 10 recognized psychologists: mention some of their writings and state their general position in the field of psychology.

16. Is space subjective or objective? Give your reasons for our answer.

17 Discuss the validity of the conclusions reached by experimental psychology.

18. Arrange a program of studies for children from three to sixteen that shall be in harmony with genetic psychology.

19. Explain the psychologic basis of "I think, therefore I am."

20. What is meant by intuitive knowledge? Give three classes of intuitions.

E. METHOD OF CONDUCTING THE WORK IN PSYCHOLOGY.

Question 10. *Use of the text-book.* Of 73 replies, 67 asserted that the text-book was the chief feature of the work, *i.e.*, the text supplied the essential information, formed the basis of recitations, discussions, etc. In 6 other schools, the text-book is the main feature of the course, but there are considerable departures into fields not therein presented. In 7 institutions, the work emphatically does not center about a text-book, but consists in a discussion of the experiences of the students and of their observations of the behavior of children. In some of these institutions a text-book is employed, but more than half of the time of the class is occupied with discussions, outside reading, and lectures by the teacher.

As an example of work which is but little confined to a text, I may refer to the extended report submitted by Prof. Frank Manny of Kalamazoo, Michigan, in the course of which he says: "In the introductory courses, the problem in each case is to learn to read the text and similar material in the library with intelligence." A handbook is issued which serves the pupils as an aid in selecting material. "We aim to introduce students to some of the literature and materials with which they need acquaintance for work in classrooms, teachers' meetings, private study, etc." About one-fourth of the time is given to "very exact 'reaction-work,' such as five or ten minutes writing on a prepared or unprepared topic, fairly extended individual recitation with a view to test ability to 'think through' and organize a definite section in its larger relations. This frees the rest of the time for more informal discussion and conference. Informal lectures take probably another fourth of the time. A physician came in and gave demonstrations on the human brain. Some simple experimentation is done along the lines suggested by Thorndike, Judd, Witmer, and Seashore."

Question 15. *Studying in the class.* In one institution nearly all the studying done by the pupils is actually carried on during the periods that the class meets: in 8 institutions the class hour is either often used entirely for studying or a portion of it is regularly so used: in 15 institutions the class period is occasionally used as a study period, *e.g.*, during the first few days or at other times when the work is difficult or during the discussion of the assignment for the following lesson: in the majority of institutions (66), however, all studying is done outside of the meeting period of the class.

Question 16. *Use of lectures.* Of 73 teachers who give lectures in the classroom, 14 confine these lectures to an exposition of the material of the text-book; 25 lecture only on topics not treated in the text; 34 lecture both on the text-book and on outside topics.

Question 19. *Lectures by non-faculty members.* No normal school reports a regular system of public lectures by 'outside' talent. In one school there are frequent, in 36 occasional lectures on psychological matters by visiting authorities.

Question 11. *Devices of method.* The compilation of the replies concerning the plans used to present psychology offers somewhat the same difficulty that was encountered in Question 21 (distribution of time to topics), although the replies are more numerous and the generalizations are correspondingly more reliable.

Table 5 summarizes the results, by number of institutions, in three groups, according as the replies were given in descriptive, absolute, or relative terms. For convenience, the chief points of interest in this Table are here restated in verbal form.

(*a*) *Lectures by the teacher.* The most common proportion of time expended in lectures is one-fifth. Most normal-school teachers of psychology make occasional use of lectures, but it is not usual to devote as much as one-half of the time to them. The single institution in which lectures are given daily is that at Danbury, Connecticut, where but 22 hours in all are given to psychology. Most of the lectures reported as 'occasional' are of an informal nature and restricted to portions of the subject-matter that offer particular difficulty.

(*b*) *Demonstrations by the teacher.* These exercises are, as a rule, used but seldom in the normal-school course. Presumably 5 minutes per day would cover the time thus expended in the average institution. In so far as reported, the demonstrations appear to be confined almost wholly to the anatomy and physiology of the brain or of the special senses, and only infrequently serve to illustrate more strictly psychological principles.[1] It is probably fair to assume that

[1]See also under question 13, in Section F. below.

TABLE 5.

Frequency of Employment of Different Methods of Presenting Psychology

	Lectures.	Demonstrations.	Experiments.	Discussions.	Dictation.	Written Exercises.	Introspective Exercises.	Observation Exercises.	Outside Reading.	Personal Appointments.	Outlines or Quizlists.
	a	b	c	d	e	f	g	h	i	j	k
Total Using	74	50	57	75	31	74	66	65	83	52	25
'Yes'	1	6	26	11	2	21	21	27	34	9	25
Seldom	8	13	18			5	16	8	7	5	
Some	16	4		2	24	5	8	7	14	27	
Much	3	4	1	22		17	11	14	25	8	
1 a Month	1					3				2	
2 a Month	2	1				5					
1 a Week	7	1	1	1		6		1		1	
2 a Week	1	1									
3 a Week						1		1			
Daily	1			5	1		3				
1/20		3	1			2	4	2			
1/12						1	1	2			
1/10	4	2	3			2			3		
1/8		2	2			3	1	1			
1/5	7	2				3		2			
1/4	6	2	4	6	4		1				
1/3	8	1		5							
2/5				1							
1/2	7	1		12							
2/3	1			4							
3/4	1		1	2							
9/10				2							

most of the 50 teachers that did not reply to this point do not use any form of class demonstration.

(*c*) *Experiments by students.* As 43 teachers do not reply to this point, and as 43 of the 57 who do reply report either merely 'yes' or 'seldom,' it is obvious that, unless a special course is given in experimental work, or a special period is set apart for it weekly, the normal-school student of psychology does little or no experimental work.

The following compilation of the subject-matter of these student experiments shows that most instructors do not use a regular laboratory manual, but pick up a few simple experiments from texts like Witmer or Titchener's *Primer of Psychology*, and that the work is confined for the most part to sensation (especially the eye, ear, and skin) and to association, memory, and reaction-time.

SUBJECT-MATTER OF EXPERIMENTS, BY NUMBER OF INSTITUTIONS.

Dealing with sensation and perception		*Dealing with other Topics*	
Sensation (not specified)	4	Attention	1
Tests of the senses	3	Reaction-time	1
Training of the senses	1	Association	5
Visual sensation	3	Memory	9
Optical illusions	3	Imagery	1
Color mixture	1	Habit	1
After-images	1	Fatigue	2
Color Vision	1	Estimation of distances	1
Hearing	3		
Temperature	3		
Esthesiometry	1		
Weber's law	1		
Space perception	2		
Localization of sound	1		

Suggestions for this work are derived from Judd (2), Sanford (1), Scripture (1), Seashore (4), Titchener (5), Witmer (7).

(*d*) *Class discussions.* The practise of holding class discussions of text or lecture is observed in a large proportion of normal schools, and in many cases such discussions occupy a large share, one-half or more, of the daily recitation period. This practise has, of course, both advantages and disadvantages, chief among the latter being the waste of time,—at least the waste of time from the point of view of the exposition of a system of psychology.

(*e*) *Dictation.* It is to be inferred from the data at hand that in 69 of the 100 schools listed, no use is made of formal dictation by the teacher to pupil. In the 24 schools where occasional dictation is reported, the material thus presented appears to be outlines or summaries. Some 5 institutions indulge in what is undoubtedly an undesirable use of this form of instruction. In one school, indeed, the work in psychology consists substantially in the dictation from day to day of the substance of a text-book on psychology.

(*f*) *Written exercises.* The normal schools make much use of written exercises in the form of topic-reviews, themes, tests, answers to questions in the text, or individual reports of various kinds. A frequently reported plan is to have some form of written exercise at the conclusion of each main topic in the course.

(*g*) *Introspective exercises.* A few schools give their pupils training in introspection in connection with experimental work: a few have regular introspective exercises: the larger number make but rare or semi-occasional use of introspective exercises. The statement of one teacher: "We give just enough to get the method" is so typical of many statements that one has the impression that the value of introspective work as a *constant accompaniment* of the course in psychology is not properly recognized by many normal-school teachers. Introspection is looked upon as something to be defined or understood, or even to be tried once and then dropped. It does not appear always to be understood that to teach psychology one must make the student *psychologize.*

(*h*) *Exercises in observation.* This form of work is used with about the same frequency as individual introspective exercises. It consists largely in some form of child-study work, usually in reports upon the children (or upon the work of the teachers) in the practise-school. In perhaps a fifth of the schools this observation-work may be said to be a prominent feature of the instruction in psychology: here the observation-work is extensive in scope; the data are tabulated and often subjected later to class-discussion.

(*i*) *Outside reading in books other than the text-book.* This kind of instruction is in almost universal use. It may take the form of assigned references upon which written reports are required from all students, or of individual reports from different books on different topics, especially if the class be large or the library small. The reading is, I judge, for the most part in text-books, though some schools make a feature of assigning reading in current psychological magazines or in the monographic literature of child-study. In one institution where no regular text is used, all of the work is based upon such reports of outside reading. Many teachers are hampered by a lack of library facilities: there are not enough reference books to 'go round,' and the files of educational and psychoogical magazines are scanty and incomplete. Herein, incidentally, lies another reason for the hesitation of the normal-school psychologist to embark upon research work or to engage in publication.

(*j*) *Personal appointments with students to discuss difficulties.* In about one-half of the normal schools the teacher of psychology has occasional personal meetings with students. As a rule these meetings are voluntary on the part of the student: if otherwise, it is either the student that is likely to fail or the advanced student with a special problem that is summoned to an appointment.

(*k*) *Printed lists of questions for use by students.* Only 25 institutions employ this useful and time-saving device. The lists reported range from occasional dictation or black-board presentation of a few questions to the use of questions printed in text-books or in training-school or state examinations in psychology. Five teachers have printed for local use fairly comprehensive lists of questions, with or without accompanying synopses of lectures.

(*l*) *Other devices.* Methods of making the presentation successful, other than those already discussed, are reported as follows:—dictation of an outline of psychology in the form of a series of questions and answers (3), note-books or outlines to be handed in by the students (2), arrangement of debates on psychological or psycho-educational questions (2), organization of a "psychological club," character study of the pupil's self, 'slip-exercises' about every other lesson, mimeographed directions and references for the conduct of simple experiments, study of charts and models, the making of drawings and charts, blackboard schematic drawings to represent the topics read about, much time devoted to definitions, 10-minute oral summary of a topic by a pupil followed by general discussion of this summary, a weekly report of reading accomplished bearing on the course.[1]

Question 12. *Which device is most valuable?* In 77 replies the following preferences are indicated: class discussions (22), observation work (13), introspective exercises (11), outside reading (7), lectures by the teacher (5), experimental work (4), printed outline of the course (3), and one each for the following: informal 'text-book talks,' individual and group work with children, application of psychology in actual teaching, oral recitations, demonstrations by the teacher, personal appointments, questions by the inductive method, "enlarge (?) drawings of the brain," blackboard diagrams of psychological principles, collection of charts, analysis of concrete instances of conduct, "use of the text through personal exposition."

F. THE USE OF EXPERIMENTAL OR LABORATORY WORK IN THE NORMAL SCHOOL.

Question 3. *Courses in experimental psychology.* Only 4 institutions report a regular course in experimental psychology viz: Duluth, Minn.; Cedar Falls, Iowa; Emporia, Kansas; and Greeley, Col. Duluth, in fact, offers two courses: those at Emporia and Greeley are more on the order of experimental pedagogy than of regular experimental psychology.

In a number of schools circumstances are such that a formal course in experimental work is out of the question, yet a

[1]This device reported from a Michigan school, deserves a short explanation. I quote as follows: "One device gives very satisfactory results: it is called the "Weekly Communication" and is due each Monday. The requirement is simple and does not need to take more than five minutes. It is that the student list any reading he has done during the week that has bearing upon the course he is taking. Many students very soon come to extend this to cover practically all their reading and also to mention lectures, sermons, concerts, observations, etc. In many cases comment is made and questions asked which lead to real communication. Where the matter is too involved for writing, there is a conference at office hours, or at my home, or on Friday afternoon tramps in the case of the boys. The time given to reading these 'communications' is a real pleasure and it always brings to my notice material of value which otherwise would have escaped me."

considerable amount of this work is incorporated in the courses in general or educational psychology.

Question 4. *The laboratory equipment.*

(*a*) *Is there a psychological laboratory?* Five of 100 schools reply in the affirmative, but only 3 (Los Angeles, Greeley, and the Brooklyn Training School) seem to have an equipment worthy of the name.

(*b*) *Any collection of psychological apparatus.* Although 33 schools report collections of apparatus (not large enough or sufficently organized to term a laboratory), these collections are for the most part but scanty and are not strictly psychological in character, but comprised of models, charts, and lantern slides illustrative of the central nervous system or of the sense-organs.

(*c*) *Its value.* From 26 replies, it is found that the average value of the laboratory or collection of material is $215, the maximal value $1000 (Brooklyn Training School), the minimal value about $25. The laboratory at Greeley is appraised at $500, that at Los Angeles at $300.

(*d*) *When established or collected.* The oldest 'laboratory' dates from 1893 (purchase of $500 worth of apparatus by the Keystone Normal School, Kutztown, Pa.): all the others date since 1903. Three institutions are starting laboratories during the present year (1908–9). It may be recalled in this connection that a large number of teachers desire to institute laboratory work in the normal schools as soon as local conditions will permit (Question 32).

(*e*) *Yearly appropriation.* Only 5 replies were received: three stated that appropriations could be secured to meet the teacher's demands from time to time. The Albany State Normal School will soon have a yearly appropriation, though the amount is not yet known. The Central State Normal School, Mt. Pleasant, Michigan, is the only institution in our list of 100 that enjoys a yearly appropriation ($250) for the equipment of a psychological or psycho-educational laboratory.

(*f*) *Housing of the collection of apparatus.* From 15 answers, we learn that 7 schools have a separate room given over to work in psychology, while in the remaining 8 the psychological apparatus is housed in rooms devoted to other laboratory work, usually to physics or biology. In a number of schools the modest equipment of charts or brain models is tucked away somewhere in a recitation-room, while several teachers regret that what little material they possess is not available for want of room.

Question 13. *Demonstrations of an experimental character given in class.* The frequency of the use of demonstrations is shown in the following list:

Reaction-time	6	Use of kymograph	1
Memory	5	Field of vision	1
Tests of association	4	Dermal sensation	1
Central nervous system	4	Sense training	1
Optical illusions	3	Color sense tests	1
Fatigue	3	Tests of senses	1
Color mixture	2	Tests of discrimination	1
Range of attention	2	Motor sense	1
Duration of attention	2	Weber's law	1
Auditory localization	2	Visual perception	1
Perception	2	Reflex action	1
Apperception	2	Imagination	1
Feeling-tone	2	Thought processes	1

The list, it will be noted, bears a close resemblance to that of the experiments performed by students (Question 11, c).

Question 14. *Apparatus deemed especially useful for demonstrations.* The order here naturally reflects in large measure the preferences of the preceding list.

Reaction-time apparatus[1]	8	Colored papers	1
Color mixer	6	Electric motor	1
Ergograph	4	Esthesiometer	1
Charts	4	Galton whistle	1
Tuning forks	4	Ophthalmotrope	1
Brain models	4	Prisms	1
Kymograph	3	Sphygomgraph	1
Pseudoptics	3	Spirometer	1
Set of lantern slides	2	Stereoscope	1
Drop-screen apparatus	2	Resonators	1
Test-weights	2	'Tapper'	1
Tambours	2	'Time of movement apparatus'	1
Monochord	2	'Test of attention'	1
Autoharp	1	Witmer's test material	1
Audiometer	1		

[1] Usually Sanford's Vernier chronoscope, although one school boasts a Hipp.

G. CONCLUSIONS AND SUGGESTIONS.

1. The normal school as an institution differs in so many respects (requirements for entrance, age of pupils, length of course, professional aim, etc.) from the college or university that the teaching of psychology in the one differs, and in all probability must differ, from its teaching in the other. It follows that what proves to be best for the college course in psychology is not necessarily best for the normal-school course in psychology.[1]

2. It would undoubtedly be advantageous to arrange *conferences* among those who are interested in normal-school psychology, for the interchange of views and for the discussion of the problems suggested by this Report. I should, therefore, recommend that this Association appoint a *Committee* to draw up an advisory syllabus of subject-matter, to supply lists of references, to outline suitable experiments and demonstrations, and, in general, to standardize, so far as may prove feasible, the normal-school course in psychology.

3. The teacher of normal-school psychology should have received at least three years of special *graduate training* in one or more universities. This training should be such as is required for the doctor's degree, and should include a thorough acquaintance with laboratory methods. The study of the science of education should have received a degree of attention at least equal to that required for a minor subject for the doctorate, and the teacher should have paid special regard to the points of contact between these two subjects.

4. It is desirable that the normal-school teacher of psychology confine his work to that field. If circumstances compel him to *teach other subjects*, it is best that these subjects be those closely correlated with psychology in the work of the institution, viz: general and special methods, school hygiene, child-study, the principles of education, observation and practise-teaching. These are, in fact, the combina-

[1] The writer keenly appreciates these differences, and has had no personal experience in normal-school teaching. Hence the propositions that follow are offered with no thought of authoritativeness, but solely with the idea that they may serve as a provisional basis for discussion.

tions most commonly found. Biology and physiology, and perhaps to a less extent, ethics and logic, if these are taught, may also be regarded as natural concurrent lines of work in the small school with few teachers.

5. Given the preparation described in Proposition 3, the normal-school teacher of psychology should have his work so arranged that he secures the time and the energy required for productive *original work.* This point is urged from the conviction that, although the normal school is essentially a training school and is conducted for the immediate benefit of its students, nevertheless, many capable teachers of psychology can undertake original work with consequent increase of their teaching efficiency and with advantage to the cause of learning, and from the further conviction that, if normal-school teaching is to offer nothing but routine class work, the better class of college and university graduates will never look to these institutions for a permanent career.

6. The teaching of psychology in the normal school unquestionably presents *difficulties* that are greater than those met with in the college. This condition follows (1) from the immaturity and meager general information of the average student, (2) from the short time available for the entire course, and (3) from the constant pressure to make everything simple and immediately practical. To meet the first difficulty the normal school should require a four-year high-school course as a prerequisite for admission: to meet the second, it should rearrange or extend its own curriculum: the third would be partially avoided by these two modifications; it may be further met by securing teachers who know enough psychology to adapt it to the situation. Just now, in not a few normal schools, lack of preparation of the teacher is probably as evident as lack of preparation of the student.

7. In schools offering more than a two-year course, it would probably be found advantageous to give to first-year students a brief, elementary, and *introductory course in psychology* designed for general orientation, and to give one or more advanced, systematic courses to the same students in the last year.

8. In the normal school it is not necessary, and probably on the whole it is not desirable, to separate *general and educational psychology.*

9. There is no need in the normal school for courses, either required or elective, in *physiological* or in *abnormal psychology.*

10. Psychology should be taken up either before, or together with, courses in the *principles of education*, child-study, and exercises in observation or practise-teaching, and the study of special methods of teaching the several studies. A course in 'general method' would appear to be quite unnecessary if the educational psychology has been properly taught.

11. The average *time given to psychology* in normal schools, 90 actual hours, is sufficient, if properly utilized, for the presentation of a sound course in psychology, general and educational. If elective or advanced courses are to be introduced, more than 90 hours are needed.

12. Classes numbering more than 50 students ought to meet in two or more *sections*, and no one of these should exceed 30 to 35 students, if the best work is to be accomplished.

13. The *general aim* or purpose of normal-school psychology is phrased in much the same way by the majority of teachers, who seek to impart such an acquaintance with mental operations as shall contribute to the success of the graduate in his professional work. But, as a matter of fact, this statement of the aim is not definite enough to determine the actual content and method of presentation of the course. The aim as thus stated might, for example, be interpreted to mean that a number of isolated facts or principles should be selected from psychology and applied to the art of teaching; or, to mean that examples of educational procedure should be analyzed in the effort to work back to the psychological principles involved in them; or, again, to mean that the course should be conducted in such a way as to develop a 'point of view' in the student's mind. For myself, I am convinced that the first aim must be to familiarize the student with his own mental processes: only when he has thus learned to 'psychologize' can he be taught to see the application of

psychology in the classroom. And I am convinced, furthermore, that the preliminary appreciation of the 'lawful' character of mind which the first aim implies can be gained best through the presentation of psychological principles in a systematic manner. The first aim is, therefore, to secure real assimilation of a system of psychology: the second aim is to develop skill in observing and in interpreting the mental phenomena of daily life in terms of this assimilated knowledge.

14. The present report sheds little light upon the *success achieved* in normal-school psychology, save that it reveals a consensus of opinion on the part of the teachers of psychology that the general aim just mentioned is attained. An investigating committee might conceivably find it worth while to collect evidence from normal-school graduates in the field as to the manner and degree in which psychology has benefited them.

15. In studying the *distribution of time* to the subject-matter of psychology, we find that there is scarcely any topic or phase of psychology that does not seem to receive relatively too much attention in some normal schools and too little in others. But it is probably more nearly true of psychology than of any other normal-school subject that no two teachers or text-book writers would agree upon the distribution of time and emphasis. The writer's own judgment would be somewhat as follows: (1) that special consideration should be given to instinct, attention and interest, habit, the process of learning, associative and organizing activities, including memory, concept-forming, judging (apperception), (2) that a moderate amount of consideration should be given to sensation and the sense-organs, the general organization and operation of the nervous system, the psychology of action, and the psychology of individual differences (with reference to the inheritance of capacities), and (3) that only occasional or brief reference should be made to philosophical problems, affection, feeling, emotion, reflex action, space-perception, animal psychology, sleep, dreams, hypnotism, fatigue, and mental hygiene.

But any such advisory list, unsupported by reasons, must of necessity appear personal and arbitrary. Again, it is to be noted that the very idea of classifying and 'pigeon-holing' mental experience is repugnant to some teachers, who, either because the notion of a 'system' implies for them something formal, 'cut-and-dried,' and arbitrary, or because mental life impresses them as being essentially unitary, prefer to study behavior in wholes, directly, and in concrete situations, or at least to avoid what they term the *disjecta membra* (sensation, imagination, memory, etc.) of the psychological texts.

I have already expressed my conviction that the use of a system is essential in the teaching of psychology, if the best results are to be secured. In support of this position, it may be said: (1) that practically every adequate text-book is cast in the form, and uses the terminology of a system; (2) that it is impossible for a teacher who knows psychology not to think this psychology in terms of a system; (3) that a system is of the highest value to the student in enabling him to comprehend and especially to relate and organize his observations and information; (4) that an intelligent student will inevitably organize his information into some system, and this system will almost inevitably be inadequate and incorrect if he is left to his own devices; (5) that a system serves to reveal the 'gaps' in our knowledge; it is worth while 'knowing what we don't know;' (6) that the special objection to system in psychology (that the several classificatory terms are interpreted by the student as entities and as isolated elements of mind, so that his whole notion of mental life is distorted and erroneous), is an objection that must, and may be met by careful instruction on the part of the teacher. It is quite possible, for instance, to avoid the "maelstrom of faculty psychology," as one writer phrases it, if memory, imagination, perception, etc., are treated as typical ways in which mind may be found at work, and if the inter-relations of these and other *types of activity* are frequently and clearly set forth,—if, for example, 'memory' is shown to be a name for a type of mental attitude or activity that is already familiar to the student through his study of organic plasticity, retention, habit, associative tendencies, 'centrally-excited sensations,' etc.

16. Every normal school should offer a specific *course in school hygiene.* The testing of the senses for practical purposes, the hygiene of study, fatigue, and allied topics should be placed in this course.

17. While, for normal-school use, some future *text-book* may improve upon those now in the market, many teachers apparently fail to select the best texts now available, or to use them with skill when they are selected. Again, it may as well be admitted that, for some students, psychology is intrinsically difficult, and that psychology is necessarily more com-

plex than other normal-school subjects, so that any text-book that treats the subject adequately will be difficult for these students.

18. Less reliance should be placed upon the text-book and more upon the teacher's expository talks or *lectures* than is at present the custom in the typical normal school. This procedure will render the text-book less of a stumbling-block than it seems now to be, and will enable the students to make more rapid progress: it will thus save some of the time that many teachers find insufficient.

19. It should be possible in any ordinary institution to arrange that *all studying be done outside the classroom.*

20. The normal-school teacher should enrich his course by the introduction of more *demonstration-experiments*, especially demonstrations of the more strictly psychological principles. The Committee contemplated in Proposition 2 might profitably collect or prepare a list of recommended demonstrations.

21. *Class-discussions* should be carefully directed, and probably less freely employed than is now the custom. These exercises are not an end in themselves, but a means of teaching psychology. To stimulate the active interest of immature students and to detect and avoid misunderstandings of the text-book, they have unquestioned value, but, in pedagogy as elsewhere, there can be "too much of a good thing." Who has not seen 'class-discussions' degenerate into aimless anecdote or blind dispute over topics that might have been presented by the teacher in a few minutes of clean-cut exposition, to the gain of the pupils in intelligent grasp of the points at issue? Normal-school teachers who find the time too short to teach psychology may well consider whether a portion of the time that they now devote to class-discussion might not be employed more profitably in other and more direct methods of instruction.[1]

[1] To take an extreme illustration, I heard recently of a young teacher who was so impressed with the virtues of the inductive method that he used this mode of procedure almost exclusively in his class in psychology, with the result that at the end of the term the class had completed the survey of the field of sensation. This class even held, 'discussions' of the structure of the central nervous system.

22. *Formal dictation* is out of place in normal-school psychology, save for occasional and special purposes, *e.g.*, the dictation of a few important definitions, of summaries, etc.

22. Exercises demanding *introspection* are of fundamental value in any course in general or educational psychology. The normal-school teacher, like the college instructor, must lead his pupils to 'psychologize,' to the end that the mental operations of which psychology treats shall be realities and not mere empty verbal assertions.[1] The formulation of a series of introspective exercises should receive careful and intelligent consideration by the teacher. Suggestions are to be found in many texts . The Committee already mentioned might again be of assistance in this connection.

24. *Observation-work* is of special value in making the operation of the salient principles of educational psychology visible to the normal-school student. If the work be properly articulated, courses in psychology and in special method ought to play into one another's hands through the medium of observation and practise-teaching. If observation-work can be arranged concomitant with the work in psychology, so much the better, but, even so, the value of the observation will be largely lost if it is not both systematized and supervised by the teacher of psychology: the novice in psychology fails to see psychology in application until he is told where to look.[2]

25. The assignment of *outside reading* is another device of value; but this, too, demands careful supervision. The difficulties are evident: students dislike to read the same thing in several different books; still more do they dislike to read apparently conflicting statements or to encounter unfamiliar terminology. If written reports are demanded, these take

[1] It is the writer's experience that it is always the student who cannot, or will not get the introspective habit, who falls back upon the verbal reproduction of text or lecture-material as the real content of the course, and that it is nearly always this student who finds psychology "too abstract," and who runs perilously near, if not over, the danger-mark in the final examination.

[2] The writer has found, for example, that the use of a printed "Guide to Observation" very greatly increases the value to college students of the 20-hours of high-school observation required for the New York State College Graduate Certificate.

much time both from student and teacher: if they are not demanded, the reading is likely to be hurried and thus to be of little permanent value. Again, if the class be large and the assignments numerous, the library is taxed to supply numerous copies of certain books and may thus be unable to purchase others that are much needed. Many students, in our experience, have almost literally to be taught to read intelligently. Yet, as the reports show, outside reading is, on the whole, a valued and a valuable adjunct: it helps particularly to overcome the limitations felt to inhere in the 'single text-book' plan.

26. If lectures are given, as we think they should be, they may be profitably accompanied by a printed or mimeographed *outline or syllabus*, This plan helps the student to follow the lectures (or to read the texts) intelligently, gives him a general perspective of the course, and compels the instructor to arrange his material in orderly fashion.

27. Whether or not lectures are given, the assimilation of the work in psychology will be distinctly facilitated by the use of *lists of questions*, preferably printed andsupplied to each student. These questions must, in the main, avoid a form that admits of answer by direct reference to the text-book, and should seek, rather, to develop introspection, observation and thought: in short, they should test the student's ability to apply his psychology.[1]

28. The *oral recitation* of the examining type (quiz on the text) is probably a necessary feature of normal-school work, but the teacher should not employ it further than conditions demand. Like class discussions, it may be made a fetich. It is a time-consuming operation, more needed in instruction on the public-school than on the professional level. The recitation when used for development-work has, of course, greater value, expecially in the hands of a skillful teacher.

[1] If I may be permitted once more to refer to my personal experience, I may state that I have found a printed list of questions, when properly classified and supplied with references to a number of texts, to be of great assistance to college students in checking up their progress in psychology. Such a quiz-list is equally useful for review or for advanced assignment: in either case it guides the student in his study, shows him what he should know, and compels his active thought and attention.

29. In general, the normal-school teacher, owing to the conditions under which he works, must search for, and use as skillfully as he may, all the *devices of presentation* that can further his aims. Notebooks, personal appointments, debates, psychological clubs, charts, diagrams, etc., are schemes that will occur to all teachers. It would be absurd to attempt to prescribe in detail which of these aids should be used, andto what extent, since it is a matter of common observation that every teacher works best by the methods that he himself devises, or in which he himself most thoroughly believes, and that a skillful teacher often achieves his success by a method that a poor teacher employs in vain.

30. The problem of the *introduction of experimental psychology*, or of laboratory courses, is the most perplexing one that confronts the normal-school teacher. Various reasons may be adduced for the introduction of such courses: the seven that follow are, of course, not exhaustive or mutually exclusive: (1) to illuminate and illustrate the subject-matter of the general course in psychology, (2) to add a certain attractiveness to the work in psychology, (3) to make clearer to students the methods by which modern psychology has been elaborated, (4) to give training in scientific procedure and acquaintance with the spirit and method of experimental investigation in science, (5) to train a limited number of advanced students to participate in minor pieces of research work in the institution, (6) to prepare students to appreciate and to participate in experimental pedagogy (as distinct from experimental psychology), (7) to train students in making examinations and tests of children in the classroom (particularly various forms of mental and physical tests, diagnostic of retardation, sensory handicap, etc.)

We have seen that, as matters stand, experimental psychology is not a feature of the typical normal-school curriculum, but that a considerable number of teachers wish to incorporate such work. It does not appear, however, that serious attention has been paid by many of these teachers to the reasons for this introduction, since the experiments that are already in use appear to have been selected, not so much because they bear upon educational psychology, as because they are stock experiments in the psychological laboratory. I yield to no one in my love for research work in psychology, in my belief that a drill-course in experimental psychology has large possibilities of culture-value, or in my conviction that the experimental method must be applied to educational problems before these can be solved, but I do not on this account believe that every kind of experimental work in psychology that is given in college classes can be offered with equal success or justification in the normal school. I see no reason, for example, why every prospective teacher should work out the constants for Weber's Law, or investigate the localization of sound, or try to measure fatigue on the ergograph, although these are perfectly legitimate and regular features of experimentation in the college laboratory.

For these reasons it seems to me that the present experimental work in the normal schools shows faulty perspective. Take, for example, two of the most used demonstration-experiments—the ergograph and the reaction-time test. These have the merit of being 'showy,' but it would be hard to select another pair of experiments that would be more complex and 'tricky' in their real meaning and interpretation. On the other hand, the central feature of educational psychology,—assuredly the process of learning—is not mentioned specifically in the list of demonstrations, although it may be covered in part by experiments in memory or apperception. Again, the demonstration of reliable tests of vision and hearing —tests which, in the absence of special courses in hygiene, demand treatment as a phase of 'mental hygiene' in the normal-school course—is mentioned in the list of but a single institution.

I contend (1) that the teacher of psychology should be drilled in laboratory methods and practice, and that he should endeavor to keep himself informed of the progress of experimental psychology at large, (2) that a selected number of experiments, mainly illustrative of the more important principles of mental elaboration, should be presented to students in the general course in psychology at a special period and in a special room set apart for the purpose, (3) that in institutions offering more than a two-year course, a more systematic course in experimental work including, the solution of simple psycho-educational problems, should be given as an elective to advanced students, and (4) that those students who display exceptional ability in experimental work should be encouraged to seek further instruction in departments of psychology and education in the university.

31. In the absence of any exercise-book or *manual* specially prepared to guide experimental work in normal schools, the teacher must adopt an eclectic plan. The laboratory and library should, accordingly, be supplied with copies of all the texts in experimental psychology. That normal-school teachers would welcome suggestions as to the best selection of experimental problems has already been pointed out.

32. The *equipment* of the normal-school laboratory for the prosecution of such work as has just been proposed need not of necessity be expensive. For an original outlay of from $200 to $750 the beginning of an experimental course can be secured: for maintenance and further extension a yearly appropriation of $50 to $100 would, perhaps, suffice.[1]

[1] The practical working out of the principles of general laboratory economy, like the successful application of methods of instruction, hinges in the last resort upon the teacher. A skilled psychologist can give an instructive demonstration with such homely materials as colored paper, strings, scissors, and cardboard, when the neophyte would be helpless without his kymograph, his electric motor, and his other "brass-ware." This is not to be construed, however, to discourage elaborate equipment, or to applaud a policy of parsimony on the part of normal-school authorities. The skilled psychologist welcomes a good color-mixer, too.

If advanced or original work is attempted, then, of course, instruments of precision become imperative. In working up an educational laboratory for research and demonstration-work at Cornell University, approximately $2000 has been invested during the past eight years, while the yearly maintenance appropriation averages about $100. An inventory of this equipment will be published in the *Journal of Educational Psychology* within a few months in the hope that it may offer some suggestions to those who are developing experimental work in the normal school.

THE TEACHING OF ELEMENTARY PSYCHOLOGY IN COLLEGES SUPPOSED TO HAVE NO LABORATORY.

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The first part of the report herewith submitted summarizes the replies to a questionary sent out to 80 colleges and universities. Replies complete enough for use were received from 47 institutions.[1] It appears from these statements that the elementary course as given in the average college is usually a required course, more often running less than a year, and often, though not always, meeting in large divisions. The course tends to be taken mainly by juniors but in considerable number also by seniors on the one hand and by sophomores on the other. It is conducted partly by lectures and partly by discussions or recitations. The students make use of a text-book but their study of it is supplemented in various ways.

The replies to the questionary are summarized in more detail in the statements which follow; and these are condensed from verified tables compiled by Professor E. A. McC. Gamble.[2] The replies concern:

[1] The list is the following: Amherst, Bates, Bethany, Boston, Bowdoin, Brenau, Butler, Colgate, Colorado College, Columbia College (South Carolina), Dalhousie, Des Moines, Dickinson, Elmira, Fayette (Upper Iowa), Georgia, Georgetown, Grinnell, Haverford, Knox, Maine, Marietta, Mercer, Middlebury, Morningside, College of City of New York, New York Normal College, Oberlin, Ohio University, Penn. State, Randolph-Macon, Ripon, Rochester, Rutgers, Simmons, Swarthmore, Tennessee, Texas, Toronto, Trinity, Tufts, Union, Vermont, Virginia, Wabash, Wells, West Virginia.

[2] To Dr. Gamble and to Miss. S. J. Woodward, who assisted her, my warm thanks are due.

I. The Academic Status of the Course (47 cases).

The course is

(*a*) Required, absolutely: Cases 30
Elective absolutely: Cases 11
Part of a group in which a given number of hours must be taken: Cases.. 5
Prerequisite to pedagogy (which is largely elected): Cases .. 1

(*b*) Prerequisite to other courses in psychology: Cases 23
Not a prerequisite to such courses: Cases.................. 6
No other courses (or no answer): Cases.................... 18

(*c*) Prerequisite to all or any courses in philosophy: Cases 29
Prerequisites to all or any courses in education: Cases.............20
Prerequisite to all or any courses in sociology: Cases...............2
Not a prerequisite: Cases.......8
No answer: Cases 5
} Separate Cases 34

II. The Length of the Course.

	ONE TERM			ONE SEMESTER		TWO TERMS				ONE YEAR				ONE-QUARTER	NO DEFINITE ANSWER	TOTAL
HOURS PER WEEK	3	4	5	3	4	2	3	4	5	2	3	6	?	?		
Cases	3	2	2	14	5	1	3	2	1	4	6	1	1	1	1	47

III. The College Rank of the Students in the Course (47 Cases).

The course is open

To seniors only: Cases 4
To juniors, or to seniors and juniors: Cases 23
To seniors, juniors and sophomores: Cases 10
To sophomores, or to sophomores and freshmen: Cases.......... 6
Without restriction: Cases 1
No definite answer: Cases 3

IV. The Numbers in the Course.

The course varies in number from less than 10 (in 1 case only) to 201–300 (in 2 cases). In only 4 cases (out of 28) in which courses number 60 or less are the classes broken into divisions. In 1 case, a course numbering 200–300 is undivided. Of 12 courses numbering more than 60, 5 are divided into 5 divisions each, 4 into 2 divisions each: while one class of 201–300 meets in 8 groups.

V. The Rank and Work of the Teacher (47 Cases).

The course is taught

By the *president* (who teaches philosophy also): Cases........... 1
By a *professor*. teaching
  Psychology only: Cases 1
  Philosophy also: Cases 15
  Education also: Cases 1
  Both philosophy and education also: Cases.................. 11
  Miscellaneous subjects: Cases 2
(No answer concerning other subjects) 2
By an *instructor* teaching
  Philosophy also: Cases 2
  Education also: Cases 2
By one man with assistants, all teaching
  Psychology only: Cases 1
  Philosophy also: Cases 2
Other cases (and question unanswered): Cases 5

The statements which follow are contrasted with those which precede in that they concern not so much the academic status, student-rank, and numbers but the avowed aims and methods of the elementary course in psychology. Part II of this paper takes special account of these results.

VI. The Emphasis of the Course (40 Cases. No reply in 7 Cases).

EMPHASIS ON	PREDOMINANT AND GREAT	MODERATE	LITTLE OR NONE
Introspection	20	17	3
Physiology	11	18	11
Biology	3	21	16
Practical Applications of psychology	12	18	10
Philosophy	2	15	23

VII. The General Method of the Course (47 Cases).

(*a*) Predominantly by lectures: Cases 8
  Predominantly by class exercise: Cases 24
  A fairly even mixture of the two methods: Cases............ 10
  No definite answer: Cases 5

(*b*) Recitation required (31 replies).
  To a considerable extent: Cases.......................... 20
  To some extent only: Cases 11

VIII. The Method of Introducing a Subject (47 Cases).

By assignment of reading in textbook: Cases	23
By lecture: Cases	8
By questions demanding introspection: Cases	5
No definite answer: Cases	11

IX. Details of Method (Cases 47, not exclusive).

A textbook is used: Cases	40
Outlines are used: Cases	23
Review questions are used: Cases	19
Collateral reading is assigned: Cases	28
(In 10 of these cases the extent of the reading is "considerable.")	

X. The Use of Experiment (Cases 47).

(*a*) Real laboratory work	7
(In 3 of these cases the laboratory work is "important;" in 2 others it is supplemented by out-of class experimenting.)	
(*d*) Directed experiments outside the laboratory: Cases	18
(In 9 of these cases in the experimenting is "important.")	
No experimental work (so far as indicated): Cases	22
(*c*) Use by the lecturer of	
Demonstration experiments: Cases	16
Physiological material: Cases	17

If we suppose that an inquiry similar to this had been conducted twenty-five years ago and if we compare its hypothetical results with our own figures, the outlook will be mainly encouraging. The course in psychology has been pushed backward from the senior year so that a student can apply his psychology in his further college study. The course is no longer taught by the college president in the intervals of administrative duty, nor as a secondary occupation by the teacher of radically different subjects; and psychology has been freed from its entangling alliance with philosophy. On all sides, also, we find evidences ofvigorous and individual teaching.

Our results are however, of chief importance in so far as they make clearer and more vivid our conception of the true aim and of the right methods of the elementary course in psychology. In the paragraphs which follow I have ventured to formulate the results of my own experience and observation, noting at various points the relation of these suggestions to the actual procedure already described. My recommendations have reference to the general course in psychology regarded as introductory not only (and not chiefly) to more advanced courses in psychology, but to courses in philosophy, in education, and in other subjects. In but one respect, I believe, may such a course differ from that elementary course which is planned to introduce the student to further psychology: the more general introduction-course may run through a semester rather than through a year. Without doubt the full year course treats the subject more adequately. Yet precisely because of the fundamental nature of psychology, it forms an important member of every group of studies, literary as well as scientific or philosophical. And in many institutions psychology can hold this central position only if it be offered as a semester course. In the opinion of the writer, such a course can be given without prejudice to scholarship by a teacher who keeps pace with his science, who distinguishes essential from accidental, who systematizes his material, and who never lowers his standards of accuracy.

With this introduction I offer four more or less obvious recommendations:

1. Psychology is psychology whatever the use to be made of it. *First courses in psychology should therefore be essentially the same in content and in method*, whether they introduce the student to advanced work in psychology or to the different problems of pedagogy, of ethics, or of metaphysics. the immediate purpose of every course in psychology is to make the student expert in the study of himself: to lead him to isolate, to analyze, to classify, and (in the scientific, not in the metaphysical sense) to explain his own perceiving, remembering, thinking, feeling, and willing. In the effort to classify and to explain, the student will of course attack the relevant facts of sense physiology and of bodily behavior He will study these however as conditions and accompaniments of consciousness. Psychology is nothing less than such a study of selves—and primarily of one's own self—in relation to the environment, personal and impersonal; and, conversely, any-

thing more than this though it may be related to psychology, is not psychology. If we are in earnest in the belief that psychology is an important, indeed, an essential introduction to the disciplines already named, we must mean by the term—psychology, and not a conglomerate of which a dilute psychology is one component only. Whatever is necessary to the study of psychology for itself is necessary, therefore, to the first course in psychology as introductory to other subjects. As has appeared, this does not mean, that the two courses need be equally long and equally detailed. In particular, laboratory experiments will probably, for reasons of time and convenience, be barred out of the general course. Yet it has, I think, been shown that individual experiment as well as demonstration may advantageously be introduced. And, on the negative side, the utmost pains should be taken not to encourage applications of psychology at the expense of the psychology to be applied. The bearing of psychology on practical problems of every day living and of pedagogy may most advantageously be emphasized by the instructor; but the applications should follow upon analytic study. Not only the purposes of scholarship but the practical aims themselves are thwarted by the tendency to form conclusions for the sake of applying them.

The last paragraph has made reference to the relation of psychology to ethics and to pedagogy. I wish to say a special word of its relation to philosophy. In my opinion the teacher of philosophy should connect the study, constantly, with psychology; whereas the teacher of psychology should exclude all discussion of metaphysical problems insisting on the purely scientific study of consciousness. He should bar out discussions of materialism, free will, and the like, pointing out that psychology is compatible with any one of the metaphysical solutions to these problems. Only by such a differentiation of the science of psychology from philosophy can we rightly study the former; and only psychology rightly studied can be of real aid to philosophy. An indirect support to this view may be found in the answer to the questions on the "emphasis of the first-year course."[1] Of the 40 instructors who answer this question, 20 lay 'predominant' or 'great' stress on introspection and only 3 set 'little or no' store by it; whereas 2 lay 'predominant' and 10 'great' stress on the applications of psychology, and 10 pay 'little or no' attention to the applications. Only two instructors emphasize strongly the relation of psychology to philosophy as against 14 who lay great stress on physiological explanation and biological relation. In this connection it may be noted that the courses of these different colleges concern themselves mainly with the problems of general individual psychology. Only 5 devote 'considerable time' to comparative psychology; and only 2 to abnormal psychology.

2. The second group of my conclusions is of a more general nature. *The class in psychology should not*, it seems to me, *be conducted by means of 'recitations,' or 'quizzes,' and the student should more often take part in discussion than listen to lecture.* I think there can be little doubt that the negative part of this statement is sound. A teacher has not the right to spend any considerable part of the time of a class in finding out by oral questions—"how does your author define perception?" or "what are the laws of color mixture?"—whether or not the student has done the work assigned to him. The good student does not need the questions and is bored by the stumbling replies which he hears; and even the poor student does not get what he needs, which is either instruction *a deux*, or else a corrected written recitation.[2] One is often reminded by the conventional oral recitation of the little boy's description of his first day at school: "An old woman asked me how to spell 'cat,' and I told her." Not in this futile way should the instructor squander the short hours spent with his students. The purpose of these hours is two-fold: first, to give to the students such necessary information as they cannot gain, or cannot so expediently gain, in some other way; second, and most important, to incite them to 'psychologize' for them-

[1] Cf. Statement VI., (p. 43 above).

[2] Of 39 instructors who reply on this point to the questionary, 35 make use of this indispensable method.

selves. The first of these purposes is best gained by the lecture, the second by guided discussion. 'Guided discussion' does not mean a reversal of the recitation-process—an hour in which students ask questions in any order, and of any degree of relevancy and seriousness, which the instructor answers. On the contrary, the instructor initiates and leads the discussion: he chooses its subject, maps out its field, pulls it back when it threatens to transgress its bounds and, from time to time, summarizes its results. This he does, however, with the least possible show of his hand. He puts his question and leaves it to the student interested to answer him; he restates the bungling answer and the confused question; he leaves one student to answer the difficulties of another. In a word, he takes advantage of every suggestion, he stimulates and trains his students by intelligent question and swift reasoning, he subordinates scattered conclusions to the advancement of the discussion as a whole.

The advantage of the discussion over the lecture is, thus, that it fosters in the student the active attitude of the thinker in place of the passive attitude of the listener. For this reason, in the opinion of the writer, the lecture should be used mainly as introduction and as summing up of a subject, not as chief method. A study of the answers to the questionary seems to confirm my estimate of the lecture as subsidiary method, for less than one-fifth of those who reply to this question (8 of 42) make predominant use of the lecture.[1] The answers, however, are either non-committal or negative as regards the far more important question of the relative merit of 'discussion' and of recitation. It is, to be sure, not easy to reduce the replies to common terminology, yet 20 make 'considerable' use of recitation and 11 more make 'some' use of it. In presenting my urgent recommendation of the guided class discussion which is neither lecture nor recitation I have, therefore, no right to claim the support of 'my constituents.'

In this connection a comment should be made on the size

[1] Cf. Statement VII (p. 69).

of classes.[1] Obviously it is simplest to teach large classes by lecturing to them. Yet a spirited and relevant discussion may be conducted in a class of a hundred or so. Of course no more than eight or twelve, or, at most, twenty of these will take even a small part on a given day; perhaps a half or two-thirds will never take part; and some will remain uninterested. But there will be many intelligent listeners as well as active participants; and these gain more, I believe, by the give and take of good discussion than by constant lectures however effective.

For the benefit, however, of large classes I am convinced that careful provision for more individual instruction should be made. It goes without saying that students with special questions and difficulties should be welcomed in private consultation hours. Yet I think that more than this is needed; and, to colleges which lack the means to establish a full preceptorial system, I recommend the division of a large class into small weekly divisions or conferences. Such conference divisions may take the place of one weekly appointment of the class as a whole; or, better, each may be treated as a laboratory hour in addition to the regular appointments. The time of the conferences may be variously used, Students disinclined to join in discussion in a large group will express their difficulties; assigned questions may be answered and the answers discussed; written review papers may be returned with comments; physiological models and preparations may be examined by each student of these smaller groups; and, more important perhaps than any specific result, a personal contact may be established between teacher and student. Obviously the success of such a plan depends on those to whom the conferences are entrusted. Able and well-trained assistants may advantageously conduct most of them; but it will be unfortunate if the main instructor of the course cannot feel the pulse of his class by himself conducting one or two of these conferences. And it is essential that he should give oversight to the conference and unity to the work, by stated meet-

[1] Cf. Statement IV (p. 68).

ings with the conference-instructors and discussions of conference methods. I find no evidence that this plan has as yet been adopted. Indeed, only 5 instructors report conferences with individual students (and 3 of these make only 'occasional' use of the method.) In one college, however, a class of 60 meets in one division for lectures and in three divisions for 'quizzes.'

3. My third conclusion is closely connected with what precedes. *It is absolutely essential that the course in psychology supplement text-book study by methods intended to secure the independent work of the student.* To require of the student merely to give back, in written or in oral form, the contents of a text-book is to run a heavy risk of missing the bull's eye of the course; and this, we have ever to remember, is to teach the student a first hand study of himself as conscious. I am not recommending that the general course in psychology be conducted without a text-book, for I believe, on the contrary, that precisely the beginner needs the aid of a book in focussing and classifying the results of his observation and reasoning. But better a thousand times no book than a book to be memorized. An essential means to the proper use of a text-book is, in my opinion, to forbid or discourage its use—and to forbear, also, to lecture on a new topic—until the student has dealt for himself with the topic of study. Thus, before, entering on the study of perception and imagination one may direct the student to 'state in writing the difference between perceiving a hat (or chair or vase) which is seen and imagining a similar hat (or chair or vase) which is not in the room. Before lecturing on the individual sense-types of imagination the student may be required to answer, again in writing, Galton's questionary or some one of those modelled on it. Before reading or listening to lectures on attention, the student may answer questions such as these: "What is the difference between attending to the demonstration of a geometrical problem and attending to the buzz of a mosquito? What bodily marks of attention have you noticed in a dog?" It is unnecessary to multiply examples. The essential point is that the student be led to observe his own experience, to

record his observation accurately—in a word to psychologize; and to make the observation before, not after, discovering from book or from lecture what answers are expected to these questions. Individual experiments should so far as possible be performed in like manner before the class discussion of typical results. In all cases the results of these introspections should be recorded in writing; representative records should be read and commented on in class; and the discussion based on them should form the starting point for text book study and for lecture.

The instructors who have answered the questionary evidently concur in the view that a text-book is necessary: only 4 of the 47 make no use of one.[1] But 23 of the 40 who have a text-book make, as I believe, a radically wrong use of it since they introduce each topic by assigned reading in the book. Of the remaining, 8 instructors introduce each subject by lecture and only 5 by what I hold to be the right method—some exercise in introspection, whether simple or experimental.

On the other hand, in almost all these college courses text-book study is supplemented in other ways: collateral reading is required in 28 cases, review questions are assigned by 19 instructors, experiments are performed or required in 25 cases.[2] All these methods have their value. Collateral reading is useful first in that it protects the student from the dogmatism or one-sidedness of his teacher or of his author or of both; second, in that it offers an opportunity to enlarge his field of observation. The student is no longer a man of one book, and can not fall into the error of regarding psychology as a closed science. In one introductory course known to the writer though not included in this study—that at Iowa University—the student is required to read three psychological text-books besides reading and working out the experiments of an experimental manual. Such a requirement obviously presupposes great skill, on the instructor's part, in the coördination of different teachings on every topic of psychology. Except in the hands of a gifted and experienced

[1] Cf. Statement VIII (p. 44).

[2] Cf. Statement IX. (p. 44).

teacher the result for the beginner in psychology might well result in a confusion of several systems and a clear knowledge of none. Fewer text-books, with assignment of collateral reading on selected topics will prove, in most cases, a more practicable plan.

The discussion of experiment in first year psychology belongs to Professor Sanford. Yet it may here be remarked that even a semester course in large divisions may advantageously include simple experiments. These may be, in the first place, experiments to be performed out of class and reported. Such are experiments in visual contrast, in the localization of temperature spots, in tactual localization. Or the experiments may be class demonstrations—of color-mixing, for example, or of beating tones. In using these demonstration experiments, as in the important demonstration of physiological material (models and charts of nervous system and of sense-organs) the important point is to keep always in mind that the experiments are in the service of psychology, that they are of use in teaching the student to classify and explain psychic phenomena, and that they are worse than useless if they keep him from seeing the psychological wood for the physical and physiological trees. One of the most interesting outcomes from the questionary is the discovery that in 25 of the 47 cases studied, some use is made of experiments.[1]

The third class of subsidiary methods is that of the review questions following on lecture and reading. Nineteen (19) instructors make use of 'review questions'. Their value as stimulus to introspection is akin to that of the questions by which a subject should be introduced. They should test the student's ability to translate the formal language of his science into concrete terms, to recognize when he meets them, experiences which he can define. Several of the well-known manuals of psychology—Titchener's "Primer," Thorndike's "Elements," Whipple's "Questions in General and Educational Psychology," and Witmer's "Analytic Psychology"—contain excellent questions of this type. And every good teacher can

[1] Cf. Statement X. (p. 70).

find in his immediate surroundings the material for questions which will lead the student to make constant applications of his psychology.

4. The exhortation to avoid mere text-book study must not be interpreted as a criticism of so-called systematic psychology. For no science can help being systematic; and my final recommendation, which may be very briefly stated, is accordingly the following: Insist on clear definition, consistent use of terms, and orderly classification of psychic facts. The definition and classification should, of course, follow on introspection; should not be accepted uncritically from instructor or from text-book: and should be subject to constant revision as fresh observations are made. The objection to system in psychology is based on a curious misconception. Definition and classification are no Procrustean bed; they form rather a scaffolding which changes constantly with the growing edifice. Faulty definitions, inconsistent conceptions, loose enumerations in place of systematic groupings, are a sheer hindrance to progress in any science. If the introductory course is to have a permanent value, if it is to provide a basis for further observation and reflection, the student must clearly identify the objects of his study, must know the precise meaning of his terms, must apprehend the likenesses and differences of phenomena.

The sum and substance of these suggestions is simply therefore: Lead your student, by some means or other, to psychologize; teach him to observe and to describe himself primarily, and then other selves, in their relations, to the environment. To that end: *First*, teach psychology primarily as you would if it were an end in itself. *Second*, eschew altogether the method of recitation; lecture in order to sum up and to illustrate different topics of study, but lecture sparingly; and cultivate constructive discussion. *Third*, bar out the possibility of memorizing text-books by requiring students to precede text-book study by the experimental introspection, and to follow text-study by the solution of concrete problems. *Finally*, do not tolerate inexact thinking, but insist on clear definition, however provisional, and on systematic grouping of facts, however incomplete the classification.

THE TEACHING OF ELEMENTARY PSYCHOLOGY IN COLLEGES AND UNIVERSITIES WITH LABORATORIES.

By E. C. Sanford,

Clark University.

To obtain data on the teaching of first year classes in psychology in such institutions, the following questionary was sent to instructors in psychology in the leading institutions east and west with the request that they would furnish the information desired, making replies to the questions, if so minded, or adopting some other form of presentation, if they should prefer.

First Year Psychology

INFORMATION IS DESIRED ON THR FOLLOWING POINTS

A. Purpose of the course as now given:

Is it looked upon as a means to liberal culture chiefly, as an introduction to philosophical studies in general, as a subject having useful applications, as a science to be cultivated for itself?

If all of these ideals enter, or several of them, please indicate their relative importance.

B. Academic status of the course:

Is it a free elective, a required part of certain groups, a required preliminary for courses other than advanced psychology, an optional course?

Does your introductory course as now given run through an entire year? How many periods a week? How much laboratory, how much class work?

How many usually register for the course? Are they handled in one or several sections? About what proportion go on to take further work in psychology?

C. Content of the course:

On what is the chief emphasis laid in the course: psychological theory, physiological and experimental matters, relations to daily life, pedagogical and other applications?

Do you try to do anything with comparative psychology (animals), child psychology, mental and nervous diseases, psychological questions of the day such as hypnotism, telepathy, mental healing, psychology of testimony and the like?

If all or several of these topics are treated, please give the relative importance attached to them.

D. Methods of instruction:

Lectures, text-book (one chiefly, or several at a time), collateral reading, reports by students, themes, discussions, seminary work, individual conferences—which of these, or which combination of them, do you find most satisfactory?

Do you give the students special pedagogical assistance in the way of outlines, reviews with or without special review questions, quizzes, or examinations other than those for the determination of academic standing?

Do you give much or little time to class demonstrations and experiments?

Is individual laboratory work required or optional? How many hours a week are given to it? How many laboratory hours are counted as equivalent to one recitation hour?

How large laboratory sections are handled at any one time? Do you work alone, or do you have regular paid assistants or student assistants?

Does laboratory work run throughout the year or is it begun after a period of class instruction?

Do you follow a systematic course of experiments required of all laboratory students—one of your own, or a text-book?

Do you think it well for all students to work at the same time at the same experiments or each individually (or in small groups) at a separate problem?

Do you have the laboratory students toward the end of the year undertake anything like minor research problems or the repetition of special experiments from research literature?

Are you responsible for classes in any other subject than psychology?

Laboratory equipment: Number of rooms, large or small.

Are you pretty well equipped or do you have to make use of a good many makeshifts? Have you a workshop?

Have you a fairly adequate appropriation for running expenses or are you cramped?

E. Possible improvements in the first year's work in psychology:

In what direction would you like to see the first year's work in your own institution develop? What would you change if you had a free hand?

What do you find the chief difficulty in giving such a course as you would like?

On the instructional side—immaturity of the students, lack of preliminary training on their part in physiology, physics and other sciences, lack of skill in introspection, no text-book of precisely the right scope, insufficient time allowed in the programme of studies?

On the material side—inadequate equipment, poor quarters, insufficient help?

Do the students fail to take psychological experiments seriously, fail to get what the experiments mean, are short in manipulative skill, are unwilling to give the time necessary for careful experimentation?

F. Philanthropical:

Have you a general list of reading in periodicals and the like, outside the ordinary psychological texts, which you have found useful in the work with first year classes and which you would be willing to contribute to a bibliography of recommended "outside reading," if one should be appended to the report of the committee?

Have you similarly any especially good demonstrations or simple experiments, not found in such good form (or not found at all) in the manuals, which you would be willing to publish in the report, or otherwise, for the general good?

Have you any special pieces of demonstrational apparatus or apparatus for student practice courses which you have found especially convenient and which have not yet been described?

Finally, have you any suggestions not extorted by the above queries as to what ought to be done in a first year's course in psychology or as to points on which this committee could obtain information that would be helpful to you?

Most of those who replied answered the questions as given, a few however took advantage of the latitude offered and gave the information in other forms. In the minds of the sub-committee such freedom of reply was more valuable than an enforced uniformity which would have allowed a statistical treatment of the data. In what follows, therefore, we shall deal with rough proportions only and not attempt tabular statements.

In presenting this report the sub-committee wishes to make grateful acknowledgements to those in charge of the work in psychology, in the thirty or more institutions responding, for the friendly coöperation without which the undertaking would have been quite impossible.

The following institutions responded: Brown, Bryn Mawr, California, Cincinnati, Chicago, Clark College, Columbia, Cornell, Harvard, Hobart, Illinois, Iowa, Johns Hopkins, Michigan, Minnesota, Mt. Holyoke, Nebraska, New York University, Ohio State University, Pennsylvania, Princeton, Smith, Teachers College (Columbia University), Texas, Toronto, Vassar, Washington (St. Louis), George Washington, Wellesley, Wisconsin, Wyoming, Yale.

The typical beginner's course in psychology is a course in "general psychology." Experimental psychology comes later though in some of the larger institutions parallel courses are offered and the student, if so inclined, may work from the start in the laboratory. Our questions covered both kinds of work and a certain amount of information has been collected with reference to introductory laboratory courses. Our report will therefore consist of two sections, the first and longer dealing with the introductory "general course," the second with the first work in the laboratory.

THE GENERAL INTRODUCTORY COURSE.

The first year's course in psychology is often a popular one in the sense that it gathers in a relatively large body of students. In many institutions some course in "philosophy" must be taken and psychology is the one selected. In about one quarter of those reporting, it is a definitely required course for all arts students and in nearly a third more it is required of students in the educational department, of pre-medical students and of those desiring certification as teachers. In more than half, however, it competes on an equal footing as a free-elective, and the size of the classes testifies to the popularity of the instructor and the subject.

The immense classes of the larger institutions 200, 300 or 400, are often handled in a single body for lectures and in a number of smaller sections for quizzes or other reënforcements of instruction. Where the lecture method is not used the handling of the class in sections is of course imperative, and these immense classes would preclude laboratory work, even if no other reasons existed for its postponement.

The proportion of students following psychology beyond this first course varies greatly, as might be expected, but less than 40 per cent of the students continue in four-fifths of the institutions reporting, and but 25 per cent or less in two-thirds of them. The fact that large classes are enrolled year after year shows that neither the subject nor the presentation of it is *per se* repellent. The small number going further shows rather that we have to do in these large classes with a student group whose major interests are in other directions and who make the course in psychology incidental to the main trend of their studies.

Psychology is an important branch of human knowledge, which has interesting bearings on every day life and vital relations to the work of certain professions, serves excellently to bring before the student certain fundamental questions of philosophy, and is for these reasons an efficient means of culture. All these functions are probably present in some measure in the minds of all instructors, but the main purpose of the course and the relative importance of its coeffects are variously conceived. In more than a quarter of even this group of institutions psychology is still looked upon as first of all the gateway to philosophy. In more than half, however, the science is presented for its own sake chiefly and the other results of its study (except perhaps the contribution to general culture, which is regarded as a natural consequence of a proper mastery of the subject) are looked upon as quite secondary. In nearly half considerable importance is attached to the presentation of a certain useful subject matter, and concreteness and applications are emphasized. In some the cultural purpose seems more directly in view, by which is meant, we infer, a somewhat less detailed and technical presentation than that used when the science is taught without ulterior considerations.

In about half the cases "psychological theory" (which, as we infer, was taken by most of those who replied to mean systematic psychology, or psychological principles presented in a coördinated way) receives chief emphasis. In about a third the chief emphasis is laid upon experimental and physiological matters; while in a few cases the relations to daily life are especially stressed. Only two correspondents speak of emphasizing the *facts* of the mental life, but this may be due to the way in which the question was phrased and it would perhaps be fair to count all that do not specify philosophical tendencies or psychological principles as tending in this direction. The central theme is of course the mind of the normal human adult and in some cases there is small departure from it. The mental life of animals and children and mental disease are referred to in a merely incidental way and for illustration. In a few cases, however, where the instructor's interest and equipment are adequate, some attention is given to borderland phenomena, child psychology and mental and nervous diseases. Topics of popular interest are often taken up with a view to combating superstition and popular error.

In about half the institutions reporting there is at least one instructor devoting himself to psychology exclusively. In the larger institutions with parallel courses, the instructor giving the general course gives courses also in one or more of the philosophical branches, the work in psychology being his less important function. The instructor giving the more special (experimental) course is usually free from such entanglements. In a few cases in the less differentiated departments the instructor in psychology takes a single class for his overburdened philosophical colleagues, usually in logic, ethics or æsthetics; or else carries work in pedagogy.

As regards method the course is usually a mixed text book and lecture course, though in some cases a text alone or lectures alone are mentioned. Demonstrations are frequent, especially when the central nervous system and sensation are under consideration, and in a few cases simple experiments are made in the class or by the students individually at home, *e.g.*, such as are found in Seashore's manual. Actual laboratory work is not usually attempted. Of the other pedagogical devices, discussions (probably the ordinary class discussions are meant) are most frequently mentioned; and collateral reading, theme writing, student reports and individual conferences follow in the order named. The quiz appears in some form in nearly every report. Sometimes it is written, sometimes oral, some times it occurs once or twice a semester and is an hour long, sometimes weekly or oftener and occupies 5 or 10 minutes. In one college all the papers turned in, in a monthly test, are corrected and returned to the students, in another, where a five minute written quiz is employed, the plan calls for the marking of but one-quarter of the papers each time. In about a third of the institutions reporting something in the way of an outline or syllabus or special set of review questions is furnished the student as an aid. *A happy variant of the review questions is that of issuing questions which cannot be answered directly from the text or lectures and which require some active response on the part of the student.* In one instance this has gone even a step further and questions are given which demand a certain amount of simple introspection or experimentation or both. In this way a real acquaintance with mental facts is cultivated.

With so much in recall of the generally familiar situation, let us hear the instructors on their chief hindrances and what they would change if they had a free hand.

Inconveniently large classes or a lack of assistance is mentioned as a serious difficulty by about a third of those reporting, but this is perhaps less an evil than the dearth of students, which is once mentioned. Lack of equipment or inconvenience in quarters is mentioned by about a third, also. More than half find the students ill prepared for psychological work, especially in being unable to use the knowledge of physics and physiology which they are supposed to possess, and in their inability to introspect. Some find themselves hindered by the mixed and uneven character of the classes in these and other respects. One or two mention the student's unwillingness to work, or his overinterest in the practical (pedagogical) aspects of the science. More, however, report no lack of enthusiasm on the part of the students, and one or two feel that immature students are the teacher's excuse for being, or state as their chief difficulties their own subjective limitations. The lack of a first rate text book is mentioned a number of times and insufficient time in the program of studies perhaps as often. Only two mention specifically the hostility of superior officers to scientific psychology.

In answer to the question as to how they would like to alter their courses if free to do so, about one-half the instructors in question reply that they would make the work more concrete and tangible and especially would add more demonstrational and experimental work; one or two would even like to see laboratory work required of every student as in chemistry and physics. Nearly a third would be glad of more assistants in order that the classes might be handled in smaller sections and the students receive more individual attention.

Some of these difficulties and desires are by no means peculiar to psychology, and could be matched from almost any department in any growing institution. Too large classes, too few assistants and unsatisfactory quarters are the common lot of nearly all instructors in nearly all sciences. The same is true in a measure of the immaturity and imperfect training of the students. The suggestion, however, that their deficiency in these particulars is only a sign that they need a teacher meets the matter in part only: there seems to be a real difficulty here and one resting with especial weight on psychology. Inability to introspect means inability to get at the subject matter of psychological science and points to an unusual danger of substituting a knowledge of words and descriptions of psychical phenomena for knowledge of the things themselves. This in the committee's opinion is a matter of first class importance and needs the attention of every teacher of beginners in psychology.

The frequent desire for more demonstrations and more experiments in the beginner's course may possibly mean a wish for a more striking lecture material only, but it ought to mean a desire for a closer envisagement of the psychic facts by the pupil.

The fact that many students whose main interest lies elsewhere are now drawn into the first year's classes in psychology and that introspection is a hard matter at the best would seem to make the opening of their inner eyes to the mental world one of the first and most imperative duties of the course. Practical bars of the most insuperable sort hinder the accomplishment of this by regular work in the laboratory. The classes are almost everywhere too large and the amount of time which the students can give to psychology too small. What is wanted is clearly some other method of bringing the student into contact with the psychical facts. Something is already being done, and often a good deal, in the way of demonstrations and class experiments and this is in every way admirable when the instructor makes use of the introspective possibilities which they offer and drives home the psychological meaning of what is presented. But the beginnings which have been made in the issuing of questions requiring simple introspections and experiments, and in the preparation of simple experimental courses which can be followed by the student at home as a part of the regular preparation for the lecture or recitation hour, are undoubtedly still more important steps in the right direction and worthy of all encouragements.

The most interesting and valuable of the information brought out by our questions was that which a number of our colleagues were kind enough to give in reply to a portion of our last question which read as follows:

"Finally, have you any suggestions not extorted by the above queries as to what ought to be done in a first year's course in psychology?" Several of these paragraphs of suggestion we desire to give *in extenso.*

SUGGESTIONS FOR THE IMPROVEMENT OF THE GENERAL COURSE.

I. "But several things are needed for the improvement of courses like A (a "general course"). . . . I might mention:

"(1) A good reliable list (a) of inexpensive demonstrational apparatus and (b) of useful demonstrational experiments, whose use with the class would occupy a very small proportion of its time, but would serve not merely for its entertainment but as a real help in comprehending the facts and principles discussed. I am not fully satisfied yet with existing lists.

"(2) A good *critical* bibliography on each phase of the subject.

"(3) A standard set of lantern slides.

"(4) A larger agreement among the leading psychologists as to what are the fixed and permanent underlying principles of the science.

"(5) An adequate text book that will emphasize these fundamental principles first of all, aiming to present them soundly and convincingly with only enough of detailed fact to make these essential principles clear; leaving to lectures and collateral reading the acquisition of further details and the application of the fundamental principles to them. At present to reach this ideal, I suppose each of us must write his own text book; for I haven't found one yet that satisfies me."

II. "I am of the opinion that a General Introductory Course that *is required should not* be a laboratory course. Too little can be covered in such a course and to my mind the advantage of the laboratory method should, under the circumstances, be sacrificed for the sake of the greater advantages of a course of wider scope, and such as can be given in the same time in lectures supplemented by copious demonstrations.

"If the course is not required and if most of the students take further work in Psychology the force of the above objection is greatly reduced. Nevertheless, even in this case, a general lecture course with demonstrations and class experiments is to be preferred by way of introduction to a strict laboratory one.

"I insist in my general required course upon the structure, the organization and the function of the nervous system. This seems to me requisite not only for a proper understanding of psychology as a science, but also for a modern understanding of man and his activities. This general culture value of a clear and not too scrappy understanding of the organization and of the function of the nervous system is, to my mind, of the first importance in a required course."

III. "1. Business-like administration is desirable in the interest of the student, who is likely to get into slovenly habits. To this end W— has found nothing better up to date, than dividing each half year's work into 5 parts, each part being a subject, such as space perception, association, memory, reaction time; the work on each part is to be completed at a previously set date, the laboratory notes to be handed in, and an examination on that part taken.

"2. Instead of assigned cultural readings W— prefers, usually, problems to be worked out by the student; as for example (a) illustrations of Weber's law from common life (usually not very successful); (b) records of trains of association; (c) exercises in finding average, constant and variable errors, etc."

IV. "Standardize by getting (and recommending) good sense organ and central nervous system slides and models. By suggesting certain sets of demonstrations. . . . (but) leaving time for instructorial bent.

"Arrange some exchanges of first year examination papers between different universities."

V. "I would suggest that the committee prepare or further the preparation of leaflets or pamphlets containing directions for single experiments or groups of experiments in order that instructors may make any combination suited to their needs. Even the smaller manuals contain much not suited to some classes."

VI. "I would suggest that a loose leaf laboratory book be prepared, corresponding to books now used in physics, chemistry, etc., describing a series of simple experiments and containing all the material necessary for performing the experiments. If the experiments were performed under constant conditions, and the records sent to some central body, we should be in possession of some psychologic norms with which we could compare the results given by our own students. I have a lot of this stuff worked out now and should be glad to coöperate."

VII. "The thing that more than any other keeps us to the old lines is the belief that the beginning student should get a firm grip on the vocabulary of psychology, even if that vocabulary is somewhat archaic, and that a study at first limited to the normal, human, adult mind, is best for that purpose."

VIII. "If my observations are correct Experimental Psychology is now taken (elected) by only a few students. The way to save this work for education is to combine its essentials with the introductory course, I sincerely believe. Experimental psychology has an 'educative' value in various ways but now this is almost entirely lost. Combining the essentials with the introductory course would also assist in giving the essentials of the text book. I used Seashore's *Elementary Experiments* in my beginning classs this year and found it decidedly helpful."

IX. "With respect to ordinary college first year psychology my main divergences from customary opinion are:

"(1) That this course in psychology is not to make philosophers.

"(2) That this course in psychology is not to make psychologists.

"(3) That this course in psychology is to make men and women better fitted to understand all the sciences and arts dealing with human nature.

"(4) That psychology is not best taught to beginners as the study of human *consciousness* but as the study of human *nature and behavior.*

"(5) That experiment dealing with realities and exercises testing knowledge and power by demanding their application to new problems are a *sine qua non* of success in teaching psychology (or anything else).

"(6) That the function of the teacher is not primarily to get a great amount of work done by the students but to get the greatest amount of knowledge, skill, etc., from a given amount of work by them.

"(7) That the actual content of the course is more important than most teachers of psychology think and the *form* or *discipline* or *point of view*, etc., less important."

X. "If conditions permitted it, I should be in favor of giving a whole year to the required course, in which the only laboratory work would be of a practice character. In such a case I would like to see the dynamic, rather than the static, aspects of mind emphasized. I would begin the course, not with a detailed analysis of sensation but with a consideration of the biological functions and place of the human mind. Then I would proceed to more careful analysis with experimental illustration. I would spend a good deal of time on the psychology of conduct, thinking, feeling, bringing out the connection between ethics, logic, æsthetics, etc., and psychological analysis. I would give some attention to abnormalities of consciousness and action and point out the practical, social, moral and educational bearing of psychology.

"Whatever may be said in favor of a structural psychology that is rigorously atomistic, as a legitimate scientific proceeding. . . . my experience as a teacher leads me to say that emphasis on the structural, static, and atomistic points of view is not desirable in a first year course.

"Whatever the value or interest of these very vague remarks, they are to be taken as the expression of a teacher who, while recognizing the independent rights of psychology, does not deem it wise, from the standpoint of undergraduate instruction and of philosophical culture, to divorce it from philosophy. In fact, as one whose interests are predominently epistemological, ethical and metaphysical, I am not at present able to draw any sharp dividing line between functional psychology and philosophy."

XI. "A satisfactory consensus of opinion, today, I think is impossible to obtain; and such as you will get I consider more likely to do harm than good, except possibly in the smaller colleges. . . . I consider the attempt to standardize courses premature; let the men in the field experiment with courses, the best will survive; but your made to order course or your average course will never be as good as the best."

XII. "I do not think that it is possible to teach a science in any other way than as a "science to be cultivated for itself." That is to say, I should give precisely the same introductory course, whatever the aims and needs of the students who took it. I doubt whether it is advisable to cut a science to suit the requirements even of a distinct professional course; I doubt, that is, if it is advisable to teach English Literature for Engineers or Physiology for Medical Students; I believe that the best results are gained, in such cases, by teaching English literature and physiology. But 'general culture' or 'liberal education'—the supposed main object of college courses in arts and sciences—is not a profession, is not at all strictly definable; and I do not see how it is possible, not to say desirable, to cut psychology to fit that requirement.

"I teach psychology, at the beginning, in an elementary manner. I make things as simple as possible; I omit phases of problems, even whole problems, where simplification to the necessary degree is impossible; I am dogmatic on points where dogmatism is strictly out of place, although the dogmatic statements are always qualified for those who have ears to hear. But I am all the while teaching psychology, as best I can, without ulterior motive or ideal. I get a good many men from engineering, medicine and law; the number of these outside students is steadily increasing; but I make no concessions to them.

"If any other ideal is followed, it seems to me that one of two things must happen. Either the teacher lapses into dilettantism; and for this there is no defence. Or the teacher substitutes his personal and private judgment for the objective judgment embodied in the actual course and growth of the science; he forces on the students his own notion of cultural or philosophical or practical application, instead of showing what has been accomplished and allowing that accomplishment to speak for itself. In general, this alternative must lead to bad results.

"I grant, of course, that a teacher, a man who is in love with his subject, will do well with students whatever method he follows and whatever choice he make. That is axiomatic. But I suppose that the aim of this questionary is to help the weaker brethren, and not to legislate for those who already know. And I should accordingly counsel the weaker brother, if he is a professor of psychology, to teach psychology. If he does this, up to his honest limit of achievement, he will find that psychology will show its own bearing upon culture, its own philosophical relevance, its own applicability. These references may then be followed up, as needs arise and as the size of the staff allows, by later courses, given either by the department of psychology or by those of philosophy and education, etc. To interject them at the beginning is to warp the mind of the student. . . .

"My ideal is the local separation of the college from the university.

With this ideal realized, I should offer (1) an introductory course, much as I do now, for graduates in other departments, and (2) research and training courses in the laboratory. I should not, by choice, be a 'college' professor. Being this, however, I have rigorously pushed my university ideals into the college work. The success is patent; my 270 elective students—if their number is compared with the number of the sophomore students in the college—are sufficient witness. To say that hard work cannot be got out of the students, or to say that scientific psychology is uninteresting, is simply to cover up one's own laziness or incompetence.

"I must here interject my ideas on the lecture system. The lecture has a twofold advantage over the recitation. (1) It is economical, since one man handles a large number of students; the method of recitation is extravagant. This fact alone will mean the retention of the lecture system, wherever it can possibly be employed with success. (2) It is educationally the better method, for the average student and the average teacher. For the reconstruction of a lecture from notes means an essay in original work, in original thinking; while the recitation lapses all too readily into text-book rote and verbal repetition.

"It is, nevertheless, true that sophomore students are on the whole inadequate to a lecture course. They cannot take notes; they cannot tear the heart out of a lecture. (They are also, I may add, inadequate to the reading of textbooks or general literature, in much the same way.) Hence one has to supplement the lecture by syllabi, by lists of questions (indexes, so to speak, to the lectures), and by personal interviews. I spend, on the average, 8 hours of time on every one of my sophomore lectures; I give 24 hours a week to a 3 hours' course. But, doing this, I secure a fail-percentage of 2 or under.

"Evidently, this method is wasteful of good material. While it costs less than it would cost to add half a dozen assistants for recitations; and while it is also, educationally, a better method than that, it still wastes good professorial time. The remedy, is, to accustom students to note-taking in the high school. In the English public schools, a part of the work in the upper forms is lecture-work, for which the boys are held responsible; they therefore go to the university in some measure prepared for the lecture courses. Now lectures are frequently given, by outsiders, to the students in American high schools; but they are looked upon as a recreation or a bore. I suggest that these lectures, given once or twice a term through the four years, might be utilized for teaching the students to take notes. The students might, in a minor way, be held responsible for the reproduction of their contents. Then, in the freshman year, the student might take, say a single lecture course of 3 hours a week; so that, in the sophomore year, he would come to the psychologist with some training.

"I speak of averages all the way through. A man may be a born lecturer, or a born user of the Socratic method; he will succeed anywhere. But we do not get congenital ability throughout our faculties. I believe in the lecture system, for the average student and the average instructor of our subject, but I suggest that the present state of affairs might be improved, on the side of the student, by better high school preparation. As for the instructor, he must be taught, as sharply as necessary, that a lecture is a work of art, and not a perfunctory delivery of information. Much of the failure to interest, in psychology, is due to the instructor's lack of education in his art: he cannot manage his voice, he has not learned what he wants to say, he does not properly enunciate, he cannot vary from narrative to exposition, to argument; his anecdotes are purple patches and not relevant lightenings of the material. Time and again I have listened to 'lectures in psychology' that made me wish I had the lecturer alone, with nothing but the moral law in my heart and the universe of stars above me. . . .

"So—to return—my chief difficulty is immaturity of training (not of mind) on the part of the students. Physiology and physics they have tasted, or can get concomitantly; introspection it is my business to teach them; text-books I can write myself; and I have my share of their time. I want them only to know how to deal with spoken or printed material.

"Our equipment I have already described. Students who lack seriousness, or understanding, or manipulative skill, or time to devote to the subject, are bowed out of the laboratory in the course of the first fortnight. Why should we bother with them? They are not obliged to come in, and we are assuredly not obliged to hold them if they do come in. We take all imaginable pains with the real student, whatever he may at first be lacking in; but the unfit are eliminated. And if one has had experience, the spotting of the unfit is not a very difficult thing.

"I should deprecate the publication of any list of first year's collateral reading. The science is growing by leaps and bounds, and the collateral reading shifts and changes from year to year. To publish a list would be to relieve the instructor, for some years to come, of what should be an integral and an interesting part of his own work and growth. The list would remain, fossilized, and students would suffer. No concessions should be made, I think, to the weakness of the instructor; every allowance should be made for the poor training of the student. . . .

"The sum and substance of my recommendations is that you provide a competently trained instructor, and let him teach psychology as he best can. What the student needs is the effect of an individuality, a personality; and the lecture system provides admirably for such effect. I should strongly deprecate the issuance of any general plan of organization, or the authoritative recommendation of any special topics or procedure, which should tend to mechanise instruction in the colleges. We are overorganized, over-businesslike already. I venture to suggest that a great danger in the Report of the Committee lies precisely in this point. If its recommendations are over-stringent, if the freest play is not allowed to the instructor's personal training and individual capacity, we shall be out of the frying pan into the fire."[1]

[1] Several correspondents would like to have information as to particular questions with reference to the first year's course—as, for example, the preferred order of presentation of the various topics in the course, whether introductory experimental work hould begin with bare sense-experiments, how far psychologists feel an unsatisfactory

Recommendations. The Committee has no such idea of its function as that implied in the warning of our colleagues who penned extracts XI and XII above. It does not desire to be responsible for an average course or to standardize the courses now given. It agrees unreservedly with the writer of extract XII that the best way in which to secure good courses in psychology is to select well trained men and give them a free hand. Good teaching is an art and the teacher must be assured the artist's freedom. The Committee's function, as we conceive it, is not to establish norms of any sort, but to formulate more clearly certain ideals on which many or all of those teaching psychology are already agreed, to point out means by which these can in some measure be realized, and finally to recommend to the Association such action as will assist those who care to work in the direction suggested.

The ideal which the Committee desires to formulate and for which it hopes the endorsement of the Association is that of the fullest possible acquaintance on the part of the student with the concrete facts of consciousness as the rational basis for a knowledge of psychology—an ideal which has been frequently mentioned by our correspondents and which is shared we believe by all or almost all of our colleagues. As means to this end the Committee urges, as indicated above, the fullest use of demonstrations and class experiments, given always in such a way that the student shall not fail to grasp their *psychological* meaning, and the development of the scheme of simple home experiments and introspections already in tentative use by at least two of our colleagues.

In furtherance of these ends it recommends to the Association the formation of two permanent committees, one on Demonstrations and Class Experiments and another on Psychological Experiments Outside the Laboratory. The functions of the first should be to publish from time to time in some one of the psychological journals, first a classified bibliography of such good demonstrations and class experiments

tendency toward practicality in the students which come to them from pedagogy, and how far they still suffer from a tendency of philosophers to dominate the departments to which the psychological courses belong.

as already exist in readily accessible literature, second to collect and *publish in full* in the same way directions for making such demonstrations and class experiments as are found only in relatively inaccessible literature, and third to collect and publish in the same way similar demonstrations, experiments and descriptions of demonstrational apparatus as are now in use in American laboratories and as yet unpublished. (Several of our correspondents have already signified their willingness to contribute to such a collection.[1]) The Committee on Experiments Outside the Laboratory should undertake the same functions with reference to their particular topic. Our purpose in suggesting that these committees be made permanent is that they may be given time to work slowly at the material to be gathered and publish from time to time as it is collected without producing at once, or waiting for, anything approaching a complete exploitation of the field; and that the experiments and demonstrations which they publish may be kept up to date.

THE FIRST YEAR IN THE LABORATORY.

In the half dozen or more institutions in which parallel introductory courses are offered, it is possible for the student to begin the subject of psychology with its experimental aspect. But even in these cases it is exceptional that a student enters the laboratory before he has taken the general introductory course, though the two courses are sometimes carried at the same time. In all other institutions where laboratory work is given at all, it follows an introductory course. The first laboratory course usually runs through a full year though in some instances it is given in a single semester.

The preferred size of laboratory sections is ten or under, though in some institutions, where the work has been thoroughly systematized larger sections (up to 30) are handled by the instructor with one or more assistants. Practical

[1] We would suggest for membership on this latter committee our colleagues Professor J. E. Lough of New York University and Professor Margaret Floy Washburn of Vassar College who have already used with success the methods in question.

reasons, especially the cost of much duplicating of apparatus, make it necessary in most cases where the laboratory sections are at all numerous, for different groups to work at different problems, but some instructors in beginning the laboratory course have all the students work at the same problem at the same time until several problems have been covered; and in exceptional circumstances it has been found best to carry this method through the major part or even the whole of the beginner's course.

After a year or so of laboratory practice several teachers set students at repeating experiments from research literature or assign them minor research problems under supervision. It is rare, however, that this can be done within the first year.

In the institutions in question the number of rooms used for laboratory purposes ranges from 1 to 32, but in the latter case of course many rooms are included which are not used for the first year's laboratory courses. The most frequent number mentioned is 5, and half of all reporting give numbers from 2 to 7. In two institutions the laboratory consists of one room only though in one of these cases other rooms may be used in time of need.

In equipment most of the laboratories supplying information (about two dozen) are at least fairly equipped for the sort of work required. Only three speak of their equipment as inadequate, and one of these is just starting.

Appropriations are felt to be inadequate and cramping in but four of twenty eight; and some small laboratories are able, by economy and by concentrating research upon a single topic at a time, to do work of excellent quality, both practice work and research, with the expenditure of relatively small sums.

With reference to the constitution of the first year's laboratory course there is a wide spread diversity. Eclectic courses are common and it is once or twice remarked that the course is varied from year to year or should be adaptable to the needs of the students taking it. It must often be limited also by the equipment, or even the physical situation, of the laboratory.

The data collected with reference to the first year's work in the laboratory, while it shows that most of the laboratories reporting are in a fairly prosperous condition is sufficient to to justify the Committee in but a single recommendation.

Recommendation. In view of the diversity of interest, requirement and possibility the Committee looks with favor upon the suggestion of one of its correspondents that a laboratory course should be prepared on the principle of the "loose leaf" courses now used for elementary laboratory work in other sciences, by means of which an instructor can vary his course with ease and within wide limits. The preparation of such a course is not the work of the Association nor of a committee working under its authority, but we suggest that, if so moved, the Association might further such an undertaking by expressing its official approval of the plan.

LABORATORY COURSES AND EQUIPMENT IN PSYCHOLOGY FOR COLLEGES AND UNIVERSITIES.

By James R. Angell.

University of Chicago.

The two points in our educational system at which advice is most often sought concerning the equipment of a laboratory for psychology are the normal school and the small college each with a modest appropriation at disposal for the purchase of apparatus. The normal schools are treated in another part of this general report. The comments which follow are directed to the consideration of collegiate institutions which possess no equipment in psychology or to those whose equipment is small and who may contemplate either systematic or occasional enlargement. What is said of research pertains primarily to the larger universities, but it is included with brevity for completeness sake. Such institutions ordinarily require little advice and would be slow to accept it, even if it were needed.

MEANS AND ENDS OF LABORATORY WORK.

Laboratory work in psychology may be designed to meet any one of several distinct aims of which three may be mentioned. (1) It may be desired to offer students laboratory methods primarily as a means of aiding them to attain direct, personal contact with psychological materials, to assist them in introspective observation and in general to supply them with a technique for discovering facts about mental processes and for arranging and presenting these facts when obtained. (2) Another proper aim of laboratory work, which includes but goes beyond the one first described, is that of furnishing students immediate acquaintance with the apparatus and methods by which important facts have been determined and significant principles established. When the opportunities of a well-equipped laboratory are improved to the utmost in this direction, students not only secure an excellent drill in general scientific method, but they also gain the ability to estimate with confidence and independence the relative reliability and certainty of specific types of procedure. In other words they begin to achieve scientific judgment. (3) A third aim is that of research, together with the giving of a thorough training in experimental technique as a preliminary for it. Many other purposes may be involved from time to time, but these three alone afford grounds for very considerable diversity in the equipment required for their realization the first differing much more from the latter two than they differ from one another.

The first aim demands much the least pretentious outfit of materials and may be attained with large success on the basis of a very slender equipment, such as can be secured at trivial expense. The exercises in introspection which certain of our texts now contain and the ground covered in one or two manuals at present on the market well supply the need for work of this kind. To be sure many of these exercises could not be called strictly experimental in character inasmuch as they simply invite attention to certain facts open to casual observation. But they shade over into a more genuinely experimental procedure by indiscernible gradations and for our present purposes they may well be ranked as experimental.

So far as our reports enable us to judge, the results gained by this type of work have been most salutary both in conveying a just impression of experimental method and in stimulating appreciation for direct psychological information. Teaching institutions of all kinds in which psychology is represented may well be urged to make use of the possibilities of work of this sort. It must not be exaggerated in importance as in any way taking the place of the varieties of work still to be described. It is in no sense justly to be considered as in competition with them. It supplies a different need for a different constituency. But it can hardly be neglected by any instructor who desires his students to secure vital ideas of the character of psychological facts. Every good teacher has used it more or less since the beginning of the experimental movement. It deserves, however, to be employed in the most effective manner possible and for this result much intelligent planning is necessary. Formal apparatus although distinctly useful is altogether secondary. In any case, whether with or without apparatus, the procedure is designed to bring out the rudimentary mental phenomena, such as those of the various sense experiences, attention, memory, imagery, feeling and the like.

In this connection should be mentioned also the class experiment which shares with this form of procedure just mentioned the essential aims designated but attempts to reach its ends by methods that permit the class to work as a group, thus economizing the time of the instructor and the expense of providing assistants such as are usually required for the supervision of laboratory exercises. This is not the place to discuss the merits and defects of this method. Suffice it to say that experience has shown conclusively that much can be done in this way to give accurate impressions of experimental method and not a little can be accomplished in the actual disclosure of unfamiliar psychological fact. The manipulation of psychological data can also be effectively taught or at least illustrated in this way.

The use of demonstrational materials of all sorts may well be mentioned here. Of the value of such devices when properly employed there can be no question whatever. The verdict of experience is unequivocal on the matter. They add to the interest of the class, increase the breadth of outlook and linger firmly in the memory by virtue of the vividness of the experience which generally attends their use. They suffer from the danger of incompetent use as do other meritorious means of instruction. If they are not used so as definitely to assist the mastery of the matters immediately in hand, if they are introduced so as to constitute a diversion, if they are used with so much frequency that the main impression conveyed by the course is one of a vaudeville character, they are not likely to prove anything but disintegrating to the final result.

The second main aim above mentioned, *i.e.*, systematic training with classical forms of apparatus, requires for its complete realization a large and relatively expensive equipment of apparatus. Such a collection must keep abreast of the time and must be well supplied with the classical pieces of apparatus many of which are inevitably costly. But the expense brings a fine return to the student fortunate enough to be trained in such a laboratory by a man conversant with his field and enthusiastic in its cultivation. No other means can afford so firm a grip upon psychological science. For a laboratory established with this second aim in view a really good shop is of the very highest value and some sort of place for minor tinkering is absolutely indispensable.

The most perplexing problem which institutions have to face in the matter of equipping for work in psychology is found in cases, which come fairly under the second main heading, where funds enough are available to permit some measure of freedom in purchasing apparatus, but where not enough is at hand to allow any completeness in outfitting. The question at once arises what to secure. It may be a part of the later work of this committee or its successor to make explicit suggestions toward combinations of apparatus where such conditions prevail. If so, no doubt such suggestion will be forthcoming. At present only a few comments on alternative courses will be offered.

One possible line of action involves a selection from among the various pieces of apparatus of such as may be thought best fitted to convey a generally accurate impression of the scope and character of experimental work. In this case personal preference as to lines of work to be emphasized will naturally differ. The private interests of the instructor will inevitably, and perhaps properly, dominate the choices. Such a collection will be frankly imperfect, but it will not be wholly one-sided.

Another line of procedure contemplates the selection of one special field of work, such for instance as that of vision, and the making of the most perfect possible collection of apparatus designed to permit thorough exploration and demonstration of all the features of that field. This course will inevitably leave one with a collection quite unrepresentative of the general experimental situation, but it will enable the giving of thorough training in some one direction, it will ordinarily permit the undertaking of stimulating research and, inasmuch as the technique of scientific method is highly similar in many fields of endeavor, it will render possible a very satisfactory training in experimentation. Certainly the advantages of this choice for the expenditure of funds in cases where the amount at disposal is quite limited, deserve much more attention than has commonly been given them. In combination with the possibilities of the first general type of procedure described above where little or no apparatus is required, it offers opportunity for most striking results.

When we come to speak of our third aim, *i.e.*, research we, find ourselves confronted by divergencies similar to those which we have just discussed. It may be laid down as a general axiom that an ideal research laboratory should be equipped to afford the best and most symmetrical training of a disciplinary kind such as has been considered under the immediately preceding heading. In point of fact, however, much good work has come from laboratories where very different conditions prevail. Successful investigation is of course partly a matter of resources, but it is much more largely a matter of the man conducting it: it has at its best the spark of genius in it and all attempts to reduce it to rules or restrictions are futile and fatuous. Nevertheless, we may certainly recognize those conditions under which it is most likely to flourish and attempt to secure them where possible.

In America the general tendency has been to precede research work by a course affording not only a general survey of the field of experimentation, but also actual drill in the execution of experiments in the several principal fields of scientific interest and accomplishment. Elsewhere a different procedure has often been followed. Research has been attempted on the basis of a very narrow acquaintance at first hand with the general experimental field. A period of service as reagent for some other student's research, together with a preliminary training of a theoretical kind in lecture or recitation room, has been the only introduction required for the beginning of original investigation. It must be admitted that not a little excellent work has been achieved on this latter basis, but it is certainly safe to say that the first method is the only one to be encouraged where means are at hand to permit its establishment.

The laboratory adequate both for general training and for original investigation is extremely expensive and is not to be thought of save by a few of the stronger and wealthier institutions. When its advantages are well used it represents the most effective organization for our work. But every one who has lived in one of these large laboratories knows full well the dangers which always lurk to ensnare the unwary. The very riches of the place may contribute to lassitude and to a dangerous sense of competency which is justified by no real attainments. The equipment too is likely to suffer from failure to clean it out often enough. A good bonfire should be an attachment of every large laboratory and on it should be offered up each year the rubbish which gradually accumulates, which has no historic value and which often misleads the tyro into cherishing respect for that which should only be forgotten. Fortunate the laboratory where only those things are cherished which are really used and which really deserve preservation.

In connection with a research laboratory a good shop with a competent mechanic in charge is one of the most important assets. The efficiency of such an establishment is increased many fold by this feature. Some of our laboratory directors have appreciated this fact and invested what might seem to the uninitiated a disproportionate part of their funds in this direction. But while it is no doubt possible to equip a shop too luxuriously, the lack of an effective shop is one of the most serious handicaps which a laboratory for research can labor under.

PLACE OF EXPERIMENTAL COURSES IN THE CURRICULUM.

Experimental training under our first heading evidently finds its place in connection with the beginner's course and the location of this in the college work is discussed elsewhere in this report. The case of experimental courses differentiated from the elementary work offers a more difficult problem. It is the almost universal practice, where such courses are given, to insist upon the taking of the elementary course in general psychology before entering on this work. If this prerequisite is enforced, the place of the experimental courses is forthwith settled. In most institutions this would in effect limit the patronage of the experimental work to the junior and senior years. Experience suggests one conclusion about the matter which seems worthy of formulation. If the patronage is open to very young students, it is difficult for a laboratory course in psychology to compete with other college courses of an attractive kind without lowering the scientific severity and rigor of the work to a point where its solidity and worth must suffer in the eyes of the better students. Many instructors prefer the most mature students possible in order to avoid the necessity of thus cheapening the work to carry the full interest of the class. Whether this situation arises from incompetent teaching, from failure as yet to develop satisfactory methods of presenting the subject matter, or from the intrinsic character of the material, whose appreciation requires a certain sobriety of interest and a certain maturity of intelligence, in any case the conclusion just indicated is an expression of the experience of many instructors. A course can be given which shall be entertaining and moderately informing without straining the student's tenacity unduly. But if the work is made to contribute in a substantial way to the mastery of adequate technique and to the achievement of accuracy and thoroughness, the tax on student patience is greater than most young undergraduates will endure. In general it may be said, then, that experimental courses beyond the elementary course which may well contain some experimental material, should come late in the college curriculum. For students who plan to go at once into graduate work of a psychological kind, this arrangement is apparently a disadvantage. But for the rank and file this disposition of the case is undoubtedly best and, even in the case of the would-be specialist, it may well be urged that his time as an undergraduate can be better employed in lines contributory to his general training than in a too early and narrow specializing.

GENERAL REPORT ON THE TEACHING OF THE ELEMENTARY COURSE IN PSYCHOLOGY: RECOMMENDATIONS.

By C. E. Seashore.

University of Iowa.

Be it said once for all that this committee regards it neither feasible nor desirable to recommend any one system ofpsychology, any fixed mode of treatment, or any exclusive set of aids to instruction. The content, the method, and the means of instruction must vary with the preparation of the teacher, the type of student, the place of the course in the curriculum, etc.; and the growth of science, the invention of methods and instruments, the appearance of new text books, etc., make it necessary to change the course from year to year.

Nor does it seem desirable to make this general report a summary of the subcommittee reports. The reports of the subcommittees have been worked out independently and no effort has been made to harmonize them. We propose that all the reports shall stand together and each supplement the other. Although this one is the general report, it does not represent the unanimous opinion of themembers of the committee in all respects. It was drawn by the chairman as an expression of his personal views after careful study of the subcommittee reports; it was then submitted to each member of the committee for criticism and thereupon revised by the writer so as to represent the conviction of a majority of the committee on each point. The committee has thus made no effort to speak with authority for itself or for the Association. Although based upon the data collected, the four reports are largely personal.

Yet psychology has now found itself in American institutions sufficiently for us to take a provisional inventory. These reports are an attempt at such an inventory of aims, interrelations, conditions, methods, equipment, etc. The committee hopes that they may stimulate interchange of ideas on the subject and prove a starting point for further coöperation toward the development of the teaching of that subject which is the foundation for all the science and art of teaching.

A word of apology must be made for the terse, brusk, and hortatory style. It has been followed in the interest of brevity and clearness, and for the purpose of stimulating discussion. There is hardly a statement that should not be qualified to meet special circumstances, and much is controversial; but an explanation of this at every point would leave the report unnecessarily cumbersome. The general scope and form of this report was determined by the purpose of setting forth a series of propostions, or tenets, for discussion in the Association. The aim has been to set out a few fundamental principles in systematic relief.

I. AIM.

(A) *Teach Psychology.*

Strange to say, this is the one exhortation most needed today. While only a few schools lag in the old rut and teach antiquated systems of philosophy in the name of psychology, the common error today is to ramble from the study of mental processes as such into sense physiology, moralizing, loose pedagogy, or logical quibble.

The first course in psychology should be essentially the same in content and method whether it is taken merely for general culture, as a foundation for philosophical studies, or in preparation for specific vocations; such as, education, theology, art, law, or medicine.

1. Teaching psychology strictly as a science in itself lays the best foundation for the final mastery of the relationships and applications of psychology.

The laying of such a broad foundation for philosophy and the numerous applications of psychology frees the teachers of all those subjects from the wasteful drudgery of having to improvise psychological foundations at each step as needed.

Nothing has been more potent for the improvement of the teaching of philosophy and psychology than the recognition of this separation for teaching purposes. The same will be seen in medicine when normal psychology is required as a preparation for the study of psychiatry, and has been clearly demonstrated in the recent development in the teaching of educational psychology.

Supplementary instruction on the relation of physiology, logic, ethics, philosophy, etc., to psychology may, and usually should, be put into the course, but only when it is made clear that it is not psychology.

2. In the first course the applications should be incidental and should be distributed among different aspects of daily life with a view to the broadening of the mental perspective of the student; as, in education, medicine, science, art, law, literature, philosophy, religion, business, play, labor, etc.

In a sustained illustration of the formation of associations in typewriting, e. g., the aim is not the pedagogy of typewriting, but the psychology of associations which enter into a thousand similar processes; in an illustration of suggestion, by a realistic case of mental healing, the aim is not to teach the art of mental healing, but the laws of suggestion which operate in countless similar situations, and to vitalize and give relief to a set of psychological facts in one coherent illustration.

(*B*) *Yet not psychology but to psychologize.*

This does not mean that we should aim to make psychologists any more than that a good first course in rhetoric should make a literary artist or the first course in chemistry a chemist, nor does it mean that the elementary student should be taught by the research method.

The primary aim is to train the student in the observation and explanation of mental facts. Other aims are secondary, such as:

1. Systematic knowledge of mental facts.

In some respects this may be coördinate with the primary aim but there is great danger of mere nominal knowledge.

2. Culture: the ability to interpret life.

Ability in interpretation is the most fundamental element in culture; with the knowledge of fact there must be the ability to judge for one's self.

3. Efficiency: the ability to act effectively.

Efficiency and culture may be one; the distinction is here made for the purpose of emphasizing the two aspects.

4. Appreciation of mental life for itself, which is the basis for the recognition of its worth and manifold bearings.

"The world is different to me since I studied psychology," is a characteristic and proper remark of students.

5. A foundation for the philosophical studies.

The science of psychology furnishes the observable facts which constitute a starting point for philosophy, logic, ethics, esthetics, etc.

6. A supplement to the material sciences.

Psychology is a study of the nature of the knower and the knowing process—the mental half of animate life.

7. Applications.

The key to applied psychology is psychology.
The student does not really understand the mental process until he comprehends it in some of its actual settings and uses.

(*c*) *A little from each of all aspects of psychology and much from a few.*

1. Give a balanced general survey of the fundamentals, *i.e.*, give a comprehensive bird's eye view of the whole subject with reference to content, point of view, relations, etc.

2. Give intensive illustrations of representative facts—often choosing insignificant objects in order that the grasp may be the more complete.

II. METHOD.

(*A*) *Secure action.*

1. Keep the student doing things, instead of merely listening, reading, or seeing them done.

Fit the course to his capacity.
Make him feel responsible for every step that he takes.
Keep him working under pressure for accuracy and detail.
Make sure that he has the means for complying with every request.
Recognize results.
Even if he is to be entertained in the course, let it be most frequently by his own activity.

2. Let there be dramatic action in the progressive realization of stages in the course.

Use concrete illustrations from the living present.
Show the meaning of each new process with reference to the whole.
Make sure that the student can follow the development.

(*B*) *Be systematic.*

1. Keep a definite and detailed outline (printed or mimeographed)—of the whole course in the hands of each student.

This will aid in: (*a*) logical development, economy, and proper distribution of emphasis; (*b*) preparation and following, economically and effectively, a specific set of references; (*c*) the orientation of the student so that he may see each day the relation of each new process to what has gone before and what is to follow; (*d*) establishing confidence of the student in the teacher and in himself; and (*e*), in short, business method and mental economy.

2. Be reasonable and specific in all assignments and demand results.

Set your task so that it may be performed when the assignment is made, and so that you may have constant information about fidelity in work and the quality of results. The sophomore is an elementary student and psychology is necessarily somewhat abstract. Nearly every young teacher makes the mistake of treating this elementary class by the method of which he has become enamored as a graduate student. There should be a radical difference in the methods of the elementary course and following courses in psychology. It is absurd to treat the sophomore in psychology as a research student.
Insist upon the mastering of difficulties. One of the greatest wastes in college teaching is that we allow the student to shift when he encounters difficulty. Show him where the difficulty lies, spur him on, and hold him to the task.
Place responsibility.

3. Follow as far as possible the following order:
(*a*) From the simple to the complex; e. g., sensation, perception, memory, thinking.

The principle, "from whole to part" is recognized only for the purpose of orientation (see II. (*A*) 2, second note above) not for determining the order of topics.

(*b*) From the known to the unknown; e. g., from the common act of seeing, lead up to the attributes of sensation.

(*c*) The course in the stream of thought; e. g., impression, elaboration, expression.

(*C*) *Use methods which develop efficiency in introspection, observation, thinking and action.*

Methods must, of course, be adapted to the instructor, the equipment, the student, etc.; and all methods chosen must be pursued with sufficient continuity to secure solid work. Method without personal power is worthless. Among the methods available for our purpose are the following:

1. The text book. Text books should be the source of information and should be used very freely.

In the first year course of three hours, the student cannot afford to get along with less than three or four ordinary text books for his private use, in addition to library sets to which he has access.

(It is very doubtful economy for a student to spend a year on one or at most two text books, or to go on aimless hunt in original sources before he knows what he wants. Those "lecturers," who recite the contents of the book to the students in order to save them the price of the book, are too extravagant.)

The student can get more good in a course from extensive use of text books than from more specialized reference work, because the text is prepared specifically as an aid to the beginner. Limited reference work should however, be encouraged.

(All teachers who cannot find good text books should at once publish their own!)

We need a source-book to use as one of the texts.

The texts in a course should represent different points of view; e. g., the functional, the structural, the experimental, the physiological, and the genetic; and the student should live in the atmosphere of different authors.

Ordinarily, the text books should not be used as a basis for oral recitation; some other means of testing work must be found.

Nor should they be a constant object to be explained; let them be so used that they help to explain the object under examination—perhaps in the way we use an encyclopedia.

No matter what other means, such as lectures, experiments, discussion, etc., are used in the class hour, we need text books with all of them; the text book is or ought to be a most serviceable outline form of presenting the subject as a whole.

2. The lecture.

The lecture is one of the best means of personalizing the instruction, yet it is one of the most abused methods.

(There are three common sources of error in a teacher's evaluation of the lecture method: (1) the warmth which the lecturer feels over having made things clear: (2) the pleasure in freedom of expression and in hearing himself, and (3) the failure to note that he has done the thinking so well that the student gladly accepts his ready made portion without thinking.)

In the elementary course in psychology the mere information lecture should be tabooed.

Among the legitimate forms of lecture, for our purpose, are the following:

(*a*) The organizing lecture.

This may occupy all or part of the hour and may answer the following purposes: arousing in the student a point of view, interest, and ambition, by setting up specific objects for search; outlining relations within the work of the course; presenting supplementary ideas and concrete illustrations; and, first and last, to put life into the course.

(*b*) The demonstration lecture.

The class demonstration is an economic method, when brief, to the point, and systematically planned.

Do not make it an object in itself, but let the demonstration of a general principle in a specific case be such that the student may see the general principle and be able to make application of it in related cases.

(*c*) The special topic lecture.

This is the true "lecture" form; it may be set, finished and artistic. These lectures give vital touch to the study by focusing attention upon some single aspect of mental life in the concrete; e. g., the psychology of play, the formation of a habit, the psychology of writing, automatism, the evolution of consciousness, the evolution of a moral instinct, the meaning of infancy, fear, a case of alteration in personality, the psychology of Helen Keller, law in illusion, vantage grounds in the development of psychology, etc. Such lectures illuminate the study of psychology, arouse interest and give the student the genuine feeling that there is more of it, and that it is all worth while.

3. Experiment.

(*a*) The individual experiment without a laboratory. (See Recommendation (*b*) in Sec. VI of this report.)

This type of experiment enables the instructor to conduct work with classes of any size, without expensive equipment, without laboratory rooms, or much help. It saves the manifolding of equipment; frees the student from technicalities incidental to the manipulation of apparatus at a time when his energies need to be conserved for the grasping of the psychological problem; and saves time for the class period.

Secure the following conditions: (1) make the experiment intensive,—"One thing well;" (2) use only such apparatus as may be at the disposal of the student, or can be supplied freely.by the instructor; e. g., paper, pins, cards, corks, watches, etc.; (3) supply full and specific directions with the necessary preliminary statements for orientation; and (4) follow each set in the experiment with printed explanation, interpretation, further suggestions, etc.

These experiments may be conducted both as class exercises and home assignments; preferably, both combined.

They may be performed in the ordinary class room, provided the room is equipped with movable desk chairs. It is good economy for the instructor to provide for the whole class such material as may be needed.

(*b*) The class experiment. (See Recommendation *a* in Section VI) of this report.

The class experiment should comply with the following three principles: (1) every individual student shall take an active and responsible part in the experiment; (2) the experiment shall be sufficiently intensive to make it vital; and (3) each step in the experiment shall be explained and interpreted in print.

This is by far the most economical form of experiment, and psychology lends itself peculiarly well to this mode of treatment.

(*c*) The laboratory experiment.

The laboratory experiment should be employed very conservatively, if at all, in the elementary course, for the following reasons:

(1) The student is not ready for it. (We can begin physics with the technical experiment, because in physics the apparatus is the one object of experiment; but in the technical laboratory psychology, a student has two objects, namely, the mental process and the apparatus to be manipulated. He cannot do justice to both and is likely to lose himself in the apparatus.)

(2) The technical laboratory experiment is worth while only to those who are both capable and willing to take psychology seriously. (Most of the elementary students lack one or both of these qualifications.)

(3) Few, if any institutions, can supply adequate laboratory facilities for elementary classes

4. Written exercises.

(*a*) The written review.

Spend about one-fourth of the study time in preparation for reviews. Cramming is bad only when it is not done often enough.

One good plan is to give three or four questions a week in advance and require the student to prepare an outline of what he proposes to write on each of the topics. Make the topics such that it will be necessary to review all the work covered since the last review, and so as to make it necessary for the student to organize the material for himself in some form in which it has not been presented. On the review day, let each student write one hour on one of the three questions, following his outline, and hand in all of the outlines with the paper.

(*b*) The written recitation.

The advantages of the written recitation are: it encourages and secures systematic analysis of the test by the student when he is at ease in his room; it leaves the class-hour for lectures, demonstrations, experiments, and discussion; it secures full and specific recitation from every student every day; it develops logical presentation.

It is erroneous to think that the outline written recitation requires much writing. The best written recitation requires much thinking and very little writing.

(*c*) Themes.

Let the theme be clearly distinguished from a report on some author or book. Assign such topics as will favor a strictly psychological treatment in terms of the student's own observation and thinking, and presuppose assigned readings.

(*d*) Ten-minute tests.

These are an excellent substitute for the oral recitation if conducted with rigidity.

5. Special exercises.

(*a*) Problems in introspection.

Introduce new topics by assigning, as a preliminary exercise, some salient feature of the topic for introspection and report. Use the same means to verify general statements.

These exercises may be small experiments, or they may be simple unaided observations under specific instructions.

(*b*) Objective observation.

Set problems frequently in the objective observation of specific expressions of mental life, mental laws, conditions, causal relations, etc.

(*c*) Topical questions.

Prepare systematic and exhaustive questions as a basis for use as references in daily work, reviews, or even as a general skeleton or full system of assignment of work in the course,

(*d*) The topical outline.

This may be effective in the same way and for the same purposes as the topical questions, and has the advantage of brevity.

(*e*) Discussion.

With the teacher who has the genius to handle it, this is one of the most effective methods of teaching in classes of not more than twenty-five; but with the average teacher and the average class, it often becomes a waste of time—an abuse of privilege. An undeserved approbrium rests upon this method, because teachers who lack resourcefulness usually fall back upon it. Incidental discussion should be strongly encouraged.

(*f*) The conference section.

Here is where discussion has its most valuable place; small sections of the class meet with the instructor at regular intervals for that purpose. All sorts of supplementary demonstrations and reports may be introduced.

(*g*) Many other devices might be enumerated; such as reports on supplementary reading, weekly "communications" on psychological facts observed during the week, acting as observers in research under the instructor or an advanced student, taking part in statistical tests, individual conferences, etc.

III. PLACE IN THE CURRICULUM.

1. In the average American college or university, the elementary course in psychology should be taken in the sophomore year.

2. In the normal school it should be taken in the first year after the high school.

3. The elementary course should run three hours throughout the year, or five hours for one semester. (Or an equivalent.)

Among the reasons for this are the following:

(*a*) Psychology is a very large subject. It is at least as large as physics, chemistry, or mathematics. Yet, if a professor in a first class institution should offer a one-semester three-hour course as the first course in any of those subjects, it would be regarded with profound suspicion by an academic faculty. Psychology, when once established, will come to the basis of a solid first course.

(*b*) Short courses are necessarily schematic, in rapid outline form, and lacking in concrete illustrations and thorough experiments. The difference between the long and the short course is not only in quantity, but very essentially in quality.

Ordinarily, both professors and students apologize for the short course.

(*c*) Granted that the student should have a certain amount of psychology, the instructor can plan that much better in one course than the student can plan his work by choosing from elective semester courses.

(*d*) The short course represents an erroneous conception of the magnitude and worth of the subject.

(*e*) If the year is divided into two independent courses, students seldom elect the second; whereas, if it is all in one, they are generally pleased with it.

(*f*) All courses after the elementary should be specialized; e.g. comparative psychology, technical laboratory course, social psychology, etc., and the student needs a thorough foundation for these.

Within the college course, there is no excuse for giving first a general elementary course and later a general advanced course.

(The principal underlying the above arguments applies to the first course in philosophy with equal force.)

4. The course should, if possible, be preceded by a course in animal biology.

5. When the course is to be followed by some form of applied psychology, it is all important that the applied psychology, e. g., education or psychiatry, shall be built actually, consistently, and unmistakably upon the elementary course of psychology as given.

To the disgrace of both the theoretical and the applied courses it is often said by students that thcy see no relation between them. This correlation is a large problem in economy and efficiency.

IV. EQUIPMENT.

1. We must distinguish between the equipment for the elementary course and the technical laboratory equipment; the former may, however, often be drawn from the latter. It should consist of:

(*a*) Apparatus for the demonstration experiments.

This should be large and portable, suitable for exhibition rather than for fine work.

(*b*) Apparatus for the class experiments.

This should be accurate, adequate, and under perfect control. The projection lantern may be used to advantage in several experiments of this class.

(*c*) Supplies for distribution among the students in experiments which require improvised material.

(*d*) Equipment for a few "special topic" lectures which may be experimental.

This is usually drawn from the technical laboratory course or the research equipment.

(*e*) A full set of illustration material, such as physiological models, charts, pictures, etc.

All the illustration material that can be reduced to charts should be used in that way. Lantern slides should not be used when charts may answer the purpose.

2. The elementary lecture room should be a well ventilated and well lighted hall furnished with movable desk chairs, blackboards, chart cases, projection lantern, gas, electric power, etc.

Students should be encouraged to work in this room when carrying on experiments as outside assignments in order that they may get incidental aid and supervision.

3. A small shop with simple equipment is essential.

4. The library should be equipped with sets of books to which the whole class may be referred for certain chapters.

V. THE TEACHER.

1. The teacher of psychology, as compared with the teachers of other college subjects, needs an exceptionally thorough preparation.

The subject is usually taken up comparatively late. It is so large and its interrelations are so complicated that it takes a long time to acquire that knowledge about mental facts, that ability in introspection, and that technique in experimentation which is essential to the effective teacher.

Comparatively few, even of those who have completed three years of specialized graduate study in psychology, are successful with the elementary class in psychology when they begin. The elementary class seems to demand a certain amount of apprenticeship. Institutions can well afford to pay for the years of experience of a well trained psychologist.

2. Psychology is perhaps unequalled by other college subjects in its power to influence the life of the student; the introduction to this subject should, therefore, be taught by mature members of the department.

Young instructors can handle advanced work better than the elementary.

3. Pyschology perhaps suggests more unsolved problems than any other science; there is, therefore, a special demand upon practical ingenuity and philosophical insight.

4. "The teacher is everything."

In this there is a great truth. As we have learned to respect the individuality of the pupil, we must learn to regard the individuality of the teacher.

Aids, in the form of equipment, favorable support from the institution, knowledge about methods, etc., are always of minor importance

VI. SPECIAL RECOMMENDATIONS.

For furthering the advancement of the teaching of elementary psychology, the committee respectfully recommends the following:

(*a*) Conferences of teachers with a common interest; e.g., state conferences of teachers of psychology; east, south, central, and west conferences on a larger scale; conferences of teachers of psychology in the normal schools; conferences of the teachers of educational psychology etc. Several such organizations exist.

(*b*) The appointment of a committee on the "Class Experiment." (See II. *C*, 3, b, above.)

(*c*) The appointment of a committee on elementary experiments without laboratory apparatus. (See II. *C*, 3, a, above.)

NOTES ON THE DISCUSSION WHICH FOLLOWED THE PRESENTATION OF THE REPORT.

By Helen D. Cook.

Wellesley College.

In the discussion which followed, Professor Pillsbury held that the chief aim of the course should be not so much to teach the student to psychologize as to teach him a body of facts. (And Professor Seashore rejoined that without learning to psychologize the student could never know the psychic fact.) Professor Pillsbury dwelt also on the necessity of a physiological basis for psychology. Professor Kirkpatrick added that psychology should be a study not of consciousness but of behavior.

Professor Thorndike reinforced the Committee's emphasis on the necessity of assigning to the student questions for preliminary study and concrete problems for solution, as means by which to avoid a psychology "which uses words which nobody knows in order to state facts which everybody knows."

Professor Warren was unequivocally of the opinion that psychology is not advantageously studied before the junior year, whereas Professor Witmer recommended the teaching of psychology even to freshmen and to high school classes. Professor Witmer strongly urged, also, a course extending over at least two years.

With reference to normal school psychology, Professor Monroe emphasized the importance in educational psychology of biological material, urged the value of teachers' conferences; and claimed that it is too early to attempt to standardize the normal school course. Dr. Rowe set forth the difficulty of combining in one course the study of psychology with that of education. (Professor Seashore held that the psychology in such a course should be taught from the point of view of education.)

On the whole, all who took part in the discussion expressed a very hearty approval of the report.

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Some Mental Processes of the Rhesus Monkey

BY

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iii

CONTENTS.

Introduction[1]

Though one of the newest of the sciences, comparative psychology, or, speaking more strictly, animal psychology, may properly claim as its father that acute observer and many-sided Greek scientist and philosopher, Aristotle. The works of this pioneer in science show him to have been an interested and critical student of the mental, as well as the more strictly zoölogical (morphological and physiological) side of animal life, of which latter sciences (morphology and physiology) he is the acknowledged father. His observations on the comparative mental and moral traits of man and the lower animals, so striking when we consider the data he had at hand, may well entitle him to the credit of being the founder of comparative psychology.

Within the past century, the evolution and anecdote schools, represented preëminently by Darwin, Huxley, Romanes, Lubbock and their co-laborers, have contributed largely to this work and their observations and critical considerations have thrown much light on the mental capabilities of animals. These men, however, depended too much on a method we now believe to be of little value in comparative psychology, viz., observation uncontrolled by experiment, and their critical considerations have been colored because they were interested chiefly in the theoretical, evolutionary interpretation of their observations. This work, however, has been invaluable in that the broader questions have been set, and especially in that it has given great stimulus to the study of the animal mind.

It has remained for the new, the conservative, experimental, school of comparative psychology, to define more

[1] The animals used in this research were supplied by Prof. Shepherd Ivory Franz, of the George Washington University, through a grant to him from the Carnegie Institution of Washington. Acknowledgment is, therefore, made to the Carnegie Institution, without whose aid the work would not have been possible.

accurately the problems of the science, and by more accurate observations and by carefully controlled experiments to solve those problems. To this more scientific and growing body of workers, to Morgan and Hobhouse in England, to Forel, Bethe and Hatchet-Suplet on the Continent, and to Mills, Thorndike, Yerkes and Watson in America, must be paid the highest tribute.

As results of the first part of the work of this newer school distinct problems have been more precisely formulated by the investigators. These problems may be divided into two general classes, each of which may be resolved into a number of concrete special problems. The first general problem is: Do animals possess the lower mental powers that man possesses? Do they have the same fundamental psychic states that man has; and, if so, how do these states differ qualitatively and quantitatively from those of man? To be more concrete, we may ask: Do the lower animals discriminate sensory qualities, do they discriminate brightness of lights, do they discriminate colors or hues, noises, smells, and tactile stimuli? Do they form and inhibit habits, do they retain impressions and have the elements of memory? In what way, if at all, do these differ from similar mental powers of man? The second general problem is: Do the lower animals have the so-called higher powers? Have they ideas, have they the ability to learn by imitation, have they general notions and reason? From any psychological consideration we may omit the question of the presence of moral judgments, a subject which is chiefly of ethical and religious interest.

Despite both the newness and the difficulties of the problems and the imperfections of many of the present experimental methods of study in this field, much work has been accomplished and many valuable results have been obtained. The results are, however, yet too few, and some of those of more general interest have led to unsettled controversies. Some of the results which at present may be considered established are as follows: Some animals, the higher forms at least, discriminate brightness values. Some classes of animals discriminate pitch. All vertebrates and some invertebrates show an ability to form habits of reaction to stimuli, and they form at least simple associations. Animals retain impressions and have a memory of some sort. Even the most conservative and hypercritical have found this to be the only satisfactory interpretation of the results of experiments, and it is almost needless to say that the adherents of the so-called anecdote school admit a high degree of memory in all mammalia.

The question on which comparative psychologists are divided are as numerous as may be expected in a growing science. Those of most interest in connection with the present study may be briefly mentioned. Have animals the same sensory equipment as man? Have animals color vision? Or, do they merely appear to discriminate colors because of differences in the brightness values of the stimuli that are used? Do animals learn by inferential imitation? Have the lower animals ideas or mental images?

In answer to these questions animal psychologists have arrived at directly opposite conclusions, although all the recent investigators believe they follow the law of parsimony enunciated by Lloyd Morgan. In regard to the so-called higher mental powers, recent experimenters have taken a less decided stand than formerly. They cease to deny to animals reason, imitation and other similarly complex processes, but they say the case is not proven and demand additional experiments and observations.

Among experimenters on animals Hobhouse is almost alone in claiming that animals have true general notions. It is true he does this after having made rather important qualifications of the term 'idea.' The latter topic has, however, not been the subject of many experiments or, rather, it has not been the subject of many reported experiments and observations other than a few illy-controlled ones. The question of the ability of animals to reason has been the subject of numerous tests; but the casual observations of the anecdote psychologists are the mainstay of those who support the view that animals show ability to reason. Hobhouse has made some experiments which he believes indicate some power or inference in monkeys and apes, but the observations that have been made by others of the group of experimental animal psychologists are in the main of a decidedly negative character. With the exception of Hobhouse, it may be said that the consensus of opinion is that the presence in animals of this so-called higher faculty is not proven.

These points we may summarize by saying that at present comparative psychologists admit the possession by animals of most of the lower powers of intelligence that man possesses. Such powers of animals may, however, differ from those of man quantitatively and in a qualitative manner. Possession by animals of the so-called higher powers is an open question, or set of questions, which have not been so widely studied, nor so clearly determined; and, respecting which the available evidence points toward a negative answer in the form of a verdict of 'Not proven.'

On account of the ease of obtaining the domesticated animals a large amount of the experimental work on animal behavior has been performed upon cats, dogs and chicks. On the other hand, the lower forms (invertebrates and non-mammalian vertebrates) have been extensively studied in respect to the simplest powers, such as tropisms. On account of expense, difficulties of care and the apparent complexity and variety of behavior, the higher mammalian types, especially anthropoids, have not been studied so consistently.

However, for studies in comparative psychology the value of using primates instead of the lower vertebrates and invertebrates is almost self-evident. On the one hand, the anatomical similarities between man and the monkey are apparent. There is the well-known similarity between both their peripheral and central nervous systems; and the similarity of arrangement of muscles and bones, especially of the extremities, must not be forgotten. Observations have made it evident that there are also similarities in a physiological way. Movements and reactions of an apparently human type are known to be present in these higher forms, which are difficult if not impossible to observe in the lower forms. We may therefore expect from the careful studies of the mental states of those animals, admitted to be immediately inferior to that of man, more light on certain problems in the psychology of human consciousness than from similar studies of others. In making such a comparison of values it is not intended in any way to detract from or to belittle the studies on the lower forms. The value of experiments with the lower orders is everywhere admitted. But at present it must also be admitted that the latter have received the attention of many more writers, and their reactions have been subjected to a much finer analysis.

Experimental

The brief historical summary of comparative psychology which we have given will, perhaps, appear as a sufficient justification for undertaking further study in the field; both in regard to the lower and to the higher faculties in the animal mind. This study of the psychology of monkeys was undertaken, therefore, with a view to add to the knowledge we now possess of the lower faculties in the animal mind, and if possible, to throw some light upon the question of whether or not monkeys possess some of the higher mental powers, and also to limit or characterize any such higher powers, if they should be found. The experiments have been much too incomplete to be entirely satisfactory to me, and, in many cases, the results are naturally lacking in definite conclusions, though they yielded, to me at least, some valuable indications in the field. It must be said, however, that the unsatisfactoriness and the indefiniteness are not peculiar to this particular study, but that they apply to any work that is performed in such a complex science. In certain of the later experiments, it will be noted, definite conclusions have been arrived at, and, had time permitted the carrying out of further experiments, it seems probable that other tests might have been concluded in as definite a way.

As tests of some of the so-called lower faculties of the monkey's intelligence, experiments were made on brightness discrimination, color discrimination, auditory discrimination, the formation and inhibition of habits, and retentive power (memory). In regard to the higher powers, observations and experiments were made on learning by imitation, on ideation, on reasoning, on adaptive intelligence and on general notions. In brief, the chief aim of the study has been, while profiting by the work of preceding investigators, and with a steady adherence to the law of parsimony, to study some of the lower powers of the animal mind, but especially by a modest study of the higher powers to seek for some light, however little, on the subject of mental evolution.

Eleven monkeys were used in this work. All were Rhesus (Macacus) obtained soon after their importation from India. So far as we could ascertain they were eight to nine months old when received and, with the possible exception of monkey 2, they appeared to be without training of any kind. Monkey 1 was a rather large, spare male, moderately active and bright looking; in his work he showed only medium capacity; when he could not perform an act and thus get food he showed signs of anger, by jumping about and by shaking the wire sides of the cage; he would sometimes jump at the experimenter and visitors who happened to be in the room, but it must be remembered he was in a cage and could not get at anyone; the movements he made were those of intimidation, so often noticed in the Rhesus and other Macacque monkeys; towards the end of the series of experiments he became cross. Monkey 2 was a medium-sized male: moderately active and of only ordinarily intelligent appearance; he showed a mediocre capacity in work; not friendly; he wore a collar about his neck when received and this would indicate that he may have been accustomed to handling by sailors or by others who previously owned him. Monkey 3 was a small female, apparently the youngest of the eleven, very active, alert; good-natured and friendly for a Rhesus; formed associations the quickest of all the animals. Monkey 4 was a medium-sized male, not very active, but bright and cunning looking; good-natured; jumped at experimenter half playfully; of medium capability in his work. Monkey 5 was a large male, dull looking, not active; slow in learning as compared with all the others; he had a way of whining when left alone; was rather ill-natured; would get angry when he failed to do a required act, and thus fail to get food. Monkey 6 was a small female; bright and active; good-natured; very cautious in all her work and actions. Monkey 7 was a large female; inactive, not very bright looking, but the most friendly of the eleven animals; proved to be quick in forming habits; very quiet. Monkey 8 was a large male; he was the master of the band; of gruff appearance, domineering, active; of not especially intelligent appearance; but was extremely cautious, appeared to like to go through the tests. Monkey 9 was a medium large female; rather friendly; quick to form habits. Monkey 10 was a large female; not active; quite friendly; fairly capable as indicated in the tests. Monkey 11 was a large male; wise looking; quiet and friendly; somewhat timid; of medium capability.

Formation of habits.

Releasing fastenings: Since the appearance of Thorndike's *Animal Intelligence* in 1898 the puzzle box method has been a favorite one for comparative psychologists in studying the formation of associations[1] by the mammalia. Partly as preliminary to succeeding work on visual discrimination, an experiment similar to those of Thorndike was made with monkeys 1, 2 and 3.

A box 2 feet long, 7 inches wide and 8 inches deep was made with wooden slats and attached to one end of the cage in which the three animals were kept. A solid wooden door, 6 by 4 inches, was arranged on each side of the inner or partition part of this box nearest to the cage. These doors had vertical hinges and were kept shut by a turn button on the side away from the animals. The hinged portions were 16 inches, the opening or button portions 8 inches apart. A space was left on the sides of the doors next to the button sufficiently wide to enable a monkey to put his hand through. The doors opened inward into the feed box, *i.e.*, outward from the monkey cage. The food was usually so placed on the floor of the food box that the animal could not see it, except by going to the extreme right or left, depending upon

[1] The term 'association' as applied to the intelligence of animals has been loosely used. Two principal usages are: (*a*) the animal's simple cerebral (or mental) processes (merely anatomical, or it may be, physiological) such as is shown in a simple adjustment to a stimulus, *e.g.*, in its learning to turn a button to open a door; (*b*) as explaining what are thought by some psychologists to be higher mental processes, *e.g.*, ideation or reasoning. When used in this paper the word is to be understood to have a connotation similar to the first meaning given above.

the box into which the food was placed. In all experiments, even in those in which no time is recorded in the paper, the times for the performance of acts were taken by a stop watch and recorded.

By reaching through the space or crack at the side of the door and turning the button, the monkey could open the door and secure the food which was placed in the food box behind one of the doors. In the beginning of the work only one of the doors was used; when the association of turning the button, *i.e.*, of opening one door, was formed, both doors were used. In the latter (two-door) preliminary experiments the food was placed in a chance order behind one of the doors, the animal not knowing which door that might be.

The records of the three animals used in this first experiment are as follows: On the fourth day, after 32 trials, monkey 1 gave evidence of the formation of the habit of opening the door. Previous to this trial he had shown many random movements, going from one part of the cage to another, getting upon a bench in the cage, stopping all work for a time, etc. On the fourth day, however, the habit of going to the door directly was apparently formed, and the method of opening was that of vigorously shaking the door so that the button was gradually moved to permit the opening of the door. On the fifth day, after 53 trials in all, he began to fumble with the button in addition to shaking the door. He continued to use both of these means with an increasing tendency to rely on the button alone. On the sixth day, after 95 trials in all, he had formed the association of opening the doors by turning the buttons alone. His average time for 10 succeeding trials on this day was 3 seconds. Monkey 2 formed the association of opening the door in much the same manner as Monkey 1. He pulled and shook it, and on the third day, after 21 trials in all, managed to get the trick of always opening it in this manner. On the fourth day, after 51 trials in all, he began to turn the button in addition to pulling and shaking the door. Also similarly to the actions of Number 1, he continued to use one or both of the means, with an increasing tendency to turn the button alone. On the sixth day, after 102 trials, he always used the button alone as the means of opening the door. His average time for ten successive trials was 1.5 seconds. Number 3 on the second day, and after 11 trials in all, formed the association of opening the door by pulling and pressing it. On the third day, after 34 trials in all, she began to turn the button. On the fourth day, after 64 trials in all, she appeared to have perfected the latter association and had inhibited all tendency to use other means (*e.g.*, shaking) for opening the door. Her average time in the last 10 successive trials on this day was approximtaely 1.5 seconds.

The method of learning in this experiment appeared to be of the 'trial and error' type. As has been mentioned, the animals scrambled about in a general way at first; they pulled and bit at the door and the adjacent parts of the partition; shook the door violently; but, *it is to be noted that after the first few experiments the attention of the animal was always directed to the door*. Even after an accidental success in turning the button, the association was not at once set, but only after a number of trials, with accidental successes many times repeated, alternating, or rather interspersed, with many errors. All the animals used in this work appeared, however, to recognize a chance success and profited by it more quickly than did raccoons under similar conditions. Another thing of particular notice in their work, in which respect also they differed from the raccoons, was their tendency to show signs of anger when they could not open the door and get the desired food. This was especially noticeable with Monkey 1.

It is also of interest to note that when, preparatory to another trial, I attempted to close the door after an animal had opened the door and obtained food, the animal would repeatedly attempt to hold the door open, as if the idea was present that 'door-being-open' meant food. This reaction is somewhat similar, I judge, to one observed by Thorndike in his experiments with cats. It will be remembered this author found a tendency on the part of the animals to walk into the opened cage from which they had just previously escaped to obtain food. The action, on the other hand, may be merely of the nature of a *reflex*.

Visual discrimination. It was formerly supposed, or rather taken for granted, that animals possess the power of discrimination of visual qualities, hues, and saturations. This assumption was based upon observations of general behavior, of reactions to objects of different color, etc., but in the past few years, however, comparative psychologists have concluded that such power of discrimination could not be assumed and they have sought by carefully controlled experiments to test the truth of the matter. While the experiments already made have yielded many valuable and apparently positive results, several investigators, among whom may be mentioned Yerkes (4) and Watson (2), await more proof of such discrimination ability. The problem has been resolved into two questions. Is there a discrimination by animals of brightnesses or intensities? Do animals discriminate objects by their color or hue qualities? In the work on visual discrimination to be recorded in this paper nine monkeys were tested for color discrimination and six for brightness (or intensity) discrimination.

BRIGHTNESS DISCRIMINATION—SIMULTANEOUS EXPOSURES: Some experiments on monkeys 1, 2, and 3 were made with the same apparatus that was employed in the preliminary experiments on the releasing of fastenings, with the addition of two cards, respectively black and white, which were placed above the doors. The cards were 5 inches square, and were placed above the doors. They were, therefore, 8 inches apart. A piece of food, usually a half or a whole peanut was dropped in the food box; the black card was placed above the door behind which the food had been placed, and the white card above the other door. The monkey was to open the door under the black card and obtain the food. The food was placed in the compartments in an irregular order, and the cards were correspondingly placed. At times the food was not placed in the box until the door had been opened by the animal, but often it was dropped in the box immediately before or after the buttons were adjusted. In this way it was possible to prevent the animal knowing which door to open from the sight of food, and since raw peanuts were used, the smell component, judging at least from human ability to smell such food was at a minimum.

These experiments were begun December 4th and continued for eleven weeks. Each animal was given from 10 to 25 trials every second or third day. In some cases there were longer intervals between the work periods. Each animal was given a total of 1000 trials. At the end of the work all were substantially perfect in opening the door under the black card *first*. No animal was ever able to inhibit altogether the tendency to open the door under the white card after the door under the black card had been opened and the food had been secured. The average time for opening the door under the black card in the last 25 trials for these three monkeys was approximately one second. The curves of learning, *i.e.*, the time-experiment curves, correspond to those found by almost all experimenters, but on account of mistakes in opening the second door they cannot have much value in this connection.

The fact that the animals formed the habit of opening the door under black first would indicate that they discriminated the cards. The quickness with which they finally performed the act would also confirm this conclusion. Moreover their looks and actions at the time appeared to indicate a knowledge that the opened door meant food. When, for example, food had been not placed in the box, after opening the door they would look at me as if they expected their reward. The closeness of the door buttons, and the great activity and ready use of the hands, explain, I believe, the opening of the second door so often. Furthermore, it was noted that while after they had opened the door under the black card, they appeared to expect food, when the door under white was opened, their actions did not indicate that they expected to be fed.

BRIGHTNESS—SUCCESSIVE EXPOSURES. For the tests with monkeys 4, 5, and 6, I used a card displayer similar to that used by Professor Cole and myself in brightness and color tests made on raccoons. This was placed outside and about 6 inches from the animal cage. The accompanying figure is a diagram of the experimenter's view of this piece of apparatus. (See Figure 1.)

The front of the displayer was formed by a board 12 inches high. A pin, 'P', on which two levers could be turned, was inserted in a hole at the back and near the lower edge. On this pin two displayers, 'W' and 'B', were arranged so as to be freely movable in the plane of the board. The two cards, black and white, were placed at the upper end of the displayers so that the raising of the displayers showed the black and white cards respectively. The card displayers were 1-inch thick so that there was a difference of 1 inch in the distances of the cards from the animal box. On alternate

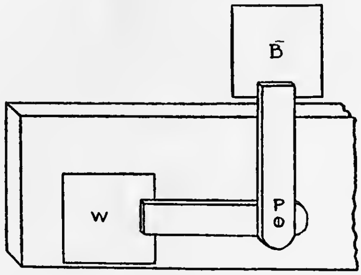

FIG. 1

days, however, the cards were changed from the front to the back lever, so the animals should not react merely to the distance position of a card. When the black was exhibited the animal was to go upon a platform arranged inside the cage and was fed, while he was not to go up at white and was not fed. Usually and except at the stimulus, the animal sat on a bench 12 inches from the floor and inside the cage. He was, therefore, in a position to look down at the card apparatus, for the upper portion of the card was about 5 inches below the horizontal level of his eyes. When the black card was displayed and the monkey had climbed to the food platform the experimenter rose from his seat and presented a piece of food to the animal. It is of interest to note, therefore, that there was no possibility of the formation of an association between the smell of food and food, rather than between the appearance of the card and food. Since the food was not placed near the cage or near the food platform until the appropriate response was obtained the animal had no smell stimulus for its guidance to an appropriate reaction.

These experiments were begun February 5th and continued for a period of seven weeks. Each animal was given from 10 to 25 trials daily, with intervals of two days, in some cases of three or four days, during which intervals no work was done with them on brightness. In all, each was given 700 trials. At the close of this work monkeys 4 and 6 appeared to have the association perfected. The average times for responding by going upon the platform when black was displayed in the last 25 trials were 1.5 seconds for monkey 4 and 1.25 seconds for monkey 6. The tendency to climb to the food platform when white was displayed appeared to be entirely inhibited. Monkey 4 was, in this regard, perfect in the last 25 trials, and monkey 6 responded to white only once in the same number of trials. Moreover, at the time of this mistake it was noted that in the trial (the 7th) in which she responded to white, monkey 6 immediately came down from the food platform with all the appearance of having knowledge that she had made a mistake; she did not appear to expect food and did not remain upon the platform for a sufficient time to have food presented to her.

Monkey 5, at the end of the 700 tests, appeared to be almost perfect. In responding to black he made only one error in the last 25 trials. His inhibition of the tendency to respond to white was not quite perfect, for during the same series he responded to the white card three times. His errors in this latter regard, however, were rather of the type shown by monkey 6. He showed by his actions that he knew he had made a mistake, for without waiting to be fed he immediately returned from the food platform to the bench.

In the later experiments, and probably for some time previous to the ones just mentioned, the movement of the card displayer appeared to set up in the animals a tendency to react, and the mistakes that were made on the last day are, in my opinion, undoubtedly due to the lack of inhibition of this reflex tendency to movement rather than to mistakes in ability to discriminate.

For the next tests in this series small pieces (about one-half inch cube) of white and rye bread were presented simultaneously on a board placed outside the cage but within reach of the animal. The breads were prepared so that the taking of one resulted in a punishment and in this way an attempt was made to have the association formed quickly. The rye bread was soaked in a solution of quinine bisulphate (about 1 per cent, although no attempt was made to keep the bitterness a constant factor). The white bread contained no quinine and was presented in a comparatively moist, fresh state, but much dryer than the rye bread that had been soaked in the solution of quinine immediately before the experiments. By taking the white bread and avoiding the rye bread the animals were to show their ability to discriminate these brightnesses.[1] In these experiments and in later ones to be reported it was assumed that monkeys disliked bitter tastes; this assumption, it will be noted, proved to be true.

To obviate the objection that the smell of the quinine or the rye bread might enable the animal to differentiate the white from the rye bread, the two pieces of bread were placed in varying positions on the board. At times they were placed at equal distances from the front of the cage and within an inch of each other. In other tests one was placed directly in front of the other, and in other tests the positions were irregularly varied, one being nearer, the other farther from the cage. It might still be objected that the rye bread which was wet from having been soaked in the solution of quinine would give them a clue. That this objection was not met in the conduct of the experiments must be admitted, but the formation of the association, *i.e.*, the positive reaction would give evidence of sharp visual discrimination.

[1] In addition to its darker appearance, there was a hue difference in the rye bread. The latter, however, is the less prominent factor.

Nine animals were tested by this method, 1, 2, 3, 4, 5, 6 7, 8 and 9. This plan of tests was very gratifying in its results. It showed, apparently, complete discrimination of the two pieces of bread. The rapidity with which the habit of taking only the white bread was formed was striking. This is especially noticeable when we compare the records of the same monkeys in this and in the preceding brightness tests. As table I shows, only from 1 to 14 trials were required for each of the eight monkeys to establish the association. This is excluding the work of monkey 9, which also discriminated the white and rye bread, but which, being in the same cage with monkey 8, could have seen the latter select the white and reject the rye, and might be said to have learned from or to have imitated monkey 8.

TABLE I.

Discrimination of white and rye breads. Twenty-five experiments on each animal.

Animals.	Mistakes in Trials		Total Mistakes.
	First Day.	Second Day.	
1	1, 2*a*, 4*b*	12*c*	4
2	1, 2*a*, 4*a*, 5, 6, 8	14*d*	7
3	1, 2*a*, 3, 4*b*, 6*b*, 8*b*, 12*a*	13*c*	8
4	1, 3*a*		2
5	1, 2*a*		2
6	1, 2*a*		2
7	1, 2*b*, 3*a*, 4*a*		4
8	1		1
9			

a Took rye bread after having taken white, smelled or tasted, or both, and dropped without eating.

b Took rye bread after having taken white, and ate both.

c First trial at second day; took rye bread but dropped it immediately without smelling or tasting.

d Second trial of second day; without smelling or tasting.

It is of some interest to note the individual reactions to the two stimuli that were presented. At the first trial each animal took both pieces of bread, placed them in the mouth and began to eat. In many cases the bitterness had a retarding or inhibiting effect, for the animal would take the rye bread from the mouth, look at it, smell it, and then either reject or reinsert it in the mouth.

Monkey 1, in addition to eating the rye bread on the first trial, took the piece of rye bread in the second trial, smelled, tasted and dropped it. The bitter bread was not taken on the third trial, but on the fourth the animal took first the white which it ate, then the rye, which it also ate. The rye bread was not taken on the next seven trials on that day. On the twelfth test (the first test on the second day) the animal first took the white bread and ate it, then took the rye bread and, without smelling or tasting, dropped it immediately. On this and on the following days all other tests were perfect in that only the white bread was taken and the rye bread not even handled, although, as has previously been noted, at times the rye bread was placed closer to the cage than the white bread.

Similar results were obtained with animals 2 and 3 as is indicated in the table. The rapidity of learning is remarkable in all animals, but particularly so in 4, 5, 6, 8 and 9. In each of these animals two tests were sufficient to inhibit altogether the tendency to take the darker bread. This finding is so at variance with the results of comparable tests on other animals that there would be ample opportunity to use the results as an indication in monkeys of some form of reasoning or of a marked activity to form practical judgments.

COLOR DISCRIMINATION—WHITE AND RED. The same general plan was followed as in the white and rye bread tests. Rice, cooked to such a consistency as to be stiff, was, however, used instead of bread. It was cut into small pieces of approximately the same size. Some of these pieces were used for the white stimulus, and others when colored with Congo red for the red. The red pieces were soaked in a quinine solution and used while wet. The white contained no quinine and was in a comparatively dry state. The precautions noted in the previous test were taken to prevent an animal obtaining a clue from either the relative positions or the smell of the pieces of food.

The nine monkeys which had been used in the white and rye bread experiments were tested in this experiment. The rapid learning to take the white rice and to reject the red rice, as in the white and rye bread test, was notable. From 1 to 3 trials were sufficient for the different animals to form the association. Animal 1 took the red rice only the first trial, and a similar result was obtained with monkeys 2, 4, 5, 6, 7, 8 and 9. Monkey 3 took the red rice only three times, on the first, second and fifth trials. Not only is the rapidity of the formation as striking here as in the case of the white and rye bread, but the fact that all but one of the animals formed the association, or, rather, inhibited the tendency to take the red rice, after having experienced its effect only once, may be taken as an indication of some form of reasoning.

Color discrimination—pink and green. A plan was followed in the next test similar to that employed in the white and red experiment. A smaller amount of coloring matter was used and some of the rice was colored a light pink. The pink rice also contained quinine. Another portion of rice was colored green, and this portion contained no quinine. The pink coloring was obtained, as has been said, by using a weak solution of Congo red, the green was made by using a solution of Malachite green. Both kinds of rice were used while wet, and in this experiment, it is not probable that discrimination could take place on account of difference in the light reflecting qualities of the two kinds of rice. Care was taken to have the two colors as nearly as possible of the same approximate brightness. To this end the two colors were mixed and compared to the colors pink and green of the same approximate brightnesss on a color scale and the mixed colors as well as those of the color scale were also tested by the minimal perception method in a dark room to insure their being of approximately the same relative brightness (*i.e.*, to the human eye).

In mixing the rice the coloring materials were not measured, nor was the relative amount in proportion to the water of the solutions kept constant, nor the relative proportions of the two colors to each other, nor to the amount of rice. The color solutions were usually made up anew each day and there was inevitably a greater or less variation in saturation and in brightness of the colors in different tests. These remarks hold true in all the color tests. While criticism may be made on any apparent discrimination on the basis of hue under such conditions, to the writer discrimination of color would be indicated by proper reaction from the very fact of the variation of intensities. This will be pointed out again in the discussion of the color experiments. As in the preceding tests the same control precautions were taken to prevent the animals from taking a cue either from the position or from the smell of the rice.

In this test the monkeys formed the habit of taking the green and rejecting the pink with even greater rapidity than in the white-red test. Table II gives the records of nine animals which were tested.

COLOR DISCRIMINATION—PINK AND YELLOW. In this test the same plan was followed as in the preceding experiments. The pieces of pink rice contained quinine, while the yellow contained no quinine. As in the preceding tests, both the pink pieces and the yellow pieces were used while wet. The pink coloring was produced by the same means as in the last mentioned experiment, while the coloring of the yellow pieces of rice was produced by the addition of lead chromate. The test was controlled by the same means as in the pink-green experiment to secure the same approximate degree of brightness in the colors pink and yellow, and to prevent the animal from receiving any cue from either the smell of the two pieces of rice, or from their relative positions or relative distances. The same animals were used as in the preceding tests, monkeys 1, 2, 3, 4, 5, 6, 7, 8 and 9.

[1] The colors were mixed and compared to the colors pink, yellow, green of the same approximate brightness on a color scale, and the mixed colors on the color scale tested in a dark room for quality of brightness. In the different experiments, the pieces of rice were also placed at varying distances from the animal to prevent his taking a cue either from the smell or the positions of the two pieces.

The following is the record for the animals for 25 trials each:

No. 1	took yellow in each trial; took pink trial	1
2		9
3		none
4		none
5		1
6		none
7		none
8		none
9		none

COLOR DISCRIMINATION—PINK, YELLOW AND GREEN. The same method was employed in this experiment as in the preceding ones. In this case the three colored pieces of rice were simultaneously exposed. The pink and yellow contained quinine, the green contained no quinine. The

TABLE II.

Discrimination of pink and green rice. Twenty experiments on each animal.

MONKEYS.	MISTAKES IN TRIALS.	TOTAL MISTAKES.
1	3	1
2	3	1
3	1	1
4	0	0
5	0	0
6	0	0
7	1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16,	14
8	0	0

coloring for the pink, yellow and green was the same as used in the preceding tests, each was used while wet. Exactly the same means to control the tests were employed as above. The same monkeys were tested as in the white and rye bread test, the white-red, the pink-green, and the pink-yellow experiments. Here again the rapidity of the rise of the habit of response is striking. Table III gives the record of each monkey for 25 trials.

TABLE III.

Discrimination of pink, yellow and green rice. Twenty-five experiments each animal. Ten experiments first day, fifteen on second day. y, took yellow; p, took pink.

MONKEYS.	MISTAKES IN TRIALS.	TOTAL MISTAKES.
1	1 y,	1
2	1 y,	1
3	1 y, 11y	2
4	1 y	1
5	1 y, 2 y, 3 y, 4 y, 11 yp, 12 y	6
6	1 y, 2 y, 3 y, 4 y, 5 y, 6 y, 11 y, 12 y, 13 y, 14 y,	10
7	1 y, 2 y, 3 y, 4 y, 5 y, 6 y, 7 y, 8 y, 9 y, 10 y, 11 y, 12 y, 13 y, 16 y, 17 y, 18 y,	16
8	1 y, 2 y, 3 y, 4 y, 5 y, 6 y, 7 y 11 y, 12 y, 13 y,	10

We see from the records and tables that the nine animals quickly learned to select the rice that contained no quinine and to leave the quinine rice alone. The learning to respond quickly was much more rapid than, so far as I am aware, that of any other monkeys which have been studied for visual discrimination. In many cases, one trial was sufficient to inhibit any tendency to take the quinine rice, and the rapidity in inhibition of the wrong response was undoubtedly due to the use of a punishment or to the association of a disagreeable sensation, *i.e.*, to the quinine in the rice.

Another factor in the rapid formation of the habit of avoiding the quinine rice, in the case of pink in the pink-green rest, was probably the knowledge or memory of disagreeableness in red rice in the white-red test immediately preceding. In the pink-yellow test and in the pink-yellow-green test, knowledge of quinine in pink in the preceding tests, was also probably a factor in their rapid discrimination. In the pink-yellow-green test the experience of the preceding tests of pink-bitter, yellow-good, green-good helps to explain the number of mistakes in yellow rice. But, with all these allowances, we still have evidence of rapid formation of the habits of selecting one color and of rejecting another, and thereby apparently discriminating the colors red, pink, yellow and green. That discrimination has taken place cannot be doubted, but the question arises: did the animals discriminate the colors as hues, or as merely brightness values?

Watson (2), in very carefully controlled tests made on three monkeys in 1908, failed to find evidence satisfactory to him that his animals discriminated colors as hues. From later tests (3) also, on monkeys, he is still not prepared to affirm whether such discrimination is of color or of mere brightness. Yerkes (4), from the results of his well-controlled experiments on the dancing-mouse, takes a view of the matter similar to that of Watson. Both condemn the use of cards, filters, etc., in experimenting on color discrimination. They believe such methods are too loose to have much value and they urge the use of more exact methods of determining the matter.

In the experiments on color discrimination reported in this paper, as has already been stated, the criticism that the method used is a loose one may be urged. The exact proportion of the different coloring materials to the bread or rice and to the amount of water in making the solutions was not determined. Furthermore, it may be said that as the coloring solutions were mixed anew almost daily, it would hardly be possible not to have some variation in the intensities of the colors at different times. This is true, notwithstanding that means were taken to control the experiments, viz., comparison with certain color standards. Shall we infer that the experiments were thereby so vitiated as to have little or no value as a test of color discrimination? Such will doubtless be the view of some, at least on first thought, but, to the writer, these inaccuracies in method point to a different conclusion.

It is admitted that the method and apparatus used in the experiments herein reported appear loose and crude when compared to the ingenious methods and complicated apparatus used in the experiments of Watson and others. It may also be urged that my experiments take no adequate account of the question of 'monochromatic bands' in the problem of color vision. To this we may reply that in the very looseness and naturalness of the tests is to be found perhaps the strongest evidence of the discrimination of color by the animals which I tested. On the other hand I would point to the highly artificial character of the methods used by some experimenters. The complication of apparatus with its reflectors, electric shocks and other appendages is artificial in the extreme and and must result in an artificial attitude on the part of the animal. The tests, with complicated apparatus, conducted in a dark room, bring about another artificial and unusual situation, viz., the necessity for dark adaptation by the animal. The method employed in these experiments leaves to the animal a large amount of freedom and places the animal in a position as natural as is possible in such work. If, as has been urged by some, the experiments with colored cards and filters may only mean that the animals react to brightness or intensity, and not to hue relations, we should expect an animal to react to a definite relation of brightness unless we admit in animals some complex form of the feeling of relation or a certain amount of inferential reasoning. Such an explanation (*i.e.*, brightness discrimination) may be justified when the red-white test be considered alone. But how may we explain the results in the pink-green test? Half of the animals used in the latter test made no mistakes. From the beginning of this experiment pink rice was avoided, although it is not possible that the animals had any experience with red or pink rice previous to the time of these experiments. Four of the animals, therefore, reacted properly to a hue of an intensity or brightness, considered from the human standpoint, very different from that to which the animals had learned to react. Do not these positive reactions indicate rather clearly that somehow or in some way the pink rice has been taken to be equivalent to or mean some thing similar to red rice? Do not these results rather lead to the conclusion that it is not a difference in brightness or in intensity which has led to the appropriate adjustment, but rather a difference in hue? Furthermore, the fact that in the pink-green test the animals took the green and avoided the pink regardless of the difference in the amount of color is evidence that color as such was an important, and probably the only, factor in the discrimination. This view receives added weight from the similar results under similar conditions (of approximate brightness and color) in the pink-green-yellow tests.

It may, however, be urged that the carrying over of a habit from a red to a pink indicates merely a dullness in discrimination, that the red and pink may have been sensed or perceived as approximate equivalents. It must be admitted that this may be so,. but the wide differences in the intensity of the red and pink would indicate rather clearly that the discrimination (or, comparison, if you will) has been due not to simple intensity relations but to hue or color similarities. For the discussion of the results in the pink-green tests, we may make two assumptions, that the two kinds of rice were of equal or unequal brightness. If we assume an equal brightness, the experiments must, it seems to me, be considered to show that discrimination has taken place because of difference in hue. If we assume the two kinds of colored rice to have been of unequal brightnesses, we must, remembering the variations in the experiments on different days, consider that the discrimination has taken place in spite of this variation. The results then lead to the conclusion that the discrimination has been due to a factor different to that of intensity on the physical side, or that of brightness on the mental. The only other factor which, in man, would produce such a reaction, is that of color.

A comparison of the results obtained in the different color tests strengthens this view. The consideration of the results in the white-red, pink-green and pink-yellow tests is instructive. Monkeys 4, 5 and 8 which had one experience in tasting bitter red rice did not take the pink rice in the succeeding tests. Now, it must be remembered that these monkeys had no previous experience with pink rice, and their avoidance of it can be accounted for only on the assumption that something in the pink rice gave a clue to the animal.

The differences (to us humans, of course) between the red and the white rice are three-fold: color, intensity, and, akin to the latter, reflecting quality or sheen. The last named was due to the differences in moisture, the red rice having been soaked in the quinine solution only a few minutes before the tests were begun. The sheen of the pink, green and yellow rice was the same, for all were equally moist at the time of the experiments. There were, therefore, only two possible differences between these colors, viz., hue and intensity.

If the avoidance of pink is to be explained, we must admit that it was due either to hue or to intensity. If it were due to a feeling of intensity difference, to an inference (taking the human standpoint, of course) that darkness means bitter and lightness means sweet we must account for a transfer from the red (very dark) to the pink (slightly dark). We must also account for the transfer in connection with the other stimuli (green) which from tests was found to have an approximate intensity equal to that of pink. So far as I can see at present, the only possible intensity explanation of this transfer is that red has a low brightness effect in comparison with other colors. However, we know that the intensities of the red and the pink rice varied greatly from each other, and we are forced to account for the phenomenon of transfer on the basis of some quality not so markedly changed. This, I believe, is the hue.

To sum up we may say that the evidence is in favor of the conclusion that monkeys have the ability to discriminate colors as such. So far as is known of the structure of the retina and of the remainder of the visual apparatus there is nothing to indicate any difference to the human mechanism, and, from this point of view, no reason exists why the monkey family may not be able to differentiate the four colors red, green, yellow and blue. The rapidity of formation of the association between the light bread and agreeableness and that between the dark bread (rye) and disagreeableness, as evidenced by their selection of the former and their avoidance of the latter, is indicative of sharp visual (brightness or intensity) discrimination. The selection of one color and the rejection of another in spite of differences in intensities speaks strongly for the discrimination of hue. The transfer of the habit from red to pink is indicative that the animals had some form of feeling of relation or of similarity between the colors as such, or that the hue (in itself and independent of intensity) acted as the appropriate stimulus to inhibit the tendency to take the particular food.

Auditory Discrimination—Noise. The apparatus used was a wooden box 22 x 18 x 10 inches and a small board or slat 18 x 3½ x ⅝ inches arranged to strike the box and thus make a noise. One end of the board or slat was fastened to the top of the box by a leather hinge. By raising the free end of the slat and suddenly letting it go, it struck the top of the box and made a sound varying in loudness with the force with which it struck. To give two sounds of different degrees of intensity or loudness two small sticks, one 3 inches in length, the other 5 inches in length, were separately used to be placed perpendicular to the box and under the free end of the board. By pressing slightly on the slat near the hinge, and suddenly removing the shorter stick, the board would strike the box and produce a noise of a noticeable intensity, and by pressing on the board as before, and withdrawing the larger stick that had been placed at the free end, the board would strike the box and produce a much louder noise. The same pressure, as nearly as possible, was exerted by the experimenter on the board in both cases. By going upon a platform arranged inside the cage when the louder noise was made, the animals were to show their discrimination of the louder and lesser noises. They were fed on the platform when the louder noise was made, and were not fed when the lesser noise was made. The noise apparatus was manipulated near the closed side of the cage in which the monkeys were kept. It was out of sight and it was not possible that the reactions were made to stimuli other than the sounds. The louder and lesser noises were made in an irregular order. Three animals, 4, 5 and 6, were tested by this method.

Monkey 4 formed the habit of responding to the louder sound and not to the lesser in eight days of 10 trials each, *i.e.*, 10 trials with each sound) or in 80 trials in all. In the first day's trials he responded to the louder sound once in the 10 trials given him, and to the lesser sound twice in the same number of trials. In 10 trials on the eighth day he always responded to the louder noise correctly and to the lesser noise only once in the same number of trials. On the ninth day he made no errors in the 10 trials given him. Monkey 6 formed the association in eleven days of 10 trials each. On the first day she responded to the louder sound three times and to the lesser four times. On the eleventh day she responded to the louder sound nine times and to the lesser sound once. She made no errors in the 10 trials for each sound on the twelfth day. In the time devoted to this work with number 5, he did not form a perfect association. In thirteen days' experiments of 10 trials each he finally came to respond to the loud sound in about 75 per cent of the trials. His inhibition of response to the lesser sound was less perfect, or about 50 per cent. Had the tests been continued it seems likely he would have become perfect in discriminating the two sounds. It is of some interest to note the apparent inferior ability of 5 in comparison with 4 and 6 in this experiment. The work of 5 appeared typical of all his work reported in this paper. Inhibition was apparently his weak point, for he responded to no matter what form of stimulus. The difference illustrates the individual variations in mental capacity of the different animals.

Sound discrimination, Pitch. Kalischer (5, p. 204 ff.) has reported experiments on sound discrimination with dogs, which animals show an ability to discrimination pitch. Although interested in the matter more from a physiological than a psychological standpoint, he incidentally obtained satisfactory evidence of discrimination of pitch by his animals. His method was to sound a certain note on an organ or harmonium as a sign that the animal should react in a certain way, such as snapping at a piece of meat. When a different note was sounded the dog was not to react and was not fed. Selionyi using a form of the 'Pawlow method' on dogs has also lately obtained evidence of discrimination by them of the tones of an organ, organ pipes and of two whistles. He also was chiefly interested in sound discrimination from a physiological standpoint.

In experiments on auditory discrimination in raccoons made by the present writer in 1906, reported by Cole (16, p. 230) evidence of pitch discrimination by those animals was obtained. In the experiments on pitch to be reported in this paper I used the same plan that I had employed with the raccoons, and which is similar to the method Kalischer has used. An ordinary German mouth harp or harmonica A was used. When I sounded the higher note, A 3, the monkey was to go upon the platform used in the preceding noise tests, and was fed there when the note was sounded. When the lowest note, A 1 (two octaves lower), was sounded he was not to go up and was not fed. The notes were sounded in an irregular order so the animal might not react in a rhythm to the sounds. Care was taken to sound the notes with the same degree of intensity, as nearly as possible. I took the usual precautions that the animal should not obtain a cue from my looks, motions or in any other manner, and react to these stimuli rather than the tones.

The records for the different animals are as follows:

Monkey 4 formed the habit of responding to the high note, A 3, and not responding to the low note, A 1, in three days tests of 10 trials each.[1] On the first day monkey 4 responded to the high note six times in 10 trials, and not at all to the low note. On the third day he responded to the high note ten times and to the low note three times. On the fourth day of the experiment (three days after the experiments just noted) he responded to the high note ten times in 10 trials and to the low note not at all in the same number of trials. When tested on the fifth day, two days later, he was perfect in 10 trials.

Monkey 6 formed the association in four days, in 40 trials in all. On the first day she responded to the high note twice in ten trials and to the low note not at all. On the fourth day she responded to the high note nine times and to the

[1] *i.e.*, ten trials of the high note and ten trials of the low note.

low note once in 10 trials of each note. In the 10 trials on the fifth day she was perfect.

Monkey 5 did not form the habit of correct response in six days' tests of 10 trials each. In the 10 trials of high and low pitch on the sixth day he responded to the high note six times and to the low note only once.

If we may generalize from the work of the animals tested in the above experiments we may conclude that Rhesus monkeys discriminate quantitative differences in noises. They also discriminate musical notes of widely different pitch. These experiments also indicate that monkeys learn to discriminate pitch with considerably more facility than do raccoons in similar tests.

Inhibition of habit.

The activities or modes of behavior of animals, including man, are of five kinds: (*a*) reflex actions; (*b*) instinctive actions; (*c*) habitual actions; (*d*) intelligent actions; (*e*) rational actions. These different actions, in varying degrees, characterize the different orders of animals. Generally speaking, the lower the order of the animal the more the *a* form of action is present, and the higher the order of the animal the more the *d* and *e* forms of action are present. The lower forms are, however, basal for man as well as for the amoeba.

Habitual actions are seen not only in the behavior of animals of the higher orders but also in the activities of man. Many apparently intelligent actions of man are of the nature of habit. To say that habit dominates man's actions to a very large degree, and reason to a very small degree is only to state the simple truth.

Previous observations have indicated to comparative psychologists that monkeys and other mammals have a marked tendency to form habits of action. This was well shown by Lloyd Morgan and many others. A certain perceived stimulus is followed by an agreeable or by a disagreeable result, and an animal soon forms the habit of reacting in the appropriate manner when the stimuli are given. The formation of habits is, however, only one way of meeting the conditions of the environment. Habits once acquired often become useless, and sometimes have to be replaced by actions opposite in character. In other words the inhibition or the replacement of a habit is often necessary to the well being of an animal or of man, and the ability to inhibit definite modes of reaction, including reflex tendencies, is important for advancement.

To test in some measure the ability of monkeys to inhibit a recently formed habit four experiments weremade. Three monkeys which had learned to discriminate white (normal) from rye (quinine) bread were reviewed on this work daily for seven days in order that the habit be firmly established. A rest period of seven days was given, and then the memory tested. The results of the latter test showed a perfect retention by all the animals. On the succeeding day each monkey was presented with some pieces of rye bread that had not been soaked in quinine. Then an experiment was performed in which the rye bread was not made bitter. These tests were continued for seven days, 10 tests on each day. The results are given in Table IV. It will be noticed that on the first day monkeys 6 and 8 disregarded the rye bread, and monkey 8 continued to disregard it for two more days. Monkey 7 on the fourth trial on the first day took the white bread first and then the rye. Five times thereafter she repeated this, and the habit of refusing the rye bread when it was simultaneously exposed with the white was broken. Thereafter, on the six succeeding days of the experiment she left the rye bread only once.

After this series a second test was made in which the white bread was made bitter with quinine. Both pieces of bread were presented simultaneously on a board, as in the previous experiments. The results of the tests, 10 experiments a day for seven days, are given in Table V. It will be seen that the animals soon learned to avoid the bitter bread. The mistake made by monkey 6 on the second day was only a partial mistake for the animal picked up the white bread, smelled it and immediately dropped it. The inhibition in the case of monkey 7 was fixed on the first day after four mistakes had been made.

A third series was then made in which the small pieces of bread (white, bitter; rye, normal) were presented successively. In these experiments the animal had a choice of taking or of leaving the single piece, white or rye. The results are given in Table VI. It will be seen that the three animals avoided the white bread on the first day of the experiments, but that on the second all took the white at least once. On the second day monkey 6 took the white bread six times, and monkey 8 four times. After these experiments both animals disregarded the white bread when it was presented to them on the board, even though they were very hungry. Only once during the remainder of these tests did monkey 6 touch the white bread. On the fourth day, when the white bread was presented, she put her hand through the wire of the cage and swept the piece off the board.

The fourth series of experiments was begun after the seven days of series 3. In this last series the breads were presented to the animal by the experimenter, to see if the inhibition effect, or the new habit of leaving the white bread was associated with the presentation of the bread upon the board outside the cage. Each piece of bread was taken by the experimenter in his fingers and held just within the wire netting of the cage. Even with this added inducement monkey 6 refused to take the white bread, but both of the other animals finally, and monkey 7 repeatedly, took it. In the 70 trials with monkey 7, she took the white bread fourteen times. At the sixth trial on the fifth day monkey 7 struck at me when I offered her the white bread, as if angry that it should be presented to her. Monkey 8 took the white bread twice on the second day, but after he had smelled it he dropped it. On the fifth day he once reached toward the white bread that was offered him, but drew back before he had touched it. On the sixth day he struck at the white bread or at me just as he had monkey 7 on the previous day.

These experiments show, on the part of the monkeys investigated, a rapid inhibition of a previously formed habit, and a rapid acquisition of a habit opposed to the original habit. The acquisition of the new habit, however, took a much longer time than that of the original habit, as can be seen from a comparison of the results. In the later experiments so many new factors, especially those of tempting the animal by single presentations, were introduced that the curves of learning cannot be directly compared. It would be fair, however, to compare the results in Table V with those in Table I. When this is done it is seen that the replacement of the association is almost, if not equally, as rapid as the formation. Should this result be true for other activities of the monkey, it would indicate a rather high degree of adaptivity, which goes far towards the production of apparently intelligent actions.

TABLE IV.

Inhibition of habits. All monkeys had learned to avoid quinine (rye) bread; no quinine in either bread for this test. W = took white, R = took rye. Ten trials each animal daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
6	W 10	W 8	W 10	W 10	W 10	W 10	W 10
	R 0	R 8	R 10	R 10	R 10	R 10	R 10
7	W 10	W 10	W 10	W 10	W 10	W 10	W 10
	R 5	R 10	R 10	R 9	R 10	R 10	R 10
8	W 10	W 10	W 10	W 10	W 10	W 10	W 10
	R 0	R 0	R 0	R 4	R 8	R 10	R 10

TABLE V.

Inhibition of habits. All monkeys had been practised on preceding tests (Table IV). Quinine in white bread, rye bread in natural state. Simultaneous presentation. R took rye, W = took white bread. Ten trials daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
	W 1	W 1*	W 1	W 1	W 10*	W 10	W 10
6	R 10	R 10*	R 10	R 10	R 10*	R 10	R 10
	W 4	W 0	W 0	W 0	W 0	W 0	W 0
7	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 3	W 1	W 0	W 0	W 0	W 0	W 0
8	R 10	R 10	R 10	R 10	R 10	R 10	R 10

TABLE VI.

Inhibition of Habits. All animals had been previously practised on experiments recorded in Tables IV and V. Quinine in white bread, rye bread in natural state. Successive presentations. W = took white, R = rook rye bread. Ten times daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
	W 1	W 6	W 0	W 0	W 0	W 0	W 0
6	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 1	W 1	W 5	W 1	W 1	W 2
7	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 4	W 0	W 0	W 0	W 0	W 0
8	R 10	R 10	R 10	R 10	R 10	R 10	R 10

TABLE VII.

Inhibition of Habits. All animals had been previously practised on experiments recorded in Tables IV, V, and VI. Quinine in white bread, rye bread in natural state. Successive presentations through wire netting of cage instead of outside. W = took white, R = took rye bread. Ten trials daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
	W 0	W 0	W 0	W 0	W 0	W 0	W 0
6	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 0	W 3	W 5	W 2	W 3	W 4
7	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 2	W 1	W 0	W 1	W 0	W 0
8	R 10	R 10	R 10	R 10	R 10	R 10	R 10

Imitation.

Whether or not the higher animals learn by imitation is a mooted question among comparative psychologists. Involving as it does the presence of ideation or of 'transferred association' in the animal mind, it is of the utmost importance that this mental function be carefully considered. Most experiments in the field of comparative psychology are inclined to deny the higher forms of imitation to animals. Morgan (7), Thorndike (8, 9) Yerkes (4), and Watson (10), take this ground, while Kinnaman (11), Hobhouse (12), Berry (13), Haggerty (14) and others believe they have found evidence of imitation, apart from mimicry which is sometimes spoken of as imitation and which may be considered a reflex adaptation to certain stimuli.

It is necessary first to define precisely the term imitation as it is applied in comparative psychology. The term imitation is used in a number of different senses. First it is used as a synonym for what may be more accurately called instinctive imitation. This is the form of imitation shown by all animals in the performance of certain necessary acts, such as that of a chick when it pecks at a bit of food on seeing another chick do so. A second use of the word is in relation to mass activities and we may therefore speak of it as gregarious imitation. This form of imitation is the performance of an act similar to that performed by another animal of a herd or flock, the latter act being due to a definite stimulus but the act of the imitator, although similar to that of the imitatee, is not produced by this same stimulus but by the stimulus of seeing or of hearing the first animal. This form of imitation is illustrated by a herd of buffaloes running off in alarm or a flock of sheep following their leader and jumping over an imaginary obstacle. Lastly, there is inferential or reflective imitation, where one individual sees another perform an act, realizes or understands the consequences of that act, and thereupon performs a similar act with the idea of getting the same results.

Of the above forms of imitation the first two are of a low order, and perhaps no comparative psychologist will deny them to many of the higher animals below man. The controversy, however, arises regarding the presence and the amount of the higher form of imitation, the imitation that has been defined as inferential. An attempt was made to test the presence of this form of imitation, and three separate kinds of tests were made.

An apparatus and plan similar to those already employed by Hobhouse (12, chap. x), and by Watson (10, p. 175), with which the former obtained positive and the latter negative results, were used by me in this work. This apparatus consists of a glass tube or cylinder, 15 inches long and $\frac{7}{8}$-inch in diameter, and a plunger of wood less in diameter but

3 inches longer than the tube. The diameter of the wooden plunger was such that it could be readily inserted in the tube and was freely movable. A piece of food, usually banana or a peanut, placed crosswise in the tube, was inserted in the tube and pushed down about half way. A piece of food was always selected sufficiently large not to drop through the tube when the latter was held in a vertical position, and yet sufficiently small that it could be readily displaced when the plunger was used to push it through the tube. This apparatus was presented in turn to eight monkeys and the imitation tests were begun after each had failed to manipulate it three times. The tube and the stick were placed in the cage with the animal to see if he could push out the food after his attention had been attracted and he had been shown by the experimenter how the stick was to be manipulated for obtaining the banana or peanut. I endeavored to have the monkeys see me perform the act at least three times before each trial, and to this end I watched carefully each animal while the exhibition was being given. If there was in my mind any doubt that the animal had paid close attention to the performance of the act by me, I repeated it. Following the three demonstrations the animal was given the apparatus for one minute, and this amount of time was allowed him in which to perform the act. In the case of some animals these tests were made daily and in the case of others there were intervals of one or two days. The variation in the time routine did not appear to have any influence upon the learning. The number of trials for each animal was also variable, from 24 to 96, each having seen the act performed three times before each trial, *i.e.*, having seen the act from 72 to 288 times.

All the monkeys failed to show any signs of imitation, and I was unable to verify on these animals results like those reported by Hobhouse. At the end of the tests all animals acted in much the same way as at the beginning of the tests and the first notes may be used to indicate the general character of their activity. They bit at the food in the tube, locked into the end of the tube at the banana, jerked the tube around, often took up the stick and bit it; sometimes picked up the stick and threw it away, dropped the tube, as if they gave it up. While I was exhibiting the mechanism an animal would watch the operation closely, and at the moment the food appeared on the end of the tube within the cage, he seized it. Sometimes they attempted to reach into the tube to get the food. In this experiment I also tried Hobhouse's plan of 'suggestion.' When an animal was wrestling with the tube I pointed to and moved the stick towards him to call his attention to it. Thereupon, sometimes, an animal took up the stick, but only to throw it away after a few seconds. In brief, throughout the three weeks of this experiment not one of the animals appeared to show any understanding of the problem, and any ability to cope with it. It may be urged that a greater number of trials than 24, the number some of the animals received, would have brought success for some of them, but, were imitation of this kind an important and a constant condition in monkeys, this number of trials would have been sufficient for them to demonstrate its presence.

A second experiment on imitation was then performed. This experiment was also similar to one made on monkeys by Hobhouse (12, chap. x) and by Watson (10, pp. 173, 174) A T-rake, consisting of a light handle 18 inches long, with a cross piece 4 inches long nailed at one end, and at right angle to the handle, was used. This was placed with the handle end through the wire side of the cage and with the T-end extending outward on the floor of the room. A piece of banana or a peanut was placed on the floor outside the cage but within the sweep of the rake when handled by an animal inside the cage. In this experiment I wished to see if the monkeys could grasp the situation of the use of the rake. Two separate factors of adjustment enter into this; first, the use of the rake for the use of grasping or reaching out towards the food, and, secondly, the use of the implement to draw the food nearer the cage and within reach of the hand itself.

Monkeys 1, 2, 3, 4, 5, 6, 7 and 8 were tested in this manner.

I showed an animal, by hooking the T around the food and pushing the rake forward, but necessarily from the outside of the cage, three times before each trial. I then let the animal try the apparatus for two minutes. Each of the eight animals was given from 24 to 120 trials in the four weeks these experiments were continued. Each animal had failed to use the rake three times, without being shown, before the imitation tests were begun.

Monkeys 1, 2, 4, 5, 6, 8, failed as completely as in the previous experiment. Each seized the end of the rake handle within the cage, jerked it around, bit it, tried to reach the food with their hands through the wire of the cage. Sometimes they became angry and threw the rake aside; in some cases they finally gave it up. I noted, however, that all soon came to pull the rake into the cage, up to the T, which was as far as the wire of the cage would permit. So far as I could tell, each animal watched me closely while I was showing them, appeared to have much interest in the proceedings, and seized the food through the wire of the cage the moment it was within reach. But what the six monkeys did *not do* was to push the rake out and hook the cross piece around the food. In all of the trials with these six monkeys so far as I could see, none gave a sign of using the rake as a tool with which to draw the food toward the cage. Here again it may be urged that a greater number of trials than 24 should have been given to all animals, but the simplicity of the apparatus was such that in a child of the same age one or two trials are sufficient for a successful imitation.

After several days tests with two of the animals, 3 and 7, there appeared some evidence of imitation. Monkey 3 learned to *push out the rake*, and, with much slashing about of the T end, would draw in the food. The securing of the food by hooking the T around it was, however, very awkward work for her. Sometimes she would knock the peanut away in her effort to pull it in. On its being replaced by me she usually succeeded in drawing it in roughly. The best that can be said of her performance is that she appeared to learn to imitate me perfectly in pushing the rake out, and awkwardly in pulling the food in with the rake.

Following is an account of the tests on the third day. It will be remembered that the method of securing the food by the use of the rake was demonstrated three times before each test. On this day, however, this demonstration was omitted after the seventh test. Trial 1 was a failure; the animal did not attempt to take hold of the rake after being shown three times. On the second trial, however, after the three demonstrations, the animal grasped the rake, and moved it about in the direction of the food. She succeeded in bringing in the nut sufficiently far so that she reached it and ate it; time, 85 seconds. In this first successful attempt she grasped the rake awkwardly and did not at first seem able to manage it, but eventually she managed to swing it around so that the nut was caught and, as has been said, pulled in the nut so that she secured it by reaching through the bars of the cage. In the next trial the movements of handling the rake were about the same as those in the preceding trial, although she had acquired some facility in the use of the tool, and managed to secure the nut in 29 seconds. On the fourth trial, during the demonstration she took hold of the rake and pushed it outwards toward the food. After this demonstration she pushed the rake outwards in an apparently purposeful manner and secured the nut in 20 seconds. The fifth trial was similar to the fourth, but with an increasing ease in adjustment and with apparent greater facility in the use of the rake. In 8 final trials on this day the demonstrations were omitted but the actions of the animal were similar to those mentioned in connection with the fourth and fifth trials. On the sixth and tenth trials the manipulation of the rake caused the nut to roll further away from the cage and each time I replaced it. The times for these 8 trials were respectively: 15, 15, 9, 12, 24, 14, 4 and 8 seconds.

Three days later, the fourth day of the tests, demonstrations were given in the first and second tests. The results, however, were similar to those on the third day, but with shortened times for the performance of the act. The times were as follows: 10, 8, 3, 4, 2, 6, 7, 9, 5, 4, 3, 9, 2, and 2 seconds. On the seventh trial she managed to get the nut only part way with the first pulling movement, but then, with apparent intention, she gave another pull and brought the nut within reach. On the thirteenth trial the nut rolled away when touched with the rake but the animal pushed the rake out farther beyond the nut and managed to secure it in the usual way.

Monkey 7 had failed to show evidence of imitation on six days of 3 trials each when I manipulated the rake and attempted to exhibit the mechanism of securing food. It was thought that the operation of another animal might be imitated, and for this reason I placed her with monkey 3, which at that time had learned to use the rake with facility. In these experiments I carefully watched monkey 7 in order to be reasonably certain that she observed monkey 3 perform the act with the rake. Only after I was sure the animal had been looking in the direction of monkey 3 while the latter performed the act three times did I begin a test of monkey 7. At these times also I called attention to the tool by pointing to it and to the food. In each trial she was permitted two minutes to perform the imitation act, and if it was not performed in that time I counted the test a failure. The first five tests were failures, and likewise the seventh and tenth. On the sixth trial she pushed the rake outwards awkardly, but with such movements that she could not secure the food. Finally, however, after 79 seconds, she managed to hook the cross pieces about the food and immediately pulled it in. The eighth and ninth trials were similar to the sixth but the food was secured in 20 and 8 seconds respectively. The tenth trial I have counted a failure, for in this case the animal pushed out the rake and pulled it in but did not manage to get the cross piece hooked around the food. In the two minutes allowed for the performance of the act, she did not manage to secure the food.

On the second day of these tests she failed to show by her actions any evidence of imitation in the second, third, sixth, ninth and tenth trials. Only ten seconds were required for the proper performance of the act on the first trial, for she immediately manipulated the rake so that it caught the nut and she pulled it inwards. The fourth, fifth and seventh trials were similar, 12, 15 and 9 seconds respectively. Although the eighth trial was counted a failure the monkey did manipulate the rake properly, but in pushing it outwards the nut was hit and rolled farther way. In the final trials of this day the animal did not make many efforts to use the rake, although two minutes were allowed. In these experiments the movements of pushing the rake outwards were well performed, but those of pulling inwards were very badly executed. This was also noted on the following day, when in 6 trials, she managed to secure the food four times, in 17, 9, 11 and 5 seconds. In subsequent trials she continued to be incoördinate in the pulling in movements, but accurate in pushing out the rake.

It may be said that monkey 7 did not make the mental connection between seeing me manipulate the rake and the idea of the acquisition of food, but that some connection or association was formed between seeing monkey 3 perform the act and such an idea (or what corresponds to an idea in the monkey mind). The impulse to handle the rake, to manipulate it and to use it in connection with food may properly be said to be due to her having seen monkey 3 obtain food by the use of this tool. This impulse or association may be explained as A, ideal, or B, imitative. In the present state of comparative psychology it is best to consider it the latter, and we conclude that the use of the rake by monkey 7 has been due to an imitative impulse, the tendency to perform the same or a similar act performed by another.

In a third experiment to test the ability to imitate the apparatus used in a previous test of analogical reasoning was again employed. A piece of banana was suspended from a pole that extended across the room. The food was placed about 4 feet from the floor, high enough to be beyond the reach of the animal when it stood upright on the floor. A light pole, 9 feet long, 1.5 inches in diameter, was loosely attached at one end by means of a pivot to a support and extended 7 feet across the open space of the room to a horizontal supporting board (*C*), which was 2.5 feet from the floor. Figure 2 shows the arrangement. On the horizontal board the free end of the pole was easily moved, and by shifting it to the position *P-X* the pole was brought under the suspended food. To bring about this change it was necessary to move the free end of the pole about 3 feet, with corresponding decreases in the amount of movement the nearer to the pivot the pole was grasped. In the position *P-X* the animal was able to reach the food if it climbed upon the pole. The problem for the animal was to slide the free end of the pole sufficiently far on the horizontal board or

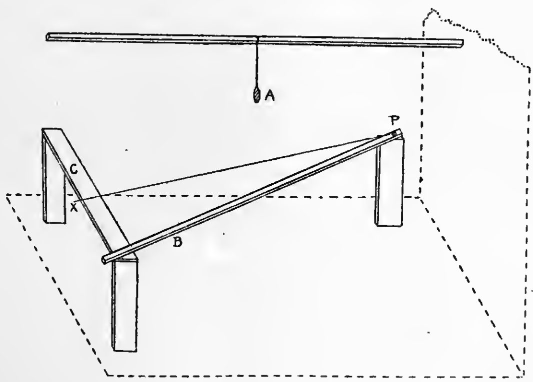

Fig. 2. Diagram of apparatus used in tests of reason and of imitation. *A*, suspended banana; *B*, sliding pole in original position; *PX*, position to which pole had to be moved in order that the animal might reach the food; *C*, supporting slide.

slide, to bring the pole under the suspended food: to go upon the pole and get the food by reaching upwards about 10 inches.

Monkey 3 had been tested with this apparatus in a series of experiments on analogical reasoning (see p. 52 ff.) and had failed to manipulate it. In this first test she was not shown how to manipulate it, but was shown when the experiments on imitation were begun. Before each test by the animal I moved the pole from its original position to that of *P-X* and permitted the animal to obtain the food each time. After having been shown in these three trials that the moving of the pole was essential for the securing of food I allowed two minutes in which the animal might perform the act. If the act was not accomplished in that time I called the test a failure and repeated the demonstrations. On the first day of this experiment she succeeded in performing the complex adjustment of pole, etc., four out of eight times. On the first trial she moved the pole awkwardly, but sufficiently far that she was able to reach the food; 30 seconds. On the second trial she moved the pole only a trifle at first, but returned to it and succeeded in moving it the required distance; 25 seconds. On the next trial she changed the movement of pushing to that of lifting the pole and pushing at the same time. In this her movements were awkward but she managed to get the food in 45 seconds. The next three trials were counted as failures, although in all of them the animal manipulated the pole and showed signs of recognition of the use to which the pole was to be put. On the fourth trial after having moved the pole a short distance and not obtaining food, she tried to get the food by going to the cross piece from which the food was suspended. On the fifth trial she pushed the pole off the slide, and could not get the food. On the sixth trial she moved the pole a short distance, and refused to complete the act. On the seventh trial she moved the pole sufficiently far to obtain the food, 20 seconds; but on the eighth trial she did not try to move the pole or to obtain the food.

On the following day she was not shown how to manipulate the apparatus the first three trials, two of which were successful, but was shown on the other seven trials. At first she pulled the pole part way, but it appeared to be difficult work, and she obtained the food in 20 seconds. The second trial she pulled the pole about 6 inches and stopped; I took hold of the pole and helped her to move it a few inches, and then she made a great effort and pulled the pole sufficiently far to obtain the food. The third trial she did not attempt to move the pole. The following seven trials were, it has been mentioned, preceded by three demonstrations each. In only the fourth trial did she take hold of the pole, at other times she did not attempt any manipulation. On the third day of the tests, six days following that just mentioned, she failed to manipulate the apparatus in six trials, although she was shown three times before each trial. Only once on this day did she touch the poll. Two days later, she failed on the first six trials. On the seventh she helped me to push the pole when I was demonstrating it, and following this she managed to get the pole moved by her own efforts in 68 seconds. On the eighth trial she moved the pole part way and obtained the food in 30 seconds. The ninth trial was a failure in two minutes. Seven days later she was given three trials and was not shown how to manipulate the apparatus. She did not exhibit any signs of ability to manipulate the apparatus and was then shown. The fourth trial was a failure; on the fifth, she took hold of the pole at the time I was moving it, and after the demonstrations she immediately attempted to move the pole, which she managed in 36 seconds. On the sixth trial she managed to get the food in 26 seconds. The seventh trial, 18 seconds, and the eighth trial in 9 seconds were successful. In the remaining 17 trials on this day she gradually lowered the time for the performance of the act although her actions were always about the same. Two days later there were no failures in 20 trials, but the actions of the animal in 2 of the trials were noteworthy. In these trials the monkey moved the pole part way, then stopped, and appeared to be observing the amount of the movement and making a judgment regarding the possibility of reaching the food from the pole in the position in which she had placed it. Then, not liking the position she moved the pole farther and climbed upon it and obtained the food.

In these tests there was a gradual learning to eliminate unnecessary movements and to perform the necessary movements in a satisfactory manner. The experiment does not, however, wholly belong to the type of learning of trial and error, for, as has been remarked, the monkey moved the pole properly the first time she attempted to do so. There was no previous groping for a something, no fumbling with anything but the pole. The attention, so far as shown in action was directed to the pole. There was an immediate grasp of the situation, and this coming after she had been shown that the food was to be reached by changing the position of the pole is taken as evidence of imitation. The fact that in later tests she failed to pay attention to the pole and to attempt to move it does not mean that she could not imitate, but it is well known that the monkey is extremely variable in its actions, and its attention is held with difficulty. We know that in the production of habit, as many observers have pointed out, there is not a gradual shortening of the time of reaction as in the case of cats and dogs, but that the time for the performance of an act is extremely variable. It is this factor of variability due to elements such as lack of hunger, etc., which seems to me to account for the lack of attention to the problem. It is further to be noted that in the successful trials she used a variety of means for the accomplishment of the end; sometimes she pushed the pole unhesitatingly through the required distance; at times she pulled it, and at times she pulled and pushed it. The variety of action indicates that the performance of the act is not like that of a habit formation, but that of attempting to accomplish an object in any manner that this could be done. The performance of the act was awkward, but there appeared to be imitation of a relatively high order.

In the experiments that have been described evidence of imitation by monkeys 3 and 7 was found in the second experiment of the series. These two monkeys and the other six failed to show signs of imitation in the first experiment and I failed to find any signs of imitation on the part of the six monkeys in the second experiment. When these results are combined, I think they indicate in a general way that some monkeys may and do learn by imitation. The amount of imitation is not shown, but the fact that in so many simple experiments negative results were obtained indicates that these animals do not imitate to the extent that has been ascribed to them. We are, I believe, justified in concluding that imitation is a mental function of the monkey or of some monkeys, although the results of my experiments may be interpreted to mean that imitation does not play a very important part in their learning process.

Ideation.

The evolution and anecdote schools attribute ideation of a comparatively high order to the animals with brains similar to that of man, and they hold that ideas similar to those of man play an important part in animal behavior. They base this belief, however, upon uncontrolled observation and upon flimsy and circumstantial evidence and not upon controlled experiments and unequivocal facts. The evolutionists also appear to believe it necessary to attribute ideas to animals, else the doctrines of a progressive mental development would not coincide with that of the physical development. On the other hand, others, mainly experimenters, hold that the casual observations do not indicate animal ideation and the same facts are interpreted differently. Special tests have given negative results and from these and from the careful observation of animals in laboratory surroundings they conclude that animal ideation is not proven, and that the present evidence tends to indicate an absence or a lack of ideas in the animal mind. Some, however, are willing to admit that animals may have ideas, although in small number, but are forced to conclude that ideas are a very unimportant element in minds below that of man. It is true that for the most part the experiments of these men have not been special tests for ideation, but tests for other reactions in which ideation, if present, should or might have been exhibited.

Before passing to the evidence of ideation, it seems well to define the term 'idea' and thus to have a precise notion of what we should look for in animal behavior. For the purpose of comparative psychology an idea may be defined in the following ways: *a*, an image or picture of a visual object which is formed by the mind; *b*, a general notion or conception; *c*, a plan or purpose of action or an intention; *d*, idea in the sense of an understanding of a certain relation or situation (as of sensible objects). If, therefore, an animal exhibits any of the mental conditions noted above we may conclude that ideation is present.

A simple test for ideation was made with animals 1, 2 and 3. A board 20 inches long and $3\frac{1}{2}$-inches wide was placed with one end against the side of the cage and within reach of the animal with the other end extending outwards from the cage and beyond reach. Food was placed at the farther end. The object of the tests was to determine if an animal would understand the situation and be able to secure the food by the indirect method of pulling at the board near the cage rather than by directing its efforts at the food or the food end of the board which was beyond reach. In the first trial monkey 3 after some testing of the openings of the wire netting, seized the board awkwardly near the end within reach, pulled the board alongside the cage and secured the food, 30 seconds. In this trial the attention for only a few seconds was directed to the food position but was directed mainly to the end of the board that did not contain the food. The actions of the animals in manipulating the board were awkward but only in the method of reaching for and handling it. The actions were directed to the board, and not to the food, and it appears that here is an example of an understanding of a situation, a direction of action to an end not of special interest in the situation. In the second and third performances she duplicated her actions, but with shorter times for its accomplishment because the awkwardness largely disappeared and because the preliminary direction of attention to the food was absent, 20 and 10 seconds. In later tests she continued to react with complete success, reducing the time to three seconds in the fifth trial, which time includes that for the manipulation of the board and the securing of the food. Monkey 1 succeeded in the third trial but showed more misdirected efforts than did monkey 3. The third animal also managed to perform the necessary acts to secure the food in the third trial, but the actions were more awkward and poorly directed than those of monkeys 1 and 3.

At the beginning of this test there were many unnecessary movements on the part of each animal, but this awkwardness or lack of understanding of how to deal with the situation was mainly that of motor adjustment. It was a difficulty in dealing with the board through the wire netting of the cage and not a difficulty or a lack of understanding of the problem, viz., of reaching for and of manipulating the board. In the first trial of monkey 3 the animal directed its attention to the board near the cage, and showed that it appreciated the relation between this part of the board and the securing of food. The later reduction in time for the performance of the act, which at first sight makes the experiment appear to be only another instance of learning by trial and error, was mainly that of a proper adjustment of arms and hands to the wire netting and not that of attacking the board at the proper place.

These results indicate the presence of ideas of the above described third or fourth class, *i.e.*, a plan of action or an understanding of a situation. It was apparent that from the first the animal understood that the food could not be reached directly but must be obtained by an indirect attack on some other part of the apparatus. There was in this case no general activity such as has been described by numerous investigators in connection with the puzzle box experiments; the trying here and the pulling there were notably absent, and the attention of the animal was concentrated on the board and on the getting the food. For a few seconds after the board and food were displayed the hungry animal would reach in vain for the food which was beyond its reach. but this unsuccessful method was given up and the attack directed to the board on which the food was placed. It is also of great importance to note that the attack was directed not to the part of the board on which the food was placed, but to the part of the board away from the food.

The simplicity of the situation may be urged as an argument against the presence of ideation in solving the problem, but I would again call attention to the ordinarily complex character of similar experiments that have given negative results to other observers. In such experiments we must steer clear of both an absolute simplicity and of a complexity abnormal to the animal. We must test the animal under conditions which are within its mental range and which will show the ability to reason. It is equally bad to set impossible conditions and to draw unsupported conclusions, and in both these ways comparative psychologists have attempted to solve the problem of animal reason and ideation. To conclude, for example, that a dog or a monkey possesses no power of ideation or of reasoning because it does not thread a needle when the needle and thread are supplied or because it does not unlock a door when the key is presented is to limit the terms ideation and reason to the ability to perform certain activities connected with a certain class of civilized man. The examples cited are, perhaps, extremes but they illustrate the attitude of a certain class of experimentalists. On the other hand, to conclude that a cat reasons because it attracts attention by scratching on the window pane or because it manages to strike an electric button for the opening of the door is to take no account of the possibility of previous training of the nature of trial and error. We should steer between the two extremes and test the so-called higher powers of animals by presenting to them conditions appropriate to the class. In the simple experiment described in this section and in others to be described in the section on reasoning (p. 52 ff.) the conditions are appropriate to the monkey family. We have presented to the animal a new problem which it must solve, a set of conditions to be dealt with in order that a resultant pleasure (hunger satisfaction) ensue. In the solution of the problem the animals took a direct path. There was none of the fumbling or groping, no trying here and there, no attempts upon other parts of the cage or its surroundings, but a direct attack upon the board that held the food.

Somewhat similar actions were observed and noted above in connection with the food box experiments. After the previously ignorant (so far as the food box is concerned) monkeys had been fed from the food box a few times so they might become acquainted with the location of the food, the doors were closed. Then began an attack upon the doors, not upon the wire netting of the sides of the cage. The animals remained active about the doors of the food boxes;

they did not sit sulkily upon the platform in the cage; the attention was directed to the doors of the food boxes. Thorndike indicates a belief that the attention of his cats was directed to the string or to the button because the movement in connection with these parts of the appliances was followed by a pleasure. With my monkeys, however, the attention of the animals could not have been directed to the doors of the food boxes on account of a pleasure in connection with a movement, for the animals did not have to perform any movements (except those of taking the food from the boxes) in the neighborhood of the food boxes, and in connection with the doors there were no movements to be performed. In these cases there was no activity in the region of the food boxes or of the doors which could become associated with a pleasure. The attention must then be due to something different from the movement-pleasure association; and it seems probable that there is some form of ideation to account for it.

In the brightness tests interesting behavior on the part of monkey 3 was observed. As has been explained above, the doors were opened by turning a button and thus food was secured. After several days' experiments monkey 3 began to close the door of her own accord instead of waiting for me to close it preparatory to another trial. Before I could close the door after she had opened it she would close it. This would expedite the getting of food. It soon became almost an habitual custom for her to close the door, and in the remaining tests she did so from three to ten times each day throughout the experiments. Why should an animal close the door if it was not with some design or intention (perhaps vague) of hurrying the food-getting process? It seems too parsimonious and even inadequate to call this kind of reaction a mere reflex. In the puzzle box experiment the same animal and two others, monkeys 1 and 2, often attempted to hold open the door with their hands and this kind of action was recorded many times throughout the series. At first I was inclined to consider this action as a reflex but it is overworking the meaning of the term reflex to use it to explain or to describe the actions of the three animals in this particular. Somewhat similar behavior in a different situation was also noted with monkey 8. In testing his color discrimination with colored rice and breads, the food was placed on a block about a foot away from the cage and then moved close to the cage so that he could readily reach the food through the wire netting. When the food was taken or rejected the block was moved backwards a short distance preparatory to placing food upon it for another trial. During the first few experiments he acted as most of the monkeys do, viz., moved away from the side of the cage as soon as the food was obtained. After he had learned to discriminate the colors, however, it was noted that he began to push the block away after having taken the appropriate food. This he continued to do during the remaining trials on that day. The actions of monkey 8 in this case had all the appearances of an understanding of the situation and a desire to hurry the getting of food. It is of considerable importance in this connection to bear in mind that the performance of this action was sudden and not a gradual growth and not the result of trial and error.

The actions of the three animals in the situation of the diagonally placed board, the actions of monkey 3 in closing the door of the food box and the actions of monkey 8 in pushing away the block on which food was placed have a similar appearance. Were we not obsessed by the law of parsimony we would immediately say that these actions show the presence of ideas in these animals, ideas of the highest form. We are, however, in keeping with the law of parsimony, quite justified in saying that the actions indicate the presence in the minds of the animals of a something very much like an idea in the human sense, a something that has for the animal a function or a use similar to an idea in man. This 'something' may be crude and simple, and doubtless it is analyzed by the animal, but it serves practical purposes. These somethings may be termed, as does Hobhouse, practical ideas; they may be partly sensory-motor reactions, and may be partly instinctive, and in part they may be accounted for by the superior equipment of the monkey in coördinated vision and in the use of the hands. The evidence of the ability to imitate supports the view of the presence of these practical ideas, and in the section of this paper on adaptive intelligence (p. 54 ff.) will be found additional evidence. The fact that only a few of the animals exhibited anything like the actions described, and each animal only a few, indicates that these 'practical ideas' play a subordinate and unimportant rôle in the ordinary life of the animal, but they do indicate that ideas may be present and have effect under certain circumstances.

Reasoning.

It was formerly held that man alone possesses reasoning power; other animals only instinct. Comparative psychology has modified the older view of instinct, viz., that it is a mysterious power, perfect at birth, unerring, unchangeable in its working, and radically different from intelligence. It has given the term a more precise definition by limiting it in various ways, *e.g.*, from reflex action and habit on the one hand and the higher mental processes on the other. But comparative psychology has not materially changed the general view that reason is confined to man. And the generally accepted position is that the observed actions of animals may be explained as the results of simple associations. Some experimentalists have found material facts that indicate to them reasoning ability on the part of some animals, but this conclusion is not accepted by others. Here again, as in considering the subject of ideation, it is necessary to define what we mean by the term reason. This word has been used in a variety of ways and the following definitions include the most important of the meanings that have been given to the term. A, implied reasoning (Harris), *e.g.*, my recognition of yonder horse; B, inference from particular to particular (James), *e.g.*, the bird which finds bread upon the window one morning comes back the next morning; C, adaptive intelligence, the ability to adapt to our purposes conditions more or less difficult and more or less unfamiliar;

D, analogical reasoning, which involves construction or creation, *e.g.*, to reach an upper window I utilize a ladder which I find; E, rational thinking (James); F, formal or syllogistic reasoning.

That the higher mammals possess the ability to reason in the first two senses, probably no comparative psychologist will deny, although the explanation of the process may differ. Implied reason is probably a function of all animals and the ability to infer from particular to particular is well shown in all the experiments in habit production. It is the higher level of constructive analogical reasoning concerning which there is dispute. Has the animal power to create or to construct? Hobhouse in some very interesting experiments, claims to have found satisfactory evidence of the presence in animals of this class of reasoning. He calls the mental states that lead to this form of reasoning articulate ideas, and he has satisfied himself that these are present in some monkeys and apes.

With the purpose of confirming the work of Hobhouse I made experiments on three monkeys which had been extensively used in the previously described work on discrimination, etc., and with which I found it most easy to experiment.

The first experiment was one similar to the box and the chair experiments of Hobhouse. Food was suspended by a string from a long pole reaching across the room, too high for the monkeys to reach or to grasp by standing or by jumping. A light box was placed near the point of suspension, but sufficiently far away that the animal could not reach the food by standing upon the box. Only when the box was moved and the animal climbed upon it could the food be secured by the animal. Each of the three monkeys were given three trials, in each of which five minutes were allowed for the performance of the act. At the time the experiments were made the animals were hungry and apparently each watched me attentively when I fastened the piece of banana to the end of the string and suspended it. When all had been arranged the animal was permitted to approach the food and to secure it if possible. The actions of the animals were similar. Sometimes the animal tried to reach the food by jumping, sometimes after the unsuccessful attempts the animal would have a puzzled look and finally gave up attempting to secure the food. None of the animals seemed to notice the box and none made the attempt to use it as a means of reaching the food. In their actions there was nothing that I could interpret as a sign of deliberation or reflection, and in this experiment I was unable to verify the results of Hobhouse. It is possible that the box appeared too heavy for them to move, but I did not notice any indication that it had been observed, or rather observed in connection with the food. That the box was not too heavy was shown by the fact that a similar and equally heavy box was being constantly moved by monkeys in a large adjoining cage. Failure to secure evidence of reasoning in this experiment may have been due to the fact that I did not continue the experiment so long as did Hobhouse.

A second experiment was then tried. This experiment has been described in connection with the observations on imitation, and the apparatus is illustrated on page 42, fig. 2. The animal had to move a long pole, pivoted at one end, under the suspended food in order to secure the food. In all the experiments of this kind (three for each of the three animals) there was no evidence of ability to grasp the situation and to solve the problem. In one trial an animal climbed upon a cage which was near and jumped from it to the food. In general, however, each animal made unsuccessful attempts, most of them directed toward the food, and in each test finally appeared to give up trying. Two months later one of the animals (monkey 3) was again given three trials previous to the imitation tests and failed.

Adaptive intelligence.

Although no direct evidence of analogical reasoning was obtained in the tests just described, other experiments on reasoning gave interesting results. Those which are recorded in this section deal with adaptive intelligence, the reason as defined in the third class.

A piece of twine was permitted to hang in front of and 12 inches away from the cage, beyond the reach of the longest armed animal which was tested. At the end of the twine a piece of banana was arranged; a thin piece of wood was pushed through the banana and turned so that one end could be grasped by an animal in the cage. By grasping and pulling the stick inwards the food was secured. All the animals were tested in this experiment. The results with all except one of the animals were similar, and a description of the actions of one will suffice to indicate the whole. As soon as the banana and stick were arranged monkey 6 put her arm through the wire of the cage, seized the end of the stick, drew it toward her and secured the banana. This experiment was repeated a number of times and in all there was a similar immediate characteristic response. There appeared to be a decided adaptation of means to end. No efforts were wasted upon random movements. It did not appear that any preliminary attempt was made to grasp or even to reach for the food, but there was an immediate movement toward the stick. The results for all animals are given in table VIII, in which is shown the approximate time for the performance of the act by each animal in each trial. The absence of hesitation, the direction of the movement away from the food and towards the stick, and the promptness with which the food was secured speak for the presence of adaptive intelligence in ten of the monkeys. It would seem that this is almost always found in these animals.

In this test the results with monkey 5 were decidedly different from those with the other animals, in that he failed in the trials given him. Monkeys 1 and 4 had considerable difficulty in getting the food in the first and second tests, but there was no gradual acquisition of the method of securing food with the other eight animals. The times for solving the problem in the second and third tests were approximately the same as those in the first tests for monkeys 2, 6 and 10, and there was not much difference in the time between the first and the later tests for monkeys 3, 7, 8, 9 and 11. Much, if not all, the difference in time can be accounted for by the better adjustment to the wire netting of the cage, the pushing of the hand through in the proper place, etc.

TABLE VIII.

Adaptive intelligence, suspended food and stick. Three or six trials each animal. Time in seconds; f = failed.

TRIALS AND ANIMALS	1	2	3	4	5	6
1	60	10	3	—	—	—
2	4	3	3	—	—	—
3	20	6	9	—	—	—
4	105	40	11	—	—	—
5	f.	f.	f.	—	—	—
6	2	1	1	—	—	—
7	6	4	3	1	1	1
8	6	4	4	3	3	3
9	5	3	3	3	4	2
10	2	1	1	1	1	1
11	5	5	3	3	3	3

A second test of the presence of adaptive intelligence was made as follows: A light wooden lever *B*, 18.5 inches long was attached by leather hinges at one end to a board *A* which rested upon the floor The hinge of the lever was 4 inches from the end of the horizontal board, well within reach of the animals. The lever was inclined at an angle of approximately 45 degrees from the horizontal, and could be moved forwards in a vertical plane. Fig. 3 illustrates the apparatus in relation to the front of a cage. The apparatus was placed outside a cage, the lower end of the lever being within, the upper being beyond the reach of the animal. A piece of banana or other food was placed at the farther end of the lever, and the problem for the animal to solve was how to secure the food which was beyond direct reach.

Eleven monkeys were tested with this apparatus, and all with the exception of monkey 4 succeeded in the first test. In the first test the animals usually took a longer time to get the food, but as in the previous experiment this delay was largely one of making the adjustment of hand to the proper opening in the wire netting and not to attacking the apparatus in the proper manner. In the case of monkey 4, he appeared frightened at the time of the first trial and I showed him that food was to be secured by moving the lever against the cage and permitting him to secure the food. In the succeeding trials he immediately attacked the lever and obtained the food in the same manner as the other animals. Monkey 9 was in the same cage as monkey 8 when the latter wasbeing tested and may have taken the opportunity to observe monkey 8. That she performed the trick may have been due to imitation, but I have credited her with having performed it in the same way as the other animals. It is reasonable to suppose that monkey 4 would have been able to

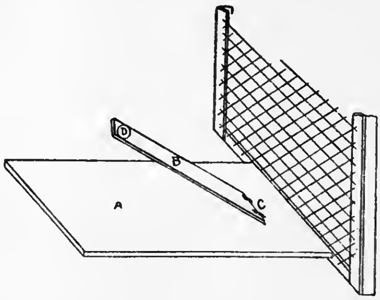

Fig. 3. Lever apparatus for test of adaptive intelligence C, leather hinges; B., lever; D, food.

manipulate the apparatus without being shown if sufficient time had been allowed. However, in whichever way it be conceived that the animal performed the trick we have a mental something very much like imitation or like adaptive intelligence. In many of the experiments the animals did

TABLE IX.

Adaptive intelligence, lever test. Time in seconds; f = failed.

TRIALS AND ANIMALS	1	2	3
1	9	4	3
2	6	4	3
3	$2\frac{1}{4}$	2	2
4	f	6	3

not use both hands for the moving of the lever, but one hand for the lever and the other for securing the food when the upper part of the lever was within reach. Table IX gives the time records of four monkeys in this experiment, those for the other animals were approximately the same as those noted in the table, although no accurate measurement by watch was taken.

A third experiment was then made. A stick 22 inches long, with 1 inch extending into the cage and the other outwards from the cage at a right angle, was arranged so that food could be secured by drawing the stick lengthwise into the cage. Ten monkeys were tested in this manner and all immediately appeared to grasp the situation for they pulled the stick and secured the food within three seconds. In the cases when the food was dislodged the animals immediately gave up the stick and turned to the food, an indication that the stick had been recognized as a means of obtaining the food.

During the progess of some experiments I noticed that monkey 7 attempted at one time to pull toward his cage a small tin bucket in which water was usually carried to the animals which had been unintentionally left near the cage. From this hint the following test was made with nine of the animals. One end of a piece of twine, 1 yard long, was attached to the bucket and the other end was left lying within the cage. The bucket was placed at the length of the twine away from the cage, and in it was placed a piece of food. In all cases the animals seized the twine immediately, drew the bucket toward the cage and seized the piece of banana. Table X gives the times for monkeys 7 and 8. These times are similar to those of the other animals.

TABLE X.

Adaptive intelligence, pulling bucket inwards by means of twine. Time in seconds.

TRIALS AND ANIMALS	1	2	3	4	5	6	7	8	9	10
7	6	4	3	3	4	3	3	4	3	3
8	0	3	3	2	2	3	2	2	2	3

A variation of the problem was then made. The end of the piece of twine which had been left lying loosely within the cage was tied to the wire netting 6 inches from the end, leaving that amount of twine within the cage. In this test the animal had to reach beyond the wire netting, seize the twine and pull the bucket. Seven animals were tested in this way by an assistant, and the notes are not so full as I could wish, but in brief the results are as follows:

Monkey 1,	first trial,	3 sec.;	average	for 10 trials, 3 sec.
Monkey 2,	first trial,	20 sec.;	average	for other 9 trials, 3.5 sec.
Monkey 3,	first trial,	20 sec.;	average	for other 9 trials, 3.5 sec.
Monkey 4,	first trial,	5 sec.;	average	for 10 trials, 3 sec.
Monkey 6,	first trial,	4 sec.;	average	for 10 trials, 2 sec.
Monkey 7,	first trial	3 sec.;	average	for 10 trials, 2 sec.
Monkey 8,	first trial,	120 sec.;	average	for last 5 trials, 5.5 sec.

In the case of the animals whose actions were noted, each pulled at the short free end of the twine at first, but soon reached beyond the knot and pulled the bucket inwards. This was done by some of the animals, notably monkey 8, for four or five trials. The appreciation of the knot condition and the ability to deal with it is plainly shown by the records given above.

The results of all the experiments described in this section appear to speak for the possession by monkeys of adaptive intelligence, of a form of reasoning. The times for the performance of the various acts are fairly conclusive, but in addition the appearances of the animals, their actions, etc., especially during the first trial in each experiment indicated an understanding of the problem. There was no fumbling with the apparatus, no appearance of learning by trial and error, but there was instant action following apparently instant understanding of the situation. The reactions of the monkeys in the situations provided for them gave diversified and relatively abundant indications of the presence of practical ideas such as have been referred to in the section of this paper dealing with ideation (p. 40 ff.).

Memory.

No one questions the fact of the possession by animals of a sort of memory for acts once or often repeated. Memory in the restricted psychological sense, however, has been denied to be present in animals but it is obviously difficult, if not impossible, to determine this. The nature of retention and recall are matters that are disputed, but that animals have an organic memory, a physiological sort of memory, is admitted. This memory acts well for practical purposes and may therefore be called practical memory. Whether or not this be merely organic, or whether or not animal memory contains a representative factor must be left for future investigation.

The monkeys observed in this study showed good ability of recognition and retention, of the practical memory. After the completion of the tests on visual and auditory discrimination no further experiments were performed with the apparatus, until many of the later experiments on imitation, ideation, and reasoning were finished. Memory tests were then made of the animals previously used in the discrimination tests with the following results.

Visual discrimination of pink, yellow and green rice: monkey 1 took the green ten times, took the pink the first trial but only smelled it, and on the fourth trial took the yellow after the green but dropped it almost immediately; monkeys 2 and 8 took only green and paid no attention to the pink and yellow rice; monkeys 3 and 7 took green each trial, and in the first trial took pink after the green and smelled but did not eat it. Auditory discrimination of noises after thirteen days. Monkeys 4 and 6 made no mistakes in ten trials each.

Lever test of adaptive intelligence. Monkey 6, after 130 days, showed perfect retention in four trials; monkeys 7 and 8, after 123 days, were also perfect in four trials.

General Summary and Conclusions.

Monkeys learn to discriminate brightnesses, but take a long time for this when the stimuli have not a direct relation to the incentive for work; only a few trials are needed when the visual qualities are a part of the objects to which they naturally pay attention.

Colors are discriminated with accuracy and rapidity when the colors are parts of the food (red, pink, yellow and green).

Three animals gave clear indications of the discrimination of different degrees of noise, and also learned to discriminate musical tones.

The habits are formed rapidly if there be the double incentive of pleasant food as an inducement to a correct response, and of an unpleasant stimulus to check a wrong response.

From the experiments recorded in this paper it appears that monkeys learn to inhibit recently formed habits of action with facility.

As far as the evidence goes, in regard to both the formation and the inhibition of habits monkeys are superior to raccoons and far superior to dogs, cats, elephants, otters and other mammals which have been experimented with.

Monkeys have a practical memory; they appear to show a good degree of retention; the representative function in memory is an unknown quantity.

Of the higher powers of mind the monkey has only rudiments. He has a something which corresponds in function to ideas of a low order and which serves practical purposes. This something we call, with Hobhouse, practical ideas.

Two of the monkeys learned by imitation, but six others gave no indication of imitation ability. It may be said that while monkeys may learn by imitation to a limited extent, imitation as involving ideation is a small factor in their ordinary learning process.

All the tested animals appeared to reach a generalized mode of action in dealing with problems but there seemed to be no evidence of true general notions. They have an adaptive intelligence, a lower form of reason, or a mental state inferior to true reason.

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