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THE PSYCHOLOGICAL REVIEW

THE RELATION OF INTELLIGENCE TO SOCIAL STATUS

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Historical

Among the many reports of investigations with the Binet-Simon scale incidental references to the influence of social status on intelligence are occasionally found; but heretofore the study of this extremely important and interesting topic has been relatively neglected, although Binet with his usual acumen did not overlook the problem.

In 1910 Decroly and Degand[1] tested forty-five children of both sexes in a private school at Brussels. They found that none of the children tested were below age, nine were at age, and the rest from one to three years above the level of their age. These results were very significant, since the Binet-Simon scale is theoretically supposed to rate equal numbers retarded and advanced with the mode and the average at age.

Decroly's and Degand's results were carefully studied by Binet.[2] He thought the best explanation of the difference between his results and theirs was found in the fact that the Belgian children came from a private school in Brussels and represented children of the well-to-do and largely professional class while the Paris children were from a rather poor section

[1] Decroly, O., et Degand, Mlle. J., 'La Mesure de l'intelligence chez des enfants normaux d'après les tests de MM. Binet et Simon,' *Arch. de psychol.*, 1910, **9**, 81–108.

[2] Binet, A., et Simon, Th., 'Nouvelles recherches sur la Mesure du Niveau Intellectual chez les enfants d'école,' *L'Année Psychol.*, 1911, **17**, 145–201.

of the city. The instruction in the Belgian school was also more individual for the classes were very small.

In analyzing the Brussels results by the individual tests, Binet found that the children from the better social class scored higher in tests involving thought in the higher sense—apprehension, criticism, comparison, etc. They also scored higher than the less favored children in the tests which put a premium on linguistic readiness—such as the description of pictures, abstract definitions, comparison of objects, absurdities, and giving words for three minutes. Binet concluded that social status must be closely correlated with mentality and reckoned from the results in the two schools an average difference of about one and one half years between children of the better and poorer social classes. He does not take into account, however, that this difference may vary with different chronological ages.

Binet, moreover, criticized[1] the work of Katharine Johnston, who had examined two hundred pupils of the Sheffield schools in England, because she had drawn her subjects from at least three distinct social groups and had not kept these groups separate in her averages.

Another study which seemed to confirm Decroly's and Degand's results was the testing done by M. Morlé[2] in a school in a poor part of Paris and compared with the results from a school situated in a wealthy section. The study was on a rather small scale as only thirty children were taken, at random, from each school. The results were as follows:

	Retarded		At Age	Advanced	
	2 Yr.	1 Yr.		1 Yr.	2 Yr.
Unfavored school	1	11	13	4	1
Favored school	1	3	10	10	6

Thus sixteen children out of the thirty tested were advanced in the favored school while only five were advanced in the unfavored school. The children from the poorer

[1] *L'Année psychol.*, 1911, 17, 195–196.

[2] Morlé, M., 'L'influence de l'état social sur le degré de l'intelligence des enfants,' *Bull. Soc. libre Educ. Psychol. de l'enfant*, 1911, 12, 8–15.

section were on the average about one fourth year behind the level of their age, while those of the favored school averaged from one fourth to one half year advanced, or a difference of about three fourths of a year between the two social classes.

In 1910 the teachers of the Breslau schools in Germany made a comparative study of children of different social classes there. The demand for a common school for all classes to replace the Vorschule and Volkschule had arisen in Germany. The Volkschule is the elementary public school attended by the children of the laboring and lower business classes, while the Vorschule is attended by the children of the higher social classes. In Prussia the children could enter the Gymnasium, which has a nine-year curriculum preparing for the university after three years of preparation in the Vorschule but only after four years in the Volkschule. The purpose of this investigation was to find whether the mental maturity of the child, as well as the curriculum, justified this. One hundred and fifty-six boys were tested from the two schools. The Binet-Simon scale modified by Bobertag was used. The boys tested were seven and nine years of age from the Vorschule and seven, nine, and ten years from the Volkschule. It was found that the nine-year Volkschule pupils scored 10 percent lower than the pupils of the same age in the better school, while the ten-year-old Volkschule boys attained only the average of the nine-year-old Vorschule pupils. The difference in average was due largely to the fact that the Vorschule pupils did nearly twice as well as the Volkschule pupils of the same age in tests above their age level. The tests at the age level were passed about equally well by both schools. This raises the question of whether children of higher social classes mature earlier than those of the lower levels.[1]

Children from three very different environments were tested in 1911 by J. and R. Weintrob.[2] There were about sev-

[1] Hoffman, A., "Vergleichende Intelligenzprüfungen an Vorschülern und Volkschülern," *Zsch. f. Angew. Psychol.*, 1914, 8, 102–120.

[2] Weintrob, J. and Weintrob, R., "The Influence of Environment on Mental Ability as Shown by the Binet-Simon Tests," *J. of Educ. Psychol.*, 1912, 3, 577–583.

enty children of both sexes in each group tested. Group *A* consisted of children from a school attended by children of the wealthy class, with every opportunity for travel, etc. Group *B* was composed of children whose fathers were wage-earners or small business men. Group *C* was composed of children from a Hebrew Orphan Asylum with no real home environment. The schools were compared as to the number of children, testing above, at, or below the norm for their age, using the Binet-Simon scale. The *A* group was found to rank highest, the *C* group next and the *B* group last. The investigators state: "Judging from the results environment does not seem to affect greatly mental capacity, if at all." Instead of the schools ranking *A* and *B* and *C* as might have been expected, the *C* school or Jewish Orphanage ranked a close second to the wealthy school. However, the question of race enters very largely in this study as the children of the Asylum were all Jewish, while those of Group *A* were predominantly American with a few Germans, Jews and Italians, and Group *B* was largely composed of Germans, Italians and some American children. It is very evident, as the investigators say, that in order to judge fairly differences in environmental influences among groups, the conditions within each group must be uniform, and the same races must be judged.

A study involving social status, incidentally, was made in Columbia, S. C. by Miss Strong.[1] Her primary purpose was to investigate the difference between the white and negro children but in order to make a fair comparison she tested white children in both the city schools and in the mill district. The results show that less than six percent of the city school children were retarded while eighteen percent of the mill district children were mentally over a year below the level of their age. None of the mill district children were above their age level, although ten percent of the city children scored above their years. Approximately the same percent of the children in each district were at the level of their age; eighty-four percent in the city schools and eighty-one per-

[1] Strong, A. C., '350 White and Colored Children Measured by the Binet-Simon Measuring Scale of Intelligence; A Comparative Study,' *Ped. Sem.*, 1913, 20, 485–513.

cent in the mill district schools. Practically the same course of study was used in the schools of both districts.

One of the most recent investigations on the subject of social status was made by Yerkes and Anderson[1] in Cambridge, Mass. In this investigation the Yerkes-Bridges Point Scale[2] was used. Fifty-four children in the kindergarten and first grade of school *A* were compared with children of the same sex and approximately the same age in school *B*. School *A* is located in a good neighborhood and the sociological status of almost all the pupils is very good. School *B* on the contrary is located in a medium to poor section of the city and the majority of its pupils live in a rather poor environment. The children compared were all of English-speaking parents.

The average number of points scored in the two schools is indicated below:

	Age				
	4 Yrs.	5 Yrs.	6 Yrs.	7 Yrs.	8 Yrs.
School *A*..................	15	27	42	49	56
School *B*..................	17	22	29	35	41

The favored school averages much higher except in the four-year group. The very young children of the unfavored group seem to have the advantage here, probably because they are less timid. The results show that there is a difference of from twenty percent to thirty percent in mental ability which may be associated with differences in sociological status.

The authors point out that in view of a difference so marked between children of different sociological levels, it is very unfair to judge them by the same norm and that further investigating should be done with the view of establishing norms for different social levels.[3]

[1] Yerkes, Robt. and Anderson, Helen, 'The Importance of Social Status as Indicated by the Results of the Point Scale Method of Measuring Mental Capacity,' *J. of Educ. Psychol.*, 6, No. 3, Mar., 1915.

[2] Yerkes, Bridges and Hardwick, 'A Point Scale for Measuring Mental Ability,' Warwick and York, 1915. Hereinafter referred to as the "Point Scale."

[3] In a book published since this paper was written, Prof. L. M. Terman discusses the influence of social status. He reports a difference of one to two years between the superior and inferior classes—a result in close conformity with those mentioned above. 'The Measurement of Intelligence,' pp. 72 and 115, Warwick and York, 1916.

Experimental

The Yerkes-Bridges Point Scale was used in this investigation and three hundred and one children were tested in two schools situated in very different localities of Columbus, Ohio. The children from school *A* situated in the better district will be designated as the favored group, while those of school *B* will be designated as the unfavored group.

School *A* is in a very good residence section near the university. The majority of the people own their homes, which are surrounded by well-kept lawns. A portion of the university campus, as well as many wooded lots afford ample playground for the children. The school building is modern in every respect, having been completed about six years ago. The children of the first and second grades spend alternate half hours in a well-equipped Portable where their play is supervised by a teacher who has specialized in this work. This school is considered one of the most desirable in which to teach in the city and only well qualified teachers obtain the positions.

The chief occupations of the fathers of the children in this district were managers, proprietors and officers of manufacturies and stores, traveling salesmen, real estate and insurance agents, and a professional group composed of professors, doctors, lawyers, architects, and ministers. A more complete analysis and grouping of the data by the occupations of the fathers will be given later in this paper. All the children of the first and second grades were tested, and as time did not permit completing the third grade, the children were taken alphabetically. All the children in the grades tested were American born and of English-speaking parents. The testing was done in a hall where occasionally some one passed but otherwise there was no disturbance; and no third party was present when the examination was made. The child's name, date of birth and father's occupation were recorded in every case and checked by the teacher's record.

School *B* is situated near the railroad in a poor factory district of Columbus. The houses average about four or five rooms and are usually built very near the street. They are often in very bad repair, having been in that part of Columbus which was flooded three years ago. The usual rent is from $8 to $10 a month. Where there are yards they are so ill-kept and muddy that the street is the common playground f r the children. The school house is old and has no inside toilet facilities or up-to-date equipment. There is one saloon on the corner opposite the school building and two others within a block and a half of it. The fathers, if still in the family, are receiving low and irregular wages and almost one half of the fathers of the children in this district belong to the unskilled and casual labor group. The remainder were in the more skilled mechanical trades or were teamsters or delivery men. The mothers are often away all day working in the factories or doing laundry work to supplement the husband's income, or in many cases to support the family entirely. The parents have little idea of the value of education and the children often stop school and go to work as soon as they can secure their working papers. The Associated Charities say that probably 50 percent of the families in this district are registered with some kind of philanthropic organization. The children are often very poorly clad, far from clean and frequently undernourished. Many of the teachers in this school are young and have not taught a great while.

In the case of several children scoring lowest, other members of the family were tested. In five cases the mothers scored only from 47 to 54 points or a mental age of about eight years, and sisters and brothers were far below their age level.

The negroes and children of non-English speaking parents were excluded in this school, but every other child in the first, second and third grade was tested, making a total of 136 children. The testing was done in a small room free from all disturbing elements.

In both schools the tests were given during school hours by one examiner[1] and in all cases doubtful credits were discussed and decided upon by the authors jointly. The child's

[1] Miss Coler.

age to the nearest month was determined by subtracting the date of the birth from the date the tests were given. A given age group includes all children from the middle of the year below to the middle of the one above. Thus, in the group of six-year-olds are included all boys (or girls) from five years seven months to six years six months, inclusive.

Results

The results for the total 301 Columbus school children will first be considered and their scores compared with the scores for Cambridge, Mass., school children of the same ages. These results are presented in Table I. The first

Table I

Age	Columbus			Cambridge			
				English		Non-English	
	No.	A.	M.	No.	A.	No.	A.
6	37	34.1	33	55	29	16	27
7	97	42.8	45	48	35	25	31
8	81	54.7	55	47	41	14	37
9	59	57.3	59	43	56	31	48
10	14	55.9	56	53	62	23	56
11	7	49.6	50	55	65	24	62
12	5	54.8	55	40	77	20	67

A.—Average.
M.—Median.

column gives the ages, the second the number of Columbus children tested at each age, the third and fourth the average and the median scores for Columbus children at each age, while columns five and six give for comparison the number and average scores for Cambridge children of English-speaking parents and columns seven and eight the same for children of non-English-speaking parents.[1]

The results are shown graphically in Fig. 1. Graph *A* is for Columbus children, both schools combined; Graph *B* for Cambridge, English-speaking group, and *B′* for Cambridge, non-English-speaking group. After nine years the number of Columbus children at each age is very small and after ten years composed of children from the unfavored school alone.

[1] Point Scale, pp. 66–67.

This selection is clearly shown by the drop in the curve after nine years, and is due to the fact that in our present school system a child is behind if he is over nine years of age and in the third grade.

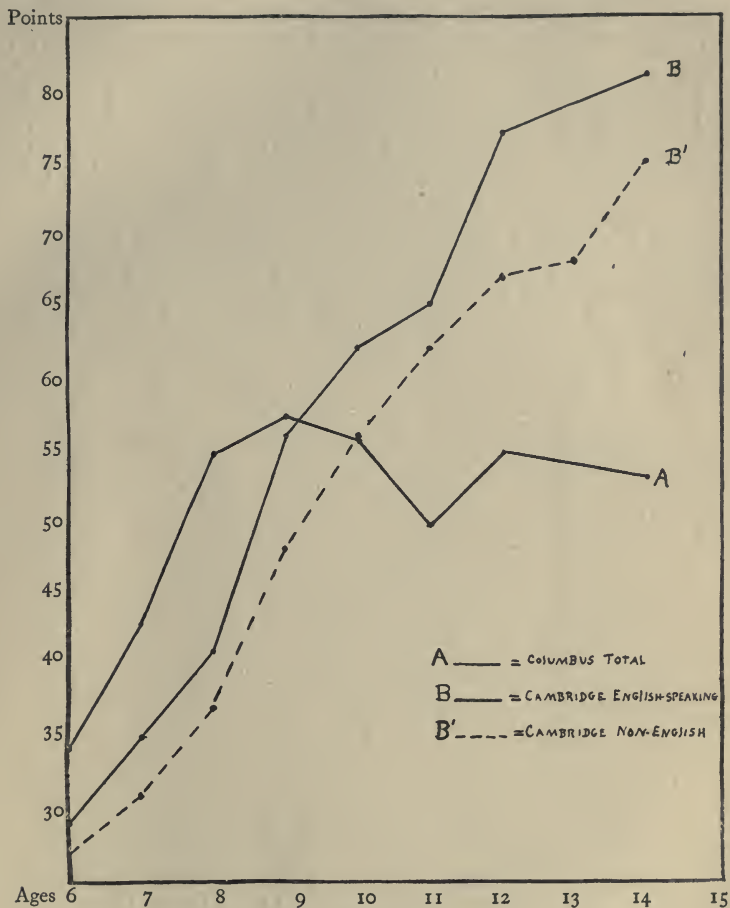


Fig. 1.

The most striking feature in these results is the evident superiority of the Columbus children up to ten years; but it will be shown in the sequel that their superiority is entirely due to the influence of the favored group and that when the groups are separated the correspondence between the children of the two cities is even closer than might have been expected.

TABLE II

		Age				
		6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Cambridge Heterogeneous	Number	71	73	61	74	76
	Score	29	34	39	52	59
Total Columbus	Number	37	97	81	59	14
	Score	34.1	42.8	54.7	57.3	55.9
Combined Average	Number	108	170	142	133	90
	Score	30.7	39.0	48	54.4	58.5

The Columbus results from the sixth to the tenth year inclusive may be combined with the Cambridge results for the same ages to help in the standardization of the scale.

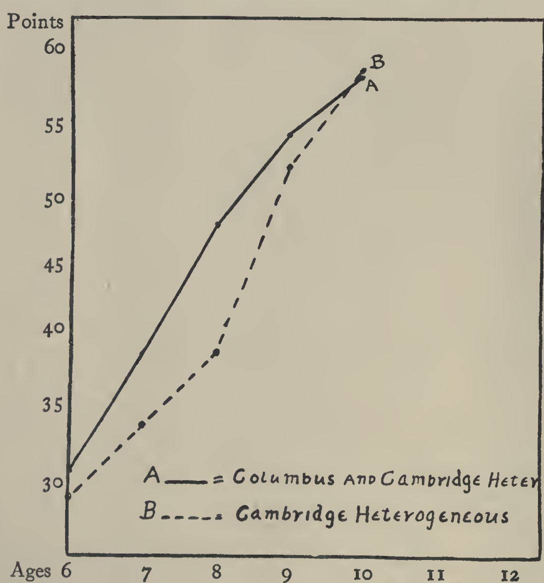


FIG. 2.

At first the Columbus results were combined with the Cambridge heterogeneous group[1] in the following way. The number of children at each age in the group was multiplied by the average score of that age for Columbus and Cambridge

[1] Point Scale, pp. 64–65.

separately. Then these results were added and divided by the total number at that age. Table II. shows the average score and number of pupils at each age in each group and the combined averages or new norms obtained. The latter are also shown graphically in Fig. 2. The solid line represents the combined norm for the total 643 children of Columbus and

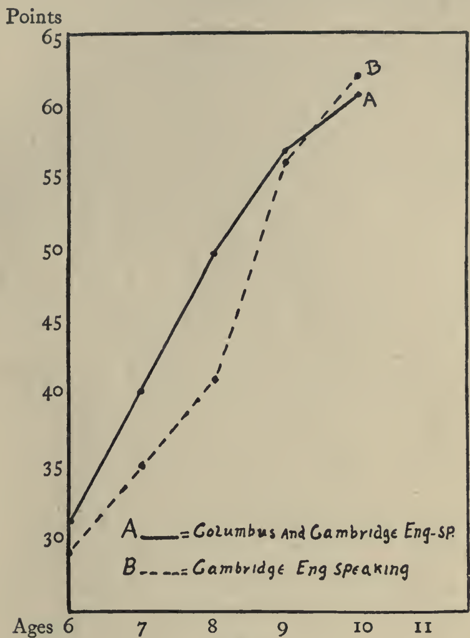


FIG. 3.

Cambridge; and the dotted line is the original norm from six to ten years for the 355 Cambridge children alone.

As all the children tested in the Columbus schools were of English-speaking parents it seemed fairer to use as a standard for judging the Columbus pupils a norm made from the Columbus results combined in the same way with the Cambridge group of children of English-speaking parents,[1] instead of the heterogeneous group.

These norms are shown in Table III. and graphically in Fig. 3. In this case the solid line represents the combined

[1] Point Scale, pp. 66–67.

norm for 245 Cambridge children of English-speaking parents and 288 Columbus children, or a total of 534 children. The broken line shows the original norm for the Cambridge English-speaking group alone. Selection entering after nine years in the Columbus results will explain the slight drop in the solid curve from nine to ten years. This curve was used as a norm for determining the mental age and coefficient of

TABLE III

		Ages				
		6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Cambridge English-speaking	Number	55	48	47	43	53
	Score	29	35	41	56	62
Total Columbus	Number	37	97	81	59	14
	Score	34.1	42.8	54.7	57.3	55.9
Combined Average	Number	92	145	128	102	67
	Score	31.1	40.2	49.7	56.8	60.7

mental ability of the Columbus children. For scores above or below the six, seven, eight, nine and ten year averages the norm for the English-speaking group of Cambridge alone had to be used.

COMPARISON OF DIFFERENT SOCIAL GROUPS

The Columbus results from Schools *A* and *B* will now be compared. Tables IV. and V. show the individual scores by sex and age for the favored and unfavored schools respectively. The average score for each age and each sex and also for both sexes combined are likewise given in each table.

These results are shown graphically by Fig. 4, curves *A* and *B*. The favored school is from 21 percent to 32 percent superior to the unfavored school varying with the chronological age. The curve drops at nine years in School *A* and at ten years in School *B*, showing that the selection previously mentioned enters earlier in the former. After ten years the numbers in the unfavored group are very small and the scores are not representative of these age groups.

As these results are very similar to those found in the Cambridge investigation it is interesting to compare them

Table IV

Favored *A*

6		7		8		9		10	
(8) M.	(9) F.	(36) M.	(23) F.	(26) M.	(27) F.	(16) M.	(14) F.	(2) M.	(4) F.
	——			——			——		
——	....		——	....	——	——	....	——	
29	31	22	32	49	36	44	54	66	39
30	36	——	....	51	41	....	55	68	——
....	——	34	36	51	....	50	61	——	49
36	39	35	38	52	45	52	62		64
——	39	35	40	52	49	56	63		75
40	41	....	43	52	49	60	63		——
42	42	40	45	53	51	61	65		
47	43	41	45	53	51	62	65		
47	....	42	49	55	55	63	67		
48	52	43	49	55	55	64	68		
....	54	45	49	55	55	64	68		
		47	49	56	56	66	71		
		47	——	58	57	69	——		
		47	50	58	58	69	72		
		48	51	——	59	——	73		
		48	51	60	59	73	....		
		48	51	62	——	77			
		49	52	63	60	79			
		——	52	64	60	....			
		50	52	65	62				
		50	53	66	63				
		52	53	66	63				
		54	55	67	65				
		54	59	69	66				
		54	....	72	67				
		54	67	....	67				
		55		75	69				
		55		76	71				
		56			73				
		56			....				
		58							
		58							
		59							
		60							
		....							
		64							
		64							
		66							
		71							
		72							
Ave........39.8	41.9	48.1	48.7	59.8	57.9	63.1	64.8	67	56.8
40.9		48.4		58.8		64		60.2	

—— = 25 percent above or below general norm.

.... = 25 percent above or below group norm.

Table V

Unfavored *B*

6 Yr.		7 Yr.		8 Yr.		9 Yr.		10 Yr.		11 Yr.		12 Yr.		14 Yr.
(14) M.	(6) F.	(24) M.	(14) F.	(14) M.	(14) F.	(11) M.	(18) F.	(4) M.	(4) F.	(6) M.	(1) F.	(3) M.	(2) F.	(1) M.
					....			....	....					
16	17	14	19	22	35	35	24	43	45	36	46	46	55	53
21	....	21	22	25	——	35	....	——	——	48	——	53	——	——
....	23	25	22	27	38	....	42	54	50	——	——	57	60	——
22	——	25	24	....	42	38	——	57	52	50		——	——	
23	28	26	....	36	44	——	44	60	61	55		——		
23	29	26	28	——	45	43	44	....	....	58				
——	29	....	——	39	48	47	46	——	——	72				
25	——	27	34	44	49	49	47			——				
25	....	27	37	45	49	52	50							
25	42	28	37	45	51	53	50							
26		29	43	46	52	58	51							
29		29	....	48	57	58	53							
33		29	44	50	57	59	54							
34		——	45	52	57	——	54							
....		32	45	——	——	....	58							
——		32	——	....	....		59							
39		35	54	62	68		61							
55		37	58	62			61							
		40					....							
		41					67							
		41					——							
		42					75							
		43												
		....												
		44												
		——												
		50												
		54												
Ave..... 28.3	27.2	33.2	36.6	43.1	49.4	47.9	52.2	53.5	52	53.2	46	52	57.5	53
28.3		34.4		46.2		50.1		52.8		52.1		54.2		53

—— = 25 percent above or below general norm.
.... = 25 percent above or below group norm.

with the two similar groups there.[1] Table VI. shows these results and they are also shown graphically in Fig. 4. Only the kindergarten and the First Grade were tested in the Cambridge favored school so the curve is short.

Table VI

	Favored School						Unfavored School				
	Age.........	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Cambridge ..	Score..	42	49	56	.....	.....	29	35	41	56	62
Columbus...	Score..	40.9	48.4	58.8	64	60.2	28.2	34.4	46.2	50.1	52

[1] Point Scale, p. 74, p. 66.

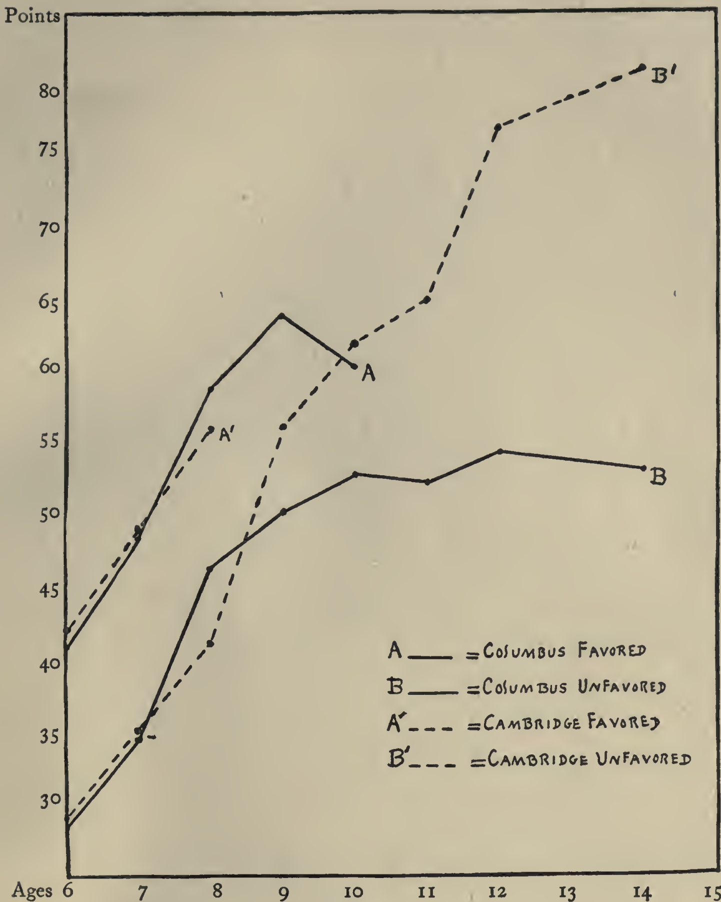


FIG. 4.

TABLE VII

		School *A*		School *B*	
		M.	F.	M.	F.
Cambridge	No.	11	13	29	26
	Score	42	40	29	30
Columbus	No.	8	9	14	6
	Score	39.8	41.9	28.3	27.2

These differences between the favored and unfavored groups and the striking similarity between the Columbus and Cambridge results are even more clearly shown by the data for the six-year-old groups given in Table VII.

Comparison of Sexes

Tables IV. and V. should be consulted for the individual scores. The averages for boys and for girls in both schools combined are shown in Table VIII.

Table VIII

	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Males	32.5	42.2	54	56.9	58
Females	36.3	44.1	55	57.7	54.4

Fig. 5 shows these results graphically. The superiority of the girls over the boys seems marked up to ten years. By examining Table IX. and Fig. 6, which give the averages for

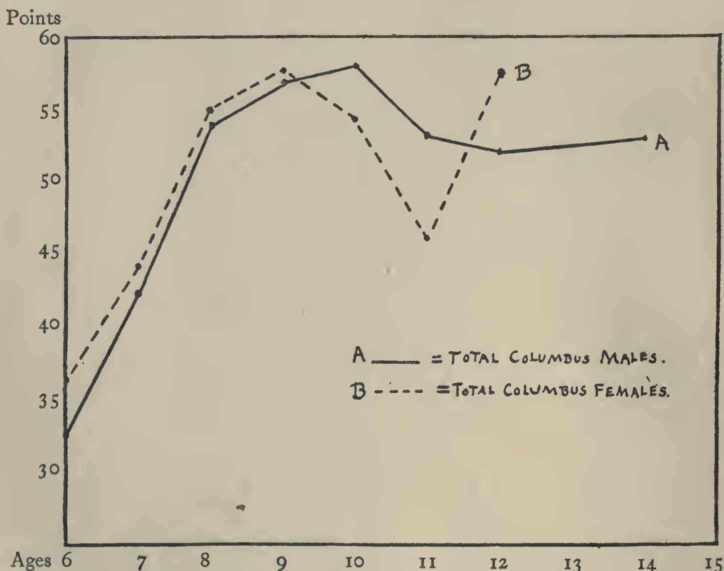


Fig. 5.

each sex in each school separately it will be seen that this superiority of the girls is brought about almost entirely by the girls of the unfavored school.

TABLE IX

	6 Yr.		7 Yr.		8 Yr.		9 Yr.		10 Yr.	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Unfavored	28.3	27.2	33.2	36.6	43.1	49.4	47.9	52.2	53.5	52
Favored	39.8	41.9	48.1	48.7	59.8	57.9	63.1	64.8	67	56.8

The difference between the sexes is particularly marked in the eight-year group of the unfavored school, where there is an equal number of boys and girls. At the same age in the favored school the boys have the advantage. These

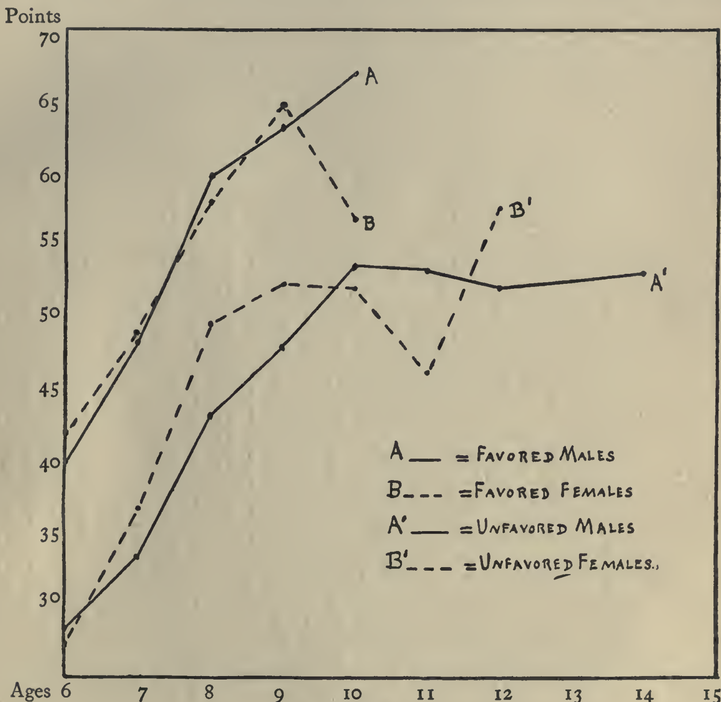


FIG. 6.

results seem to bear out the theory held by Thorndike[1] and others that females deviate less from the norm than males; for there is less difference between the performances of the girls in the two schools than there is between the performances of the boys.

[1] 'Educational Psychology', second edition, 1910, pp. 33–43.

Table X

	First Grade				Second Grade				Third Grade			
	No.	M.	A.	Age	No.	M.	A.	Age	No.	M.	A.	Age
Favored	57	47	45.3	6–7½ mo.	55	55	56.2	7–8 mo.	53	64	63.5	8–8½ mo.
Unfavored ..	52	27.5	29.1	6–9½ mo.	44	45.5	46.2	8–5 mo.	40	54.5	55.1	9–9 mo.
Difference...	...	19.5	15.7	2 mo.	...	9.5	10	4½ mo.	...	9.5	8.4	1 yr.

M.—Median.
A.—Average.
Age—Chronological age in years and months.

Table Xa

First Grade				Second Grade				Third Grade			
Favored		Unfavored		Favored		Unfavored		Favored		Unfavored	
M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
57		52		55		44		53		40	
22	31	14	17	44	36	29	24	50	49	43	44
29	32	16	19	49	39	35	35	52	49	45	45
30	36	21	22	50	41	36	37	52	51	48	46
34	36	21	22	51	45	37	42	53	54	50	46
35	38	22	23	51	49	39	42	53	55	53	50
35	39	22	24	52	49	40	43	56	56	53	50
36	39	23	28	52	49	42	44	58	57	54	50
40	40	23	28	54	51	43	44	60	58	55	51
40	41	25	29	54	51	43	44	60	61	57	51
41	42	25	29	55	51	44	45	61	63	57	52
42	43	25	34	55	52	44	47	62	63	58	53
42	43	25	37	55	53	45	49	62	64	58	54
43	45	25	38	55	55	46	49	63	65	58	54
45	45	25	42	56	55	46	52	64	65	59	55
47	49	26	45	56	55	47	54	64	65	60	57
47	49	26	45	56	55	48	57	64	67	62	58
47	50	26	48	58	59	49	57	66	67	72	59
47	51	27		58	59	50	58	66	68		60
47	52	27		58	60	50	67	67	68		61
48	52	27		59	60	52		68	71		61
48	52	28		63	62	52		69	71		61
48	53	29		64	62	53		69	72		68
48	54	29		65	63	54		69	73		75
49	59	29		66	66	55		72	73		
50	63	32		66	67	62		73	75		
52		32		66	67			76			
54		33		72	69			77			
54		34		75				79			
55		35									
60		35									
64		36									
71		38									
		39									
		41									
		41									
Ave.......45.3	45.4	28.1	31.2	57.7	54.8	45.6	46.8	63.8	63.2	55.4	54.8
45.3		29.1		56.3		46.2		63.5		55	

Comparison of Grades

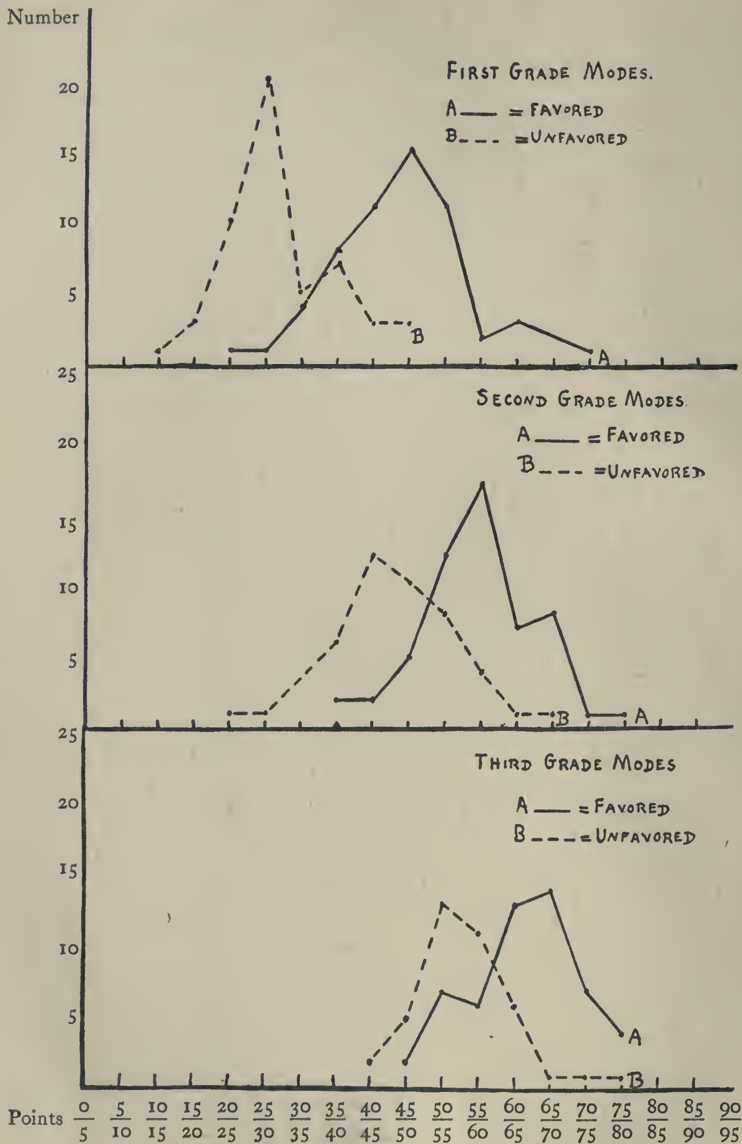
Table X. gives in convenient form the number of pupils, the median and average scores and the average chronological ages for each grade in each school. The individual scores for each school and each grade are given on Table X*a*.

It will be seen that the superiority of the favored school is much greater for the first than for the second or third grades.

By examining the Figs. 7, 8, and 9, showing the modes for the three grades, the same thing will be found to be true. The favored school, first grade, has a mode of from 45 to 50 points and the unfavored school from 25 to 30 points or a difference of 20 points. The difference in the second grade is 15 points and in the third the difference lies between 10 and 15 points as the mode is not distinct here.

These results seem to show that the difference between the children of the two schools is greatest when they enter the school and that it becomes less from grade to grade. This would mean that the school work tended to equalize some of the original difference.

It is necessary, however, to examine further to see if there may be another explanation before accepting this conclusion. The average chronological ages for the three grades are also given in Table X. The ages are nearest the same in the first grade where there is only two months difference, but this difference increases to over a year in the third grade. This age difference is brought about by the fact that in the unfavored school so many children are in a low grade for their years. In our present school system a child enters at the age of six and would therefore be in the third grade at the age of eight to nine years if regularly promoted. Out of the forty children in the third grade of the unfavored school there were eight in the ten-year group, five in the eleven-year group, four in the twelve-year group and one in the fourteen-year group. That is, about 45 percent of the children in that grade are older than they should normally be in the third grade. On the other hand there are only five older than the nine-year group in the third grade of the favored school and none of these is above ten years six months.


Number
First Grade Modes.
A —— = Favored
B - - - = Unfavored
Second Grade Modes.
A —— = Favored
B - - - = Unfavored
Third Grade Modes
A —— = Favored
B - - - = Unfavored
Points
0/5 5/10 10/15 15/20 20/25 25/30 30/35 35/40 40/45 45/50 50/55 55/60 60/65 65/70 70/75 75/80 80/85 85/90 90/95

Figs. 7, 8, and 9.

If we now compare the average scores for each age group in the two schools we find that the differences from year to year are fairly constant. These figures are shown in Table XI.

TABLE XI

	6 Yr.	7 Yr.	8 Yr.	9 Yr.	10 Yr.
Favored	40.9	48.4	58.8	64	60.2
Unfavored	28.3	34.4	46.2	50.1	52.8
Difference	12.6	14	12.6	13.9	7.4

The difference is practically the same for the six- and seven-year groups and the decrease at ten years is probably due to selection entering in both schools at this age. Moreover there are too few ten-year-old children for a fair comparison. There is thus very little diminution of the initial difference with age; and the fact that the difference in mental ability between the two schools is less in the second and third grades than in the first, must therefore be largely due to the older children in the second and third grades of the unfavored school who tend to raise the average of points scored in that grade and so make it approach nearer to the favored school's results.

A coefficient of mental ability for each child was obtained by dividing the number of points actually scored by the number of points which should be scored at his chronological age. The standard used was the one obtained by combining the total Columbus results with the results of the Cambridge children of English-speaking parents.[1] The mental age of the child can also be obtained by referring his score to the age at which this score should be obtained.

The average coefficient of mental ability, average mental age and average chronological age for each grade in each school are shown in Table XII.

There is a uniform difference of about a year in the average chronological age of the children from grade to grade in the favored school. On the other hand there is a difference of almost two years between the average chronological

[1] See Table III.

age of the children in the first grade and those of the second grade in the unfavored school. This indicates that the children of the first grade in the unfavored school remain on the average two years in that grade before being promoted. Moreover the mental age for the children of the unfavored school is about a year lower than their actual chronological age. It would therefore seem that the children of the unfavored group are not mature enough mentally when they enter school at the age of six to be able to do the prescribed

TABLE XII

Grade	Favored			Unfavored			Difference		
	I		III	I	II	III	I	II	III
C. A.	6–7½ mo.	7–8 mo.	8–8½ mo.	6–9½ mo.	8–5 mo.	9–9 mo.	2 mo.	4½ mo.	1 yr. 5 mo
M. A.	7–9	9–3	10–4	5–9	7–9	8–11	2 yr.	1 yr. 6 mo.	1 yr. 5 mo
C. M. A.	1.24	1.26	1.18	.77	.98	.93	.47	.28	.25

C. A., chronological age.
M. A., mental age.
C. M. A., coefficient of mental ability.

work. The children of the favored group, on the contrary, are mentally a year ahead of their chronological age and so these children might equally well enter school at an earlier age and be able to do the required work. It is also interesting to note in this table that although the difference in the mental age between the two schools decreases from grade to grade, the difference in their chronological age increases, thus keeping a fairly constant difference of two years between the schools in the grades tested. Binet estimated that there was a difference of about one and one half years between children of different social classes, and M. Morlé found about a year's difference in the groups he studied in Paris.

COMPARISON OF SEPARATE TESTS

Table XIII. shows the average scores for each of the twenty tests making up the Point Scale. Scores for the favored and unfavored boys and girls are given separately and the combined average and the difference between the schools is also given. The difference, although sometimes very small, is always on the side of the favored school.

Table XIII

Test	Fav. Boys	Unfav. Boys	Fav. Girls	Unfav. Girls	Total		Diff.
					Fav.	Unfav.	
1	2.90	2.62	2.98	2.91	2.94	2.77	.17
2	3.73	3.18	3.88	3.22	3.8	3.2	.60
3	2.97	2.77	2.98	2.71	2.98	2.74	.34
4	3.18	2.48	3.19	2.67	3.19	2.55	.64
5	3.27	2.02	3.24	2.67	3.26	2.35	.91
6	2.22	1.87	2.20	1.94	2.21	1.91	.30
7	6.76	5.90	6.76	6.	6.76	5.95	.81
8	1.73	1.46	1.54	1.57	1.64	1.52	.12
9	4.58	3.06	4.28	3.59	4.43	3.33	1.10
10	5.42	3.98	5.18	4.64	5.30	4.31	.99
11	2.09	1.48	2.25	1.32	2.17	1.40	.77
12	2.43	2.06	2.27	2.25	2.35	2.16	.19
13	1.51	.78	1.64	1.40	1.58	1.09	.49
14	1.66	.52	1.16	.98	1.41	.75	.66
15	3.40	2.27	3.50	2.40	3.45	2.34	1.11
16	1.65	1.09	1.38	1.45	1.52	1.27	.25
17	2.05	.79	2.01	.86	2.03	.83	1.20
18	1.38	.39	1.37	.88	1.38	.64	.74
19	.89	.42	1.19	1.05	1.04	.74	.30
20	1.30	.75	1.35	.90	1.33	.83	.50

The five tests in which the greatest superiority of the favored school is shown are given here in the order of the amount of difference.

Test number 17, absurd statements, is described by the authors[1] of the Point Scale as primarily a test for 'logical judgment based on imagination, analysis and reasoning.'

Number 15, comprehension of questions, tests 'practical judgment involving memory and imagination.'

Number 19, comparison of familiar objects involves 'analysis and comparison of remembered objects and attention.'

Number 10, concrete definitions, tests 'ideation (association) and analysis.'

Number 5, which shows the fifth greatest difference between the two schools, consists of counting backward from 20 to 1. Here the mental traits involved are 'memory, imagination and attention.' Probably the reason for the higher average score in the favored school is the fact that games involving counting backwards were played by the children of this school and when they were given this test

[1] Point Scale, p. 8.

they knew what was expected, with little explanation; while counting backward seemed a new process for most of the younger children of the unfavored school.

The results of test number 19, abstract definitions, would probably have shown a greater difference between the two schools but for the fact that the average for the unfavored school was raised because a greater number of unfavored children were able to define 'charity.' The familiarity with this term is easily understood, as charity in some form is extremely common in the unfavored district.

With the exception of number 5 it seems, then, that the greatest difference in the performances of the children of the two schools lies in the tests involving primarily analysis and abstraction. This agrees with Binet's analysis of the difference between the scores of the children in the private school at Brussels and those from the poorer section of Paris. Binet found the Belgian children superior in tests involving criticism, comparison, abstract definitions, absurdities, or in those involving thought in the higher sense. He pointed out that some of these tests probably put a premium on the ready use of language and that the children from the higher social class had the advantage in this respect.

The five tests which show the least difference between the two schools will now be considered. Number 8, arranging weights, is described as having to do with 'kinæsthetic judgment, ideation and attention.'

In number 1, æsthetic judgment, the difference between the two schools is also very slight. This is described as having to do with 'æsthetic judgment involving perception, association and analysis.' As this is probably the easiest of all the tests, it was seldom missed by either school, which probably accounts for the small difference in the average scores.

'Motor coördination and visual perception' are the traits involved in Number 12, copying a square and a diamond.

Number 3, comparison of lines and weights, tests 'discrimination' of the visual type in the first part and of the kinæsthetic type in the second.

Number 16, drawing of designs from memory, involves 'visual memory, perception, attention and motor coördination.'

With the exception of number 1 the above tests all involve sensory-motor functions to a great extent and have to do primarily with kinæsthetic judgment and motor coördination.

To summarize: The results from the single tests show the greatest difference in tests involving analysis and abstraction and the least difference in those involving primarily motor coördination and kinæsthetic judgment. This agrees with Thorndike's view that individuals differ least in sensory motor functions and most in analysis and abstraction.[1]

Individual Scores

The individual scores will now be considered as to the number of children who are twenty-five percent above or below the norm of their age, when judged by the general norm and when judged by the average of their own school. Tables IV. and V. show the individual scores by sex and age groups for each school. In each group the number of individuals whose scores depart by twenty-five percent or more

Table XIV

		No.	Percent of Total
Using general norm.....	Favored group, 25% below the norm...	3	1.8
	Favored group, 25% above the norm...	73	44.2
	Unfavored group, 25% below the norm.	44	32.4
	Unfavored group, 25% above the norm.	11	8.1
Using separate norms for each school..........	Favored group, 25% below the norm...	10	6.2
	Favored group, 25% above the norm...	10	6.2
	Unfavored group, 25% below the norm.	19	16.5
	Unfavored group, 25% above the norm.	16	13.9

from the norm for that age is indicated. The solid lines indicate the individuals who deviate twenty-five percent or more from the general norm for that age group. The dotted lines indicate the number who deviate twenty-five percent or more from their own group norm. The exact numbers are shown in convenient form in Table XIV.

[1] Thorndike, E. L., 'Educational Psychology,' second edition, 1910, pp. 218–223.

As will be seen, nearly one third of the children of the unfavored school have a coefficient of mental ability of .75 when judged by the general norm. This has been suggested by Dr. T. H. Haines[1] as a criterion of feeble-mindedness. He has shown that it is a more lenient criterion than four years' retardation above the thirteenth year. For the ages here considered it is roughly equal to about two years' retardation. The number of the favored group twenty-five percent or more below the general norm for their age is only 3. On the other hand the favored school shows over 44 percent, twenty-five percent or more above the norm, while the unfavored school has only eight percent. If these results are compared with those of Cambridge[2] where practically the same numbers are twenty-five percent above and below the average, it appears that the unfavored school is greatly weighted by subnormals and the favored school by supernormals. The question now arises whether it is fair to judge both schools by the same standard. Let us see what the results would show if the unfavored and the favored schools were judged by their own norm or average. In this case the standard will of course be lowered for the unfavored group and raised for the favored group. The dotted lines indicate this in Tables IV. and V. The averages of the pupils were only used up to and including nine years, however, for beyond nine years the scores were so low and there were so few cases that a fair average could not be obtained. Up to the ten-year group, then, the number twenty-five percent above and below the norm for each school is about equal, as was the case in the Cambridge schools.

Considering the great number which must be classed as very inferior intellectually, if not feeble-minded, in the unfavored school if the same standard is used for judging both schools, it seems unfair that groups of children from such different social classes should be judged by the same norm. If the sociological factor is not considered in clinical diagnosis, it seems probable that too high a standard will be expected

[1] Haines, T. H., 'Relative Values of Point Scale and Year Scale Measurements of 1,000 Minor Delinquents,' *J. Exp. Psychol.*, 1, 51–82.

[2] Point Scale, p. 55.

of the unfavored individuals and so the degree of mental deficiency which might exist would be overestimated.

Study of Occupations

The children will now be grouped in the two schools by the occupation of the father. The 165 children in the favored school were classified as follows:

(1) Professional group	32
Professors	17
Doctors	6
Lawyers	3
Editors	3
Architects	2
Ministers	1
(2) Proprietors, officers and managers of manufactories and stores	32
Proprietors	16
Managers and officers	11
Building contractors	5
(3) Traveling salesmen, insurance agents and real estate dealers	39
Traveling salesmen	33
Salesmen	5
Insurance agents	6
Real estate dealers	6
(4) Clerical workers	21
Clerks	13
Bookkeepers and accountants	5
Cashiers	3

(5) The remaining forty-one children were classified in a miscellaneous group.

The main groups in which the 136 children of the unfavored school were classified are as follows:

(1) Laborers, unskilled	60
This group includes odd-job workers and all unskilled and casual laborers.	
(2) Skilled mechanical trades	45
Railroad engineers and mechanics	13
Metal workers	12
Building trades	10
Electricians	3
Shoe cutters	3
Miscellaneous skilled workers	4
(3) Teamsters and delivery men	19

The remaining twelve children in the unfavored school were put in a miscellaneous class.

In comparing the groups in the favored school, the children from one group were matched with children as nearly as possible the same age in the other group. The results are here shown for the professional group compared with the traveling salesmen. Thirty children from each group were matched in this comparison.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Professional group	7 yr. 7 mo.	9 yr. 8 mo.	1.42
Traveling salesmen	7 yr. 7 mo.	9 yr. 3 mo.	1.26

The average chronological age is thus the same, but the professional group averages 5 months superior to the traveling salesmen group mentally.

The clerical workers were rather a mixed group. Many of the clerks were chief clerks and the group as a whole is small. Comparing seventeen from this group with seventeen from the manager group of corresponding ages, the following results were obtained.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Clerical	7 yr. 10 mo.	9 yr. 1 mo.	1.22
Managing class	7 yr. 8 mo.	9 yr. 5 mo.	1.24

The managing class averages two months younger chronologically, but shows about four months' superiority mentally.

In the unfavored school thirty-six children were matched from the skilled and unskilled laboring classes.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Skilled	7 yr. 11 mo.	7 yr. 6 mo.	.93
Unskilled	8 yr. 1 mo.	6 yr. 11 mo.	.80

The unskilled group here has the advantage of being two months older; nevertheless its average mental age is seven months less and its coefficient of mental ability .13 less than the average for the skilled group.

When a group was selected from laborers to match the ages of the eighteen children in the teamster group, the following results were obtained.

	Av. Chron. Age	Av. Mental Age	C. M. A.
Teamsters	7 yr. 10 mo.	7 yr.	.83
Unskilled laborers	7 yr. 10 mo.	7 yr. 2 mo.	.88

The teamsters appear to have as a class an even lower mentality than the unskilled laborers, but this is a very small group and results might be different if larger numbers were compared.

The following is a summary of the results for the various occupation groups, irrespective of schools and ages:

	No.	Av. Chron. Age	Av. Mental Age	C. M. A.
Professional	32	7 yr. 3 mo.	9 yr. 8 mo.	1.42
Traveling salesmen	39	7 yr. 6 mo.	9 yr. 2 mo.	1.26
Proprietors, etc.	34	7 yr. 10 mo.	9 yr. 1 mo.	1.21
Skilled	63	8 yr.	7 yr. 10 mo.	1.12
Unskilled	60	8 yr.	7 yr. 1 mo.	.83

It is noteworthy in this table that although the chronological age increases from group to group, the mental age decreases.

Conclusions

Our results corroborate the conclusions of Binet in France, Hoffman in Germany and Yerkes *et al.* in United States that there is a very considerable dependence of intelligence upon sociological condition. We have further shown that when children are classified according to the occupations of their fathers, a striking correlation is shown between intelligence quotient and occupation group. Hence, if mental age rather than chronological age were used to determine the time for beginning school, the children of the professional group, for example, would begin school two years earlier than the children of the unskilled labor group; for the former mature intellectually much earlier than the latter.

Incidentally the results have shown that the correlation of intelligence and social status is probably higher for boys than for girls. The girls of the poorer school are considerably superior to the boys; but the boys of the better school are only at one age noticeably superior to the girls.

The superiority of the better classes is most evident in tests that involve higher mental processes like analysis and abstraction; but it is also shown to a lesser extent in sensory motor functions.

We have not discussed the causes of this relation of intelligence to social status for the very good reason that our data do not contribute anything towards a solution of the problem. They aim merely to establish the fact and amount of the difference, and could be used by adherents of the "Environment Theory" as well as by advocates of "Inheritance." Thus, the former could emphasize the quite evident differences in home and school environments, teaching staff, etc.; while the latter would point to the just as evident differences in the character and intelligence of the parents. It is worth noting that in the few cases where the mothers were tested, they showed a mental age about equivalent to that of their children. If intelligence quotient could be obtained for a number of successive generations with different environments, such data might contribute to a solution of the problem.

We have also omitted discussion of the percentage of feeble-minded in the different social groups, and have concerned ourselves only with the variations in intelligence; for we consider the diagnosis of feeble-mindedness and the measurement of intelligence two quite distinct though related problems that had better not be confused. Diagnosis depends upon a number of other considerations as well as the psychological. The physical aspect can not be wholly neglected; and the importance of the patient's life history is generally acknowledged, especially if amentia is to be distinguished from dementia.

There are also sociological considerations which the advocates of a purely psychological concept of feeble-mindedness must acknowledge as soon as they consider the problem of the dividing line between normal and feeble-minded intelligence. The various criteria: two to four years' retardation, an intelligence quotient below .75, the 'lowest three percent' of the population,[1] etc., are all ultimately based upon sociological, or socio-legal considerations. They are merely statements of the limits below which an individual fails to attain certain social standards of living.

Now, since these standards of living vary greatly from group to group, it seems only reasonable that the above mentioned limits (and intelligence norms) used in diagnosis should vary too. Otherwise, we might be obliged to classify whole races as feeble-minded. All Hottentots would probably be feeble-minded, if judged by Anglo-Saxon intelligence norms; and similarly the majority of the children of the unskilled labor group might be classed feeble-minded if judged by norms for the professional group. The facts discussed in this paper should therefore find a place among the various considerations upon which careful diagnosis depends; but we have preferred to confine ourselves to the strictly psychological problem: the measurement of the intelligence of different social groups.

[1] Pintner, R. and Paterson, D. G., 'A Psychological Basis for the Diagnosis of Feeble-mindedness,' *J. of Crim. Law and Crim.*, 7, May, 1916.

MENTAL TESTS WITH DELINQUENTS AND AUSTRALIAN ABORIGINAL CHILDREN

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During 1915 two groups of delinquent boys were examined by a new series of mental tests.[1] The first group consisted of boys ranging in age from nine to fourteen years who had been committed to the care of the Boys' Home at Burwood, Victoria. Some few were merely neglected children but the majority had been before the children's courts for various minor offences, chiefly truancy and petty thieving. Under ordinary environment these may be considered criminals in the making.

The tests given are based on the maze plan and the subject is required to find a way through the maze in a given number of trials under certain conditions.[2] Success requires the exercise of prudence in action, forethought, and general mental alertness. The tests are graded for the mental ages from three to thirteen years. It is not claimed that they enable us to arrive at the general mental age of the subject, though in the majority of cases there is a close correlation between results by these tests and by the Binet-Simon. In another investigation, out of one thousand normal children examined 70.6 percent passed by these tests within one year of their Binet ages.

Mental age per the Porteus tests means that, in the capacities of foresight, prudence, resistance to suggestion, and sustaining the attention, the child has reached the average development of the age assigned to the tests passed under the given conditions. Children below ten years of age are most often placed about one year higher in mental age by the Porteus tests than by the Binet. This difference is accounted for

[1] Tests published by C. H. Stoelting Co., Chicago.

[2] See *J. of Exp. Ped.*, June, 1915, or *Amer. J. of Psycho-Asthenics*, June, 1915.

mainly by the fact that the former are motor tests and therefore make a more universal appeal to child interest and secondly because they were arranged so as to permit of a comparison between the mentally deficient and the dull normal child. This explanation is necessary to meet the possible objection that the tests were too difficult.

Since impulsive and ill-considered action had been characteristic of the delinquent boys' social behavior it was thought likely that their performances in the tests would reflect to some degree the same faults of disposition. How this expectation was fulfilled may be seen by reference to Table I. below. In some cases where foresight was shown in the subject's preliminary study of the problem before beginning its working, failure resulted through a too imprudent trust in the memory. Not a few looked at the maze before beginning the test and remarked "I see the way out," and then went impulsively to work only to find that they had lost the plan and had taken a wrong turning, which, of course, meant instant failure, since corrections are not allowed. It was very rare indeed to find a child with the most intelligent method of attack, viz., a preliminary sizing up of the problem and then a careful and deliberative working—prudence and forethought in combination.

The following is a summary of the scores of the boys in the tests as compared with their chronological ages.

TABLE I

Passed Test	
Above age	0
At age	5
1 year below chron. age	3
2 years " " "	5
3 years " " "	5
4 years " " "	3
5 years " " "	1
Total	22
Average chronological age	12 years 8 months
Average test passed	10 " 4 "
Average deficiency	2 " 4 "

It is significant that no boy passed a test above his chronological age while less than 25 per cent. passed 'at age.'

In Table II. some interesting individual records are given together with a brief report by the superintendent of the Home as to each boy's character.

Those in whose personal reports there is a favorable entry are placed in Section A. Those whose social dispositions are not satisfactory are shown in Section B.

Table II

Section A

Case	Chron. Age	Test Passed	Deficiency	Superintendent's Report
C. D.	11 yrs.	10 yrs.	1 yr.	Quick witted, fairly reliable, and moderately intelligent.
G. T.	12½ yrs.	12½ yrs.	—	Truant, good open disposition fairly intelligent, conduct good.
A. A.	11 yrs. 9 mos.	12 yrs.	—	Fairly intelligent, good disposition.
E. S.	11 yrs.	11½ yrs.	—	Rather dull, but improving wonderfully.
W. G.	13 yrs.	12½ yrs.	½ yr.	Splendid memory, intelligent for his age.
C. C.	12 yrs.	11½ yrs.	½ yr.	Truant. Conduct good, fairly intelligent.
N. S.	12 yrs. 10 mos.	11 yrs.	1½ yrs.	Truant, easily led. Quiet disposition, fairly intelligent. Conduct now good.

Section B

Case	Chron. Age	Test Passed	Deficiency	Superintendent's Report
J. D.	14 yrs.	11 yrs.	3 yrs.	Associated with bad companions. Habit of petty thieving.
J. R.	12½ yrs.	8 yrs.	4½ yrs.	Illegitimate. Rather untrustworthy and dull.
F. M.	13½ yrs.	9 yrs.	4½ yrs.	Truant. Fairly intelligent but of a sly disposition.
R. F.	11½ yrs.	9 yrs.	2½ yrs.	Petty thief. Rather sullen disposition.
W. P.	11½ yrs.	9 yrs.	2½ yrs.	Neglected at home. Fairly intelligent but sly disposition.
C. J.	12 yrs.	10 yrs.	2 yrs.	A truant and of plausible disposition. Fairly advanced in school.

It will be seen that the tests have brought out to a rather remarkable degree the differences in character in the boys of the two groups. The total deficiency by the tests is for Section A—3½ years; for Section B—19 years. This accounts for 13 boys out of 22. In the remaining nine cases there is nothing noteworthy in the superintendent's reports other than the references to their intelligence.

It should be stated that these personal reports must be considered thoroughly independent. None of the results of the testing were known to the superintendent at the time the reports were furnished.

Reformatory Boys

The next investigation was undertaken at the Royal Park Reformatory, Melbourne. In the light of their records the subjects must be considered as youthful criminals. Table III. summarizes their results and speaks for itself.

Table III

Passed Test	
At age	0
2 years below chron. age*	2
3 years " " "	3
4 years " " "	4
5 years " " "	3
6 years " " "	5
7 years " " "	2
8 years " " "	1
Total	20

Average chronological age	15 years	6 months	
Average test passed	9 "	10	"
Average deficiency	5 "	8	"

Note.—In this table 15 years was taken as the upper limit of chronological age so as to provide a basis of comparison with the mental age. As a matter of fact the average age will be seen to have been above 15 years.

By comparison with Table I. it will be seen that, though the average age of the Reformatory boys was higher than that of the boys at the Home, they passed on the average a lower test. Some of the individual records are so interesting that I may be pardoned for giving some details.

Some of the offences charged against these lads were house-breaking (6); serious assault (causing grievous bodily harm) (3); larceny (7); obscene language (1); street gambling (1); obtaining money under false pretences (1); murder (1). The numbers in brackets here indicate the number of such offences.

The housebreakers showed on the whole better success in the tests than the others, the mental ages of the six being

12, 11, 11, 11, 10 and 8 years respectively, whilst their total deficiency amounted to $23\frac{1}{2}$ years. Those who had been convicted of serious assaults had the mental ages of 11, 9, and 8 years respectively, whilst the murderer's mental age was 9 years. Those four, however, who had committed crimes of violence had a total deficiency of 23 years, an average of $5\frac{3}{4}$ years as against the average of 4 years for the house-breakers. It would seem as if the more talented in this group of youthful criminals were already qualifying for a place in the aristocracy of crime, the skilled burglars. The clumsy insensate crimes of violence were left to boys of a much lower mental level. Three of the four mentioned above were sentenced for kicking a companion nearly to death. The murderer had decapitated his victim with an axe when they were returning from a joint hunting expedition.

It is most significant that only four of this group of 20 lads were suspected by the penal authorities to be at all deficient mentally. Among these was the murderer. But the point that should be emphasized is that there were other lads of a lower mental level who were considered fully responsible. When they have committed equally atrocious crimes, their mental deficiency too may be realized.

Provided that the offenders show an amount of low cunning in the commission of their crimes the police appear very loath to admit the fact of mental deficiency. An interesting instance of this was the case of S. B., aged $15\frac{1}{2}$ years, examined by me at the Melbourne Gaol. He had been committed for stealing letters during their transit from the post office to the city railway station. His plan was to represent himself to the driver of the mail van as an employee of the post office, sit at the back of the van, cut open the mail bags and abstract the contents. The police appeared very loath to agree to a theory of mental deficiency in his case and he was examined.

The highest test that he could pass in the Porteus series was that for 8 years and he required the two trials allowed for tests for 6 and 7 years. When examined per the Binet, his mental age was also eight years. He failed to tell the differences between a butterfly and fly, etc., he could not reckon the value of the stamps nor repeat five numerals correctly. In the nine year tests he could not give change nor arrange the weights and he failed in ten year tests throughout.

The dynamometer records were 17 kilos right hand and 10 kilos left hand—very low for a youth of 16 years of age. In the Fernald-Healy construction test he required two minutes thirty seconds.

The diagnosis of feeble-mindedness was, by the foregoing results, indisputable.[1]

Aboriginal Children

For comparison with the delinquent groups I am taking the examination results of some aboriginal children examined by myself at the Mission Station, Point MacLeay, South Australia.

Few of them were full-blooded, though in many cases they were the offspring of marriages between full-blooded aborigines and half castes. Results are summarized in Table IV.

Table IV

Passed Test	
3 years above chron. age	1
2 years " " "	1
1 year " " "	4
At age	12
1 year below chron. age	7
2 years " " "	1
3 years " " "	2
Total	28

Average age	10 years 2 months
Average test passed	9 " 9 "
Average deficiency	5 "

[1] Since writing this article, I have learned that this boy was examined by a medical officer at the gaol. He was found to be syphilitic but the equally important fact of his mental deficiency was unnoticed by the doctor, or was, at any rate, unmentioned! This implies either carelessness in examination or ignorance of modern methods of psychological examination. The question of the lad's mental and moral responsibility was not raised at the trial and he was sentenced to two years' imprisonment. The diagnosis of deficiency was not disputed but ignored.

On examining the individual records it was found that the younger children succeeded, relatively, far better than the older ones. Of 12 children who were over 11 years of age, 9 are retarded by the tests, the total deficiency being 16 years. Of the 16 children under 11 years of age, 6 passed tests above their chronological age, 9 passed 'at age' and 1 was retarded one year.

The increased difficulty of the tests for the upper years is not sufficient to explain this failure of the older children. This result bears out the view that the period of mental development is, in aboriginal children, comparatively short. This also accords with the experience of the teacher of the school, who says that he finds little difficulty in bringing his pupils through the lower grades but finds it very difficult indeed to educate them beyond about the fourth grade, or midway through the school course.

Dr. Gertrude Halley, chief schools' medical officer, who examined these children physically found them well developed for their ages, weight and height being above the average. Puberty appeared to be rather early established. The prepubescent period seems to be the most favorable for mental development, but is succeeded during early adolescence by a period in which the common racial characteristics of indolence, shiftlessness, and lack of foresight become apparent. This view was strengthened by the results of the tests. It would be interesting to discover by continuing the tests whether the white race's superiority over the aboriginal is mainly due to the shorter period of mental development in the latter, force of heredity in the white enabling him better to withstand the physical strain associated with the onset of adolescence, and allowing mental development to proceed equally with that of the body.

By comparing Tables I., III. and IV. it will be seen that relatively speaking, the aboriginal children passed far better than the Reformatory boys and considerably better than the boys of Section B of Table II. By the use of these tests and of similar ones which no doubt will soon be developed, it is hoped that we may reach a stage when we can definitely state by means of an examination that a child is abnormal not only in intelligence but in disposition and that he thus lacks the potentialities of good character forming. If, in addition, psychological tests will enable us even approximately to assess the importance of these deficiencies in their bearing on conduct, then we will have achieved a decided step forward towards the ideal of making training and education ensure the conservation of the child.

Application of Tests to Deaf Children

Interesting results were obtained by the application of the tests to the children at the school for deaf and dumb, Melbourne.

For children above nine years of age the plan adopted was to illustrate the working of the six and seven year tests, the subjects beginning with the test for eight years. It was rarely found necessary to make any further explanations or to give further instructions.

On the whole, the deaf children found the tests somewhat difficult. Temperamental peculiarities were often apparent. In some cases the subjects gave up the task, signing that it was beyond their powers. Very many showed extremely quick perceptions, and worked the tests at a great pace with a resultant tendency to error through impulsiveness. Generally they were very quick to notice their errors. A fairly large proportion were definitely feeble-minded, and a number, judged by ordinary standards, must be considered dull. In every case in which the tests showed the child to be feeble-minded the teacher's judgment concurred. As regards the boys, there was a close agreement between the teachers' estimates of intelligence and the verdict of the tests. That is to say that the tests, considered as intelligence tests, were satisfactory. In the case of the girls, however, the agreement was not so close, many failing to reach the standard that the teachers expected.

This partial failure of the tests may be explained in two ways. The first explanation is that girls on the whole do not test as high as the boys. Five hundred and eight girls (normals) were tested in a recent investigation and their Binet ages compared with their ages per my tests. By the latter, 36 percent tested above their Binet age whilst 43 percent tested below, the remaining 20 percent testing the same. For 492 boys the corresponding figures were: Above the Binet = 44 percent; Below, = 40 percent; Same, = 15 percent.

It will be seen that girls test, on the average, lower than boys. This is probably due to greater impulsiveness and to a lesser development of foresight in girls than in boys.

The second factor influencing results is that the training of the boys at the school tends to accentuate the above-mentioned sex differences. Organized games and sport generally are made a special feature of the boys' training at the school. This is not the case with the girls. Undoubtedly the effect of this training on the boys is to make them more self-reliant, resourceful, and more mentally alert than the girls. It was noticed that the boys who played best, scored, as a rule, highest in the tests.

Notwithstanding the foregoing objections, I am convinced that the tests do enable us to make a useful estimate, within fairly correct limits, of the deaf child's native intelligence. They certainly allow him to display his ability in a much fairer light than any other tests do. Even if the Binet test is adapted to the deaf child's examination, it remains, principally, a language test, a test of comprehension. It must be admitted, however, that the successful application of the Porteus tests depends on the subject's thorough understanding of what is required of him. Once this understanding is gained the application is easy.

The dynamometer records were also taken. The right-hand and left-hand grips were added together and the ranking of the children in the tests and their ranking in individual grip records were correlated by the Spearman Footrule method.

The correlation was:

$$R = 0.65 \text{ (Boys)},$$
$$R = 0.66 \text{ (Girls)}.$$

Considering the dissimilarity existing between motor intelligence tests of a merely physical measurement, such as power of grip, I think this correlation may be considered fairly high. A noticeable feature was the large number of cases in which the grip of one hand approximated the grip of the other.

INDIVIDUAL RECORDS: DEAF AND DUMB CHILDREN

Section A

Case No.	Actual Age	Age per Test	Dynamometer			Case No.	Actual Age	Age per Test	Dynamometer		
			R.	L.	Total				R.	L.	Total
1	$17^{2}/_{12}$	13	47	48	95	19	$9^{1}/_{12}$	9	21	21	42
2	$17^{6}/_{12}$	13	45	48	93	20	$11^{7}/_{12}$	12	20	17	37
3	$17^{7}/_{12}$	12	46	43	89	21	$10^{10}/_{12}$	10	18	18	36
4	$17^{3}/_{12}$	12	43	44	87	22	$9^{8}/_{12}$	10	20	16	36
5	$15^{3}/_{12}$	13	40	37	77	23	$8^{3}/_{12}$	10	17	16	33
6	$16^{10}/_{12}$	13	38	36	74	24	$11^{4}/_{12}$	11	17	16	33
7	17	11	32	42	74	25	$9^{10}/_{12}$	8	16	16	32
8	$14^{8}/_{12}$	11	37	36	73	26	$11^{10}/_{12}$	8	17	15	32
9	$17^{10}/_{12}$	12	33	38	71	27	$10^{8}/_{12}$	11	14	14	28
10	$16^{5}/_{12}$	11	32	37	69	28	$12^{2}/_{12}$	9	14	13	27
11	$15^{6}/_{12}$	$11^{6}/_{12}$	35	33	68	29	$10^{9}/_{12}$	10	13	12	25
12	$16^{10}/_{12}$	$11^{6}/_{12}$	34	31	65	30	$8^{8}/_{12}$	11	13	12	25
13	$15^{5}/_{12}$	13	26	28	54	31	9	9	13	10	23
14	$12^{5}/_{12}$	10	26	24	50	34	$10^{10}/_{12}$	10	10	11	21
15	$16^{1}/_{12}$	13	26	23	49	35	$7^{3}/_{12}$	8	11	10	21
16	$13^{9}/_{12}$	12	23	21	44	36	$6^{11}/_{12}$	7	8	7	15
17	$13^{5}/_{12}$	11	20	23	43	37	$6^{2}/_{12}$	7	7	8	15
18	$12^{9}/_{12}$	12	23	20	43	38	$6^{5}/_{12}$	$6^{8}/_{12}$	5	7	12

Section B

Mentally Deficient

Case No.	Actual Age	Age per Test	Dynamometer			Case No.	Actual Age	Age per Test	Dynamometer		
			R.	L.	Total				R.	L.	Total
1	$16^{2}/_{12}$	10	35	28	63	7	$11^{2}/_{12}$	8	13	13	26
2	$14^{10}/_{12}$	9	31	26	57	8	14	7	16	10	26
3	19	11	23	27	50	9	$9^{5}/_{12}$	6	12	14	26
4	$15^{11}/_{12}$	10	25	24	49	10	$11^{3}/_{12}$	7	11	10	21
5	$14^{4}/_{12}$	8	19	20	39	11	$11^{3}/_{12}$	7	9	9	18
6	$13^{3}/_{12}$	9	19	20	39	12	$8^{8}/_{12}$	4	5	5	10

A close approach to ambidexterity appeared in about sixty percent of cases, a difference of less than four kilos. in strength in each hand appearing in that proportion. In this particular the deaf appear to resemble closely the mentally deficient, a recent investigation by the author amongst feeble-minded revealing a similar condition. It may be said of the latter that they are generally equally weak in each hand, while of the normal deaf it may be said that they are, generally speaking, equally strong in each hand.

In the following summary the normal boys' results have been separated from those of the boys who were considered feeble-minded. In the table showing individual records the figures for the latter are given in Section *B*.

Table V.

Normal Boys

Average age	$12^7/_{12}$ years
Average test passed	$10^7/_{12}$ years
Average deficiency	2 years

Feebleminded Boys

Average age	$13^2/_{12}$ years
Average test passed	8 years
Average deficiency	$5^2/_{12}$ years

Girls

Average age	$11^6/_{12}$ years
Average test passed	$9^5/_{12}$ years
Average deficiency	$2^1/_{12}$ years

Individual Records: Girls

Case No.	Actual Age	Age per Test	Dynamometer			Case No.	Actual Age	Age per Test	Dynamometer		
			R.	L.	Total				R.	L.	Total
1	$14^5/_{12}$	10	34	26	60	15	$12^1/_{12}$	9	16	13	29
2	$13^{10}/_{12}$	11	26	25	51	16	$11^8/_{12}$	10	15	13	28
3	$13^4/_{12}$	12	25	23	48	17	$9^8/_{12}$	10	15	12	27
4	$12^5/_{12}$	10	25	23	48	18	$12^8/_{12}$	8	14	13	27
5	$17^6/_{12}$	12	24	23	47	19	$12^{10}/_{12}$	11	13	12	25
6	$15^{10}/_{12}$	11	25	21	46	20	$8^2/_{12}$	8	13	10	23
7	13	13	22	20	42	21	$8^3/_{12}$	8	10	9	19
8	$13^5/_{12}$	9	21	19	40	22	$6^8/_{12}$	6	10	9	19
9	$13^{11}/_{12}$	11	20	18	38	23	$7^1/_{12}$	6	10	9	19
10	$12^9/_{12}$	12	18	19	37	24	$9^1/_{12}$	7	12	6	18
11	$13^9/_{12}$	11	20	15	35	25	$6^4/_{12}$	6	8	8	16
12	$12^1/_{12}$	10	18	14	32	26	$5^1/_{12}$	5	6	6	12
13	$15^9/_{12}$	9	16	16	32	27	$7^3/_{12}$	$8^6/_{12}$	11	11	22
14	$13^{10}/_{12}$	11	18	13	31						

In each of the above investigations the number of individuals examined is certainly small. The results are reported in order to show the applicability of the tests to abnormal children generally. Results with mentally deficient and with normal children have been elsewhere reported.

THE PSYCHOLOGICAL CONCEPT OF CLEARNESS

BY E. B. TITCHENER

In 1913 C. A. Britz published a thesis for the Zurich doctorate entitled *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit.* Circumstances over which I have no control postponed my first-hand acquaintance with the work to 1916. I have regretted this the more because Britz deals in detail with two psychological systems in which the notion of clearness holds a prominent place, Wundt's and my own. Wundt he chose for obvious reasons; myself, because my 'Standpunkt stellt quasi ein Extrem dar.' Both of us receive a severe mauling: which would be wholesome enough—since the criticism is objective and based upon quotation—if only Britz had followed a sound method. He has not. He struggles with the Wundtian concepts of *clearness* and *degree of consciousness* on the basis of the sixth edition of the 'Physiologische Psychologie' and the seventh of the 'Grundriss'; and it never occurs to him that the key to their understanding is a genetic study of his author. He attacks my concept of sensory clearness on the basis of my 'Text-book' and of Hillebrand's review of my 'Feeling and Attention' in the *Zeitschrift;* he has not referred, incredible as the thing appears, to the *Feeling and Attention* itself.

External circumstances may be in part responsible. The thesis was undertaken at Schumann's suggestion, and the experimental portion was apparently completed under his direction at Frankfurt. The author, however, returned for his doctorate to his old university, and the thesis was accepted by G. F. Lipps of Zurich. It is a fair assumption (is it not?) that Schumann was chiefly interested in the experiments, and that Lipps was generously disposed to a bit of work originated and approved by a psychological colleague elsewhere. In that case the critical chapters, about seventy percent of the whole paper, would have fallen, so to say, between the two professorial chairs. Even so it is astonishing that Britz should not have learned for himself the essentials of scientific method.

I

I do not imagine that Wundt will find time to defend his system against these latest charges, and I do not propose to undertake the business for him. I shall, however, try to set forth, in the light of a genetic study, the use and meaning of the technical terms here in question. Such a study is doubly instructive. It shows our modern psychology in the making; the long series of Wundt's books reflects the recent history of the science. It shows also the manner of Wundt's own progress from logic to psychology, from activity to content. His fundamental ideas have remained, for the most part, unchanged; advance is made, habitually, by modification in detail, by expansion and contraction, by redistribution of topics and change of emphasis. Even when the system suffers a decided innovation (we shall have a case presently), there are always hints of the new departure, if we look closely enough, in the previous work. Here, of course, I have space only to give results. I hope to be able, nevertheless, to clear up the difficulties which Britz and, perhaps, other readers have found in the sixth edition of the 'Physiologische Psychologie.'

We are to ask, accordingly, what Wundt means by consciousness and degrees of consciousness, by clearness, by degrees of apperception, and especially what is the relation between degree of consciousness and degree of clearness. We begin with consciousness.

The keynote of Wundt's psychological treatment of consciousness is the notion of *synthesis*. Consciousness, the condition of all inner experience (1–5)[1] or, more empirically, the total contents of our immediate experience (6), cannot be defined in psychological terms. We must be satisfied to determine its conditions (1), that is, the phenomena that

[1] By these numbers I indicate the editions in which the particular phrase occurs; minor verbal changes must here be disregarded. References are given in later notes.

invariably accompany its manifestation in experience (2–5); more exactly, we must be content to give the conditions under which we observe such phenomena as we attribute to a consciousness (6). There are, now, two psychological processes which are bound up with consciousness and may be regarded as its essential characters. The first of these is the formation of ideas (and real feelings) from sensations (and simple feelings). Our consciousness of ideas consists in the *act of synthesis* whereby sensations are brought into temporal and spatial form (1); in every act of ideation there is effected a connection of elementary sensations (2–4). Ideas and real feelings arise from a psychological synthesis of elements, and this *connection of elements* is therefore one of the two characteristics of consciousness (5, 6). The other is to be found in the processes of reproduction and association of ideas (and feelings). The connection of ideas takes place *in* consciousness (1); it is only by way of reproduction and association that consciousness can become aware of itself as persisting without change through all the change of ideas (1–3); this changing flow of ideas is itself aware of consciousness as a *synthetic activity* connecting present ideas with those that have gone before (1–3). Consciousness is empirically demonstrable only on condition (*unter der Voraussetzung*) of a connection of the ideas (and feelings) which follow one another in time (4, 5); reproduction and association are therefore an universal concomitant of consciousness (6). An orderly connection of ideas (1–4) or, in more general phrase, a connection of immediate experiences (5, 6) is, indeed, the condition under which alone consciousness appears.

If, however, the primary thing about consciousness is synthesis, then we must recognize the possibility of *degrees* or grades of consciousness, since such connections as that of sensations in the temporal or spatial idea may exist at various levels (1). Self-observation reveals these degrees. "Whenever we incorporate an impression but loosely in the context of our ideas, or later remember it but imperfectly by reason of this looseness of connection, we credit ourselves only with a lower degree of consciousness at the time in question" (2–6).

In these instances, capacity for the connection of ideas (or psychical contents) is taken as measure of degree of consciousness (2–5); or, as Wundt puts it in his final phrasing, "the *connection* of psychical contents is a certain measure of degree of consciousness" (6). Every connection of inner states (or psychical elements) manifests some degree of consciousness (2–6).

That is the Wundtian doctrine of consciousness and its degrees, as set forth in the various editions of his great work.[1] The intention of the discussion, from the very first, is psychological; even in 1874 Wundt is combating the logical tendencies which showed their full force in the *Vorlesungen* of 1863.[2] His effort is not immediately successful; but, by degrees, changing here a little and there a little, he moves away from logic toward psychology, away from synthetic activity toward observable connection. There is no reason to think that he was, at every stage, fully aware of the significance of the changes; he probably chose the wording that seemed, at the time, best to express his thought—his original no less than his present thought. In point of fact the corrections of the early text serve in sum to change the whole atmosphere of the discussion.

We find the same sort of progress in the treatment of attention. The discussion of 1874 begins as follows: "In the synthesis of sensations and in the association of ideas consciousness apprehends itself as active. Thus arises that expression of consciousness which we name *attention*. It makes itself known in direct self-observation by the fact that the interconnection of ideas, with which consciousness is correlated, is by no means present to it at all times in the same manner; consciousness is directed upon certain ideas in higher measure than upon others." Later we have: "Beside the coming and going of ideas we perceive within us not infrequently (in varying fashion) and more or less plainly an (inner) activity which we designate *attention*" (2–6).

[1] *Phys. Psych.*, 1874, 707 f., 711 ff., 717; 1880, II., 195 f., 199, 201; 1887, II., 225 f., 229, 231; 1893, II., 255 f., 259, 261; 1903, III., 320 f., 324 f.; 1911, III., 296, f.; 299 f.

[2] See *op. cit.*, 1874, 708 ff.

Degree of apperception is gauged by the subjective activity with which consciousness turns to a particular sensory stimulus (1–5). The simile of *Blickpunkt* and *Blickfeld*, the statement that the *Punkt* is really a small *Feld* of varying extent and that the main field darkens in proportion as the central field brightens, and the distinction of perception and apperception are present in all editions. Passive and active apperception are distinguished in the second, the limen of consciousness and the limen of attention only in the fourth edition. All these things are familiar, and need not be dwelt upon. What now of clearness, which ultimately becomes the sole objective criterion of apperception?

The brightening of the *Blickpunkt* means, as we have seen, that consciousness is directed upon certain ideas in higher measure than upon others (1–4); certain contents become *more conscious* than others (5); we observe in consciousness *different degrees of conscious status*, variously and varyingly distributed over its contents (6). If we consider the apperceived contents themselves, we find the following progression. We begin with a clearness of ideas, dependent partly on the intensity of the ideas and partly on adaptation of attention (1–3). Presently this clearness, dependent now upon the intensity of the sensations composing the ideas and upon adaptation of attention, is paired with distinctness; clearness is predicable of an idea in its own right, distinctness of an idea in its relation to other ideas. Feelings may be distinct, but apparently can not be clear (4). Later still this same clearness attaches to all complex conscious contents: to ideas and feelings as wholes, and also to particular elements within ideas and feelings (5, 6). Clearness, which originally belonged to ideas alone, thus remains to the end a character of complex contents. "Clearness and distinctness are exclusively characters of ideas, and may be transferred to sensations only when these are considered as constituents of ideas" (6).[1]

All this is fairly straightforward, though I must warn the

[1] *Op. cit.*, 1874, 717 f., 720, 722, 725, 729; 1880, II., 205 f., 208, 209, 212; 1887, II., 235 f., 238, 239, 244; 1893, II., 266, 267, 269, 271, 272, 282; 1903, III., 331, 332 f., 336, 337 f., 339, 348, 349; 1911, III., 306, 307 f., 312 f., 314, 322, 323.

reader that I have passed over certain passages which will occupy us later. The pairing of distinctness with clearness offers no difficulty; distinctness is always the subordinate concept, and does not appear in the final summary of the part-processes in an apperception.[1] The irruption of the new theory of feeling, in the fifth edition, does create a difficulty—as I pointed out in *Feeling and Attention*—but it is not one that directly concerns us here.

We are therefore ready to take up, in a preliminary way, the relation of consciousness to attention and of degree of consciousness to degree of clearness. The first of these questions is easily answered. Consciousness, for Wundt, is always wider than attention. In 1874 he wrote: "The theory that consciousness and attention are identical is not tenable." In 1911 he writes: "An impression that has sunk below the limen of apperception does not therewith disappear from consciousness;" and his whole treatment of the two topics, from first to last, implies this distinction.[2] Consciousness is the total contents of our immediate experience; attention is the range of clear experience.

The second question may be answered, to begin with, by the statement that degree of consciousness and degree of clearness have, logically, no connection with each other. Degree of consciousness is degree of organization of conscious contents. Let us imagine (if we can) a consciousness without attention. Such a consciousness would still show degrees of consciousness, because the complex contents and the groups of complex contents which make it up would differ in closeness of connection or organization. There are passages in the first edition which seem to come very near to such an imaginary consciousness;[3] and the recurring phrase "It is always association that puts ideas at the disposal of apperception" at any rate suggests it.[4] In reality, however,

[1] See *op. cit.*, 1911, III., 316.

[2] *Op. cit.*, 1874, 725; 1911, III., 314.

[3] *Op. cit.*, 1874, 795, 835.

[4] *Op. cit.*, 1880, II., 212; 1887, II., 244; 1893, II., 279. I do not find the phrase in the two last editions; and indeed it goes too far. See 1903, III., 524 f.; 1911, III., 498 f.

consciousness comes to us in other guise. It makes its own history from the very outset, and carries that history with it; and the history is constantly interfering, so to say, with its present course.[1] Or, to put the same thing from another point of view, it is organized, at whatever level and in whatever degree, as an attentive consciousness; associations are formed in passive, apperceptive connections in active attention.[2] While, then, degree of consciousness (or of organization) and degree of clearness (or of conscious status) may be distinguished logically, empirically they are bound together in the most complicated fashion. One might suppose, perhaps, that the difference between range of consciousness and range of attention should be directly observable. Wundt does not deny it, though he seems to think otherwise; it is a subsequent apperception that ordinarily makes us aware of the obscure fringe; and he does deny that the simultaneous method is adequate to range of consciousness.[3] One might suppose, again, that degree of consciousness and degree of clearness should run parallel; and in many cases, at many moments of the history of consciousness, they doubtless do; but we must remember that the one tends to be stable and the other is essentially instable. Consider, indeed, any case of active attention. The complex contents that now lie in the obscurity of the *Blickfeld*, and that therefore have no conscious status whatever, were once (probably, many times over) given in passive apperception, in so far as they are organized at all; and if their organization is high, as it may be, they were given in active apperception. The complex contents that occupy the *Blickpunkt* and therefore possess various degrees of conscious status vary in degree of consciousness, from moment to moment, according as apperception is integrative or disruptive and their organization is correspondingly strengthened or weakened. Or consider observation itself. Observation is always apperception; and we cannot become aware of a low degree of consciousness unless

[1] *Op. cit.*, 1893, II., 284.

[2] See W. B. Pillsbury, *Amer. J. of Psychol.*, 1897, 8, 329 ff.

[3] *Op. cit.*, 1911, III., 324, 330; cf. 1903, III., 351 ff., and the stronger statements of 1874, 726; 1880, II., 219; 1887, II., 261; 1893, II., 305.

we give the poorly organized contents a high conscious status.[1] So the empirical relation of the two degrees (and I have, of course, greatly oversimplified it in this brief account) is complex in the extreme. Neither can exist without the other; yet, since they do not run on parallel lines, their separate treatment is a matter of practical convenience, if not of necessity; a full account, historical and descriptive, of any given consciousness implies constant reference to both. If Wundt inclines, even in his latest writing, to make connection the fundamental character of consciousness and to regard attention as an activity within consciousness[2]—when we might expect him to give the two factors equal rank—the reasons are historical, and not least among them is his reaction against unconscious ideas.

I believe that these answers to our two questions are fair, and that they represent the essentials of Wundt's doctrine; I confess that I have rounded off some rather prickly passages.[3] We have now to consider those divergent statements to which I have already referred.

[1] After a good deal of vacillation, Wundt settles down in the sixth edition to the definite terminological distinction of *Bewusstseinsstufe* or *Grad des Bewusstseins* and *Grad der Bewusstheit*: 1911, III., 299, 307. I have made this distinction throughout, and have translated *Bewusstheit* by 'conscious status.'

[2] *Op. cit.*, 1911, III., 301.

[3] Let me give an instance! Wundt teaches that the contents at the *Blickpunkt* (an area, be it remembered) are variously clear, and the contents in the outlying *Blickfeld* obscure. In 1903, III., 353 (1911, III., 326) we are introduced to *Grade der Verdunkelung*, degrees of obscurity. It looks, then, as if the contents below the limen of attention might possess something more than degree of consciousness (which is all that I have allowed them in the text), something that is, after all, very like conscious status. I have, however, pointed out in 'Feeling and Attention' that there is here a confusion of apperception with cognition, of attributive with cognitive clearness, and that a recent worker in Wundt's own laboratory has called attention to it (see 237 ff., 369, and cf. 230 f.). I have already remarked that Wundt's progress is from activity to content: it is not till the fifth edition that 'consciousness' ceases to be 'directed upon ideas,' and that 'contents become more conscious': cf. 1903, III., 333 with 1893, II., 267.

Again, in 1902, I., 323 (1908, I., 382) we are told that change of clearness, as distinguished from change of intensity, alters the relation between contents; clearness thus seems to be confused with distinctness. But the passage in which this statement occurs is not represented in 1893; it harks back to 1887, I., 237, an edition in which the distinction of clearness and distinctness had not yet been drawn. The clearness of 1902 and 1908 (in these particular sentences) is therefore an undifferentiated clearness *and* distinctness.

The discussion of the apperception-center in the fifth edition surprises us by a reference to the clearness of *sensation:* surprises us all the more because the earlier editions spoke in the same context only of the clearness of ideas and impressions; because the same volume teaches that sensations are constituted solely of intensity and quality; and because the same edition, in a later volume, retains the orthodox view that clearness and distinctness are exclusively characters of ideas. This third volume adds, however, that clearness may be predicated of sensations when they are considered as constituents of ideas (the fundamental of a compound tone, the color of a visual form); and so it seemed possible to interpret the sensations which become clear in apperception as sensations-in-ideas.[1] That was a way out of the difficulty; it was not the way the offending passage read. Those who know the 'Physiologische Psychologie' historically know, however, that its exposition is continually changing in detail, and that the details are likely to prove important; I have made the point earlier in this paper. So one hoped for more light in a sixth edition; and the light came with a vengeance! The reference to clearness of sensations in the discussion of the apperception-center is now justified by entirely new matter, which introduces the chapter on Intensity of Sensation. There are (we learn) intensive psychical magnitudes, which accrue only to the simple elements of the mental life, and there are extensive psychical magnitudes, which result from the composition of elements. The three intensive magnitudes are intensity, quality, and—clearness. And these three characters are three coördinate dimensions of the psychical elements; they are, that is to say, *attributes of sensation.* Compound contents show different degrees of clearness in their different parts; degree of clearness is unequivocal

[1] *Op. cit.*, 1902, I., 322 f., 353; 1903, III., 338, 349; cf. 1880, I., 218; 1887, I., 233; 1893, I., 228. These passages are to be sharply distinguished from the casual and physiologically motived reference to the apperception of sensations (correlates of the excitation of a sensory center) which occurs in 1902, I., 324, and which appears in all editions from the second to the sixth.

only in regard to the elements, to simple contents. Wundt's statements are as definite as they could well be.[1]

Here, then, is a new kind of clearness, different from the original clearness of ideas and real feelings, different also from the clearness of the sensations-in-ideas. Wundt has not made a clean sweep of things, however, even in this volume; the old statement, running through all the editions, that sensations are constituted solely of intensity and quality,[2] is allowed to remain. A pure oversight, no doubt! The few references to clearness in the second volume, of 1910, are neutral. But then we come to the third volume, of 1911, and there we are back again in the familiar atmosphere, with the express assurance that clearness and distinctness are exclusively characters of ideas! Could ever anything be more bewildering?

Well! our bewilderment is at any rate less than Britz's. For our genetic study proves that the two clearnesses do not stand on a level. The clearness of the third volume, of 1911, is the traditional clearness of the Wundtian system, deeply rooted in nearly forty years of thought and expression; the clearness of the first volume, of 1908, is a new phenomenon, only casually foreshadowed in the corresponding volume of 1902. Something, it appears, was moving Wundt's ideas, even at the earlier date, towards sensory clearness; and something happened, between the fifth and sixth editions, to precipitate and crystallize his ideas.[3] Thereafter, in the interval between 1908 and 1911, his interests turned away from this something; he had forgotten all about his intensive

[1] *Op. cit.*, III., 1908, I., 539 ff. I myself urged as early as 1898 that clearness should be recognized as a sensory attribute, but printed no extended discussion of the question before this same year, 1908. See *Phil. Rev.*, 8, 461 f.; 'Feeling and Attention,' references to 'Clearness' in index.

[2] Together (for a time) with feeling-tone. See 1874, 273; 1880, I., 272; 1887, I., 290; 1893, I., 282; 1902, I., 353; 1908, I., 412.

[3] I am in this paper expounding Wundt, and neither criticizing his views nor trying at all completely to trace their motivation; the first thing to do with an author (and it is what Britz has failed to do) is to understand him. I think, however, that it is safe to connect Wundt's new paragraphs with the revival of psychophysical interest shown by the works of Müller (1904), Lipps (1903, 1904, 1906), Titchener (1905), Bruns (1906), Keller (1907) and others. In particular, G. F. Lipps was at Leipsic from 1903 on.

magnitudes that accrue only to the elements; and so he contented himself with the customary revision of the former text. Natural enough, after all, in view of Wundt's age and multifarious activities: the wonder is not so much that he should have forgotten as that he should have had, in 1908, the energy and the open-mindedness to attack once again the whole problem of mental measurement, and in doing this to effect a radical change in one of his most elaborate systematic constructions. There is no possibility of reconciliation of the two volumes; Britz's efforts are wasted labor.[1]

It would plainly be useless to reopen our questions of the relation of consciousness to attention, and of degree of consciousness to conscious status, in the light of Wundt's new definitions. The clearness which is an intensive attribute of sensation is at the same time degree of apperception (the objective aspect of degree of attention) or of keenness of apprehension.[2] We may work out, if we will, what this statement logically implies for the treatment of consciousness and attention in the third volume; or we may wait patiently for a seventh edition. At present the questions can be answered intelligibly only if we ignore the intruding passages. They can be answered, that is, only in the preliminary way in which they have been answered above. This conclusion seems to me to be a positive result, which justifies our recourse to the genetic or historical method. It far outweighs, just because it is the result of a sound method, any conclusion reached by Britz.

II

I must now say something in reply to Britz's polemic against my own doctrine of clearness. If only Britz knew accurately what he is talking about! I give a few examples to show that he does not.

(1) Britz finds five principal "criteria" of the sensory attribute. I am said to rely only upon two, inseparability

[1] But what was Klemm about, who read the proofs, that he did not call his chief's attention to the discrepancy? See *op. cit.*, 1908, I., X; 1911, III., V.

[2] *Op. cit.*, 1908, III., 541 ff. The italicized *Apperzeptionsgrade* of p. 541, l. 16 should be *Aufmerksamkeitsgrade*.

and independent variability; and I am further said to rely mainly upon the second. Turn to 'Feeling and Attention'! I there begin by criticizing the 'common definition' of an attribute. From this definition I accept the criterion of *inseparability*. As a mark of inseparability I instance the reduction of the whole sensation to zero when a single attribute reduces to zero. Britz, who raises this special case of inseparability to the rank of a separate criterion, speculates as to what I should make of the argument if I used it! From the same definition I get the criterion of *independent variability*, and point out that in fact "there are bound attributes as well as free," so that "the test of independent variability, useful enough for a preliminary survey, must be applied with caution when we demand accuracy of detail." Yet this is 'Titchener's Hauptmerkmal'! Hillebrand, from whom Britz derives my two criteria, states the case correctly; so that Britz has even misread Hillebrand.[1]

(2) Britz does not hesitate to criticize Bentley's experiment with intensities of sound on the basis of the passing reference in my 'Text-book.' What series were carried out, what intensities of stimulus were employed, what precautions were taken, of course he does not know.[2]

(3) I never use the phrase 'degree of consciousness'; from my point of view it is as nonsensical as 'degree of matter' or 'degree of material existence' would be in physics. Britz disregards my definition of consciousness, and his discussion of the place of *Bewusstseinsgrad* in my system is consequently all in the air.[3]

(4) Britz credits me with 'the assumption that the number of degrees of clearness is not the same for all departments of sense.' What I say is that we have 'to determine, introspectively, how many degrees of clearness can be distinguished [how many just noticeable differences of clearness there are]

[1] Britz, *op. cit.*, 14, 24; 'Feeling and Attention,' 8 ff.; F. Hillebrand, *Z. f. Psych.*, 1910, 58, 141.

[2] *Op. cit.*, 26; 'Text-book,' 1910, 280; 'Feeling and Attention,' 361 ff. I take this opportunity to correct a misprint. In Table II., p. 364, the second rubric under Height of Fall should be 74.4–89.6 cm.

[3] *Op. cit.*, 41 f.

in the various departments of sense'; I assume neither that the number is the same nor that it is different.[1]

(5) I have just said that Britz misreads Hillebrand: here is another case. "Hillebrand has shown in detail," he writes, "to what absurdities we are led by the identification of attention and clearness when we make clearness an attribute of sensation." Hillebrand has shown no such thing. He raises the question "whether every attribute has its own clearness, as an attribute of the second order"; if it has, he says, "that would lead to the absurd consequence that the complete disregard of any one attribute brought with it, to say the least, the disregard of all the others. . . . Here Titchener seems to me to have overlooked obvious difficulties." Perfectly fair criticism! but there is one absurdity hinging on an 'if,' not an *ausführliche Darlegung* of absurdities in general.[2]

It is naturally disappointing, when one is made the part-subject of a doctorate thesis, to find one's views thus caricatured. But enough has been said on that matter. Let us now see if Britz makes any positive contribution to the discussion of sensory clearness.

My thesis is that clearness or vividness (I am not yet sure which is the better term, and there is historical warrant for both) is one of the intensive attributes of sensation. Britz complains that I say very little about its actual nature; and in a sense that is true. You cannot say much about a thing that you regard as ultimate to your science; any attempt at a definition runs over, by force of circumstances, into what Wundt would call a *tautologische Umschreibung*. I have had recourse to a number of these periphrases; but I have tried, above all, to exhibit the thing itself, to state conditions under which it may be experienced and identified in experience. Quality and intensity are here in the same box with clearness. You can exhibit qualities or intensities, as you can exhibit clearnesses; but when you attempt to define them, you find yourself talking round them. If Britz had performed Geiss-

[1] *Op. cit.*, 41, 42; more correctly stated on p. 12; 'Feeling and Attention,' 277 f.; 'Text-book,' 295 f.

[2] *Op. cit.*, 42; Hillebrand, 146 f.

ler's simple experiment with the two metronomes, equated for quality and intensity of sound, he would have discovered at first hand what I mean by sensory or attributive clearness.[1]

My thesis is, secondly, that sensory clearness is the elementary phenomenon in what is ordinarily called attention. Just as sensory extension is the elementary phenomenon in spatial perception, and sensory duration in temporal perception, just so, *mutatis mutandis*, is sensory clearness the unique thing, the psychologically ultimate thing, in attention. Hence I remark in the 'Text-book' that "in the last resort, and in its simplest terms, attention is identical with sensory clearness." Analyze an attentive consciousness, and everything is familiar to you but the one thing, which you finally arrive at—this sensory clearness or vividness; that is new and characteristic.

The importance of such a view for experimental psychology is, I think, plain on the surface; a new road is opened, and a road that by all analogy should take us an appreciable distance to our goal, for an experimental attack upon attention. In 'Feeling and Attention' I speak accordingly of a 'simplified' or 'elementary' psychology of attention; I suggest that we start out, not from the gross facts of the attentive consciousness, but from the 'rise' of the single sensation, the absolute temporal limen, the carrying power of clearness under simple conditions. "How far this elementary psychology of attention could be carried it is, evidently, impossible to predict," though there is no lack of specific problems; in any case, "the results of experiment in these fields must be 'interpreted' by a psychology of attention; the factors that make for clearness must be separated from the other conditions involved, and must if possible be separately estimated or 'weighted.'" That is my view; and I am correspondingly surprised to find Britz, who quotes correctly the sentence from the 'Text-book' given above, asserting in several places that I identify outright clearness with attention. If that were the case my

[1] *Op. cit.*, 40, 41, 44; 'Text-book,' 53, 279; 'Feeling and Attention,' 26, 183 ff.; L. R. Geissler, in *Amer. J. of Psych.*, 1909, 20, 510. Britz devotes a special section to Geissler, as he does also to Wirth and Jaensch; all three will, I expect, find something to say for themselves.

chapter on Attention would hardly have been written as it is. But having made this identification, it is easy for my critic to show that the introductory examples of attention—the shift of interest due to the visit of a friend or to the receipt of a telephone message—involve more than sensory clearness, and that I am therefore faithless to my theory before I have got it formulated.[1]

I hold, thirdly, that clearness is not an attribute of the simple feeling; and as clearness is an intensive attribute, ranging from liminal obscurity to terminal clearness (just as intensity ranges from the very weak to the very strong), this means that feeling is for me neither clear nor obscure, but only qualitative, intensive and durative. The traditional 'obscurity' of feeling rests, I believe, upon the customary mixture of logic and psychology. I realize that the whole psychology of feeling is debatable ground; but, after all, the discussion in 'Feeling and Attention' is seriously written and deserves to be taken seriously. Britz gives a single sentence to the matter. We ought, he says, to enquire carefully whether the clearness of my two examples (the friend's visit and the telephone message) is not applicable to feeling; "nach meinem Dafürhalten kann er [der Begriff] angewendet werden." But *that* clearness is evidently cognitive as well as attributive; the distinction is clearly drawn, again, in 'Feeling and Attention.'[2]

I hold, lastly, that in cross-section the attentive consciousness is arranged, for many and perhaps for most of us, at two main levels, the upper of which certainly, and the lower probably, are 'wrinkled' by minor differences of sensory clearness. "A two-level type," Britz remarks, "seems to me to be altogether beyond the range of psychological proof; it too obviously contradicts all and every experience." Oddly enough, it seems to me to *represent* my experience. Britz may very well belong to the multi-level type, though he does not appear to have gone beyond casual self-observation. He continues: "Within the apperceived

[1] *Op. cit.*, 12, 40 ff.; 'Text-book,' 266 f.; 'Feeling and Attention,' 209 f., 251, 372.

[2] *Op. cit.*, 42; 'Feeling and Attention,' 237 ff.

(*beachteten*) complex there are degrees of clearness which may be lower than the highest upon the low level; therefore it is not permissible to speak of a 'niveau,' 'level,' plane or surface." I do not know how he gets his data; but it is surely plain that, if there are *anywhere* in a given consciousness processes less clear than the clearest of the lower level, these processes must be for me *at* the lower level. To say that they are 'within the apperceived complex' means, if it means anything, that my critic is thinking of the unitary object of attention; and that means that he has fallen into a form of the stimulus-error.

Hillebrand's comment here is more to the point. He asks why, if clearness is an intensive attribute of sensation, there should be only two main levels of clearness at any moment instead of an unbroken section of the attributive continuum. I do not know, though I might if I knew more physiology. I do not know either why the constant of attention is 6; our theories of attention are still nothing better than more or less plausible hypotheses. I am trying only to ascertain the psychological facts.[1]

We are not reaping an abundant harvest. Nor shall we fare much better if we ask, as I now proceed to do, why Britz objects to clearness as an attribute of sensation. He first examines the criterion of inseparability, and finds that the attribute of sensation is not separable by sensory attention but is, of course, separable by abstraction. Then 'as regards clearness' he adds: "not every phenomenon that is inseparable by sensory attention is thereby given immediate status as attribute of sensation." This statement, in the absence of examples, is a little cryptic; Britz may be thinking of some form of 'inseparable association.' We need not guess, however, since the conclusion is simply a *non liquet*. He asks, secondly, whether the sensation disappears as a whole when clearness becomes zero, and replies that, for Wundt, it does not; Wundt has two limens, the one of consciousness and the other of attention. How this reply bears upon my position I do not understand; nor, by his own admission, does

[1] *Op. cit.*, 53; Hillebrand, 148.

Britz. He examines, thirdly, the criterion of independent variability, and finds that clearness is not an independent variable. He forgets (though Hillebrand had told him) that I recognize bound attributes as well as free. He asks, fourthly, whether a reference to clearness is necessary to the complete description of a sensation, and decides that it is not. "Can we characterize a sensation completely without recourse to the notion of clearness? I must answer this question in the affirmative." Yes, but he does not show us in a concrete case how the thing may be done. Further: a supposed attribute may prove to be analyzable into a number of really primary attributes. Yes, and I have been on my guard; witness my treatment of *Aufdringlichkeit* and of tonal quality. Britz quotes no cases. Further: very few psychologists have regarded clearness as an attribute of sensation. Yes, again: and how has attention fared in the history of psychology? Listen to Ebbinghaus: "Attention is a real perplexity in psychology. Both in the general run of English associationism and in certain comprehensive works down to the present day it is altogether ignored. In other books it bears the strangest relation to the systematic presentation of the whole subject, and sometimes an author seems to be entirely helpless." And when psychology came to deal with sensations? I quote Ebbinghaus once more. "All statements of any exactness regarding sensations, their attributes, their liminal values, etc., imply from first to last—as everybody always understands without being specially told—that a high degree of attention was given to them." In other words: so long as psychology dealt with the full attentive consciousness, we made no solid progress; and when psychology acquired methods of precision, attention was taken for granted. Now that a suggestion for the beginnings of an exact psychology of attention are forthcoming, one would think they were worth a trial. Very few psychologists have agreed, as a matter of fact, upon *any* general view of attention.[1]

There remains, fifthly, the empirical side of an issue al-

[1] *Op. cit.*, 23, 24, 30, 31; 'Text-book,' 54 f., 95; 'Feeling and Attention,' 26 f., 326 f.; H. Ebbinghaus, 'Grundzüge der Psychologie,' 1902, I., 585 f., 588.

ready raised theoretically. *Is* clearness analyzable and not simple, derivative and not primary? Britz replies, on the ground of critical discussion and of experimental work, with an emphatic Yes. Clearness (both Wundt's and mine, apparently) is a very mixed concept, deriving partly from the metaphysical philosophy of Leibniz, partly from popular psychology (we talk of 'clear' colors as we talk of 'pure' tones), and partly from the properly psychological distinction of degrees of consciousness; it may thus be very variously employed, under various empirical conditions, and its employment always implies a process which is of the nature of judgment.

I have, now, said something of the value of Britz's critical discussion, and I could say a good deal of the value of his experiments. He worked with the tachistoscope (not knowing, of course, what I had said of that in 'Feeling and Attention,' though he had read Mittenzwey); he required his observers to cognize and name (*erkennen und benennen*) the colors exposed; and he employed a *wissentliches Verfahren* to the extent, at any rate, that they knew the nature of his problem. What the tachistoscopic analysis of the *Erkennungsvorgang* has to do, in any direct way, with the study of attributive clearness, it is difficult to see.[1] I shall not, however, enter into detailed criticism; experiment is best met by experiment; and while a repetition of Britz's work will hardly help us to a psychology of clearness, it may throw light upon the psychology of *Eindringlichkeit* or insistence.

No higher honor can be paid a scientific theory than critical discussion based upon experiments which are conceived and carried out expressly to test its validity. Here, however, is a discussion that leaves out of account the original statement of the theory, and relies wholly upon secondary sources; and here are experiments that fall into line with the work of Schumann and his school, but by the same token are directed upon a complex process of assimilation. It is a great disappointment.

[1] *Op. cit.*, 40, 42 f., 54 ff., 67 ff., 75; K. Mittenzwey, *Psych. Stud.*, II., 1907, 386 ff. ("Im Begriff der Assimilation findet sich von einem Merkmal der Klarheit zunächst gar nichts.")

Postscript.—Since writing this paper I have learned that Dr. Britz is numbered among the victims of the war. It goes against the grain to criticise thus sharply an author who can no longer reply. Yet I am sure that Britz would have wished his work to be seriously considered; and since the points really at issue are not personal, but scientific, it is perhaps not too much to hope that some other pupil of Schumann or Lipps may carry further the study of clearness which Britz began.

COMPOUND SUBSTITUTION IN BEHAVIOR

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The average man does not seem to care much how his brain works. There are many inquiring minds, however, that are striving to add to the sum of human knowledge of the brain and its operations which serve to adjust the man's inner relations to his outer relations. The search for facts in this field is certain to be guided more or less by theories that are based on the results of preceding investigations.

These theories, of course, must be remodeled from time to time. At the present day we find that there are certain theories of nervous mechanisms that are given some acceptance in scientific circles. In brief outline these theories show that each nervous impulse enters the nervous system at a receptor that is either external or internal. It then passes from neurone (nerve fiber) to neurone across the connecting synapses or junctions. Following the course that is most open to it, it tends to arrive at one or more muscles or other effectors. The theories give a clear account of reflex action. They also account in some degree for associative memory in its elementary forms. They show for example an explanation of substitution of one stimulus for another and an explanation of memorizing a series of words.[1]

In this article it is proposed to extend these theories and seek to explain more complex responses or rather responses to more complex stimuli such as appear in first lessons in mental arithmetic, for example. We will begin by noting some effects of greater numbers of neurones. We will then discuss briefly the formation of simple associations and the substitution of one stimulus for another through association and also the linking of movements into a definite order or series. The relation of such a movement series to what is known as

[1] Watson, John B., 'Behavior,' New York, Holt, 1914, pp. 272, 274.

delayed reaction will then be taken up. With this preparation we will attack the main problem before us; *i. e.*, responses to complex stimuli.

To make a proper beginning we will start with a brief definition of the more important terms we shall use.

Selective reaction is a form of simple association. It is the type of behavior shown when a child points to an object upon hearing its name pronounced. It is always the result of training or experience.

Substitution will be used to express simple substitution and is the type of behavior where one stimulus takes the place of another in provoking a given movement. It is the result of training as in the case of young chickens running up at the sound, 'Here chick, chick,' without waiting to see the food.

Delayed reaction is the term used for a case of selective reaction or substitution when the response does not come immediately after the determining or substitute stimulus. Several seconds or several minutes may elapse between the stimulus that determines the movement and the corresponding response.

A memorized series is a series of movements made in a definite order from previous training. When a dog is made to 'fetch' he performs a memorized series that he has been taught.

Compound substitution is the same in apparent form as simple substitution but requires the coöperation of several stimuli to provoke the response. Herbert Spencer in explaining instinct[1] used the term compound reflex action to describe automatic behavior in which complex stimuli produce complex movements. In compound substitution we have a similar term. When you teach a boy to give a correct answer to the question "Two and one make how many?" you are developing a mechanism for compound substitution. If we attempt to analyze this case after the habit is fixed, we see that the sound of the word two cannot have a great tendency to provoke the utterance of the word three. We see

[1] 'Principles of Psychology,' New York, Appleton, 1894, Vol. 1, p. 432.

that the sound of the word one cannot have a great tendency to provoke the word three. The same is true for the other words of the question asked, when taken separately and yet taken together, the words do provoke the response 'Three.' How may we account for this?

We see at once that it is much harder to account for compound substitution than it is for simple substitution. It is not so very difficult to think of connecting nervous pathways that would allow one stimulus to excite a movement which before training required a different stimulus. Neither is it very difficult to think of connecting paths that would allow a stimulus from one receptor to open up a passage for a stimulus from another receptor, as in a case of selective reaction. When we come to compound substitution, however, we find a different matter. We must now provide for coöperation of stimuli, or, if you prefer it, for the resolution of nervous impulses. It would seem that a new principle is required. This is the problem which we are to discuss. If the theories we now have covering association and substitution are worth demonstration, the extension of these theories to cover compound substitution must be also worth while.

Having this much knowledge of what we are seeking and why, let us proceed with the search. Having noted what a great gap there is between simple substitution and compound substitution, let us now hunt for the missing links.

In the course of this discussion it will be shown that the key to the mechanisms for a memorized series will prove to be a missing link as it were.

Let us here briefly consider the effect of numbers of nerve fibers in the system. The greater the number of nerve fibers that are simultaneously aroused and tributary to a given muscle the stronger will be the contraction if any occurs and moreover the greater will be the tendency to contract actually. The superiority of the human mind over the brute mind must be largely due to the greatly superior number of the neurones that control movement. A dog can be trained to respond to a simple command but not to a long speech. A child can be trained to respond to a simple command and by further training, he comes to respond to very lengthy instructions.

The act of a thoughtful man is due to the joint excitation of many neurones. To educate a child, we first train him to respond to simple commands from the effect of association. For each act thus learned, one or more association nerve fibers must be developed. We may say that each step of learning is the development of a particular association fiber. Step by step, the child learns to talk. Step by step, he learns to write, to read, and to count.

By way of further preparing a foundation for our discussion, let us agree that selective reaction and substitution can be explained by the dual common path theory, which is that for every unit association of which the nerve system is capable, there is a common nerve path which is, we will say, an association fiber. Moreover each association fiber has two tributary fibers or private nerve paths coming from two different receptors or sensory terminals.

Each impulse that follows the common path leaves a resistance temporarily lessened for a subsequent impulse. Each impulse from a private path goes by way of the association fiber to a certain muscle or other effector. If the resistance is low enough, a feeble impulse will reach the effector. Let us call such an impulse a scout impulse when it only serves to lower the resistance for the next impulse. Let us also, on the other hand, call an impulse that follows a scout impulse and is strong enough to aid in causing movement, a worker impulse for convenience in discussion. When a volley of worker impulses excite one of a pair of antagonistic muscles, movement will take place unless there be equal excitation of the opposed muscle.

By way of illustration, let us think of a child writing under the direction of his teacher. A certain movement may be prompted by a group of worker impulses from his eyes which are guiding his hand together with a similar group from his ear due to the teacher's words and another group of tactile impulses from the feel of the penholder, etc., and still another group from his hand and arm; *i. e.*, kinæsthetic impulses from the muscles and joints due to movements just made. All these worker impulses reinforce each other and compel the contraction of a certain muscle. This illustration shows how some of the so-called voluntary movements are caused. Such movements are determined by previous training as each worker impulse comes by way of an association fiber which has been developed by previous impulses. It is plain that the formation of a single letter is due to a large number of association nerve fibers working together like the instruments of a great orchestra.

To apply the theory of the dual common path to a typical case of selective reaction, we may use the following illustration. The sound of the word 'ball' spoken by the nurse excites a short series of scout impulses in the boy's brain, each opening up some common path. The sight of the ball produces worker impulses that follow along the common paths opened by the scout impulses. Of course these paths were developed by the repetition of impulses in some previous experience. The worker impulses now provoke the movement of pointing at the ball. The result is due to recency and frequency of stimuli in altering resistance to conduction.

In the case of substitution, the application is similar. Let us take this illustration: Show a horse an ear of corn and then call him, thus giving him a lesson. After several lessons, he will come at your call without any corn. The explanation is that the lessons opened up the dual common paths so that the call after training produces a sufficient volley of worker impulses to provoke a forward movement. To express it in a different way the effect of the lessons is to set up a 'conditioned reflex' as it is called.

In his presidential address given in the March, 1916, number of this journal Professor John B. Watson showed how the conditioned reflex has been used in behavior experiments and suggested its use for investigations of association reaction, etc. Let us here note that in the cases he described the conditioned reflex is the result of simple substitution and appears when the substitute stimulus provokes a sufficient volley of worker impulses to produce reaction. In the case of compound substitution, however, the time element is a factor. Hence, although we find a resemblance in the operations, the term reflex may be thought inappropriate.

In order to help the reader keep in mind the way impulses tend to follow recent impulses from other receptors, the following illustration is offered: Suppose that Mr. Brown leaves his home in the suburbs in the morning after a snowstorm, on the way to the railway station. He breaks a path through the snow. A little later his neighbor Mr. Jones leaves his home and soon strikes Brown's trail. His easiest course is to follow Brown's track to the station. We may say that Brown represents a scout impulse and Jones is like a worker impulse that coming from a different source soon after the scout impulse, tends to follow the same nerve path because it offers less resistance.

Having now a conception of how elementary nerve mechanisms are constituted, let us think how they may be coördinated so as to account for a memorized series in which the movements follow the same order in which they were made before, in a similar situation. Each movement causes afferent or kinæsthetic impulses, some of which may be worker impulses that excite the next movement while others are scout impulses that open up the paths for other movements to follow. The kinæsthetic impulses have become substituted for other stimuli that were received in early training before the habit was fixed. In this way the movements become linked together so that it is only necessary to provoke the first movement of the series and if the external conditions are right, the other movements will follow automatically in their proper order.

On consideration we see that the important factors for a memorized series are the kinæsthetic impulses and the association nerve fibers leading to the muscles that must act. An important thing for us to remember in this connection is that the scout impulses sent through by one movement of the series will facilitate the worker impulses for a movement that follows after intervening movements and a considerable interval of time.

Let us now pass on to the behavior known as delayed reaction.

Let us suppose a small dog placed in a box so constructed that he can see two openings in front of him, one to the right and one to the left but cannot reach either until the bar in front of him is removed. Over each opening is a green light that can be switched on or off by the operator. One opening always leads to food, the other does not. Sometimes one opening and sometimes the other leads to food. To train the dog the operator turns on the light over the opening with food. After giving the dog time to notice the light he turns it off and half a minute later releases him. After a number of trials the dog becomes trained so that when released he goes at once to the opening where the light was seen. We may explain the behavior in this way: The sight of a light over the right-hand opening excites certain nerve paths. That is, it causes impulses that follow certain nerve paths. The performance of going to the right-hand opening is a series of movements due to impulses following certain nerve paths. Let us suppose that there are particular association nerve paths that are common to each group. That is to say, there are particular association nerve fibers that are aroused in executing the movement series that are also aroused by the sight of the right-hand light.

The common paths will get two excitations whenever the dog makes the proper turn at the proper time. The double excitation may be assumed to have the useful effect of opening these common paths; *i. e.*, of lowering their resistance. Each succeeding lesson will further facilitate the appropriate behavior. When the common paths are sufficiently developed we may explain the dog's actions by saying that the right-hand light produces scout impulses that temporarily open up further the common paths and the operation of releasing the animal produces worker impulses that follow these common paths and provoke the movement of turning to the right. It appears then that delayed reactions are explained by the dual common path theory.

We may say that delayed reaction is by nature only a special case of selective reaction. As a further aid to understanding the matter, we may note that a case of delayed reaction is like a memorized series where some of the movements are omitted so as to leave a gap in it. For in every movement series that is habitually made, the movements in the first part of the series are associated with each movement in the last part of the series. In other words each movement causes impulses that facilitate or insure any movement that has a place further along in the series as we have already observed.

We may say then that delayed reactions are special cases of memorized series behavior. Or we may say with equal propriety that a memorized series is a compound of delayed reactions, meaning by this reactions that are more or less delayed. For each movement of such a series, to some extent, is determined by impulses that preceded it by a considerable interval of time.

Behavior students have measured the comparative intelligence of animals by the delayed reaction and by the memorized series as shown in escaping from a maze or opening a puzzle box. The more intelligent an animal is the quicker it will master a given maze or puzzle box and the longer it will remember what it has learned.

The principle of learning is no doubt the same with the animal that learns in a few lessons as with the animal that requires many. The difference must be in the nerve structure with which he begins the task. The animal of high intelligence must have more association neurones to take part in the proceedings.

From this brief study it would appear that the delayed reaction can be explained in some measure by the same mechanisms that are used for simpler forms of behavior.

We have considered four types of behavior, selective reaction, substitution, the memorized series, and delayed reaction and we find in all of them it is a matter of association nerve fibers that register the frequency and recency of impulses traversing them in the range of experience and then in turn regulate the passage of impulses that provoke movement.

To put it another way association is the characteristic feature of the four types of behavior.

This brings us to the main question, the explanation of compound substitution.

Let us consider the case of a boy learning mental arithmetic. In the beginning he is taught simple problems. He is taught to make a certain response to a certain series of ear stimulations. For example when he hears the question: "Two and one make how many?" He must respond, "Three." The reader will see that we have here a case of compound substitution. As the boy advances in the study, the series of excitations is increased in length so that there are more factors to be kept in his mind as it were. In mastering each series he really develops certain association fibers; *i. e.*, his experience opens up certain nerve paths. The associations formed in the earlier lessons take part in working out the later problems.

But let us look into the matter more closely. Suppose that the boy learns to add by counting on his fingers. At the sound of "two" he holds up two fingers. At the sound "one" he holds up one finger of the other hand. In response to "How many?" he puts his hands together and counts "one, two, three" and answers "Three." Here we have a memorized series in which eye movements, finger movements and vocal movements are linked together by association. Let us note that quite a large number of association neurones are stimulated in the course of the performance. After the boy has mastered this series, he can be trained to modify it by counting in a whisper and uttering the final word three aloud. Then he can be taught to suppress the finger movements so that they are incipient only. The eye movements also become incipient. It is fair to suppose that the number of neurones stimulated though lessened, is now nearly as great as when all the movements were actually made, so we still have a large number of neurones taking part in the response.

On consideration we see that a certain series of auditory excitations provokes a memorized series composed of movements or incipient movements that lead up to the desired vocal response. In the case where the intermediate movements are only incipient, we may say that the series of excitations given by the question heard, provokes a series of scout impulses and the effect upon the *nerve muscle system* produces a group of worker impulses that excite the series of actual movements included in the proper response. These nervous operations by long practice become greatly abbreviated, but we may believe that the successive character of the operations still remains.

This shows that a case of compound substitution may be evolved from a memorized series, so we see how an elementary case of compound substitution may be explained as a modified memorized series so to speak. The same reasoning may be followed to build up cases of more and more complexity. The behavior of a skillful lawyer engaged as referee in a legal contest is largely determined by compound substitutions which are due to his special training. His ability to keep in mind an astonishing array of pros and cons has been built up step by step. Each step in the case means more movements linked in habit series and a greater number of association neurones stimulated. When the great lawyer finally gives his decision for the plaintiff or for the defendant, the deciding movement is prompted by the discharge of a large number of association neurones which have been duly prepared in the course of the proceedings.

A performing elephant will respond to his trainer's signal by one trick or another depending upon the stage setting at the time. In the same way, the great lawyer responds to his stage setting; *i. e.*, the pros and cons of the legal problem. The lawyer's response, however, is due to a far greater number of association neurones and to a far greater number of steps in the ladder of learning.

It may be noted that the above given explanation of compound substitution rests to some extent upon an assumption that all incipient movements can in some way cause afferent impulses similar to the kinæsthetic impulses from actual movements.[1]

If our view is the correct one, a case of compound substitution may be regarded as a memorized series that has become modified by suppressing the movements so that of the different stimuli concerned, each takes part in determining the resultant response and thus the stimuli do coöperate. It is evident too that in the case of compound substitution of a long series of stimuli, delayed reaction plays an important part for the reason that it takes part in all memorized series as we have seen.

It is hardly necessary to point out that language habits are the basis of most compound substitutions in human behavior. Writing movements also are an important aid in forming the required associations.

From this demonstration we may conclude that a rational theory can be formulated for nervous mechanisms for compound substitutions in behavior if we may start with a very large number of association nerve fibers to many of which there are tributary afferent fibers coming from the muscles. These fibers are developed by practice through the influence of frequency and recency. As brooks come together to form a river so do the worker impulses meet and provoke movement. These nerve mechanisms are of great service in adapting behavior to environments containing changeable features. In the instinctive behavior of all highly organized animals they have an important part. They are especially important in child education. In fact we might almost say that in any animal that has a cerebral cortex, behavior is mainly determined by such mechanisms or at least modified by them.

If our theory is the true one, we shall find that to have a greater intelligence, the number of association fibers must be increased in much greater ratio.

If we take a broad view of the subject, compound substi-

[1] A discussion by the writer of 'The Function of Incipient Motor Processes' will be found in the PSYCHOL. REV., 1915, 22, 163–166. The other main assumption made in this article and termed the dual common path theory is discussed by the writer in the PSYCHOL. REV., 1916, 23, 235.

tution in behavior may be defined as an increase of the correspondence between the organized individual and its environment in speciality and in complexity. Furthermore it is the coördination and integration of correspondences, so to speak. The subject is such an extensive one that the demonstration given herein, to some readers, may appear quite inadequate. On the other hand, to some readers, it will seem a lengthy exposition of a simple proposition. It is not practical in a short article to do more than offer a contribution to the general study of this extensive subject with sufficient elaboration to make the main idea plain to those who are searching for mechanisms to explain mental growth.

THE DELAYED REACTION IN A CHILD

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Introduction

The present paper derives its chief significance from the fact that the child tested did not possess vocal language and probably not gesture language either.

In the work with the delayed reaction on children previously published,[1] five subjects were used: M., 8 years, Hd., L., and H., each 6 years, and F., 2½ years. All of these children possessed vocal language. M. received 38 trials on delays; Hd., 46 trials; H., 15 trials; L., 41 trials. All of them succeeded with delays as great as 25 minutes. There was some indication that the children first encountered particular difficulty in the intervals from 4–6 secs. The child F. received 507 trials on delays. Her maximal delay was 50 secs. The periods of greatest difficulty were at 5 secs., 7 secs., 10 secs., 15 secs., 30 secs., and 40 secs. Although F. was given 45 trials on the one-minute delay and failed, it is possible that more prolonged training would have enabled her to master the interval.

When the present tests were begun, the writer hoped to secure subjects who would fill in the great gap between the child F. and the older children. Success has not attended these efforts mainly because of a lack of convenient material. One three-year-old child was tested, but the data are of so little value that they will not be presented. The second and fundamental purpose of this study was, however, to study the delayed reaction in a child too young to possess vocal language. This is of particular importance because of Watson's insistence that the delayed reaction, if solved by internal factors other than orientation, must be solved by vocal language.

[1] Hunter, Walter S., 'The Delayed Reaction in Animals and Children,' *Behav. Monog.*, 1913, 2, No. 1, pp. 52–62.

The Subject Tested.—The subject of these tests was the writer's daughter, Thayer, thirteen to sixteen months of age from first to last of the experimentation. She was a normal, healthy child physically—a little slow perhaps on the behavior side, due to the lack of the constant attention that many children receive. She learned to walk alone rather suddenly at about 15 months; and by 16 months, she could indicate with a little certainty her eyes, ears, nose and mouth. When 13 months old, she could 'throw a kiss' and wave 'bye-bye.' Even before this, as early as the tenth month, she swayed and waved her arms to music. These observations are presented as a sample of her best accomplishments. She had no vocal language. She made many sounds, some of which were in response to definite stimuli; but in no case did she use the sounds spontaneously and in no case did she use them as symbols. Her equipment was not large in the first place, and what there was was purely of a stimulus-response nature. During the period covered by the experimentation, the following vocal behavior was present: 'Dăddy' she said whenever a distant door was heard and I was away, or when she heard me coming up the steps. 'Whĭtte' came in response to the striking of the clock or the ringing of the door bell. She could say 'bōōb-bōōb' for the dog; 'dāy-dāy' for the duck; 'm-m-m' for the cow; a funny noise for the donkey; and 'y-gŏb, y-gŏb' for the turkey. These were in response to the specific questions, "What does the dog say?" etc. Some of these she got mixed up and later forgot all but the duck, the turkey, and the cow. In addition to these vocal responses there occurred only the more conventional baby noises.

One gesture might possibly be termed language, viz., raising her arms to be taken up. It is impossible to say, however, that this was not in response to present stimuli. It is also impossible to say with any certainty whether or not the child used a vocal cry specifically "to attract attention" to her needs. I have no evidence to indicate that such was the case. Experimentation was purposely stopped before the first signs of language (in the conventional sense) appeared.

Apparatus and Method.—The apparatus used is shown in Fig. 1. It consists of three boxes placed upon a stand whose top is 6 inches above the floor. Each box is approximately 3 inches deep, 4 inches wide, and 5 inches long. Each is covered by a hinged top. The apparatus was wired for electric lights, but these were never used. Another type of apparatus more nearly approximating that used with children in the previous work was constructed. It, however, proved unsuited to Thayer and was used only with the three-year-old child.

The only features necessary to secure in devising an apparatus for the delayed reaction are these: (1) It must be adapted to the size of the subject and to its mode of response—walking, reaching, swimming or flying. (2) It must provide a means for presenting a stimulus in one of several places. (3) These stimulus positions must be equally accessible to the response. And (4), the stimulus and the method employed should be such as to present no differential cues to the subject during the intervals of delay. These requirements, although rigid, are simple and can be met for practically all organisms. I am therefore unable to agree with Professor Yerkes when he says[1] that the multiple choice method of studying ideational behavior is superior to all others in: (1) applicability to a wide range of conditions; (2) susceptibility to standardization; (3) quantitative nature of results; and (4) intelligibility of data acquired. Nor can I acquiesce in the claim that "It is already obvious that the method enables us to compare, as has never before been possible, the responses to certain standard situations, of human and infra-human, normal and abnormal, mature and immature subjects." Both the multiple choice and the delayed reaction methods are valuable for the study of human and animal behavior; but they are applicable, I think, to very different problems.

In the experiments reported in this paper, the following method was used. Thayer sat in front of the apparatus, as shown in Fig. 1, and the stimulus object was placed in her

[1] Yerkes, Robt. M., 'Methods of Studying Ideational Behavior in Man and Other Animals,' Psychol. Bull., 1915, 12, 330–331.

hand. A great variety of things were used for stimuli: dolls, keys, rattles, shoe-buttoners, small books, etc. Every effort was made to keep up the child's interest. Occasionally two different stimulus objects were used in the course of the day's work. Such methods are necessary if a child of this age is even to approximate to the vigor with which a hungry

FIG. 1. Thayer opening box *c*.

animal attacks its problem. The stimulus object was taken away from the child almost as soon as she received it and was placed in one of the boxes. The lid of the box was left open and Thayer was pushed over (or permitted to lean over) and made to look into the box. Often she tried to reach in and get the stimulus; but in every case, her hand was withdrawn and she was raised back to an upright position. The lid of the box was now closed. Save for a few instances to be mentioned in due time, the subject was distracted during the interval of delay. Distraction took either one of several forms: (1) I might place my hands over her eyes and rock her body back and forth from right to left. (2) She might be stood up, turned around with her back to the apparatus, kept there awhile and then put down. (3) I might cause her to turn her head by speaking to her. She would remain in this position and imitate animals for me (as described above under vocal habits) as long as any delays here used required. About 2 secs. before the end of the delay period, her body was straightened around and she sat facing the middle box, *b*, entirely free from contact with me. (She almost never looked back at me; and when she did, she paid no further attention to the problem. I never spoke to her during the delay. These facts together with the child's inability to reach delays of a minute or more, indicate that she was not deriving cues from the experimenter.) Thayer was now left to her own devices until she opened the box containing the stimulus object. In all but a few cases, she began to hunt for the stimulus as soon as she was straightened around. Time was taken with an ordinary watch and was counted from the moment the box lid was closed until the subject made some movement toward one of the boxes. In the records particular attention was given to the orientation at the time of response and to the behavior during the delay. A reaction was counted wrong if the child opened any box save that containing the stimulus.

Experimental Results

In this experiment there was no period of learning the association between the stimulus object and the three boxes. Seeking for objects that had disappeared was already a part of the subject's behavior equipment. I noticed as early as her eleventh month that if I showed her a toy and then hid it behind something, she would immediately reach or creep toward the spot. I have no doubt, however, that this type of reaction occurred earlier. The present test was more complex than this in that the toy might be in either one of *three* different places.

I quote the following from my diary records indicating the results at the very beginning of the work. "Date, 10–23–15. Trial 1. Toys put in middle box and door shut. (All this done by Thayer on her own initiative.) I now put my hands over her eyes and shook her head and whole body playfully but thoroughly. No orientation of upper part of body retained. 13 secs. delay from time she straightened up after closing box until she reached toward *b*. She reacted correctly, straight to middle box."

"Trial 2. Toys put in right box, *a*. 12 secs. delay with distraction as above. Reacted correctly."

"Trial 3. Same as No. 2, 13 secs. delay. O. K."

"Trial 4. Thayer preferred right or middle box. I had her put toys in left one, *c*. Distraction by standing her up and turning her to me. 17 secs. O. K."

"Trial 5. Middle box used. Baby tired of test. 14 secs. Distracted as in first trial. Reaction wrong."

In these tests made on the first day, Thayer missed one of five or 20 percent. The delays ranged from 12–17 secs. Distraction was always used. The same orientation at the moment of release was held for all, viz., orientation to *b*. These long delays were very startling and held out a promise of very rapid development through training. This was the last of October. A longer stage of delay was not successfully reached and consistently maintained until after Christmas. This fact is shown in Table I.

TABLE I

Delay in Secs.	Correct Trs.	Wrong Trs.	Percent. Correct
3	3	0	100
4	1	0	100
5	6	0	100
6	3	0	100
7	2	2	50
8	4	2	66
9	4	1	80
10	11	4	73
11	6	2	75
12	6	3	66
13	5	0	100
14	6	8	42
15	13	11	54
16	5	5	50
17	4	3	57
18	2	1	66
19	1	1	50
20	9	15	37
21	1	5	16
22	2	3	40
23	1	0	100
24	2	3	40
25	0	3	0
26	1	1	50
30	3	5	37
35	2	0	100

This table shows the size of the delays and the number of correct and incorrect reactions made. All trials given the subject through December 2 are included. The statement is not chronological. The reason the intervals of delay increase so gradually is that within a few seconds variation, Thayer herself determined when she would begin the reaction. All I could do was to place her facing the apparatus and await results. Table II. groups the delays into five

TABLE II

Delays	Percent Correct
3–7 secs.	88
8–12 "	72
13–17 "	55
18–22 "	37
23–35 "	44

classes which may be called the 5 sec., 10 sec., 15 sec., 20 sec., and 25 sec. intervals. The 10 sec. interval may be regarded as mastered, but no higher interval. (This is understated, as will be indicated below.) Table III. gives data gathered

TABLE III

Delay in Secs.	Right	Wrong	Percent Right
5	4	0	
6	0	1	77
7	3	1	
8	2	0	
9	1	0	
10	10	3	82
12	2	0	
15	25	8	75
20	24	10	70
25	2	2	50

from January 2 through January 10. An interval of one month had elapsed during which no tests were made. A comparison of Tables II. and III. indicates a marked gain in ability to deal with the 15 sec. and 20 sec. intervals. In Table III. the 15 sec. interval can be regarded as mastered and the 20 sec. interval as practically perfected.

The tables just given are valuable in showing just what the child actually did in the work *as a whole*. Her achievements are much obscured, however, by such a presentation, inasmuch as poor incentives and position factors frequently dragged her total percentages down. Here, *e. g.*, is the diary record for the ten tests made November 12. (Data included in Table I.) The delays were all above 20 secs., and 7 trials of 10 were successful. Where the word 'distracted' is used,

TABLE IV

Box with Toy	Delay		Behavior
a	20 secs.	distracted	*a*
a	22 "	"	*bca*
c	21 "	"	*abc*
c	20 "	"	*c*
b	35 "	"	*b* very slow and 'careful.'
a	22 "	stood her up	*a*
c	26 "	distracted	*c*
b	20 "	"	*b*
c	22 "	"	*c*
c	24 "	stood her up	*ac*

Thayer's eyes were covered and her body was shaken back and forth; or she was induced to look up at the ceiling and listen to me count while I waved her arms about. The letters in the last column indicate to which box the reactions were made. In every case the subject was oriented, body and face, to *b* at the moment of reaction. This day's record is better than any that preceded it. Two weeks previously, she had succeeded with intervals between 11 and 19 secs. But during those two weeks that followed, she was largely the victim of position habits. The day following the above diary record, she again fell back into position habits. I kept holding pretty well to 15 and 20 sec. delays with an occasional one at 30 secs. The task was too difficult, however, and she shifted from one position habit to another.

Work was discontinued for a month. By the end of this time, the following changes had occurred in the child: (1) old position habits were temporarily lost; (2) new interest was taken in the problem; (3) greatly increased control of her own body appeared—shown mostly in walking and balancing; and (4) a stronger aversion to being held during distractions had developed.

Table III. above summarizes the results for this period. It also understates the subject's behavior. On January 5, she made the entire day—9 trials—at 15 secs. without error and with no correlation between orientation and direction of response. She now fell into a position habit, but recovered and made 20 secs. delay correctly 5 times in succession, again with no dependence upon orientation.

Thayer's best delays may be recorded as 20–24 secs. The child F. used in the earlier work reached a delay of 50 secs. F.'s record would probably have been higher had she been tested with a method similar to the one here employed, *i. e.*, a method where the satisfaction is derived from the stimulus object and not from an associated food supply. The gap between Thayer and F. would undoubtedly be bridged in a gradual manner by a continuous increase in periods of delay. Greatest interest now centers on children of less than one year of age. How early ontogenetically does this ability to react independently of orientation appear?

It remains to comment upon the position habits and errors that appeared. The frequency with which these stereotyped forms of response interfered with the work and *the fact that the child if permitted would watch the box containing the toy during the interval of delay*, indicate the great importance of kinæsthesis in the response. Position habits occurred with each of the three boxes so that during a particular position habit period Thayer always chose a particular box first. I made no tests where the choice lay between two boxes as opposed to three. Time is limited both by the speed with which a baby grows and even more by the necessity of staying within the limits of the child's interest and patience. Table V

TABLE V

					Total Reactions Made
Order of response.........	*cba*	*cab*	*ca*	*cb*	
No. made................	17	13	8	28	66
Order of response.........	*abc*	*acb*	*ac*	*ab*	
No. made................	6	3	2	11	22
Order of response.........	*bac*	*bca*	*bc*	*ba*	
No. made................	7	3	7	9	26

analyses all incorrect responses and gives the relative number of times the subject followed the different possible orders. Thus when an error was made, 17 times Thayer first opened *c*, then *b* and then *a*. The table shows that three times more errors were made beginning with box *c* than with any of the others. When the subject opened *c* first, she opened *b* next 45 times out of 66, or 68 percent of the time. When she opened *a* first, she chose *b* next 15 times out of 20, or 75 percent of the time. When *b* was opened first, *a* was chosen next 16 times out of 26, or 61 percent of the time. In other words, when the reaction began at the end of the apparatus the tendency was to take the boxes in order until the solution was reached. Only six times in all did the subject go to the same box twice in the same trial. These cases are distributed throughout the entire period of experimentation. The following is a record of the order of the boxes chosen:

cccab
bcba
cacb
cacb
bcba
cacacab.

Of the 114 errors recorded in Table V., 32 (28 percent) occurred when the box containing the toy on the last previous trial was re-selected. Inasmuch, however, as such a mode of response often led to success the percentage is very low. This form of behavior as well as that of the six instances above given is apparently far less current in the present subject than in Hamilton's dog.[1] The later study made by Hamilton[2] reports the case of a child 26 months old. Out of 38 trials, 60.53 percent (34.21 plus 26.32) of the reactions involved the type of behavior given just above as occurring but 6 times during the present work, 264 trials. Since Thayer missed 120 trials (66 plus 22 plus 26 plus 6), her percentage is 5.

[1] Hamilton, G. V. T., 'An Experimental Study of an Unusual Type of Reaction in a Dog,' *J. of Comp. Neur. Psychol.*, 1907, 17, 329–341.

[2] Hamilton, G. V. T., 'A Study of Trial and Error Reactions in Mammals,' *J. of Animal Behav.*, 1911, 1, p. 51.

This extreme difference in behavior is undoubtedly due to one or both of the following causes: (1) the guiding influence of the absent stimulus in the delayed reaction tests; and (2) the fact that only three boxes were used here as opposed to Hamilton's four. It would be very interesting to determine whether a variation in the number of boxes would result in a corresponding variation in 'reaction tendencies.' If this were true, the possibility of phyletic correlations would be pushed still farther back than appears in Hamilton's work.

It will be valuable to put beside this work, similar data gathered on rats and raccoons in 1910–1912. The records here given are representative and include only tests made with three boxes on periods of delay. The following indicates the maximal delays attained by the animals whose records are used in this paper:

Rat No. 9, maximal delay 10 secs.
Rat No. 2, " " 1 "
Dog Blackie, " " 5 mins.
Raccoon Bob, " " 30–35 secs.[1]

Table VI. summarizes the errors made by these four animals. It includes for comparative purposes the data for Thayer. The raccoon's records include delays from 1 sec. through 20 secs.; those for the dog, from 1 sec. through 7 secs.;

TABLE VI

Animal	No. of Trs.	Total No. of Errors *A*	3 Place Errors *B*	Persistent Errors *C*	Percent. of *A* to *C*	Percent. of *B* to *C*
Thayer	264	120	54	6	5	11
Raccoon, Bob	720	209	78	29	13	37
Dog, Blackie	570	127	75	25	19	33
Rat No. 9	575	144	42	13	9	30
Rat No. 2	345	152	69	47	32	60

those for rat No. 9, from the third stage of delay (turning light off just as animal was released) through 7 secs.; and those for rat No. 2, from the third stage of delay through 1 sec. I have included the data represented by 'percent of *B* to *C*' because Hamilton's percentages are based only on those reactions that included all the boxes of his apparatus.

[1] These data are taken from my 'Delayed Reaction,' pp. 35–38.

The column '3 place errors' includes the trials that involved a testing by the animal of each of the three boxes. By 'persistent errors,' I mean all errors that involve trying any one box more than once each trial. These were all 3 place errors. This column corresponds to reactions belonging to Hamilton's types *D* and *E*.

The only one of Hamilton's human subjects whose percentage in *D* plus *E* rose above 6.45 percent was a 26-months-old child whose grade was 60.53 percent. Of the animals below man, the lowest grade (best record) was 22.58 percent made by a dog. Hamilton's results and my own here presented indicate a marked difference between man and other animals in reactive tendencies, *i. e.*, in forms of kinæsthetic habits. (There is, I think, no clear evidence as yet that the tendencies are instinctive.) Whether this is caused by phyletic factors, or by experimental and environmental conditions is a matter undecided. My infra-human animals are essentially on a par. And so I think are Hamilton's in that practically all of them made their highest percentages in what I here term 'persistent errors.' (His curves would be quite different, naturally, if *D* and *E* were combined and if *B* and *C* were combined.[1]) Much work is undoubtedly needed to determine how minute a classification of reaction tendencies can be and still be significant for animal ability.

Theoretical Considerations

There is very little in the way of interpretative comments that I can add to what has already been said in other papers.[2] The delayed reaction problem can be solved at least in two ways:[3] (1) by the maintenance of bodily orientation in whole

[1] Hamilton, *op. cit.*, 1911, p. 54.

[2] 'Delayed Reaction,' pp. 62–79 and Hunter, W. S., 'A Reply to Some Criticisms of the Delayed Reaction,' *J. of Phil. Psychol., &c.*, 1915, 12, 38–41. See also Watson J. B., 'Behavior,' 1914, pp. 224–227; and Ch. X.

[3] If there is a present determining external stimulus, the reaction is not delayed. A. C. Walton, 'The Influence of Diverting Stimuli during Delayed Reaction in Dogs,' *J. Animal Behav.*, 1915, 5, 259–291, has shown that dogs can react successfully to three boxes after delays of 30 secs. when they have been distracted during the intervals. This is better than I had been able to show. Before deciding that the dog belongs in a class with the raccoons—as perhaps he does—it will be necessary to

or in part during the interval of delay or by the chance recovery of the proper orientation just at the moment of release; and (2) by the use of some intra-organic factor which is *non*-observable by the experimenter. In the first method, the animal always responds in accordance with orientation; in the second, he does not. The cue used in the second method may or may not be retained in the focus of neural activity during the delay. It is highly improbable that such retention occurs under conditions of distraction. What one has, then, is a system of processes or cues which 'stand for' certain differential responses as a result of association. These cues are susceptible to selective re-arousal and subsequent successful functioning in initiating responses. This is the condition which I have previously found in raccoons and in one child, F., 2½ years old. It is the condition here presented by Thayer, ages 13–16 months. This second method of solution which I am describing may be mediated by any type of intra-organic process which can be re-aroused without the presence of the external stimulus, toys as used in the present study. Inasmuch as kinæsthetic factors can be so aroused,[1] and inasmuch as they have been demonstrated to have great importance not only in animal reactions in general but in the delayed reaction in particular (position habits and maintenance of orientation), it is most probable that the intra-organic factors are kinæsthetic in nature. (The genetic relations of sensation, image and imageless thought are discussed in 'The Delayed Reaction,' pages cited.) In certain cases this type of process has its locus in the vocal organs with frequent resulting audible sounds. This we term vocal language as it occurs in normal human adults and in children of a certain development. In other cases the observance of the sounds and their accompanying behavior

have data on the animals' orientations at the moment of response. This Walton unfortunately does not give. The only facts that we have are that the animals did not maintain their orientations during the delays. If it should appear that the animal is able to recover the proper orientation in a large number of instances after a thorough distraction and can then react correctly, this fact will itself be of great significance and will require careful analysis.

[1] See the all too brief comments in my review of Calkins' 'First Book in Psychology,' Psychol. Bull., 1915, 12, 189–190.

does not indicate that the organism uses either the sound or the parallel kinæsthesis as a substitute of the type above described. This is the situation in all animals that indulge in vocalization. But this intra-organic kinæsthetic factor may arise elsewhere than from the throat. Some part of the general bodily musculature may be the origin. Here, when the behavior is overt, we speak of gesture language. When it is not overt, the delayed reaction method has proved serviceable in detecting it. Language is ideational in function. So also are the cues which function in many responses of raccoons, of children and possibly of dogs (Walton). The resulting conception of these cues is that they are kinæsthetic sensory ideas. This line of reasoning leads one to conclude that a true language non-vocal in character appears phylogenetically and ontogenetically prior to vocal language. Such language, although undoubtedly of great service to the individual in controlling his reactions, is of little social significance.

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THE PSYCHOLOGICAL REVIEW

THE LAWS OF RELATIVE FATIGUE[1]

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The temerity that ventures to speak of fatigue laws may well arouse a critical attitude. But I shall not be quite so indiscreet as the title might be misconstrued to imply. For reasons that will presently appear in detail, I have no expectation that the laws of mental fatigue will be formulated in the immediate future. Oeffner's so-called laws of fatigue are obviously only empirical generalizations and summaries. My subject is really much less pretentious. It concerns only the relativity of fatigue. The laws of relative fatigue that we shall discuss might with equal propriety have been called the laws of fatigue relativity.

My excuse for selecting so threadbare a matter as fatigue for the subject of the presidential address is largely personal. As some of you know, I have been working on various phases of mental fatigue experimentally for a number of years—too long for self complacency. More than once it has seemed to me that I was following a clear experimental path out of the maze of fact, only to find myself back again at the starting point, facing the same fundamental questions. But however personal my interest in fatigue may be it certainly is not exceptional. I venture the guess that there is not a member of this Association but has made fatigue the subject of direct, indirect, or projected investigation. Certainly few psychological subjects have so widely interested investigators in the allied sciences. Few seem to have at once such far-reaching

[1] Address of the President before the American Psychological Association, New York Meeting, December, 1916.

bearings on psychological theory and the conduct of human affairs. Few present such a bewildering literature, with such an array of apparently mutually contradictory experimental results. None is more confused with an equal pressure for practical working rules. Confusion and eagerness for practical results make a situation fraught with grave peril to science. If anything could, they justify this attempt to clarify and systematize the fundamental concept of mental fatigue.

It would be an impracticable as well as an uncongenial task for me to attempt a review of the literature of fatigue, even if this were a fitting occasion. Our time limits, our precedents, and my personal interests persuade me rather to attempt what I hope may prove to be a more generally useful undertaking, namely, a substantive analysis of the problem. The first part of that task, as I apprehend it, is to clear the problem of some misleading assumptions by which faulty analogy and practical interests have confused the real issues. Thus simplified we shall try to redefine the psycho-physical problem on a scientific rather than on a practical basis.

Mental fatigue is one of those scientific problems that has suffered from too much practical importance. In the enormous number of investigations that have appeared since the publication of Mosso's "epoch-making" book, just a quarter of a century ago, educational, medical, and more recently social and economic interests have given the dominant motifs. It was indeed an alarming arraignment of the schools that they ruined the health and impaired the eyes of pupils by their excessive demands. An investigation of such charges was a direct obligation on experimental pedagogy. Scarcely less important than the school problems is a just determination of the proper duration of an industrial day, with a fair consideration for the welfare of the laborer and for the exigencies of competition. None of us, moreover, is entirely free from more personal practical difficulties in our desire to exploit most effectively the time and energy at our disposal. Now I would not for a moment be misunderstood to depreciate the importance of these practical studies. My only contention with respect to them is that they all suffer more or less from an inadequate scientific basis. But in spite of all the confusion of alleged fact, all the premature and unverifiable pronouncements, most of us still believe that an adequate answer to such practical questions is both desirable and possible.

Less insistent and obvious, but none the less real and important, are the scientific problems of mental fatigue. For the present at least it may be of some advantage to keep separate the two lines of investigation, the practical and the scientific. The former is, in the main, quite independent of the latter. However the questions as to the nature and laws of mental fatigue may finally be answered, careful generalization from experience as to the expediency of certain work and relaxation sequences will deserve and will receive careful consideration in planning the day's work. So too the most advisable length and distribution of recesses may be settled purely empirically, entirely without reference to any of the underlying bio-chemical processes. For all such practical purposes the concept of fatigue is an accident. Its function is not to recall the implications that it has in bio-chemical science; but merely to serve as a vehicle for practical maxims, a class name for all sorts of unanalyzed hindrances to effective work. The hindrances would be just as real and the practical maxims just as valuable even if it were proven that fatigue had nothing to do with them. The scientific problems as to the real nature and conditions of a supposititious mental fatigue are quite independent of all such questions of practical expediency. Scientifically we must know the differential characteristics by which mental fatigue can be distinguished from the other limitations of the work curve; as well as its elementary forms and their interrelations. We must follow its implications as an indicator of the relationship between mind and body; and correlate it with other bio-chemical facts. It is these latter problems that appeal to me personally with especial emphasis. Mental fatigue if it exists in the physiological sense, must be connected in some direct way with the energy transformations in nervous tissue, and the fundamental problems of inner psycho-physics. The great problem whether our mental life conforms like the rest of the organism to the underlying postulate of thermodynamics, the conservation of energy, must be answered if at all by the psycho-physics of work and fatigue. While I sincerely hope that adequately equipped attempts to explore these fundamental questions are not too far distant, there are related problems that can be examined by simpler techniques. Again mental fatigue if it exists ought to furnish us with an instrument of dynamic analysis of the mental complexes, reaching the inner mechanisms of our mental life. To estimate its possible usefulness, one has only to think of the analogous use of fatigue or adaptation in sense analysis; when we adapt out one sense quality and note the effect of its loss on the other qualities, or on perception in general. So, for example, the relative composition of two purples might be shown even if we had no other method, by adapting out spectral blue, and comparing their resulting appearance. It would seem that a similar process ought to be applicable to mental experiments when we have no other means of experimentally eliminating the various factors. One might even outline the working postulates of such an analysis as follows: I. Whenever in mental processes fatigue of one is regularly accompanied by fatigue of another there must be some dynamic factor common to both. II. Conversely, whenever the fatigue of one mental process does not show as fatigue of another, the two must depend on different dynamic conditions. III. Whenever fatigue in one process is accompanied by the improvement of another process then the two are probably related in the sense that the fatigued factor in the former was inhibitory to the second. That such postulates have borne little fruit hitherto, is not due to any inherent logical unsoundness, but rather to our misapprehension of the character of mental fatigue. At present their application to the problems of analysis would be handicapped by the very richness of the alleged correlations. It has proven embarrassing to more than one of us to teach our students on one occasion the very slight correlation between mental processes that seem very much alike; and on another occasion to teach them how mental fatigue in general may be measured by the pulse, the ergograph, addition, reaction time, the dermal threshold, and other apparently disconnected events through a long list of accredited extrinsic tests. To be sure the reliability of these and other so-called tests is not universally admitted. But the gross discrepancies between genetic and dynamic correlations might well be taken a little more seriously. Before any of these scientific tasks can be undertaken with promise of success, we must know what mental fatigue really is, if there is any such thing, and how it is conditioned.

The Concept of Mental Fatigue

The concept of mental fatigue is so familiar that a precise analysis of its differentia has seldom seemed necessary. Statements of its meaning, when they occur, regularly emphasize a diminution of some product of mental activity per unit of time, incident to continued activity, and as Thorndike insists, recoverable through rest. Actual recovery or the capacity to recover through rest seems to me to be irrelevant. On the one hand it excludes extreme fatigue; on the other hand it fails to exclude all sorts of intercurrent disturbances. But the diminution of production consequent to continuous work with or without recoverability, is I believe an untenable criterion of fatigue.

If the word fatigue has any scientific propriety in connection with our mental life, it seems to me that it should refer to the metabolic conditions of mental action, not to any predetermined characteristic of its consequences. This is very much the same point that I made recently concerning mental work. While that was not received with the unanimity that I had hoped, in the case of fatigue at least, failure to realize the dynamic implications must lead to gross confusion. Obviously psychology or pedagogy is entirely competent to ignore the physiological concept of fatigue and to develop its own empirical concept as decreased returns of mental work. But if it ignores the metabolic implications at the beginning, it may not assume at the end that physiological and pathological fatigue processes parallel the decreased returns. It is such gratuitous assumptions concerning matters of fact that make psycho-physical parallelism a dangerous working hypothesis. Moreover, such an independent psychological concept would be scientifically defensible only to the degree that work decrements, consequent to work and eliminated by rest, prove to be homologous processes with regular and definable antecedents. If on the contrary work decrements show a large variety of types, or follow any considerable variety of conditions, it would seem to be good sense and sound science to enquire whether any of the varieties of mental work decrement correspond to physiological fatigue processes. These alone would then seem to have a natural right to the name mental fatigue. From this standpoint other decrements would be regarded as pseudo-fatigues.

In order to conserve our time let me be quite direct and frank. I regard it as improbable that any of the mental work decrements so commonly treated as mental fatigue, are ever simply conditioned by true fatigue processes in nervous tissue. Conversely real fatigue may not appear as a decrement at all. Some of the evidence for this position can only be indicated here. Some of it must be given in more detail.

First one must note the physiological fact that nervous tissue *in situ* has been found quite resistant to fatigue and exhaustion under normal cirumstances. The axis cylinders apparently never fatigue except under experimental conditions when their environment is freed from oxygen, or when they are narcotized so that they are unable to use the oxygen that is present. Cell bodies are likewise resistant to fatigue under normal circumstances. They can be exhausted in experimental animals only under strychnine poisoning, after the withdrawal of normal blood supply. Langfeld has shown that in humans prolonged fasting produces no correlated decrease of neural efficiency. Reflexes like the knee-jerk and the protective lid reflex show no decrement after long series of elicitation, if care be taken to prevent intercurrent general depression of the nervous system. In those cases where fatigue decrement of the reflexes does occur, there is evidence that neither the muscles, the nerves, nor the nervous centers have lost their irritability except to the particular stimulus to which they have become adapted. On the contrary hyperexcitability is a common if not a regular phenomenon of extreme so-called mental fatigue. At any rate it would seem that the complete cessation of mental processes, like the inability to recall an opposite, to complete a sentence, to recite a series of nonsense syllables, or to multiply four-place numbers by mental arithmetic cannot possibly mean the fatigue of nervous tissue to the corresponding degree of completeness.

A second ground against the traditional differentiæ of fatigue is their failure to exclude normal psycho-physical rhythms. In more than one respect it was an unfortunate accident that the paradigm for the interpretation of the phenomena of mental fatigue was the fatigue of a nerve-muscle preparation. Undoubtedly there are many and important analogies between the action of lower spinal arcs and cerebral processes. But after all the main task of physiological psychology begins when it seeks to understand the differences between the simpler processes and cerebral action. Similarly, in connection with a supposititious mental fatigue the regularly increasing work-paralysis of nerve-muscle preparation may be and in some respects must be a misleading model. One of the great differences is that while the extirpated preparation changes only slowly under experimental conditions when unstimulated, normal mental life precludes unchanging neural conditions. In the complex interconnections of human cortical processes the one statement that can be made with completest conviction is that the experimental subject never remains constant, quite apart from the intended experimental changes. Even under the best possible experimental conditions, the experimental change is only one of the changes that we know to be occurring. The constitution of these non-experimental changes in any given case we know only in part. We believe that consciousness itself is a process which involves more or less continuous inherent change. We know that there are also various intercurrent physiological rhythms, cardiac, vascular, respiratory, intestinal, glandular, and muscular. Cortical action may also initiate non-rhythmic changes in the glandular, circulatory, and respiratory systems with far-reaching reactions of those changes on the cortical action that originated them.

From the standpoint of the importance of the accompanying mental changes, perhaps the most significant of the rhythms is sleep. The fatigue-hunting enthusiasm that finds in sleep the daily climax of fatigue is without physiological justification. On the contrary, we have learned from experimental investigations that for some persons evening may be the time of most effective mental work. Moreover, it is neurological commonplace that in serious extreme fatigue, sleep may be impossible. Physiologists would welcome any insight that we could give them into the causes of sleep. The fatigue climax assumption simply is not tenable. Whatever they may be, we know that the conditions of sleep are not simple. Habit, the absence of stimuli, probably widespread inhibitions, and possibly gland products and vaso-motor changes coöperate in its production. Sleep may come from restriction of activity quicker than from over-exertion. Lecturers never go to sleep. The audience may. In view of such complication of the conditions of continuous work decrement the assumption that all diminished returns consequent to work and eliminated by rest are fatigue seems to me utterly untenable.

A third ground of suspicion against the true fatigue character of most so-called mental fatigue is found in the means that are commonly used to induce it. In nerve-muscle fatigue experiments one isolates a specific tissue and stimulates it successively in the same manner. In mental fatigue experiments, on the contrary, repetition of the same stimulus is systematically avoided. The more carefully one analyses the assumptions of this anomalous technique, the more incongruous it appears. Let us take a concrete instance from what Thorndike has taught us to regard as one of the purest forms of mental work, mental arithmetic. If we strictly followed the analogy of physiological fatigue experiments some association in mental arithmetic, say the multiplication of two times two, should be repeated until work decrement or paralysis indicated fatigue of the association process. As far as I know that is never done. It seems absurd. The experimental device of constantly changing the stimulus in fatigue experiments is defensible only on the assumption that all multiplication processes affect the same general group of tissues, and that continuous multiplication of different digits increases the sum of the fatigue of the whole. But neurologically the assumption is certainly a strange one that the nervous tissue which was involved in one association fatigued more when a variety of different associations were made than it did when all the burden fell on the same associative elements, operating continuously or in rapid succession. Moreover, there are no facts available to show that restriction to a single field like multiplication will produce greater work decrement than rapid change from one field to another. On the contrary there is evidence that the greater the complexity of the mental task the more pronounced is the decrement. Such decrement, however, is more probably due to a confusion between different paths of discharge than to fatigue of any particular path. That confusion is real and a common experience every introspective account is evidence. Theoretically it should be expected from the operation of the known laws of association. Suppose, for example, that after adding various digits to seven we come to the task of adding four. The right associate is by hypothesis well known and thoroughly practiced. But if other numbers have recently appeared in the series they also tend to be reproduced on the basis of recency. It is at least conceivable that the true associate in such a case might be difficult to recall, not at all from fatigue of the corresponding tissue but from effectual inhibition because a more recent associate appears in its stead. The necessity for inhibiting irrelevant and false associates is certainly a common experience in the elementary mathematics of some of us. But the tendency of recent ideas to recur is not in any sense a fatigue or exhaustion process, but is probably a matter of residual excitation and summation. Such work decrement then is not fatigue but mere association rivalry.

A fourth ground against identifying work decrement and fatigue may be found in the operation of incidental inhibitions. Theoretically, every mental operation arouses more or less widespread associated reverberations which manifest themselves in the sequences of actual associative recall, and may on occasion, as we have just seen, operate to confuse the regular sequence of work by a kind of associative rivalry. Theoretically also, every actual association process involves more or less widespread inhibitions of undesirable associations. Now it is conceivable that these useful inhibitions of the irrelevant might operate to produce a pseudo-fatigue work decrement in any extrinsic test. For example, I have published experimental evidence that the most intense mental work of an examination period commonly follows the first reading of the examination questions. It is the period of adjustment to the examination as a whole, when widespread association systems are being organized. Such activities are not possible in fullest degree without corresponding inhibitions. Ordinarily distracting stimuli pass unnoticed. Even physical discomfort and pain may for a time be ignored. Now it is conceivable that if at such a time the fatigue tester should request the examinee to add digits for two minutes as rapidly as possible, the response might show a degree of work decrement that bordered on total incapacity. Or again, suppose we would measure the fatigue of a Wall St. broker, hour by hour, with the æsthesiometer test. And supposing as the hour struck we should interrupt a selling campaign that was taxing his skill as a broker by the request that he submit to our compass point test. The chances are in favor of some rather vigorous verbal defensive reactions with no discrimination at all. But if we were able to hold him to a promise and actually start the test, is there any guarantee that gross decrements in the measured function, all due to previous work and remediable by rest, might not be due to his inability to give his attention to our petty tests while his fortune was at stake on the floor? Of course the whole situation is absurd. The most enthusiastic believer in space threshold tests would hesitate to use such results as an indication of the broker's general mental fatigue at that time.

We freely admit that these are extreme cases, and that they break the most elementary rules for experimentation. But have we any guarantee that similar discriminations against some seemingly unimportant task might not occur just after recess, or just before school lets out, when the afternoon's escapades are in the making, or any time at the interruption of seemingly important processes? Conversely is there any guarantee that the interruption of annoying or even fatiguing work by a few moments of trivial testing might not be a joyous relief, giving results that might entirely hide a supposititious real mental fatigue of the interrupted work? I am not arguing that such inhibitions would not be very much worth knowing; but merely that it confuses their real bearings to call them all fatigue.

In addition to these specific inhibitory processes which are commonly classed in psychology as phenomena of attention, we are acquainted with secondary inhibitions through a diminution of the supporting organic processes, glandular or circulatory. Of the glandular changes I have no direct knowledge. The initial increased pulse frequency, whenever complete relaxation is interrupted by any mental activity, is commonly followed by a gradually decreasing heart rate in any prolonged experimental task. We may regard this as a kind of adaptive process, an habituation to the task at hand. It is difficult to conceive of it without reference to the gradual elimination of extrinsic excitations, in which an initial general excitation is followed by an inhibition which restricts the excitement to selected processes. I have been able to demonstrate that something of this sort occurs in every normal reading pause. That continuous fixation of a trivial object is inhibitory is shown by its familiar hypnagogic tendencies. It is one of the methods of producing hypnosis. With some probability we can predict a diminution in the organic conditions for metabolism in all relatively unused neural centers during monotonous mental work. In extreme cases continuous disuse leads to atrophy, muscular, neural, and glandular. To regard work decrement which is due to more or less complete atrophy of unused paths as fatigue would be a manifestly absurd confusion of concepts. But work decrement from secondary trophic deficiency, as in unused parts, is just as surely not fatigue. Just as in periods of excitement and important readjustment, there are undoubtedly vascular and glandular changes which increase the activity of the whole neural mechanism, reflexly reinforcing the processes that initiated them; so it is probable that general depressions of glandular or vascular origin accompany monotonous mental work, in which even the centers that are most active finally participate. But this again is not fatigue in any physiological sense.

In as far as these various processes represent work decrements or decreased returns that might be mistaken for neural fatigue they may properly be called pseudo-fatigues. We have described pseudo-fatigues of intercurrent rhythms, of residual excitation and rivalry, and of specific and trophic inhibition. The pathological evidence that work decrement is no true indicator of nervous fatigue is not new. Even to summarize it would extend our paper too far. But I think that without it, we have established the thesis that decreased returns resulting from work and recoverable by rest if you will, cannot be employed as simply and directly in the higher neural systematizations as it can in simpler tissues. Arbitrarily to define mental fatigue as work decrement is effectual self-banishment from physiological tradition as well as from clearly defined fields of investigation of the utmost importance.

Having divested the mental fatigue concept of its irrelevant content as vehicle of the various work decrements, it is now in order to inquire whether there is in our mental life a real fatigue phenomenon. I believe that there is, but its manifestations differ from the paradigm of nerve-muscle fatigue in two important particulars. These are: first, the inconstancy of the stimuli in mental work; and second, the interaction of competing paths. These two differences combine to emphasize the relativity of all mental fatigue.

The Relativity of Fatigue

In the nerve-muscle fatigue experiment, the stimulus is always simple, and usually constant in intensity, given at regular time intervals. For a variety of reasons the stimulus that is most used is the faradic current. It is capable of fine adjustment, may be held at constant intensity over long periods, and is exceedingly effective in quantities that do not damage the tissue. No physiologist would start a fatigue experiment with stimuli of unknown and variable intensity. Unfortunately, that seems to be the only practicable method at present in so-called mental fatigue experiments. Nobody knows the relative stimulus value of two different mathematical sums. But what is vastly more embarrassing, nobody knows how to follow or to evaluate the ever-changing inner factors in the total mental stimulus, such as the force of the instructions, the personal interest of the subject in the scientific aspect of his task, in its bearing on the particular exigencies of his academic career, and so forth. It was one of the great services of Kraepelin in his analysis of the work curve to show how these inner stimuli may change during an experimental period. The meaning of that analysis as I apprehend it is not given in the precise variables or spurts that he found, nor in the assumption that they are always present, but rather in the demonstration that variables in the inner stimuli may occur and must be reckoned with. It would not take us long to add to his objectively defined list many others taken from our experimental experience, such as competition and personal pride, repetition of the instructions, encouragement and persuasion, the presence of the instructor, rewards and penalties of various sorts, and the unanalyzed mass of obligations.

I am not unaware that this matter of the inner stimuli to mental work is packed with problems that we have no adequate techniques to investigate. But that is no excuse for ignoring them. It is our business as scientists to try to see things as they are, even if they are complex. There is at least some ground for the suspicion that most if not all our real mental fatigue of the work decrement type is really a fatigue of the inner stimuli rather than of the capacity to react. This at least would account for the extraordinary correlations in the fatigue of the most diverse functions. In many so-called mental fatigue experiments the only common factor discernible to introspective analysis is the intent to keep working as fast as possible to the neglect of competing interests.

Now in the physiological experiment fatigue may be shown in two ways, either by a rising threshold or by decreased response to a constant supra-threshold stimulus. Only in the latter case is there an obvious work decrement. The former case implies a constant work output with a gradually increasing stimulus intensity. In mental work we are often distinctly aware of similar changes in the intensity of the inner stimuli that keep us at a disagreeable or monotonous task. Mere interest in the task may lose its force comparatively early. Then the task is continued from stubbornness, the dislike to fail, sense of obligation, honor, fear of ridicule, or hope of reward, etc. All of these may operate in succession. In the end all of them may lose their force and we say, "I do not care what happens, I cannot go on with this thing any longer to-night." There may have been no important work decrement until the break, as Yoakum calls it. But the process is none the less a real fatigue if the continuation of work depends on a change of the stimuli.

All of this emphasis on the importance of the neglected factor of changing stimuli in the fatigue concept is probably sufficient to justify the formal statement of a necessary correction in the traditional definition of mental fatigue. We may call it the first law of relative fatigue, neglected rather than new. Without pretending to give it final formulation we may express it as follows: Within physiological limits, all fatigue decrement in the results of work is relative to the intensity of the stimulus.

Education and society have a very practical interest in this phase of the fatigue problem.' They make use of a large number of incentives in which as Thorndike wisely points out the changes in satisfyingness may be a real cause of work decrement. The adequate adjustment of stimuli to the development of the individual and the needs of the case would seem to be a very real problem in the training of backward and gifted as well as normal children. It seems strange that we have so little experimental knowledge of the relative value of available reinforcements. Autogenic reinforcement is, I believe, at least one factor in the underlying psycho-physics of James's 'reservoirs of power' which may be quite as significant for psychology as the action of adrenin to which Cannon has introduced us. That continuous activity under the reinforcement of emotion or even in the educational use of play may be a source of serious fatigue we have been warned by Kraepelin. Some other reinforcements are conspicuous for their insistence. Such a one is worry. It would seem to be no accident that this is so closely connected with exhaustion psychoses.

I believe that the relative value of the various inner stimuli would repay the closest study. Just now it seems to be interesting the abnormal rather than the normal psychologist. Practical experience is full of rough approximations. Their refinement by experimental techniques would not seem to be an impossible task.

It is possible that we can study relative fatigue not merely by the changes that occur during long series of repetitions but more expeditiously in the relative refractory phase which the genius of Verworn proved to be identical with the fatigue process. Since the relative refractory phase is common to all nervous tissue, I have asked the question whether we can find in mental processes a similar phenomenon. This is undoubtedly the case. In fact every mental process shows something analogous. Repetitions of all sorts seem to be avoided whenever practicable. The repetition of questions, courses, lectures, phrases, and even words is possible enough, but except for special reinforcing circumstances, it is postponed until the effect of the initial case is somewhat worn off.

The routine is regularly less alluring than the unusual. Mankind in general prefers new scenes, new plays, new walks, new jokes, new styles, new investigations. Possibly the decreased effectiveness of over-memorization is a case in point. Possibly even the loss of attention to frequently repeated processes, which is commonly regarded teleologically as a freeing of consciousness for new adjustments, may be caused by the longer refractory phase of the more complex systematizations of attention, so that the rapidly repeated task is dynamically excluded from conscious emphasis.

Works of art on the contrary are characteristically resistant to the refractory phase. Possibly this results in some way from their origin. Certainly one of the marks of good art is the constancy of its appeals. The popular song, the clever phrase, the good joke, soon finds us refractory to the point of desperation, though it is notable that we become refractory to their reception much quicker than to their execution. We like to tell old jokes better than to hear them. But the great classics in music and literature may be heard over and over with increasing satisfaction. It is not impossible that Aristotle's catharsis by dramatic representation of suffering and evil really operates by developing a refractory phase, and a kind of relative fatigue. How far this principle operates in habituation to environment, indifference to shocking conditions of poverty and morals, to suffering, and to the horrors of war, as well as to luxuries "when the novelty has worn off," I am not prepared to estimate.

It would seem that some of these or analogous phenomena ought to yield data for a scientific study of the intensity of the inner stimuli in connection with fatigue if we only knew how to use them. But the very difficulties of technique emphasize how far we are from a real knowledge of relative mental fatigue.

The simplicity of the nerve-muscle paradigm of mental fatigue is further misleading in that it gives no indication of certain important complications which are characteristic of higher systematizations, and which Sherrington called their competition. In a nerve-muscle preparation the impulse has only one possible path. In the higher nervous system on the contrary any afferent impulse may theoretically activate every efferent path. Just which motor process it finally initiates, is determined by a kind of competition. Competition appears in the spinal reflexes though less conspicuously than in cortically conditioned action, where it is the rule. Unfortunately, however, just where it is most significant it can seldom be followed objectively by our present means of investigation. But there are clear evidences of its operation in associative thinking, in attention, and in perception as well as in conduct.

The relatively fixed tendencies of competition in the cord are probably determined very simply by neural growth and development. In higher systematizations the outcome of competition tends to follow habitual patterns which have originated in the varying life-history of the several competitors. At any given moment in a developed system of this sort, the outcome may be modified by a variety of reinforcing and inhibiting accidents. Among the latter we must count fatigue. In closely balanced competition the absolute degree of fatigue need not be high to make it a deciding factor. Indeed it is conceivable that if the balance of the other factors is close enough, an infinitesimal fatigue, or the slightest trace of a refractory phase may totally change the character of the response, just as intrinsically trivial reinforcements or accidental inhibitions may be the arbiters.

This relation of fatigue to balanced competition gives us a second type of fatigue relativity. Fatigue is relative, not only in the relation of apparent work decrement to stimulus, as expressed in our first law; it is also relative in the sense of a proportionate fatigue of the various factors in a competing group. We may tentatively express this second type of fatigue relativity in the form of a law which for want of a better name we may call the Second Law of Relative Fatigue, because it implies a higher systematization than the first law. In any complex of competing tendencies the relatively greater fatigue of one tendency will tend to eliminate it from the competition in favor of the less fatigued tendencies.

Unfortunately the mechanism of competition cannot be studied at all in simple preparations and only imperfectly in the reflexes. The most characteristic systems are the least accessible. In the search for accessible human systems of greater complexity than the reflexes, it occurred to me, something over ten years ago, that the motor apparatus of the eyes offered some unique advantages. There we may study twelve intimately related and delicately adjusted final paths which are directly connected with reactions of considerable biological importance. Furthermore, every variation of their interaction is capable of being recorded on a single plane, without complicated mechanical devices, and without the distortions incident to the moving of heavy masses, like the limbs. Since that time the eye-movements have proved to be unusually valuable indicators of neural conditions in some forms of insanity and under the action of alcohol. In experiments that are now in progress they give promise of being the most consistent indicators of general neural conditions. In the early hopes of using them for an analysis of fatigue phenomena, I took a considerable number of binocular records of rapid successions of eye-movements after the model of the ergograph. Though reported on informally from time to time these records have never been published before because of my inability to account for some of their most conspicuous peculiarities. As these difficulties have gradually been experimentally cleared, the records have been seen to illustrate in a remarkable way some of the characteristic phenomena of mental fatigue, and pseudo-fatigue. In particular they admirably schematize the second law of relative fatigue and the "breaks" that it conditions.

Let me assume your familiarity with the technique of photographically recording the eye-movements from the corneal reflection. For the present records the eyes moved horizontally through an arc of sixty degrees, fixating successively two knitting needles which were situated thirty degrees on either side of the primary position of the line of regard. Each dot or dash on the records represents one phase of the alternating current, and a time interval of about eight thousandths of a second.

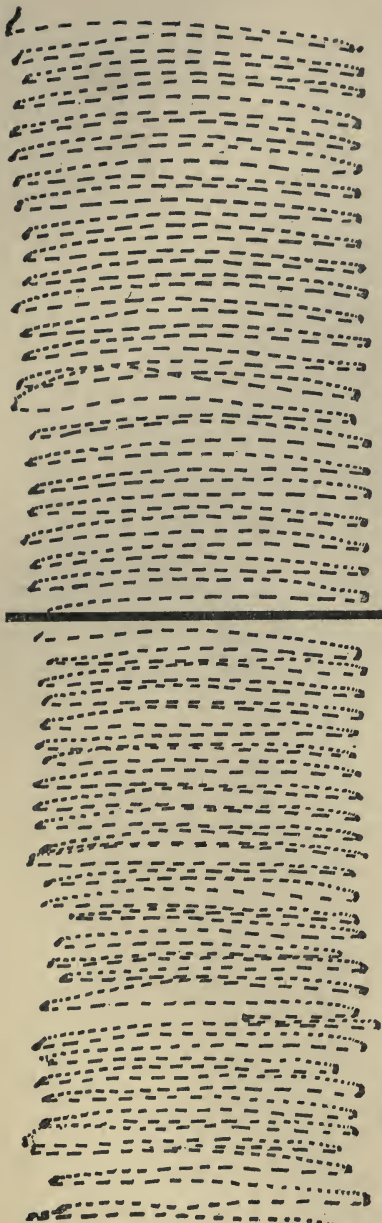

FIG. 1.

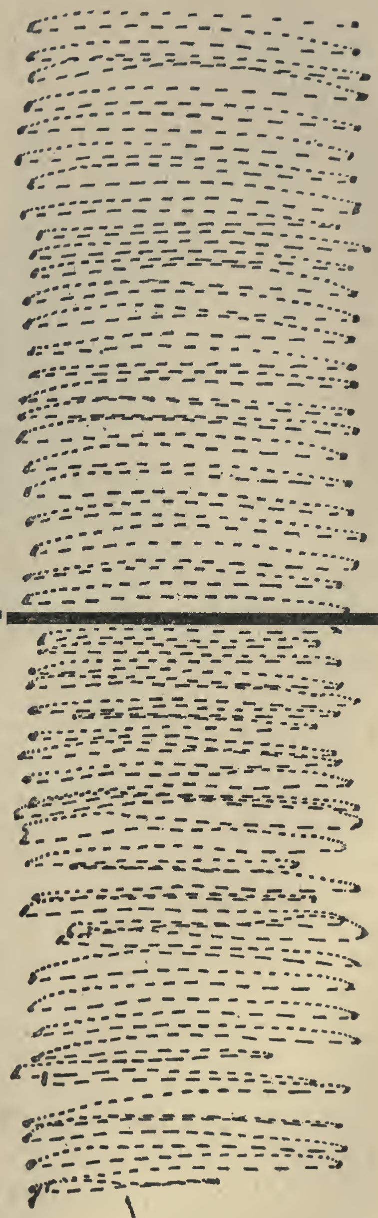

FIG. 2.

The succession of eye-movements in the records that are here reproduced was as rapid as practicable with subjectively adequate successive fixation of the two fixation marks. Some of the more characteristic fatigue phenomena which they show are: (1) The speed of movement becomes less towards the end of the series; (2) the fixations become less accurate; (3) and finally the line of movement itself becomes more irregular. Fig. 2 shows the climax of these processes in a break. The gradual decrease in angle velocity corresponds to the work decrement of extirpated muscle. But in this case, in view of Sherrington's demonstration of the reciprocal inhibition of antagonistic eye-muscles, it doubtless involves something more. The greatest angle velocity of eye-movement could only occur when the relaxation of the antagonistic was perfectly coördinated with the contraction of the agonistic muscle. The pseudo-work-decrement in this case then is not purely muscular but is in part a matter of defective coördination. The increasing errors of coördination have a similar origin. That is, the total elaboration of the contraction impulse and the corresponding relaxation of the antagonistic becomes less exact in successive repetitions of the act of fixation. But the coördination is not limited to the internal and external recti as one might expect them to be in horizontal movements of the eyes. All the records of 60″ eye-movements, which I have ever seen, show a vertical factor. In all my records this vertical factor results in an elevation of the line of regard. But it varies from movement to movement. That these vertical components are not accidents of purely muscular origin is shown by binocular records. Since the disturbances are homologous for both eyes, their origin must lie in the central nervous system. While occasional gross disturbances occur early in the series of movements, they become more and more conspicuous as the series progresses. The vertical components represent the intercurrent action of related and competing, but this is a case of non-inhibiting systems. When they become extreme they tend to interrupt for a moment the main rhythm of horizontal movements. In some cases these various disturbances produce a moment of confusion and a break in the process, which in ordinary mental fatigue experiments would be interpreted as complete fatigue or exhaustion.

Our eye-movement schema for the relative fatigue of competing systems is particularly free from complications through extrinsic facilitations and inhibitions. Retinal fatigue or adaptation is reduced to a minimum by the eye-movement itself, and the consequent shift of the area of stimulation. The homologous fixation marks, under constant illumination present the same stimulus for each reaction in the same direction. Cortical conditions of the successive reactions, such as interest, attention and motives to continue at work, cannot of course be guaranteed to remain constant. But the experiment itself introduces no obvious distractions like the physical discomfort of the ergograph. Moreover, all our relative fatigue phenomena appear during short experimental periods.

In order to protect our conclusions from the dangers inherent in a single line of experimental evidence, I sought other similarly complicated coördination systems. While thus far no other has been found with all the advantages of the horizontal eye-movements, those movements of the index finger which Bergström recommended for ergographic work show a similar complication. Undoubtedly the strongest and best practiced oscillatory movements of the index finger are the flexion-extension movements. Considerably less easy for most of us are movements of the finger sideways in the plane of the hand. In any event, rapid oscillation of the finger in this direction is always disturbed by intercurrent action of the flexors and extensors. Their action prevents rectilinear movement, decreases the angle velocity, and finally may so confuse the process as to produce a break in the sequence of oscillations, quite like the disturbances of the eye-movements.

It was the phenomena of these relatively accessible complex systematizations that forced me to a re-analysis of the mental fatigue concept. I believe that our eye-movement paradigm gives us the clue not only for a more intelligent experimental investigation of mental fatigue, but also for the interpretation of previous investigations. The very irregularity of the traditional results may be an expression of the laws of relativity. But I hope that the time has passed when an experimenter will be content to give us only the work decrement as datum for the measure of fatigue. Certainly the break can no longer be regarded as a temporary exhaustion of a function. Perhaps the least expected change that the new paradigm will make in our tradition is the place of the interfering sensations of weariness. These may, after all, turn out to be subjective indicators of real fatigue. Their effect in apparent work decrement, however, will be determined by their relative importance in the group of competing tendencies. Under normal conditions at least I doubt if we should call weariness a pseudo-fatigue.

It will be noted that the eye-movement paradigm is still much too simple to apply directly to our mental processes. In place of its anatomically restricted competition to the nuclei of the third, fourth, and sixth cranial nerves, we have reason to believe that cortical competitions are as indefinitely complicated as the various active association tendencies. That a variety of tendencies to associative reproduction are normally aroused as the effect of a mental stimulus is indicated by the facts of the association experiment. This normal spread of excitation, coupled with the effect of psychophysiological rhythms, and the complication of simultaneous stimuli from the different receptor fields, gives the competition in mental operations an almost chaotic complexity. But in addition to all that, we must extend our notion of competition and relative fatigue to those more slowly changing inner determinants of action that we call motives, controls, and the like. Indeed it seems probable that these inner factors, in so far as they are the only continuously acting factors in mental work, are more apt to be the locus of absolute fatigue than the several discrete association tendencies which are involved only occasionally in the mental task.

But aside from the obvious differences in complexity our paradigm adequately represents the fundamental processes. However long a mental process may be continued and however insignificant the decrement in returns, there comes a moment when it stops. It may be interrupted by demands for food, for sleep, or by some competing task. It may be interrupted by the gradually increasing insistence of inhibiting sensations like thirst, eye-strain, muscle pains, or pressure pains from sitting still. In any case, the work decrement of the consequent break can never be fully understood if we regard it as a direct product of fatigue, but only in connection with the intercurrent competing tendencies. Fatigue may be a contributing factor, but the apparent decrement of the break will bear no regular relation to the degree of absolute fatigue in the tissues which performed the discontinued task.

This enables us to understand why in pathogenic nervous exhaustion, the physician in search of a therapeutic measure may seek to strengthen some competing interest. He may even try to develop some fad, philanthropy, golf, the calculation of food calories, or what not, to compete with the old system and its emotional, business, or religious reinforcements. Most normal lives seem too full of competing tendencies. In my own case I have been interested in observing how every prolonged period of monotonous work like correcting papers, for example, finds before its close some insistent demand for interruption. If I successfully suppress one demand, more insistent ones arise, until finally effective voluntary reinforcement of the main task suddenly ends. The voluntary reinforcements may have developed such sensations of strain that the surrender to a competing impulse brings great relief. I know that the interruption is not permanent. I consent to it to get the lesser matter off my mind, expecting to return presently to the main task, freed from the incubus of that particular competitor. In very much the same way, after lying awake for a time on one side we turn over, not because we could not lie on that side longer, not because we expect any great improvement from the change, certainly not because we expect to lie on the other side forever. The displacement of the body mass is scarcely the product of fatigue. But in the complex of competing tendencies a little relative fatigue becomes the occasion for an entirely disproportionate result. Possibly social unrest follows a similar course. They seek a change in the government, or the social and labor conditions, not because the present is really unendurable, not because they expect a permanent betterment. In many cases at least, they act from relative fatigue, to shift the pressure. I suppose all the phenomena of restlessness and the corresponding attractiveness of change finally reduce to competition and the relative refractory phase. They operate in work and play, in social and economic activities, in politics and in religion. Without their interference in our lives, unwelcome as it often is, we must have continued indefinitely in the direction of our first activity, with the consequent loss of that vital equilibrium on which the organism as a unit of different parts depends for its continued existence. Without their interference the initial process must always work itself out to the final collapse of complete exhaustion.

Relative fatigue, then, is not a mere limitation of human efficiency. It is not exhaustion, but prevents it. It is a conservator of organic equilibrium, as well as a condition of organic development. The incapacity of the young child for long-continued monotonous tasks may be a symptom of an active, developing mind. Lack of competition would result in mental deformity, or absolute exhaustion, just as truly as the lack of stable reinforcing systems in the adult would mean perpetual infantilism. Thus it seems to me that the principles of relative fatigue have a direct bearing on the practical problems of education which the traditional doctrine of fatigue as apparent work decrement entirely missed. The development of the capacity to sit still, to continue long at routine work, the adequate response to all formal discipline demands more than the strengthening of the corresponding neural bonds. It demands the weakening or elimination of competing tendencies. At least one of the perils of routine education arises from this depression of spontaneity. But I have expressly disclaimed any right or intent to discuss the practical side of the problem.

I cannot quite resist the temptation, however, in closing, to point a methodological moral. There has seemed to me to be something almost humiliating in the eagerness with which tests of mental fatigue have been sought, while there is still so much that is uncertain in our knowledge of the fundamental nature of the process that we would test. If it is not too great a strain on presidential license at a meeting like this, when the program is so largely devoted to the matter of tests, I would sound a note of warning that in my opinion any tendency to supplant psychological investigation by tests would contain a serious menace to the future of psychology. Both have their proper place. But it can only lead to confusion and work to the discredit of our science if the search for practical tests blinds us to the necessity for studying the dynamics of the processes that we hope to test. We cannot afford to develop a new phrenology.

MORE CONCERNING THE TEMPORAL RELATIONS OF MEANING AND IMAGERY

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The controversy between the imageless thought adherents and their opponents has been lent a new aspect by the work of Dr. T. V. Moore.[1] He has ingeniously devised a simple but most fruitful method for investigating the temporal relations of meaning and imagery. By the use of this method, he has brought forward striking evidence which seems to support the contentions of the imageless thought school. The present investigation makes use of Dr. Moore's method, but by applying it to a greater number of subjects has obtained data which point to a qualification, if not a contradiction, of Dr. Moore's conclusions.

In the part of Dr. Moore's work which directly concerns us, he presented to his subjects the names of easily visualizable common objects, such as Zange, Fernglas, Pfeil, Messer, Lampe, etc.,[2] and asked them to react according to either one of two instructions. One time he would instruct them to react just as soon as they had obtained the meaning of the word, another time just as soon as they had obtained a visual image of the object which the word named. Nonsense words were occasionally introduced as a check to make sure that the subject was reacting to real meanings. By averaging up the times separately for the two kinds of reaction, he found whether, on the average, the subject could obtain meaning or visual image in shorter time.

Of the 9 subjects to whom he applied the method, all but one gave unambiguously shorter average reaction times for meaning than for visual image. In the case of the one,

[1] T. V. Moore, 'The Temporal Relations of Meaning and Imagery,' Psychol. Rev., 1915, 22, 177–225.

[2] The work was performed at Prof. Külpe's laboratory in Germany.

the figures showed a tendency in the opposite direction. The average reaction times were slightly shorter for visual image than for meaning. The trustworthiness of these figures was, however, called into question by the fact that this subject was never cured of a habit of reacting to nonsense words as readily as to real words. This fact led Moore to reject the figures of this subject as inconclusive, and to draw all his conclusions from the results of the 8 subjects who agreed.

Introspections were asked for after each reaction and it was found that the introspection of these subjects bore out the testimony of their objective reaction times. They all agreed, that is, in reporting an awareness of simple meaning which appeared in every case prior to the image. Further introspection indicated that this awareness of meaning was a totally different kind of content from image.

From these facts, combined with similar ones obtained from experiments on the time relation of meaning and *verbal* imagery, in which pictures of objects were shown the subject, and he was sometimes instructed to react when he obtained the meaning of the picture, and sometimes when he obtained a verbal image of the name of the object, Dr. Moore concludes that meaning as a psychological content is sui generis and independent of imagery.

The present writer was led to question these conclusions because of the conviction that his own consciousness of meaning depended in no small part upon visual imagery. With that conviction in mind, he attempted by a method essentially similar to Dr. Moore's (to be described below) to put the matter to test. Great was his surprise, however, to discover that he himself, when the experimental conditions were thus controlled, substantiated the results of Dr. Moore's subjects, in that, on the average, he obtained meaning in less time than he did visual image. In the course of more or less haphazard experimenting, however, the writer, largely by accident, discovered a subject who *did* fulfill the prediction he had made for himself; a subject, namely, who obtained visual image, on the average, in as short a time as she obtained meaning, and who declared that introspectively the image was a part of, or essential to, the meaning.

This purely chance discovery suggested to the writer that, if a large enough number of subjects were to be examined, a small proportion might be found whose results would agree with those of the subject just mentioned rather than with those both of the writer himself and of Dr. Moore's 8 subjects. In pursuance of such a possibility, an investigation was undertaken of as many Northwestern University students as possible who were at the time taking either the introductory course in psychology or the laboratory training course.

The method employed was slightly different from that used by Dr. Moore. Instead of presenting a purely chance list of names, names of black or of white objects only were presented. The subject was given two keys, one for the right hand and one for the left, and instructed to react with the right hand if the object were black, and with the left hand if the object were white. The following typewritten instructions, which were read by the subject before the beginning of the experiment, will make the method clearer:

"You will either be shown the name of something which is black or of something which is white. If it is black, you are to press the right-hand key; if white, the left-hand one. Sometimes you will be told beforehand that you are to react (*i. e.*, press the appropriate key) the instant you *know* whether the object is black or white, irrespective of how you know it. Other times you will be told beforehand that you are to react the instant you *see from your visual image* whether the object is black or white. Introspection will be asked for from time to time during the course of the experiment."

Each word was typewritten on a slip of paper which could be fastened to a piece of black cardboard; the latter was cut so as to slip into place directly behind a pair of shutters. These were made to swing open towards the subject by means of a camera-bulb. When opened, they exposed a black field in the center of which appeared the slip of paper with the typewritten word.

A Bergström pendulum chronoscope was used.[1] This was

[1] Described as Model No. 2 in Psychol. Rev., 1910, 17, 1–18.

arranged so that the opening of the shutters closed a circuit which by means of a magnet released the pendulum. The pressing of either reaction key closed another circuit, which by means of another magnet stopped the pointer carried by the pendulum. The scale over which the pointer passed was calibrated to be read directly to thousandths of a second. The complete swing of the pendulum lasted 2 seconds only; any time longer than 2 seconds could not, therefore, be recorded.

The chronoscope was tested by means of a seconds pendulum before the beginning of the investigation, and the strength of the currents in the two magnet circuits were found such that a complete swing on the seconds pendulum registered on the chronoscope correctly to within 0.005 of a second. These strengths of current were noted and established throughout the course of the experiments by means of rheostats which were included in each of the two circuits.

Each subject was presented the same list of 24 words. The reaction times of the first 4 were rejected. The remaining 20, the times for which were counted, were the following: coffee, lime, steam, snow, mud, coal, swan, iron, diamond, crow, plaster, negro, cinders, raven, milk, print, teeth, jet, cement, lard.

It will be observed that 10 of them are names of white objects, and 10 names of black objects.

The programme was arranged so that for 5 of the 'white' words the subject was instructed to react to meaning, and for 5 instructed to react to image; the same held for the 'black' words. But different sets of 5 were used for meaning and image, respectively, in presenting this same list to successive subjects.

The testing of each subject took about 30 minutes, and 49 subjects in all were tested.[1] In Table I. we present the final results for all the subjects.

[1] The writer wishes to express his indebtedness to Mr. Leslie B. Bunch and to Mr. Wilbert C. Keiser, who helped as experimenters throughout this part of the investigation, and to the latter also for his assistance in the preliminary experiments and in setting up the apparatus.

If, for any reason, there was a slip on the part either of the experimenter or of the subject, so that no reaction was obtained for one of these words, this was usually rectified by making the subject react in the same way to another word of the proper color at the end of list.

Table I

Men			Women		
Subject	M.	V. I.	Subject	M.	V. I.
Mr. Bark	.823	1.385[1]	Miss C.	.742	.752
" Bu	1.199	1.784[1]	" E.	1.378	1.888[1]
" Ca	.641	1.014	" Go	.952	1.056
" Co	.617	.684	" Gr	.778	.944
" De	.868	1.039	" Gu	Could not visual	obtain images.
" Dy	.796	1.051	" H.	.834	.942
" E.	.764	1.027	" Ed. J.	.983	1.266
" M.	.794	.954	" El. J.	1.058	1.145
" N.	1.524[2]	2.000[3]	" S. J.	.892	1.266
" O.	.887	1.289	" Ka	1.154	1.318
" Pa	.843	.932	" Ll.	1.003	1.247
" Ril	.884	.918	" Mas	.756	.770
" Rit	.792	1.047	" Mi	.654	.841
" Si	.617	1.044	" Pa	.916	1.335
" St	1.233	1.457	" Pe	1.193	1.973[1]
" T.	.664	.932	" Po	.853	.891
" We	.811	1.439[1]	" Rea	.682	.890
			" S.	.870	.889
" Bart.*	.972	1.014	" A.*	.717	.719
" L.*	.816	.757	" B.*	.784	.743
" Mi.*	.893	.724	" Ki.*	.775	.734
" Milln.*	1.060	.802	" Kn.*	.561	.528
" Pe.*	.888	.761	" Li.*	.827	.821
" Wa.*	1.136	.975	" Mac.*	.736	.698
			" Rei.*	.820	.754
			" V.*	.862	.912

[1] Some of the individual reaction-times from which this mean was computed were over 2 secs. But, since, as before mentioned (see above p. 117), the maximum range of the chronoscope used was only 2 secs., we reckoned them at only 2 secs. each in computing the mean. The direction of the results, it will be noted, however, were not obscured by this method.

[2] In the case of Mr. N., two of the individual reaction-times for "meaning" exceeded 2 secs., but were reckoned at only 2 secs. in computing the mean.

[3] In the case of Mr. N., all of the individual reaction-times for "visual image" exceeded 2 secs., but were reckoned at 2 secs. in computing the mean.

The second column on each side gives the arithmetical mean time for the subject's reaction to meaning; and the third column the arithmetical mean time for his or her reaction to visual image.

It will be observed that by far the greater majority of the subjects (those whose names are not starred) showed a decidedly shorter reaction time for meaning than for visual image. And this holds in about equal proportions for men and women. They belong, in short, to the type represented both by Dr. Moore's 8 subjects and by the writer himself. Their introspections also bear out this conclusion. They all agree in reporting a meaning which appeared before visual image.

Turning now, however, to the subjects whose names are starred, we find a group for whom the results are quite different. All but two of them, Mr. Bart. and Miss V., gave mean reaction times which were actually longer for meaning than for visual image, and these two gave introspections of a character which, combined with the closeness of their reaction time, suggested that a longer and more careful examination of them might have resulted in their cases also, in a longer reaction time for meaning than for visual image.

Group I

Table II

Subject Miss B.

Meaning	T.	V.	Visual Image	T.	V.
Fork	1.030	.092	Square	1.490	.337
Turkey	.885	.053	Ring	.692	.461
Banjo	.800	.138	Lion	1.025	.128
Rose	1.345	.407	Candle	.598	.555
Fly	.872	.066	Steamer	.872	.281
Book	.814	.124	Circle	1.362	.209
Nest	.651	.287	Sofa	1.370	.217
Boot	.822	.116	Tree	.915	.238
Heart	.945	.007	Stocking	1.370	.217
Jug	.658	.280	Stairs	.906	.247
Oven	.782	.156	Crown	.990	.163
Knife	.752	.186	Tower	1.230	.077
Cat	.965	.027	Spoon	1.505	.352
Cradle	.690	.248	Cherry	1.350	.197
Rooster	1.525	.587	Brush	1.006	.147
Mouse	.772	.166	Drum	.960	.193
Snake	1.012	.074	Hammer	1.308	.155
Sled	1.356	.418	Pear	1.140	.013
Mask	1.125	.187	Peacock	.978	.175
Flag	.580	.358	Skull	1.708	.555
Letter	.772	.166	Flask	1.620	.467
Rabbit	1.268	.330	Chain	.868	.285
Anchor	1.300	.362	Tiger	1.240	.087
Apple	.782	.156			
Total	22.503	4.991	Total	26.503	5.756
Mean =	.938	.208	Mean =	1.152	.250
Median =	.847		Median =	1.140	

Table III

Miss L.

Meaning	T.	V.	Visual Image	T.	V.
Turkey	.560	.044	Square	.710	.136
Banjo	.532	.016	Ring	.518	.056
Rose	.511	.005	Lion	.655	.081
Candle	.391	.125	Steamer	.605	.031
Fly	.633	.117	Circle	.723	.149
Nest	.330	.186	Book	.520	.054
Boot	.482	.034	Sofa	.660	.086
Heart	.404	.112	Tree	.589	.015
Jug	.478	.038	Stocking	.560	.014
Oven	.505	.011	Stairs	.360	.214
Knife	.405	.111	Crown	.482	.092
Tower	.390	.126	Cat	.568	.006
Cradle	.590	.074	Spoon	.630	.056
Rooster	.582	.066	Cherry	.620	.046
Hat	.605	.089	Scissors	.515	.059
Mouse	.545	.029	Brush	.532	.042
Snake	.672	.156	Drum	.515	.059
Sled	.665	.149	Hammer	.705	.131
Peacock	.510	.006	Mask	.510	.064
Ladder	.520	.004	Flag	.555	.019
			Skull	.512	.062
Total	10.310	1.498	Total	12.044	1.472
Mean =	.516	.075	Mean =	.574	.070
Median =	.516		Median =	.560	

To make sure of this result, however, we subjected these 14 subjects to further tests, and, in doing so, we decided to copy Mr. Moore's method exactly, rather than to use the preceding 'discrimination between black and white' method.[1]

The words presented to the subjects were one and two syllable names of common objects. The subjects were sometimes instructed to react as soon as they obtained the meaning of the word, sometimes as soon as they obtained a visual image of the object which the word named. The instructions 'react to meaning,' and 'react to visual image' were in the case of any one subject distributed irregularly, but with approximately equal frequency. A majority of the same words

[1] The black and white method was adopted originally with the idea that it would set a more definite check, in the case of reaction to meaning, by making sure that the subject really understood and not merely recognized the word. The results, however, were so exactly similar to those obtained from Mr. Moore's method, that it was thought as well to adopt the latter.

were used over again for the successive subjects in such a way that the same word would with one subject call for the instruction 'react to meaning,' and with another that of 'react to visual image.' Introspections were asked for after about half the reactions only, owing to insufficient time, as each subject could spare but a little over an hour, and it was desired to obtain from each as long an objective series as possible. Nonsense words, introduced occasionally, served as checks to make sure that the subject was reacting fairly. The results of the first few reactions in the case of each subject were discarded. The writer served as sole experimenter throughout this series.

Tables II.–XV. inclusive present the results for the different subjects. In the first column are the words on which the subjects received the instruction 'react to meaning'; in the second column are the reaction times for these words; and in the third column are the deviations of these

TABLE IV

MR. MILLN.

Meaning	T.	V.	Visual Image	T.	V.
Turkey	.965	.052	Fork	.968	.022
Banjo	.748	.165	Square	1.172	.226
Lion	.856	.057	Ring	.942	.004
Candle	.865	.048	Rose	.905	.041
Circle	.930	.017	Fly	.818	.128
Book	.952	.039	Steamer	.995	.049
Sofa	1.040	.127	Nest	.992	.046
Tree	.701	.212	Boot	.872	.074
Stocking	.962	.049	Heart	.972	.026
Stairs	.950	.037	Jug	.965	.019
Crown	.917	.004	Oven	.905	.041
Knife	.924	.011	Cat	1.102	.156
Tower	.868	.045	Spoon	1.005	.059
Cradle	1.086	.173	Cherry	.818	.128
Snake	1.283	.370	Rooster	.840	.106
Hammer	.790	.123	Scissors	1.016	.070
Sled	.950	.037	Hat	.814	.132
Mask	.692	.221	Brush	.873	.073
Peacock	.940	.027	Mouse	.985	.039
Letter	.818	.075	Drum	.970	.024
Flask	.940	.027			
Total	19.177	1.916	Total	18.929	1.463
Mean =	.913	.091	Mean =	.946	.073
Median =	.930		Median =	.967	

Group 2

Table V

Mr. L.

Meaning	T.	V.	Visual Image	T.	V.
Banjo	.698	.021	Ring	.785	.030
Lion	.680	.039	Fly	.640	.115
Rose	.750	.031	Circle	.810	.055
Candle	.660	.059	Book	.722	.033
Steamer	.760	.041	Nest	.925	.170
Sofa	.927	.208	Boot	.718	.037
Tree	.768	.049	Heart	1.378	.623
Stocking	.773	.054	Ear	.775	.020
Jug	.720	.001	Stairs	.640	.115
Oven	.735	.016	Crown	.812	.057
Hen	.600	.119	Snail	.760	.005
Basket	.830	.111	Eye	.620	.135
Table	.713	.006	Bag	.653	.102
Star	.665	.054	Horse	.770	.015
Nose	.658	.061	Trunk	.620	.135
Spade	.748	.029	File	.598	.157
Shoe	.750	.031	Picture	.628	.127
Balloon	.615	.104	Scissors	.822	.067
Egg	.554	.165	Rooster	.672	.083
Chair	.782	.063			
Total	14.386	1.262	Total	14.348	2.081
Mean =	.719	.063	Mean =	.755	.110
Median =	.728		Median =	.722	

times from the mean. Similarly, in the fourth, fifth, and sixth columns are the words, times, and deviations for the instruction 'react to visual image.'

Examining these tables we discover, first, a group of 3 subjects, Group 1, Tables II., III., and IV., who appear to belong after all to the type represented by Dr. Moore's 8 subjects. For all 3 both the mean and the median times for reaction to meaning are shorter than for reaction to visual image. Next, we note 3 subjects, Group 2, Tables V., VI., and VII., whose figures begin to point in the opposite direction, in that, whereas their median times are shorter for meaning, their mean times are shorter for visual image. Finally, we note 8 subjects, Group 3, Tables VIII. to XV. inclusive, who substantiate our original hypothesis, in that for them both median and mean times are shorter for visual image than for meaning.

In order to be sure of these results, however, we must compare the introspections. In parenthesis, be it noted that our subjects were untrained,[1] and hence the value of their introspection is subject to qualification. The attempt was made, however, to make their introspective task as definite

Table VI

Miss Mac.

Meaning	T.	V.	Visual Image	T.	V.
Square	.600	.011	Fork	.580	.013
Ring	.589	.022	Turkey	.550	.043
Rose	.535	.076	Banjo	.781	.188
Candle	.530	.081	Lion	.640	.047
Fly	.479	.132	Steamer	.598	.005
Circle	.623	.012	Book	.489	.104
Nest	.970	.359	Sofa	.861	.268
Boot	.501	.110	Tree	.542	.051
Heart	.546	.065	Stairs	.542	.051
Stocking	.390	.221	Oven	.633	.040
Jug	.522	.089	Crown	.710	.117
Knife	.530	.081	Cat	.462	.131
Tower	.443	.168	Spoon	.515	.078
Cradle	.778	.167	Cherry	.443	.150
Scissors	.575	.036	Rooster	.590	.003
Brush	.760	.149	Hat	.442	.151
Mouse	.733	.122	Drum	.600	.007
Snake	.732	.121	Hammer	.478	.115
Sled	.740	.129	Pear	.660	.067
Mask	.518	.093	Peacock	.598	.005
Flag	.580	.031	Ladder	.655	.062
Skull	.491	.120	Letter	.561	.032
Rabbit	.713	.102	Flask	.648	.055
Anchor	.785	.174	Tiger	.500	.093
Apple	.635	.024	Hand	.608	.015
Hen	.656	.045	Basket	.633	.040
Snail	.605	.006	Table	.696	.103
Eye	.560	.051			
Total	17.119	2.797	Total	16.015	2.034
Mean =	.611	.100	Mean =	.593	.075
Median =	.585		Median =	.598	

and easy as possible. The method we chose was to ask them such specific questions as: "Did you get a visual image?" (in case of reaction to meaning). "Did *you* get a meaning?" (in case of reaction to visual image); and "Which came first?" Occasionally, in the case of a subject who seemed to

[1] The great majority of them were but just finishing their first course in psychology. See above, page 116.

show native aptitude for introspection, he was asked to describe, if he could, his consciousness of meaning; *i. e.*, whether it seemed to him reducible to images or something unique and not further analyzable. Descriptions of the visual images obtained were also asked for from time to time. Any other specific questions put to the subjects will be noted along with the answers obtained.

TABLE VII

MISS REI.

Meaning	T.	V.	Visual Image	T.	V.
Fork	.808	.061	Turkey	.910	.232
Square	.908	.161	Banjo	1.132	.454
Ring	.790	.043	Rose	.704	.026
Lion	.632	.115	Fly	.716	.038
Candle	.663	.084	Circle	.620	.058
Steamer	.680	.067	Nest	.762	.084
Book	.685	.062	Boot	.620	.058
Sofa	1.012	.265	Tree	.603	.075
Heart	.748	.001	Stairs	.538	.140
Stocking	.678	.069	Oven	.695	.017
Jug	1.002	.255	Crown	.872	.194
Cat	.905	.158	Knife	.755	.077
Cherry	.580	.167	Spoon	.772	.094
Rooster	.660	.087	Sled	.511	.167
Hat	.690	.057	Pear	.728	.050
Brush	.630	.117	Mask	.920	.242
Mouse	.745	.002	Peacock	.722	.044
Drum	.900	.153	Flag	.601	.077
Letter	.698	.049	Ladder	.700	.022
Flask	.682	.065			
Rabbit	.590	.157	Total	12.881	2.149
Total	15.686	2.195	Mean =	.678	.113
Mean =	.747	.105	Median =	.716	
Median =	.690				

Turning to the three subjects, Group 1, whose results agreed with those obtained by Dr. Moore, we find substantial agreement between their introspection and that of his subjects. When instructed to react to meaning, our three subjects reported, almost without exception, that a consciousness of meaning appeared first, and that a visual image did not come until after this meaning. An interesting thing is, however, that with these three subjects the visual image almost always did come finally. Miss B. reported only

3 cases, Miss L. one case and Mr. Milln. no cases in which a visual image did not follow the meaning. This shows what ready visualizers the three subjects were, and how near they were to crossing the border beyond which visual image comes before, or at least simultaneously with, meaning. When instructed to react to visual image, their introspections were

Group 3

Table VIII

Miss A.

Meaning	T.	V.	Visual Image	T.	V.
Spoon	.550	.020	Tower	.325	.087
Cradle	.538	.032	Cherry	.370	.042
Hat	.595	.025	Brush	.422	.010
Mouse	.390	.180	Snake	.681	.269
Drum	.558	.012	Pear	.900	.488
Hammer	1.000	.430	Mask	.392	.020
Sled	.484	.086	Peacock	.455	.043
Flag	.579	.009	Skull	.504	.092
Flask	.505	.065	Letter	.345	.067
Rabbit	.560	.010	Chain	.545	.133
Tiger	.572	.002	Apple	.408	.004
Hen	.582	.012	Eye	.366	.046
Hand	.589	.029	Table	.305	.107
Snail	.692	.122	Crab	.379	.033
Basket	.491	.079	Nose	.212	.200
Bag	.805	.235	Spade	.375	.037
Star	.670	.100	Pig	.245	.167
Horse	.417	.153	Balloon	.315	.097
Trunk	.498	.072	File	.278	.134
Shoe	.482	.088			
Egg	.560	.010	Total	7.822	2.076
Chair	.417	.153	Mean =	.412	.109
Total	12.534	1.924	Median =	.375	
Mean =	.570	.087			
Median =	.559				

practically the same as those just discussed. They reported a consciousness of meaning which came first, and a visual image which came afterwards.

Below we give sample introspections for both kinds of instructions.

INTROSPECTION—GROUP I

Instruction: Meaning.

Subject Miss B.[1]

Cat: "Thought of cat at home and then saw it."

Snake: "Thought of crawling thing, visual image afterwards."

Subject Miss L.

Banjo: "An idea of a banjo. After pressing key saw an image."

Rose: "No image until long afterwards."

Nest: "An idea of a nest. Then image of a nest in a tree."

Instruction: Visual image.

Subject Miss B.

Tower: "I thought of the meaning before I saw it."

Spoon: "I got meaning before image. I think I always do get meaning first."

Subject Miss L.

Steamer: "Saw the one I have so often seen on the lake. Had a general idea of a steamer first. I think the general idea always comes first."

Scissors: "A general idea, then saw all sorts of scissors, then selected one of those, a pair of embroidery scissors."

TABLE IX

MR. MI.

Meaning	T.	V.	Visual Image	T.	V.
Square	.662	.087	Fork	.460	.049
Banjo	.480	.095	Turkey	.370	.139
Lion	.593	.018	Ring	.543	.034
Candle	.593	.018	Rose	.299	.210
Steamer	.438	.137	Fly	.410	.099
Book	.570	.005	Circle	.650	.141
Nest	.430	.145	Sofa	.310	.199
Tree	.610	.035	Boot	.415	.094
Stocking	.680	.105	Heart	.632	.123
Stairs	.576	.001	Jug	.670	.161
Crown	.565	.010	Oven	.592	.083
Cat	.642	.067	Knife	.610	.101
Hat	.633	.058	Scissors	.562	.053
Mouse	.865	.290	Brush	.430	.079
Mask	.600	.025	Sled	.468	.041
Flag	.528	.047	Peacock	.682	.173
Letter	.538	.037	Skull	.521	.012
Rabbit	.555	.020	Flask	.475	.034
Tiger	.362	.213	Chain	.565	.056
Total	10.920	1.413	Total	9.664	1.881
Mean =	.575	.074	Mean =	.509	.099
Median =	.576		Median =	.521	

[1] This subject and Mr. Milln. did report a few cases in which they seemed to get the image first, but the majority of their introspections were like these quoted.

Table X

Mr. Pe.

Meaning	T.	V.	Meaning	T.	V.
Basin	.891	.078	Fork	.608	.130
Square	.715	.098	Banjo	.700	.038
Turkey	.814	.001	Ring	.562	.176
Lion	.910	.097	Candle	.948	.210
Rose	.910	.097	Fly	.620	.118
Steamer	.688	.125	Circle	.882	.144
Book	.700	.113	Nest	.742	.004
Sofa	.970	.157	Boot	.678	.060
Tree	.728	.085	Heart	.718	.020
Stocking	.825	.012	Jug	.840	.120
Stairs	.805	.008	Oven	.704	.034
Crown	.673	.140	Cat	.680	.058
Knife	.690	.123	Spoon	.769	.031
Tower	.915	.102	Cherry	.741	.003
Cradle	.790	.023	Scissors	.778	.040
Rooster	.925	.112	Drum	.773	.035
Hat	.988	.175	Snake	.658	.080
Brush	.840	.027	Hammer	.970	.232
Mouse	.848	.035	Pear	.829	.091
Sled	.708	.105	Peacock	.688	.050
Mask	.798	.015	Ladder	.621	.117
Flag	.836	.023	Letter	.706	.032
Skull	.750	.063	Rabbit	.752	.014
Flask	.904	.091			
Chain	.708	.105	Total	16.967	1.837
Total	20.329	2.010	Mean =	.738	.080
Mean =	.813	.080	Median =	.718	
Median =	.814				

Table XI

Mr. Bart.

Meaning	T.	V.	Visual Image	T.	V.
Basin	.800	.172	Fork	.603	.004
Square	.660	.032	Turkey	.882	.275
Banjo	.672	.044	Ring	.522	.085
Lion	.310	.318	Candle	.768	.161
Rose	.792	.164	Steamer	.625	.018
Fly	.516	.112	Book	.530	.077
Circle	.680	.052	Sofa	.573	.034
Nest	.608	.020	Tree	.445	.162
Boot	.648	.020	Ear	.508	.099
Heart	.610	.018	Jug	.613	.006
Stocking	.622	.006	Oven	.602	.005
Stairs	.623	.005	Crown	.615	.008
Total	7.541	.963	Total	7.286	.934
Mean =	.628	.080	Mean =	.607	.078
Median =	.636		Median =	.603	

TABLE XII

MISS KI.

Meaning	T.	V.	Visual Image	T.	V.
Basin	.510	.045	Finger	.450	.077
Turkey	.465	.090	Fork	.452	.075
Ring	.390	.165	Square	.519	.008
Lion	.750	.195	Banjo	.612	.085
Fly	.408	.147	Rose	.440	.087
Circle	.570	.015	Candle	.469	.058
Boot	.345	.210	Steamer	.695	.168
Tree	.594	.039	Book	.520	.007
Ear	.480	.075	Nest	.708	.181
Jug	.985	.430	Bear	.530	.003
Crown	.658	.103	Heart	.590	.063
Hen	.609	.054	Stocking	.496	.031
Hand	.730	.175	Stairs	.573	.046
Snail	.472	.083	Oven	.563	.036
Basket	.506	.049	Eye	.495	.032
Crab	.569	.014	Cow	.698	.171
Glove	.582	.027	Table	.482	.045
Star	.450	.105	Leaf	.482	.045
Horse	.482	.073	Bag	.250	.277
Trunk	.550	.005	Nose	.518	.099
Total	11.105	2.099	Total	10.542	1.504
Mean =	.535	.105	Mean =	.527	.075
Median =	.530		Median =	.519	

Subject Mr. Milln.[1]

Hammer: "Thought of hammer as something to drive nails, then saw a hammer, no specific hammer."

Mask: "Thought right away of mask as something to prevent detection, then a visual image of black mask that covered about half the face."

Mouse: "Thought of mouse as disagreeable, then saw a little mouse on the floor."

Hat: "Meaning that hat was to cover head, then saw a felt hat."

We may conclude that these 3 subjects, both from their objective results and their introspections, belong, generally speaking, to the type represented by Mr. Moore's 8 subjects.

Let us turn now to the three subjects, Group 2, whose results did not point definitely in either of the two directions. In the cases of two of them, Mr. L. and Miss Mac., this inconclusiveness of the objective results is supported by their introspections. In the case of the third, Miss Rei., the introspection does not bear out this inconclusiveness of

[1] This subject and Miss B. did report a few cases in which they seemed to get the image first, but the majority of their introspections were like those quoted.

TABLE XIII

MISS KN.

Meaning	T.	V.	Visual Image	T.	V.
Cradle	.690	.067	Cat	.732	.152
Rooster	.588	.035	Tower	.690	.110
Scissors	.698	.075	Spoon	.470	.110
Brush	.615	.008	Cherry	.490	.090
Mouse	.765	.142	Hat	.472	.108
Snake	.735	.112	Drum	.663	.083
Sled	.618	.005	Hammer	.748	.168
Pear	.860	.237	Flag	.653	.073
Mask	.625	.002	Ladder	.568	.012
Peacock	.578	.045	Skull	.581	.001
Flask	.590	.033	Letter	.663	.083
Chain	.522	.101	Rabbit	.650	.070
Tiger	.530	.093	Anchor	.500	.080
Finger	.780	.157	Apple	.480	.100
Basin	.963	.340	Key	.350	.230
Easel	.751	.128	Fork	.708	.128
Banjo	.503	.120	Square	.780	.200
Rose	.420	.203	Turkey	.552	.028
Candle	.740	.117	Ring	.520	.060
Steamer	.693	.070	Lion	.528	.052
Book	.500	.123	Fly	.478	.102
Nest	.640	.017	Circle	.670	.090
Bear	.616	.007	Sofa	.670	.090
Heart	.580	.043	Boot	.571	.009
Stocking	.632	.009	Tree	.482	.098
Stairs	.449	.174	Ear	.662	.082
Oven	.590	.033	Jug	.582	.002
Eye	.500	.123	Crown	.525	.055
Table	.480	.143	Hen	.645	.065
Crab	.575	.048	Hand	.498	.082
Glove	.482	.141	Snail	.510	.070
			Basket	.460	.120
Total	19.308	2.951	Total	18.551	2.803
Mean =	.623	.095	Mean =	.580	.088
Median =	.615		Median =	.570	

objective results, but suggests rather that the subject belongs with the last group, *i. e.*, those for whom visual image definitely comes before instead of after meaning. We will reserve the introspection of this third subject to consider in that connection, and will examine now the introspections of Mr. L. and Miss Mac. only.

Mr. L.'s introspection is unique in that for him the question seemed to be one not so much of meaning *vs.* visual image as of some sorts of images *vs.* other sorts of images. When instructed to react to meaning, he never obtained

Table XIV

Miss V.

Meaning	T.	V.	Visual Image	T.	V.
Finger	.750	.129	Key	.935	.107
Basin	1.142	.263	Fork	.702	.126
Square	.760	.119	Turkey	.813	.015
Ring	.846	.033	Banjo	.861	.033
Lion	.790	.089	Candle	1.045	.217
Rose	.913	.034	Circle	.502	.326
Fly	.960	.081	Nest	.712	.116
Steamer	.930	.051	Sofa	.652	.176
Heart	.870	.009	Boot	.732	.096
Scissors	.540	.339	Jug	1.045	.217
Book	.692	.187	Hand	.690	.138
Tower	.752	.127	Bag	1.045	.217
Crown	1.290	.411	Star	.852	.024
Horse	.938	.059	Nose	1.000	.172
Trunk	1.005	.126			
Total	13.178	2.057	Total	11.586	1.980
Mean =	.879	.137	Mean =	.828	
Median =	.870		Median =	.833	

Table XV

Mr. Wa.

Meaning	T.	V.	Visual Image	T.	V.
Key	1.240	.247	Finger	.840	.061
Fork	1.200	.207	Basin	.860	.081
Square	.780	.213	Turkey	.584	.195
Ring	1.008	.015	Banjo	.712	.067
Lion	1.148	.155	Steamer	.930	.151
Rose	.890	.103	Circle	.803	.024
Candle	.985	.008	Book	.652	.127
Fly	1.065	.072	Nest	.745	.034
Boot	1.108	.115	Sofa	.808	.029
Tree	.918	.075	Heart	.800	.021
Bear	.899	.094	Stocking	.715	.064
Jug	.692	.301	Ear	.890	.111
Oven	.975	.018	Stairs	.788	.009
Total	12.908	1.623	Total	10.127	.974
Mean =	.993	.125	Mean =	.779	.075
Median =	.985		Median =	.800	

anything imageless, but merely images of one kind or another; sometimes visual, but often cutaneous or kinesthetic. When instructed to react to visual images, he obtained visual image. The following are typical introspections:

INTROSPECTION—GROUP 2

Subject Mr. L.

Instruction: Meaning.

Steamer: "Visual image of white steamer out on lake. Feeling of tension, *i. e.*, a kind of mental muddle before image."

Stocking: "Kinæsthetic image, followed by visual image. They came almost simultaneously. Meaning not really made clear until the visual image."

Spade: "Combination of visual kinæsthetic image. Visual came first."

Instruction: Visual image.

Heart: "A feeling of tension or strain, then visual image. No consciousness of meaning before the image."

Horse: "Saw a dappled gray horse."

Turkey: "A feeling of tension then a visual image."

Occasionally for 'meaning' he did report the presence in consciousness of the 'idea' of the object named. When asked, however, to analyze this 'idea,' he invariably reduced it to images; kinæsthetic, cutaneous, or even visual, but in that case an image of something other than the immediate object itself. The following are typical:

Instruction: Meaning.

Nose: "Idea of sniffing (seemed kinæsthetic and cutaneous)."

Star: "Idea of star as a source of light. (This idea a visual image of light.)" "Visual image of a star after reacting."

We conclude that as far as his introspection goes meaning for this subject was always image, of one sort or another. If this be true, it suggests that the reason his objective time for meaning averaged about the same as that for visual image might be due to the fact that the various kinds of images which he got for 'meaning' took on an average about the same time to develop as the purely visual images which he got when reacting to 'visual image.' In addition, however, to suggesting an explanation of his objective reaction times, this introspective testimony to the effect that meaning was always image of one kind or another, would carry, of course, a direct answer to our original problem. It would settle it against the adherents of imageless thought. We cannot, however, be certain of this decision until we are sure of the absolute reliability of this subject's[1] introspection. And for

[1] This particular subject happened to be one of the better-trained (as well as one of the most intelligent) students, since at the date when the test was performed he had just completed a year's course in laboratory psychology, in addition to having had two previous courses in psychology.

that we lack proof. We will not, therefore, attempt to make any final statement, but simply draw attention to this subject's introspection and to the way in which it points.

Turning now to the introspection of Miss Mac., the second subject for whom the objective time appeared approximately the same for both meaning and visual image, we find a somewhat different state of affairs. This subject did not succeed in analyzing her consciousness of meaning. The presumption was, therefore, that it was unanalyzable because imageless. The approximate equality of her times for meaning and visual image, however, is explainable by the fact that, introspectively, this awareness of meaning appeared sometimes before the visual image and sometimes afterwards. Out of 16 times in which she was asked to introspect upon reaction to meaning, she reported 6 times in which meaning came before the visual image, and 10 times in which image came before meaning. When instructed to react to visual image, she reported 8 times in which meaning came before the visual image and 8 times in which the visual image came before the meaning. Below are typical introspections:

INTROSPECTION—GROUP 2

Miss Mac.

Instruction: Meaning.	*Instruction:* Visual image.
Snake: "Image first, then knew what it was, knowing a sort of memory of snakes that I have seen."	*Drum:* "Knew what it was, then saw drum being carried in a parade."
Cradle: "Knew what it was when I saw it."	*Peacock:* "Saw it, then meaning."
Hen: "Visual image first; then recognized the image. I remembered that I had seen things before that looked like that."	*Ladder:* "Knew what it was, then image."

This subject's introspection, as far as it goes, substantiates her objective results and we see why, in her case, reaction to meaning and reaction to visual image required about equal average times.

Combining her results with those of Mr. L., we note that these two subjects taken together constitute a transition group in that for them both reaction to meaning seems sometimes though not always to depend upon visual image. In Mr. L.'s case meaning sometimes actually *was* visual image; in Miss Mac.'s case it sometimes followed the visual image and was dependent upon the latter.

We may turn now to the introspection of the third and last group of subjects, the group for whom reactions to meaning, as we shall see, did always depend upon a visual image. Miss Rei., it will be remembered, belonged introspectively to this group. In considering the introspection of these 9 subjects (including Miss Rei.) we find it possible to divide them, roughly speaking, into two subgroups: first, a subgroup, *A*, composed of subjects who, as a rule, tend to make a distinction between meaning and visual image; and second, a subgroup, *B*, made up of subjects who, as a rule, do not tend to make such distinction, but for whom meaning *is* visual image.

Subgroup *A* comprises Miss A., Mr. Mi., Mr. Pe., and Miss Rei.

Subgroup *B* comprises Mr. Bart., Miss Ki., Miss Kn., Miss V., and Mr. Wa.

The following are typical introspections for subgroup *A*.

GROUP 3—SUBGROUP *A*

Instruction: Meaning.	*Instruction:* Visual image.
Subject Miss A.	
Rabbit: "Saw a rabbit hopping. Realized it was a small animal, then reacted."	*Letter:* "I saw a letter lying open. No particular letter."
Drum: "First saw a picture of a man beating drum in orchestra, and then thought, 'it is a musical instrument.'"	*File:* "Saw both a letter file, and a file, the tool."
Subject Mr. Mi.[1]	
Tree: "Saw a bunch of trees, then thought what a tree really is."	*Scissors:* "Image first; didn't think what it meant till after I pressed the key."
Stairs: "Saw stairs, then got a meaning, *i. e.*, stairs something that you go up on."	*Skull:* "Image and that was all."

[1] This subject reported some instances in which he thought he obtained a meaning without a preceding image. His introspection tends, therefore, to class him to some extent in group 2 with Miss Mac rather than here in group 3.

Subject Mr. Pe.

Cradle: "Saw a cradle first, and then the idea to rock came on."

Rooster: "Saw a big black rooster we used to own, and then thought of him as something to eat."

Scissors: "Saw scissors lying on a sewing-table, that belonged to my mother."

Snake: "Visual image of a little green garter snake running through the grass."

Subject Miss Rei.

Cherry: "Saw a red cherry, then thought of it as something to eat."

Drum: "Saw a drum and then thought of it as being a musical instrument." (In answer to question) "I think the image is an aid to meaning."

Mask: "Saw a black mask, nothing besides the image."

Ladder: "Saw a tall painters' ladder, no consciousness of meaning."

Peacock: "Saw a peacock going up a hill. It was at Lincoln Park."

Examining these introspections, we note that in the case of Subgroup *A* visual image was always a precursor of meaning, and that the meaning itself seemed to depend upon the image. Turning now to Subgroup *B*, the following are typical introspections:

Group 3—Subgroup *B*

Instruction: Meaning.

Instruction: Visual image.

Subject Mr. Bart.

Lion: "A mixed image of the zoo, *i. e.*, the line of cages in Lincoln Park. No other process in consciousness detectable."

Fly: "Visual image of fly paper with flies stuck on it."

Finger: "Image of a finger with ring upon it (no particular finger)."

Ring: "Image of a key ring (no particular key ring)."

Subject Miss Ki.

Fly: "First saw a black fly, then something flying."

Basket: "Saw image of basket at same time that I got meaning. Meaning to me *is* image."

Crab: "I saw a single crab and the beach behind him filled in."

Nest: "Image of a nest which I saw this morning and then of other nests of all different kinds."

Bear: "Image of a bear in a cage."

Book: "Image of a red book seen at an angle."

Subject Miss Kn.

Nest: "I said the word to myself. I realized it wasn't a nonsense syllable; then I obtained a visual image of a nest on a tree (not any particular nest); then I reacted."

Hen: "Image of a black and white hen."

Snail: "A greenish brown snail shell. Nothing previous to this image.

Oven: "Got an image first thing. As if looking into an open oven."

Stocking: "An image of a new black stocking the first thing in consciousness. Nothing between image and reacting."

Basket: "A market basket; nothing before the image."

Subject Miss V.

Eye: "Made up my mind beforehand that this time I would get a meaning before image, but got an image of an 'eye' almost before I saw the word."

Snail: (N. B. reaction to this word took over two seconds and was discarded but the introspection is significant.) "Could not get an image for a long time. Had no meaning until I got the image. Felt just as if I were looking at a nonsense syllable."

Candle: "First I saw a candle, no particular candle. Then I saw a particular candle, viz., the one I saw last."

Nest: "Image of the nest which I saw last Sunday."

Bag: "Image of travelling bag, also of paper bag."

Subject Mr. Wa.

Ring: "Visual image of ring on my finger. Nothing previous to this image. Afterwards an image of a circle on the ground."

Heart: "Visual image of a human heart."

(In answer to specific question) "I feel that I go through the same process each time no matter whether meaning or visual image is asked for."

In the case of Subgroup *B* the testimony seems to be unanimous that the visual image *was* the meaning. No other process which might play the part of meaning was ever detected either before or after the image.

The common point in the introspection for both subgroups, it will be noted, is that visual image was the first thing which came. The two subgroups disagree, however, as to the extent to which this visual image was important for, or a part of, the meaning. Subgroup *B* declared that it *was* the whole of the meaning. Subgroup *A* reported merely that it always came before the meaning but that meaning itself was something more. This disagreement, together with the fact that for the vast majority of subjects visual image is entirely unneeded for meaning, sets us a problem.

Three solutions suggest themselves. We may try either an out-and-out imageless position, an out-and-out image position, or some sort of a compromise. The out-and-out imageless position would have to contend that no matter what were the objective reaction times to the contrary, nor what the apparent introspective evidence, the visual image can never have been really, in any true sense, a part of or even necessary for the meaning. Such a position would have to claim that the objectively shorter reaction times for visual image in the case of both subgroups did not prove that the visual image was necessary to the meaning, but that it was merely an adventitious circumstance. It would have to claim also that the introspective testimony from both subgroups as to the subjective precedence in time of the visual image was no additional proof of the prerequisiteness of the image. The out-and-out imageless position would, in short, have to deny every one of the evidences afforded by the results of our third group of students. In support, it would have only the results of the non-visual[1] subjects, *i. e.*, of Mr. Moore's 8 subjects and those of our investigation who were like his. But since the method we have used is one which traces the importance of the visual image only, the support of the not extremely visual subjects carries little or no weight. We feel justified in concluding, therefore, that our results render the out-and-out imageless position untenable.

We turn now to the out-and-out 'image' position. Results which directly support it are the introspections of Subgroup *B*, that meaning is image, and image is meaning. For it to be completely supported, however, two further demands would have to be satisfied. The objective reaction times for meaning and visual image in the case of Subgroup *B* would have to be the same. This demand was not fulfilled; the reaction times for meaning averaged longer than for image, which implies that meaning for Subgroup *B* as for Subgroup *A* involved something more than mere image. Second, all cases of 'meaning' reported ought to be analyzable by better introspection into images. This would demand that the 'meaning' of Mr. Moore's 8 subjects and of the

[1] Using 'non-visual' in a merely relative sense.

majority of our own original subjects, *i. e.*, the 'meaning' which came before visual image, must really have been imaginal. It must have been made up, that is, of verbal, organic, kinæsthetic, or other images, which these subjects failed to recognize. It would demand, similarly, that Miss Mac.'s 'meaning,' which came sometimes before and sometimes after the visual image, must have been made up of images. And, finally, it would demand that the 'meaning' of Subgroup *A*, which came after the visual image, and which was often similar to an awareness of definition must likewise have been made up of images. None of these demands would be insurmountable, if we were strongly prejudiced in favor of an out-and-out 'image' position. It would be possible to assume, as has been done before, that better introspection would eventually show images in processes where as yet nothing but imageless awarenesses have been discovered. Such an assumption, however, has at the present stage of psychology little but theoretical preconceptions to support it. So that at present we consider it safer to conclude that our results, while they do not completely contradict an out-and-out 'image' doctrine, do nothing actively to support one.

Finally, we may turn to the consideration of an intermediate doctrine which would both allow an essential importance to the image, and yet admit an imageless component as also necessary. Such a doctrine is directly suggested by the results of Subgroup *A*. They, it will be remembered, obtained first of all the visual image and then a 'meaning.' It appeared that for them the visual image was a prerequisite of the 'meaning,' but that the 'meaning' itself was something different from image. It does not seem improbable that a similar situation may have existed also in the cases of the other groups. With the subjects of the Group I who obtained 'meaning' first, this 'meaning' may have come after kinæsthetic or organic images which were not identified. And in the cases of Subgroup *B* the doctrine would explain the longer reaction times obtained for meaning than for image. We would simply have to assume that the 'meaning' which followed the visual image was not recognized by this subgroup, as it was by Subgroup *A*, as something distinct from the image, but was confused with the visual image itself. Such an assumption does not seem at all a difficult one. Further experiments with subjects of the type of Subgroup *B*, *i. e.*, those who introspectively declare meaning to be identical with visual image but who objectively require a longer reaction time for meaning than for image, ought to throw light upon the matter. We hope to be able in the near future to report the results of such experiments.

We may sum up. The results of the 7[1] subjects for whom both objectively and subjectively visual images came first, render an out-and-out imageless position untenable. But the fact, on the other hand, that for 2 of these 7 (Miss A. and Mr. Pe., Subgroup *A*) meaning was distinct from the visual image, as well as that for the great majority of all subjects 'meaning' appears as something not analyzed into images, gives no direct support to an out-and-out 'image' position. A compromise position, therefore, which assumes that 'meaning' depends upon image but is itself distinct from the latter, is the one most nearly suggested by our results.

In conclusion, let us emphasize that the value of the present investigation has lain not so much in the direction of a positive proof of one or the other theory, as in showing that, *if a large enough sample of subjects be taken*, Dr. Moore's method in no way lends support to the out-and-out imageless position.

[1] This excludes Miss Rei. and Mr. Mi. The former's objective times and the latter's introspection left it doubtful whether they really belonged in this third group or in Group 2. See pp. 128–9, and footnote, p. 133.

EXPERIMENTS ON THE RELATIVE EFFICIENCY OF MEN AND WOMEN IN MEMORY AND REASONING[1]

BY ARTHUR I. GATES

The majority of psychologists and educators who have expressed themselves on the subject are of the opinion that women, as a rule, are considerably more efficient than men in memory work and less efficient in applying the facts learned, in self-expression, and in reasoning power. For example one writer says:[2] "Girls excel in learning and memorization accepting studies on suggestion or authority, but are often quite at sea when set to make tasks or experiments that give individuality and a chance for self-expression, which is one of the best things in boyhood."

Opinions similar to these seem to prevail generally among psychologists, educators, and laymen. Many, moreover, are of the opinion that women, in addition to having quicker and more tenacious memories, are as a rule more diligent and painstaking in their work; the boy may often be satisfied with a fair knowledge of the general principles underlying a lesson, while the girl seeks a more detailed and exact knowledge. If such is the fact it should be taken into account in any attempt to determine the sex-differences in memory, for obviously the differences in the time spent on the work might easily account for the differences in the reproduction of the ideas.

The experiments to be described presently were performed first in 1913 and were repeated in 1914 and 1915, using as subjects a class in elementary psychology consisting of from 158 to 275 students of both sexes of the sophomore, junior, and senior years in the University of California.

[1] From the Psychological Laboratory of the University of California.

[2] Hall, G. S., "Youth: its Education, Regimen, and Hygiene." New York, 1912, p. 287.

The data were obtained from the answers to three sets of questions. Each set of two questions comprised the regular weekly examination of the class. The first set called for a somewhat detailed reproduction of facts presented in the lectures of the week preceding. The second set called for the application of facts or principles given in the lectures, the purpose being to call into action a mental process as closely as possible identical with that involved in reasoning. All the questions were framed by Professor Stratton, who was in charge of the classes, and who endeavored to make the tests as nearly as possible adequate to the purpose of the experiment. The papers in all cases were graded on a basis of ten, but the averages below, for the sake of clearness, are made on the basis of one hundred. All papers were corrected by the regular 'readers,' who were in no case aware that the results were to be used for experimental purposes. It happened, moreover, that each of the nine sets of papers was graded by a different 'reader.'

The following table shows the results in the case of memory questions.

TABLE I

MEMORY

	1913		1914		1915	
	No. of Individuals	Grade	No. of Individuals	Grade	No. of Individuals	Grade
Women	95	77	162	89.2	154	86.4
Men	59	73	102	85.0	98	81.0
Diff. in favor of women	....	4.0%	....	4.2%	....	5.4%

The women show a slight superiority in memory work, amounting on the average to 4.5 percent. While the percentile difference is rather small, its reliability is indicated by the fact that it appears in all cases, although different questions were given at different times to three entirely different groups of individuals.

Table II. shows the average grades obtained by men and women to questions that involved reasoning.

The evidence indicates a slight superiority of the men in this sort of work. The average difference is approximately

Table II

Reason

	1913		1914		1915	
	No. of Individuals	Grade	No. of Individuals	Grade	No. of Individuals	Grade
Women	90	77.5	153	83.3	154	88.4
Men	58	79.5	103	86.4	99	89.2
Diff. in favor of men	....	2.0%	....	3.1%	....	0.8%

2 percent, a difference which is so small as to have but little significance were it not for the fact that it is repeated by the three separate groups.

Table III. shows the results of tests in which the subjects were given free choice between a memory and a reason question. The two questions, constituting the regular weekly examination as before, were presented and the students were permitted to take their choice.

Table III

One Memory and One Reason Question

1913

	Memory Question		Grade	Reason Question		Grade
	No. of Individuals	Percent of Individuals of That Sex		No. of Individuals	Percent of Individuals of That Sex	
Women	60	72.3	85	23	27.7	86
Men	19	28.8	82	47	71.2	87
Diff.	....	43.5	3	....	43.5	1
1914						
Women	129	84.8	70.4	23	15.2	77
Men	80	78.4	64.5	22	21.6	80
Diff.	....	6.4	5.9	....	6.4	3
1915						
Women	144	91.8	88.4	13	8.2	87.2
Men	85	74.2	87.0	16	15.8	89.8
Diff.	....	17.6	1.4	....	7.6	2.6

Although both sexes show a distinct preference for the memory question, the preference is much greater in the case of women. The men show more willingness than do the women to take the reason questions, although the actual number of either sex that take these questions is small. In

1913 and 1915 twice as great a ratio of men, and in 1914 a ratio one third greater of men than of women chose the reason question. The grades received in the memory tests confirm the earlier finding that the women excel in this kind of work. The women excel in every case, although in two (1913 and 1915) the differences in their favor are very small. The grades received on the reason questions also confirm the earlier finding that the men excel slightly in this type of work. Although the superiority of the men is small it appears in every case.

Our general conclusions from the experiment thus far are as follows:

1. The women excel the men in memory work.

2. The men excel the women, but to a less degree, in reason work.

3. Both sexes prefer memory work but more men show a willingness to do reason work in lieu of memory work.

To let the experiment remain as it stands and to accept without further question the conclusions just enumerated would be hazardous and would certainly not take into account all of the factors which have an influence here. There is at least one possibility which if proven to be a reality would force us to modify the conclusions at which we have just arrived. It is possible that the apparent superiority of the women in reproduction from memory is due merely to a greater amount of study and not to an innate superiority of memory.

To take into account this possibility the following test was employed. The news item given below was read to the class at the beginning of the lecture hour, the students being warned to pay particular attention to the contents, without being informed, however, of the purpose of the test. The item follows:

THREE HOUSES BURNED[1]

Boston, September 5. A serious fire last night destroyed three houses in the center of the city. Seventeen families are without a home. The loss exceeds fifty thousand dollars. In rescuing a child, one of the firemen was badly burned about the hands and arms.

[1] See Whipple, G. M., 'Manual of Physical and Mental Tests,' Baltimore, 1910, p. 504.

The students were first requested to write down all the facts they could recall from the article. Following the free account, they were asked to answer the following questions:

1. In what city did the fire occur? Boston
2. What was the date of the item? Sep 15
3. When did the fire break out? Night
4. How many houses were destroyed?
5. In what part of the city were these houses? center
6. How many families were left homeless? 17
7. What was the total loss (in dollars)? 50,000
8. Who was burned?
9. On what part or parts of the body was this individual burned?
10. What was this individual doing when the burns were received?

The data used were obtained from the answers to the ten definite questions, for it was found that the additions or alterations of these answers from the free accounts were so slight as to be negligible. The papers were graded on a basis of ten, one unit being allowed for the correct answer to each question. Table IV. gives the average results.

TABLE IV

	Percentage Reported	Percentage Correct	Percentage Positive Errors	Ratio of Pos. Errors to Amt. Reported	Ratio of Pos. Errors to Pos. Errors Plus Amt. Not Reported
1913					
Women	97.4	84.4	13.0	.133	.833
Men	90.0	80.0	10.0	.100	.500
1914					
Women	98.4	86.4	12.0	.121	.888
Men	94.2	83.1	11.0	.116	.650
1915					
Women	94.1	82.5	11.6	.123	.662
Men	88.0	76.4	10.6	.120	.443
Average of above.					
Women	96.6	84.4	12.2	.125	.782
Men	90.7	79.8	10.5	.112	.519
Diff.	5.9	4.6	1.7	.013	.263

The women in every case report a greater amount of the content of the item, as well as a greater amount of it correctly. On the average the women report 96.6 percent of the item and 84.4 percent of it correctly while the men report but 90.7 percent with 79.8 percent correct. The men, however, make fewer mistakes. The actual number of errors made by the women is greater in every case, although the differences between the sexes is small. The ratios of the number of errors to the total amount reported show even smaller differences because of the fact that the women in all cases report a larger amount. But the ratios of the amount of positive errors to the total amount of positive errors plus the amount not reported—*i. e.*, to the field in which suggestion and kindred forces could operate because the ideas were not correctly remembered—were much larger for the women. That is to say, the women, much more than the men, were likely to make erroneous statements rather than mere omissions. This ratio is, on the average, about one third larger for the women.

The general conclusion from this test is that the women in immediate memory tests can correctly reproduce more of the detail of a given group of facts but at the same time make more mistakes.

A question, however, may be raised with regard to the application of the results gained by this method to the determination of the relative ability shown by men and women in the tests first considered, because the present method tests immediate memory, or immediate reproduction, rather than delayed memory which is the function operative in the examinations.

Accordingly, the same students were requested, one week or five weeks after the immediate-memory test, to write, without previous warning, all that they could remember of the news item given above. The same set of ten questions as before was used. Table V. gives the results.

In delayed as well as in immediate memory the women have a greater range of report, a greater number of details are reported correctly, and more positive errors are made. The amount by which the sexes differ is about the same in both types of memory.

The experiments with the news item justify the following conclusions:

TABLE V

DELAYED MEMORY

1913. After 5 Weeks

	Percentage Reported	Percentage Correct	Percentage Positive Errors	Ratio of Positive Errors to Amount Reported	Ratio of Positive Errors to Positive Errors Plus Amount Not Reported
Women......	82.0	64.0	18.0	.219	.500
Men........	72.0	57.0	15.0	.208	.349
1914. After 5 Weeks					
Women......	89.0	68.0	21.0	.236	.655
Men........	79.0	60.0	19.0	.240	.475
1915. After 1 Week					
Women......	94.4	80.7	13.7	.145	.650
Men........	89.8	75.8	12.0	.137	.495
Average of Above					
Women......	88.5	70.9	17.9	.200	.602
Men........	80.3	64.3	15.3	.195	.440
Diff.........	8.2	6.6	2.6	.005	.162

1. The women excel the men in tests of immediate or delayed memory, at least in so far as the amount of material correctly reproduced is concerned.

2. The women, however, make more positive errors in reporting.

The results obtained by other investigators are for the most part in harmony with the present findings. A summary of such experiments will be found in Whipple[1] who concludes: "Sex differences in this test [memory for ideas], as in the rote memory test, are in favor of girls."[2]

A final consideration is the possibility that the women employed in these experiments constitute a more select group than the men. It is possible that these women are on the whole more capable, or that their previous training has better adapted them to the particular subject of psychology. There is no obvious reason why this should be the case, but in order to throw some light upon it the average grade in the course has been computed for each sex.

[1] Whipple, G. M., 'Manual of Mental and Physical Tests,' Part II., 17–43, 149–223.

[2] *Op. cit.*, p. 213.

TABLE VI

	1913	1914	1915
The women received an average grade of..........	77.0	75.5	74.0
The men received an average grade of.............	75.5	75.0	72.0

The women have slightly the higher grade. The mass of experimental evidence from other investigations, however, indicates that in groups of men and women of equal endowment and training, the women usually excel in memory work. We have found that in the three groups just considered, the women excel in memory. It seems that the small amount by which the women excel the men in the grades received in the course may be accounted for by the great predominance of memory work in the weekly and final examinations on which the grades are based. The women who apparently excel in memory work should in a long series of tests of that nature, come out with a somewhat better average.

The three main conclusions that the investigation seems to justify are as follows:

1. The women excel the men noticeably in either immediate or delayed memory work.

2. The men excel the women, but to a less degree, in reason work.

3. Both sexes prefer memory work, but a greater relative number of men show a willingness to do reason work in lieu of memory work.

Results obtained by other investigators and the supplementary tests for possible sources of error have brought forth no evidence contradictory to the conclusions we have reached.

INDIVIDUAL DIFFERENCES IN JUDGMENTS OF THE BEAUTY OF SIMPLE FORMS

BY EDWARD L. THORNDIKE

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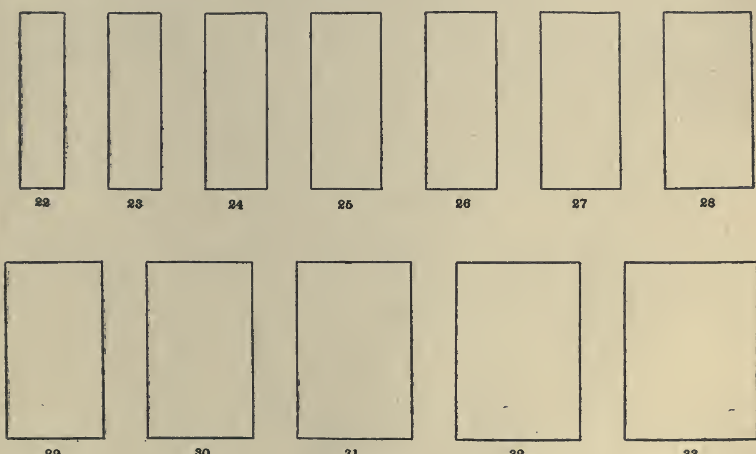
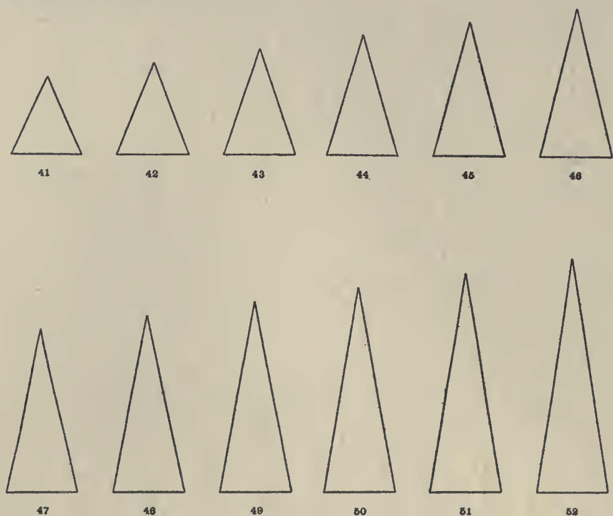
Students of esthetic appreciation have commonly been especially interested in the general drift or average tendency toward this or that preference and have perhaps given an impression of greater uniformity than exists. The diversity of the judgments whose average favors the golden section, for example, is really very great. It seems worth while therefore to report certain rather extensive measurements of esthetic preference which I have made.

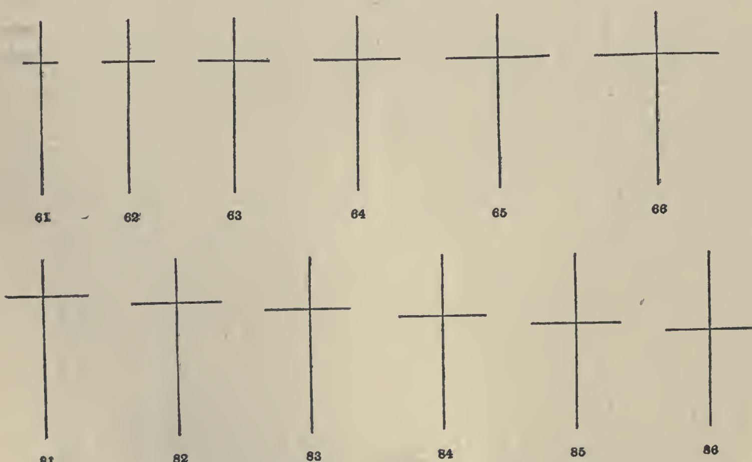


(A)

The subjects of the experiment were college juniors and, with few exceptions, of the female sex. The judgments made were of the order of esthetic merit (the question being, "Which rectangle do you like the looks of most? Next most? etc.") of (*A*) rectangles 22–33, (*B*) triangles 41–52, (*C*) Crosses 61–66 and 81–86, (*D*) designs *A–L* and (*E*) the 24 unnumbered designs. Each set was shown as here save that the dimensions were in each case double those here (quadrupling the areas).



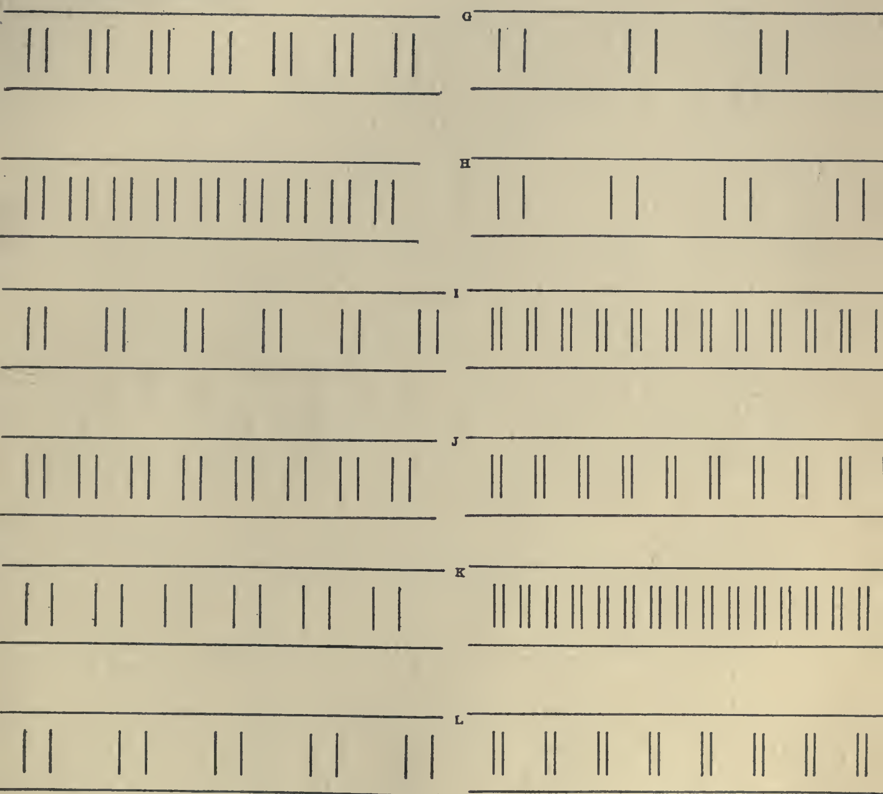
(B)



(C)

I give the facts for from *100* to *250* individuals who made the judgments, in the form of the percent of them assigning a given form to a given position.

In the case of the rectangles it will be observed that 27, 28, and 29, those most liked, still have some ratings in the lowest position of all; and that 33, the one least liked, still has ratings in the highest position. In only 3 cases out of 144 do over 25 percent of the ratings give a rectangle the same position.

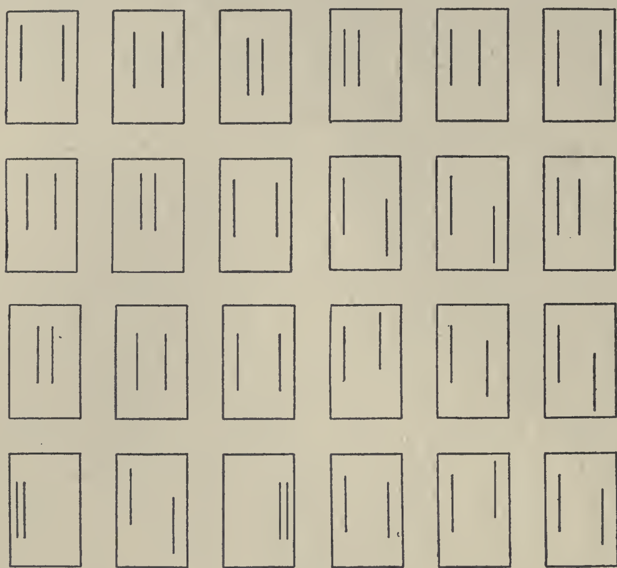


(D) (D)

In the case of the triangles there is a pronounced drift of opinion against the tall triangles, but even so almost every position has votes in the case of each. This is still more the case with the crosses.

In the case of the designs where the sequence by proportions is more hidden, the variability becomes enormous.

Although any one person may feel very decided preferences, these are never shared by enough of his fellows to make



(E)

TABLE I

FREQUENCIES OF EACH POSITION FOR EACH RECTANGLE COMPUTED FROM ORDERS OF MERIT REPORTED BY 200 INDIVIDUALS: IN PERCENTS

Positions	Rectangles											
	22	23	24	25	26	27	28	29	30	31	32	33
1	4.5	5	3.5	8.5	6.5	15	10.5	15	16	6.5	4.5	4.5
2	1.5	7	6	7.5	14	7	16	17	11	8	3.5	1.5
3	4	2.5	8	11	9.5	15	15	11	8.5	7	5.5	3
4	2.5	3.5	11	12	10	16.5	13.5	7.5	11	7	4.5	2
5	2	4	5.5	12	18	15.5	8.5	12	8	7	4	3
6	2	4.5	9	14.5	22	9	12.5	10	7.5	5.5	3	1.5
7	5.5	7.5	12	15	4	14.5	9.5	6.5	9	11	4.5	1.5
8	6	9	15.5	6	12.5	2.5	5.5	11	8	11.5	10	1.5
9	9.5	15	12	10	1.5	2	2	5	15	11.5	11	5.5
10	11.5	18.5	14	1.5	1.5	2.5	1	1.5	4.5	22	14	7
11	15	21	3	2	.....	.....	3	2	1.5	3	35	14.5
12	36	2.5	0.5	.....	0.5	0.5	3	1.5	.....	.....	0.5	54.5

anything like universal agreement. In the series of 12 designs, not one has 25 percent of ratings in any one position.

In the series of 24 designs, in only about one case out of thirty are there 10 percent or more of ratings in any one position.

Table II

Frequencies of Each Position for Each Triangle Computed from Orders of Merit Reported by 250 Individuals: In Percents

Positions	Triangles											
	41	42	43	44	45	46	47	48	49	50	51	52
1	26.0	14.0	21.2	14.0	7.2	4.8	3.6	2.0	1.2	3.2	.8	1.6
2	10.8	31.2	16.8	12.0	7.6	6.8	7.2	2.8	2.8	.....	1.6	.8
3	8.8	14.8	30.4	16.4	12.0	3.6	3.6	3.6	3.2	2.0	1.2	.8
4	9.6	7.2	6.8	33.2	14.0	11.2	8.0	4.0	1.6	3.6	.8	.4
5	6.4	5.6	7.2	7.2	36.0	12.0	9.2	8.0	4.0	2.4	1.6	.8
6	7.2	3.2	4.4	6.4	6.4	41.2	12.0	7.6	7.2	.4	2.4	1.6
7	4.4	5.2	3.2	2.4	4.8	6.8	45.6	12.4	6.8	2.8	2.4	2.8
8	2.4	4.8	1.2	1.2	6.8	4.8	4.8	49.2	12.0	8.0	2.4	1.6
9	5.6	4.8	2.4	4.8	2.4	2.8	1.6	5.6	52.0	10.0	4.0	3.6
10	5.2	2.0	4.4	1.2	1.2	3.6	2.0	2.0	4.8	61.2	9.2	4.0
11	6.8	4.8	1.2	.4	.8	2.0	1.6	.8	3.2	4.8	67.6	4.4
12	5.6	2.8	0.8	.4	.8	.....	.4	.8	1.2	1.6	5.6	76.8
?	.....	.....	.....	.....	.....	.....	.4	1.2	.....	.....	.4	.8

No great value attaches to the general drift of the consensus, since the responses to the objects displayed as they were and with criteria of symmetry so strongly suggested

Table III

Frequencies of Each Position for Each Cross Computed from Orders of Merit Reported by 140 Individuals: In Percents

Positions	Crosses											
	61	62	63	64	65	66	81	82	83	84	85	86
1	2.1	.7	10.0	15.0	24.3	7.1	6.4	19.3	5.7	2.1	2.1	5.0
2	2.1	3.6	10.0	22.1	16.4	4.3	5.0	14.3	12.9	1.4	7.1	1.4
3	.....	6.4	8.6	15.7	10.0	11.4	4.3	18.6	8.6	10.7	1.4	4.3
4	3.6	3.6	10.0	15.0	9.3	5.0	15.0	13.6	15.0	2.1	6.4	1.4
5	.....	7.9	15.7	7.1	10.7	6.4	13.6	15.0	15.0	4.3	3.6	1.4
6	3.6	8.6	11.4	9.3	5.7	8.6	13.6	7.9	12.1	11.4	3.6	4.3
7	1.4	5.0	11.4	3.6	11.4	12.9	12.1	4.3	15.7	10.7	5.7	5.0
8	5.0	10.0	3.6	2.1	6.4	17.9	10.7	3.6	2.9	24.3	9.3	5.7
9	2.1	11.4	5.0	6.4	2.9	9.3	7.1	2.1	6.4	17.1	24.3	4.3
10	11.4	10.0	13.6	2.9	.....	5.0	5.7	.7	4.3	8.6	17.1	20.7
11	15.0	32.1	.....	.7	2.1	5.7	5.0	.7	1.4	3.6	17.1	15.7
12	53.6	0.7	.7	.....	.7	6.4	1.4	.....	.....	3.6	2.1	30.7

may be different from the responses to the same objects in isolation or in different surroundings. However, it may be of interest to some to record that: The most liked rectangles

Table IV

Frequencies of Each Position for Each Lettered Design Computed from Orders of Merit Reported by 100 Individuals: In Percents

Positions	Designs											
	A	B	C	D	E	F	G	H	I	J	K	L
1	14	6	12	9	2	5	6	2	15	13	7	10
2	12	8	12	8	2	2	1	9	13	16	5	11
3	16	2	12	10	10	5	3	5	12	14	4	9
4	16	11	11	11	9	10	3	2	5	9	5	9
5	14	9	12	11	10	7	3	5	7	6	4	12
6	8	6	13	15	6	17	5	6	5	9	1	10
7	11	13	7	6	7	10	11	7	6	6	7	8
8	5	9	11	13	8	8	7	10	4	11	6	7
9	1	6	6	7	19	12	13	6	7	.....	10	12
10	2	7	2	5	12	16	7	19	10	11	3	5
11	1	10	.....	5	11	5	17	22	15	3	7	5
12	.....	13	2	.....	4	3	24	7	1	2	41	2

Table V

Frequencies of Each Position for Each Unnumbered Design of the First Two Rows Computed from Orders of Merit Reported by 250 Individuals: In Percents. The Results for the Other Two Rows Show the Same Variability

Positions	Designs											
Row	1	1	1	1	1	1	2	2	2	2	2	2
Number	1	2	3	4	5	6	1	2	3	4	5	6
1	2.8	10.0	12.8	2.8	1.2	4.4	2.0	.8	.4	7.2	2.4	1.2
2	4.0	9.6	8.8	2.0	.8	5.2	6.4	4.4	3.2	4.0	4.4	1.2
3	3.6	9.2	8.8	3.6	.....	6.8	4.4	2.8	2.8	4.8	2.8	.4
4	5.6	10.2	7.2	2.4	1.6	3.6	4.4	3.2	3.6	3.6	4.0	3.6
5	6.0	8.0	4.0	2.4	.4	8.0	3.6	6.8	5.6	3.2	2.0	1.6
6	4.8	8.4	3.6	2.4	3.6	8.4	9.2	7.2	7.2	2.8	1.6	2.4
7	8.0	4.4	7.2	3.6	4.8	4.4	7.6	4.8	4.8	4.8	2.8	3.6
8	7.2	3.2	2.0	2.8	4.4	8.4	7.2	6.0	6.0	4.4	4.8	2.8
9	6.0	4.4	1.6	3.6	4.8	7.6	4.8	4.0	4.8	6.4	6.0	3.2
10	3.2	3.6	2.8	4.0	6.0	5.2	4.8	6.8	10.0	4.0	5.6	3.6
11	5.2	2.4	2.8	4.8	5.2	4.8	4.8	4.4	7.2	6.0	4.0	4.4
12	3.2	4.4	4.0	2.8	6.0	3.6	5.2	4.4	6.4	7.2	6.4	5.2
13	5.2	4.8	2.8	6.4	4.0	4.4	4.8	3.6	2.8	3.2	6.0	6.0
14	4.8	3.6	3.2	2.8	4.0	3.2	5.2	2.8	3.6	8.8	5.6	6.4
15	4.0	1.6	2.8	3.2	4.0	3.6	3.6	5.2	4.0	4.8	10.0	4.4
16	4.4	1.6	1.2	4.0	4.8	2.4	6.4	4.8	4.0	4.8	6.0	4.4
17	2.8	2.0	3.2	2.4	4.8	1.6	4.4	3.2	5.6	6.0	3.6	2.4
18	2.0	1.6	3.2	5.6	6.8	1.6	2.0	6.0	2.8	1.2	5.2	4.4
19	3.2	2.0	4.0	4.8	8.4	4.0	1.2	3.2	3.2	4.0	3.6	4.0
20	2.0	1.6	3.2	8.4	5.6	1.6	1.6	4.4	3.2	1.6	4.0	8.4
21	2.8	1.2	3.6	10.8	6.8	2.0	2.8	5.2	2.0	2.4	2.4	10.0
22	3.6	.....	1.2	11.2	3.2	1.6	2.0	4.4	.8	1.2	3.6	10.0
23	1.6	.8	3.2	1.6	3.6	2.4	.8	1.2	1.2	.8	2.4	2.4
24	4.0	.8	1.2	1.2	4.4	.8	.8	.4	4.4	.4	.4	4.0

Table VI

Order of Merit Assigned by the Consensus of College Students

Rectangles	Triangles	Crosses	Lettered Designs	Unnumbered Designs. The Numbers Here Follow the Order of Printing*
29	43, 44	64, 82, 65		
28	42	81, 83, 66	A	14
27 and 30	41, 45	84, 63	J	2
26	46	62, 85	C D I L	3
25 and 31	47	61, 86	B F	6, 15
24	48		E	1, 7, 13
23 and 32	49		G. H.	8, 9, 10, 20
22 and 33	50		K.	11, 17, 22, 24
	51			4, 5, 12, 18
	52			16, 23
				19, 21

* That is, the first design in the second row is 7, the next is 8; the first design in the third row is 13, the next is 14, etc.

had, as the ratio of altitude to base, 1.83 to 1. The most liked triangles had, as similar ratios, 1.6 to 1 and 1.7 to 1 (43 and 44 being equally well liked). The most liked of the crosses had a bar half of the length of the upright and such a bar is best liked when it cuts the upright so as to leave one fourth above and three fourths below. A bar two fifths of the length of the upright is nearly as well liked. The most liked of the unnumbered designs is the second one of the third row. The first and third of the fourth row are the most disliked. In the lettered designs the space relations may vary widely so long as the design remains obvious, and so long as neither bareness nor crowdedness is suggested. *A* and *J* are liked about equally; *G*, *H* and *K* are disliked about equally.

The order of merit of the consensus is given for each group of designs in Table VI.

PRELIMINARY REPORT ON THE RELATIVE INTENSITY OF SUCCESSIVE, SIMULTANEOUS, ASCENDING, AND DESCENDING TONES

BY A. P. WEISS

Ohio State University

The attribute of tone intensity has been relatively neglected in experiments in audition because of the technical difficulty in producing pure tones which may be varied in their loudness or intensity in a definite and measurable manner.

The apparatus with which the experiments in this paper were performed was developed at the University of Missouri under the guidance and suggestions of Dr. M. F. Meyer. The extended report showing the details of the apparatus construction and the manner in which the experiments were conducted is being published as a Psychological Review Monograph.

The apparatus makes it possible to produce tuning-fork tones which meet the following conditions:

1. The tones are pure in the sense that no lower or upper harmonics can be detected.

2. The tones can be quickly varied from weak to strong in any number of steps and each degree of intensity can be repeated as often as necessary.

3. The tones 'come in' and 'go out' at their full intensity without disturbing, starting, or stopping noises.

4. The phase relations of the tuning forks is under control.

The nature of the experiments may be understood from the following illustrations.

1. Relative intensity of successive and simultaneous tones: Suppose we have the tone 200 which, during a given trial, is always sounded at a medium and constant intensity. Another tone 250 can be easily varied from weak to strong

(ascending order)[1] or from strong to weak (descending order). Suppose we sound 200 and 250 *alternately* and vary the intensity of 250 (either ascending or descending) until it seems to have the same intensity as 200; the question now arises, if 200 and 250 are sounded *simultaneously*, are they still of the same intensity?

2. Relative intensity of ascending and descending tones: Suppose 200 is kept constant in intensity and 250 is varied in *descending* order, will the point at which 250 is considered equal to 200 be the same as when 250 is varied in *ascending* order?

The tones used in this experiment were the four tones 150, 200, 250, 300. Each tone was compared with each of the other three tones in four ways.

1. Successively, with the comparison tone varying in ascending order.

2. Successively, with the comparison tone varying in descending order.

3. Simultaneously, with the comparison tone varying in ascending order.

4. Simultaneously, with the comparison tone varying in descending order.

Both the lower and the higher tones were used as standard in each pair. Each pair of tones was further compared for ten degrees of intensity ranging from a weak tone which was nevertheless clearly heard, to a strong tone which was not, however, so loud that it became disagreeable. That is, the range of conveniently obtainable intensities was divided into ten steps and the various tone combinations were compared for each of these steps.

The method of making the judgments was that of "Selbsteinstellung." One tone (the standard) was kept at constant intensity while the observer varied the intensity of the comparison tone until it seemed equal in intensity to the tone which was being used as the standard.

[1] The numbers 200 and 250 refer to the vibration rates. Ascending order or ascending tones refer to tones which are varied from silence to weak to strong. Descending order or descending tones refer to tones whose intensity is varied from strong to weak.

The tones were produced by resonators suspended over silently vibrating tuning forks of constant amplitude and the objective intensity of the tone was measured by the distance of the mouth of the resonator from the prongs of the tuning fork.

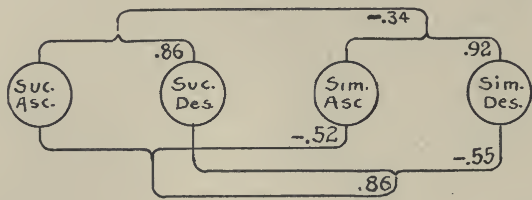


Fig. 1.

The above diagram shows the results obtained. The number in the right arm of each brace indicates the intensity relations between the series connected by the brace. Thus the number .86 in the brace connecting the circles of the successive-ascending and successive-descending series, means that when a tone of constant intensity was compared *successively*, first with an ascending tone, and second with a descending tone, the descending tone was made .86 step stronger than the ascending tone. Subjectively this implies that the descending tone was actually heard weaker than the ascending tone, since if it had been set at the intensity of the ascending tone it would have been judged weaker than the standard tone.

A negative sign in the diagram means that the series was made weaker (objectively) than the companion series. In subjective terms this means that the tone was actually heard stronger. This opposition between "made stronger objectively" and "heard weaker subjectively" is rather confusing and it was thought worth while to add the subjective implications parenthetically in the statement of the conclusion.

Each of the conclusions which follow are based on at least 4,800 judgments or reactions. The term 'step' refers to one tenth the total range of intensities used in the experiments, or one tenth of the range of conveniently obtainable intensities.

1. When compared *successively* with a tone of constant intensity, *descending* tones are made .86 step stronger (or heard .86 step weaker) than *ascending* tones.

2. When compared *simultaneously* with a tone of constant intensity, *descending* tones are made .92 step stronger (or heard .92 step weaker) than *ascending* tones.

3. When compared with a tone of constant intensity, *ascending* tones, when compared *simultaneously*, are made .52 step weaker (or heard .52 step stronger) than when compared *successively*.

4. When compared with a tone of constant intensity, *descending* tones, when compared *simultaneously*, are made .55 step weaker (or heard .55 step stronger) than when compared *successively*.

5. When compared with a tone of constant intensity in mixed simultaneous and successive order, *descending* tones are made .86 step stronger (or heard .86 step weaker) than *ascending* tones.

6. When compared with a tone of constant intensity in mixed ascending and descending order, *simultaneous* tones are made .34 step weaker (or heard .34 step stronger) than *successive* tones.

The deviations of one half of the intensity judgments above or below the objective intensity was .74 step. This seems to indicate that within the range of conveniently obtainable intensities used in this experiment 15 steps might have been discriminated. Taking very weak and very loud tones it seems that 25 steps should be possible.

The value .74 is also an indication of the reliability with which intensity judgments can be made. This is about the same for all the intensities used, being somewhat less for the medium intensities than for the extremes, as might have been expected.

The variability of the intensity judgments is not influenced as much by difference in vibration rates as was expected. The greatest difference between any of the tones of this experiment was an octave (150 vibrations) and the comparisons between these two tones were no more variable than where the difference was 50 vibrations. This seems to show that even between tones whose vibration difference is considerable, the intensity judgments can be made with a degree of accuracy which promises well for an experimental analysis of the sound intensity reaction.

DISCUSSION

A NEW METHOD OF HETEROCHROMATIC PHOTOMETRY—A REPLY TO DR. JOHNSON

In the September number of this journal appears a discussion entitled 'A Note on Ferree and Rand's Method of Photometry,' by Dr. H. M. Johnson, of the Nela Park Laboratory. This discussion, we may perhaps be pardoned for noting, is remarkable chiefly for its numerous mistakes and incorrect or misleading representations, a few of which we take opportunity here to rectify. The net service of the discussion is to call the authors' attention to the omission of a decimal point in the original article, for which they duly acknowledge their debt.

1. In his opening paragraph Dr. Johnson says: "The authors claim for their method that with respect both to sensitivity and reproducibility it surpasses the equality of brightness method, even when the photometer head used is of the best Lummer-Brodhun type." In regard to this statement we beg to point out that Dr. Johnson has omitted from what was actually said all that makes a difference between a reasonable and an absurd claim. We had claimed in our paper greater reproducibility of setting for the method in question as compared with the equality of brightness method *only in case of heterochromatic photometry*, in which respect as is well known the equality of brightness method is notably deficient. The possibility of a service to heterochromatic photometry alone is the reason given in the paper for applying to the rating of artificial lights a principle formerly used by us for an entirely different purpose. Also the special reference to heterochromatic photometry was featured in the title.

2. Dr. Johnson next says: "The authors assumed that the two elements making up the photometer screen 'received equal amounts of light from the source to be measured.' Even if the elements were equidistant from the lamp . . . the truth of this assumption does not follow from the data given. In some of the work the results of which are presented in the authors' table, the angular separation of the compared elements was 14° to 15° at the source. Now the radiation from a carbon or tungsten lamp is not equal in

all directions as is that from an ideal point source. In fact, for lamps of such types, differences of several per cent. in different directions normal to the long axis of the lamp are the rule, and a considerable difference might occur in a range of 15°."

With reference to the above statements we wish to note in the first place that it was never assumed by us that there were only two elements in the photometer screen. This erroneous interpretation of the principle on which the method is based can be attributed to Dr. Johnson only. Secondly, that when the angular separation of the elements referred to (the stimulus patch and the measuring disc), is correctly computed from the data contained in the original article it is found to vary between 4.5° and 11°,[1] and not to have a range of 15°. And thirdly, that when the question of the influence of the distribution curve on the general applicability of the method to working practice was raised by us in a paper presented to the Philadelphia Section of the Illuminating Engineering Society in February, 1914,[2] it was the consensus of opinion in the discussion that followed that the possibility of error from this source is of negligible consequence in a field presenting so many difficulties as heterochromatic photometry, and that the effective check on these and many other points which were raised by us at that time—in addition to those now raised by Dr. Johnson—must come in a comparison of the results with those obtained by the equality of brightness method. Because of this confirmatory opinion of a group of specialists fully familiar with all the technical and working details of photometry and because of the check experiments we had run on the point to convince ourselves of the negligible influence of the factor for the conditions under which we worked (see this paper, p. 165), we had not considered it necessary to raise the discussion in the preliminary exposition of the principles on which the proposed method is based, contained in the article in question. However, since the point has been raised by Dr. Johnson, the following comments may not be out of place.

[1] It is assumed here of course that Dr. Johnson referred to the angle for the colorless light. There can have been no reasonable doubt in his mind that the colored light was not obtained from the naked carbon or tungsten lamps to which his comments on distribution refer. (See footnote, original article p. 9).

[2] With reference to the foregoing point and to others taken up in this discussion it is scarcely needful to state that principles and descriptions of conditions of a technical nature were taken up in a fuller and more detailed way when a statement of the method was presented to auditors technically interested in photometry than was done in the article criticized by Dr. Johnson.

(*a*) A general statement of the type which Dr. Johnson has made about the inequality of distribution of carbon and tungsten lamps is incomplete to the point of being somewhat misleading. As is well known, the distribution curve of an incandescent filament lamp depends upon the shape of the filament. While, for example, the single oval filament of the ordinary carbon lamp gives considerable unevenness of distribution, if wide enough angles are considered, the single loop tungsten filament of the Mazda lamp, series type, gives a curve which deviates so little from

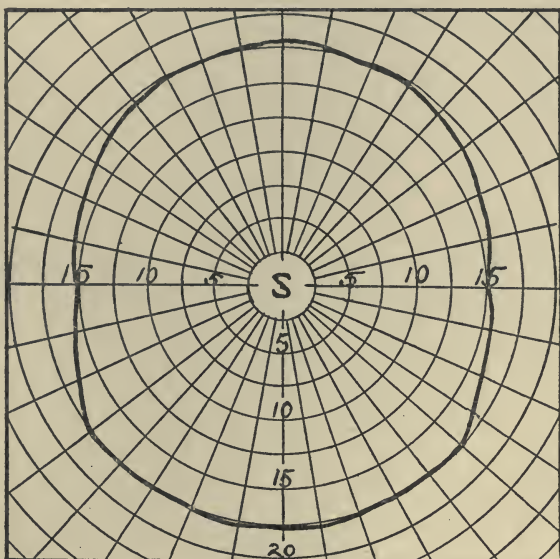


FIG. 1. Showing the distribution curve in the horizontal plane of a 50-watt carbon lamp, single oval filament—readings taken at 5 ft. radius; lamp operated at 6.8 horizontal cp.; watts per horizontal cp., 2.97.

a circle as to be scarcely detectable with the exception of a very small region in the plane of the filament. The curves for these lamps are appended in Figs. 1 and 2. In Fig. 3 is given also the curve for the ordinary type B Mazda lamp.[1] This curve shows more variation than the series lamp but it is so nearly uniform as to be considered circular for practical purposes. However, neither this nor the single oval filament carbon lamp have ever been used

[1] The determinations represented in these curves were made by the photometric laboratory of the General Electric Co., Schenectady, N. Y.

by us in connection with the method of photometry in question without some device to secure greater uniformity of distribution of light. In case a naked lamp were used at all it has always been of the series type, single-loop tip-anchored filament, and care has been taken to have the lamp set on the bar so that the light was taken at right angles to the plane of the filament or from the most uniform part of the curve. But even were a carbon lamp used and the arrow

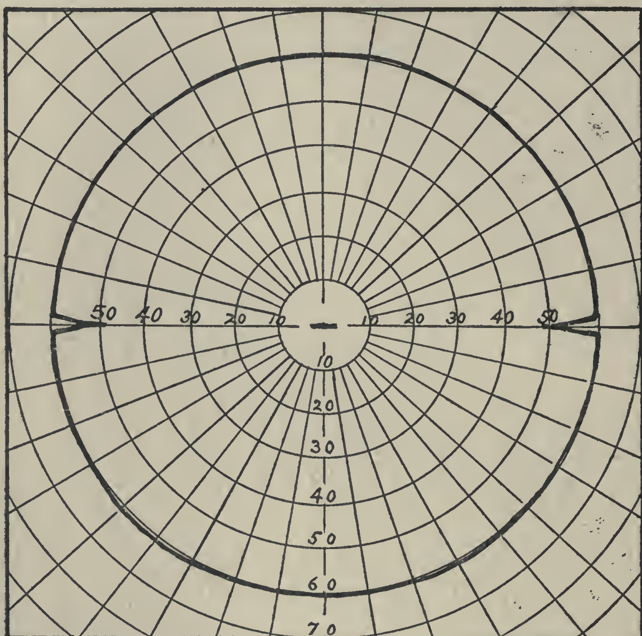


Fig. 2. Showing the distribution curve of a 60 cp. series Mazda lamp (clear), single loop tip anchored filament, 6.6 amps.—readings taken at 5 ft. radius; lamp operated at 60 horizontal cp.; watts per horizontal cp., 1.18.

or 'fiducial' mark scratched in a plane at right angles to the plane of the filament, the distribution would fall off so evenly on either side (see Fig. 1)[1] that the difference in the illumination of the stimulus patch and measuring disc, not exceeding 5.5° on either side, should be negligible.

[1] It should be noted that in making the cuts for the curves in Figs. 1 and 3 the true deviations from uniformity have been exaggerated by small but considerable amounts.

(*b*) So far as the question of uniformity of angular distribution of light is concerned, stress seems to be laid in the criticism on the equality of illumination of the stimulus patch and the measuring disc alone from the lights to be photometered. This is not at all in keeping with a correct interpretation of the method, for the photometric balance does not consist in the judgments of the actual amounts of light falling on the stimulus patch and measuring disc.

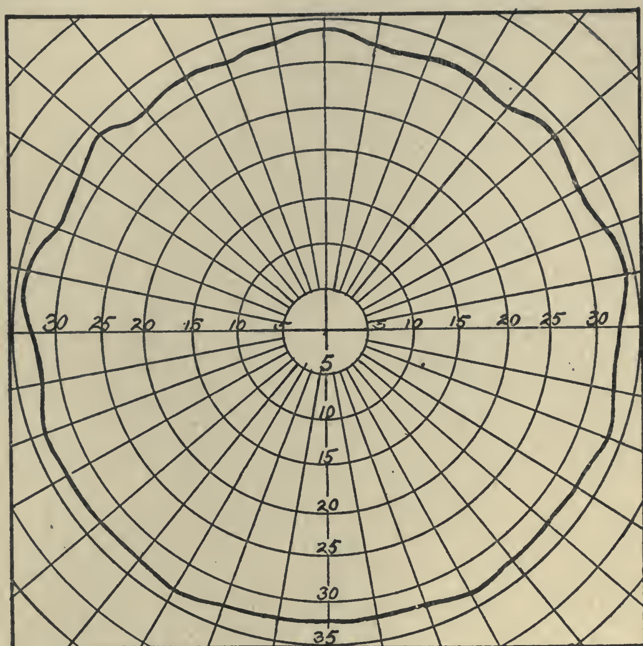


FIG. 3. Showing the distribution curve in the horizontal plane of a 40-watt G. E. Mazda lamp (clear), regular type small bulb, 110 volts—reading taken at 5 ft. radius; lamp operated at 32.5 horizontal cp.; watts per horizontal cp., 1.23.

The apparent brightness of the stimulus patch is, for example, the result of three factors: the actual amount of light falling on the stimulus patch, the amount falling on the surrounding screen (rather in both cases the amount reflected to the eye), and the physiological induction caused by the difference in the brightnesses of these two surfaces. Dr. Johnson, however, as indicated above, in considering the question of the distribution of the illumination and its probable effect on the results of the method, seems throughout his discussion to take into account only the relative amounts of light received by the stimulus patch and the measuring disc, and in so doing shows a fundamental misunderstanding of the principle on which the method is based. The illumination of the field *surrounding* the stimulus patch is just as important as the illumination of the stimulus patch itself, for it is an equal factor in producing the induction and is, so far as any one knows, effective for induction up to the measuring disc; and there is, it is scarcely needful to point out, not an *angular separation of 15°* between this screen and the measuring disc. The important point is rather that there shall be no effective difference in the collective situation influencing the induction and its measurement for the standard and the comparison lamp. That is, although the two surfaces are compared in each judgment, the comparison of the two light sources is based on the results of two judgments, and if there is no difference in the collective situation influencing the two judgments, no injustice is done to the lights compared. If, therefore, we were considering with Dr. Johnson the relative illumination of stimulus patch and measuring disc to the exclusion of other factors, and to what degree this relative illumination is influenced by the distribution curve of the light source, the important item is not that there *is* an angular separation between them of a given number of degrees and a possible difference of illumination in consequence, but *how much this varies* for the position of the standard and comparison lamps on the photometer bar. For the nearest position of the standard lamp, the difference in the angular separation for the two lamps was 11°; for the farthest position for the distances as given in the table, the difference would have been 4.5°. However, for the greater distances that would have been required for the standard lamp from the screen to establish a balance with the less intense colored lights, a part of the reduction was produced by sectored discs, because in the form and set-up of apparatus employed for that work, distances of light from screen of 134–160 cm. (Table I., original article, p. 9) could not conveniently be attained owing to the angle of the shadow cast by the observer's head. This reduction was converted into terms of the law of squares to make the results comparable in the table with those obtained by the equality of brightness method. The actual setting of the lamp on the photometer bar for the greatest of these distances was 104 instead of 160 cm. The difference between the angular separation of stimulus patch and measuring disc was in this case, therefore, 7°. The actual range of variation of angular separation of stimulus patch and measuring disc was thus only from 7° to 11°. There is, it is obvious, considerable difference between these values and the 15° with which Dr. Johnson confronts us. Furthermore, in the course of the original work we ran a series of check experiments to determine whether this difference in angular separation in case of the standard and comparison lights produced any significant error. That is, in these check experiments both lights were kept in the same position and the light for the more intense, the standard, was reduced by means of sectored discs very accurately cut from sheet aluminum, the open sectors of which were measured with a protractor provided with a Vernier scale reading to minutes. The results of these experiments are given in Table I.

Table I.

Showing a Comparison of the Results Obtained for the Lights Represented in the Original Table when the Photometric Balance was made (*a*) by Changing the Setting of the Lights on the Photometer Bar; and (*b*) by the Use of the Sectored Disc

Source of Colored Light	Color	Distance of White Light Giving Equality of Illumination, Cm.	Ratio of Candle-power. Color: White	Value of Open Sector Giving Equality of Illumination with Distance of White and Colored Lights Equal	Ratio of Candle-power. Color: White	Difference in Ratio	Difference in Per Cent. Candle-power
87 cp. 41 cm. distant from photometric screen	Red	66.6	0.379	137.5°	0.382	0.0030	0.785
	Blue-green	59.5	0.4748	172.0	0.4778	0.0030	0.628
52 cp. 38 cm. distant from photometric screen	Red	82.2	0.2137	77.75	0.2160	0.0023	1.06
	Blue-green	70.5	0.2905	105.5	0.2931	0.0026	0.887
13 cp. 38 cm. distant from photometric screen	Red	160.0	0.0564	20.5	0.05694	0.00054	0.948
	Blue-green	134.9	0.0793	28.85	0.08014	0.00079	0.985

Moreover, so far as inequalities of illumination of stimulus patch and measuring disc are concerned, we may point out that a naked lamp was not even used in the experiments the results of which are given in the original table. Partly because the colored light was secured by means of colored filters, and partly as a precaution against unevenness of illumination of stimulus patch, measuring disc, and surrounding field for a height and breadth sufficient for the purpose of the experiment, the light was placed in a lamp-house (see original article, footnote p. 9).[1] This lamp-house was 24 cm. high, 14 cm. wide and 14 cm. deep. At the lower end of the lamp-house was an opening 5 cm. square through which the light passed to the screen. The lamp-house was lined with mat white paper so shaped as to round off the edges and corners and to give as much as possible in the lower part of the enclosure the effect of the segment of a sphere. No light passed directly from the lamp to the screen as the tip of the lamp was for the different lamps used from 2 to 9 cm. above the opening for the emission of the light. Owing to the high absorption of the Wrattan and Wainwright filters the light from the lamp used to establish a balance with that transmitted from the filters had to be greatly reduced at the opening of the lamp-house by means of colorless absorbing screens, which served further to diffuse the light. To determine whether or not any serious difference in the distribution of light to measuring disc and stimulus patch was present in case of this device, the light was photometered at stimulus patch and at measuring disc for each position of the lights on the bar. No difference could be detected for these two positions by the equality of brightness method. Also the distribution curve for the light coming through this opening was found to be circular through an angle greater than the 11° in question. Our statement in the original article then was correct that the equality of distance of the measuring disc and stimulus patch on either side of the photometer bar guaranteed that they receive equal illumination from the light source employed. It would also be true within any reasonable margin of error for the single loop filament series lamp set as described above (without a lamp house), and even for the single oval carbon filament within a margin of error quite acceptable for work in heterochromatic photometry.

3. In a later paragraph (p. 394) Dr. Johnson conveys the impression that we claim an agreement between the results of the new method and those of the equality of brightness method within

[1] The lamp-house is not shown in the photograph of apparatus given in the original article. The photograph was a part of the general description of the method and the apparatus that might be used with it. In making this photograph the apparatus was regrouped, the object being merely to show the type of bar used, the screen and the measuring disc. In this photograph it will also be noted that the apparatus was not even shown in the position in which it is used in making the determinations. The use of a lamp-house is mentioned in another part of the article, namely the part treating of the results that were given as a sample of what might be obtained with the method. In the first photographs that were made the lamp-house was included, but its size and position in the foreground made it appear so disproportionately large that it was decided to omit it and to give the photograph the general character mentioned above.

a fraction of one per cent.[1] Of this we have to say that no numerical value whatever was assigned to the agreement in the original article nor was any general statement made that would warrant the inference that we claimed an agreement within so small a margin. All that appears in the article in this connection is a very brief table of results containing no reference whatever to the point in question accompanied by an 8-line paragraph stating that the table is appended as a *sample* of the results obtained, that the results are averages from 25 determinations, etc. It is the custom in photometry when a numerical expression is made of agreements, mean deviations, etc., to give these in per cent. illumination or per cent. candlepower. When this is done for the table in question, the agreement shown by the data given falls within 1.5 per cent. instead of 'within a fraction of 1 per cent.' as is stated by Dr. Johnson. And this it will be remembered, is an agreement in the average. When the individual determinations are compared, the deviations reach values of the order of + 10 and − 12 per cent. Some idea of this may be had from an inspection of the per cent. mean variations appearing in the table for the results obtained by the equality of brightness method. Thus it will be seen that the actual closeness of agreement of results is not surprising. It has been made to appear so only by our critic's method of presentation.

[1] On p. 393 Dr. Johnson says: "The authors do not describe their mode of procedure in making their measurements by the method of direct comparison. I assume, therefore, ... Under these conditions and working with the lamps beyond certain minimal distances from the photometer head, the luminous intensities of the compared sources would be *inversely* [italics ours] as the squares of their distances from the photometer screen at valid settings for equality of brightness on the two halves of the photometer field." We did not suppose that in an article on photometry it was necessary to give a description of the equality of brightness method over 100 years after its principles were laid down for all time (Pierre Bouguer, 1760, and Sir Benjamin Thompson, Count of Rumford, 1793). However, we do wish to say now that Dr. Johnson has raised the question that we conformed to all that is essential in his very elementary directions with the exception that we chose rather to follow the custom to which we know of no exception either in practice or recommendation, of calculating the luminous intensities of the light sources on the basis of the *direct* squares of the distances of these light sources from the photometer head, instead of the *inverse* squares. In replying to an advanced criticism on photometric method, one should not have to point out that the law of inverse squares applies to the intensity of illumination at different distances from a given source; while the converse of this relation, namely, the direct squares, applies to the comparative intensities of two sources which produce equal illumination on a given screen or photometer head. That is, the former is used in the computations of intensity of illumination: foot-candles, meter-candles, etc.; and the latter in the computation of the relative intensities of light sources: candlepower, lamberts, millilamberts, etc.

4. Also on p. 394 Dr. Johnson presents a table in which it is represented that the measuring disc in the work for which our table of results was submitted was 3 cm. nearer to the observer than the plane of the screen containing the stimulus patch. Applying the law of inverse squares he demonstrates that the illumination of the stimulus patch and measuring disc was in case of each light source unequal. Since the colored lights were all nearer the screen and measuring disc than the standard white light in proportions varying from 41 /59 to 38/ 160 (actually 41/ 59 to 38/ 106 because, as stated earlier, a sectored disc was used for the lights requiring the greater distance of setting from the screen), the 3 cm. caused a greater difference between the illumination of the measuring disc than of the stimulus patch for the colored lights than for the white light by percentages ranging from 5.4 to 15.5. From the showing of this table without further inquiry into causes, it was concluded that 'the authors' procedure in making the settings was faulty,' the 'method is insensitive' and that the evidence of agreement of the two methods is 'spurious,' for the explanation of which latter point there seems to have been no hypothesis worthy of mention but that the settings of one method were biased by a knowledge of the settings of the other—a smashing and uncompromising arraignment truly! However, we beg in passing to say a word of this table ourselves. In the first place, as a matter of only minor consequence to the present discussion, we wish to point out that in all of the computations given by Dr. Johnson of the deviations in per cent. from proportionality of illumination of stimulus patch and measuring disc, errors have been made, and that in 5 out of a total of 6 cases appearing in his table these errors have ranged from 1.8 to 11 per cent. of the correct value, with a leaning in some of the most important cases towards the advantage of the critic. This, we may be pardoned for noting, is under the circumstances somewhat surprising, and is of value perhaps chiefly in demonstrating that it is possible for mistakes to occur even in a critique levelled at the accuracy of the work of others without furnishing a justification for the impugning of motives and integrities. And secondly we wish to state that, as might have been suspected by our critic himself,[1] the 3 cm.

[1] The above statement is made for the following reasons. (*a*) It is obvious on *a priori* grounds to one having even the least rudimentary knowledge of the principles on which photometry is based, that a just balance could not be established between the colored and white lights involving so wide a difference in setting on the bar if the measuring disc was 3 cm. in front of the photometer screen. And (*b*) even an approximate set-up of the apparatus with the lights in position demonstrates at a glance that

was a typographical error. In the original data still in our possession, the distance of the measuring disc from the screen is given as .3 cm.[1] When the law of inverse squares is applied to this, the discrepancy of illumination of stimulus patch and measuring disc for the distances used by Dr. Johnson in his computations ranges from .464 to 1.22 per cent., and for the actual distances used, from .464 to 1.03 per cent.—an amount which the experienced photometrist will, we think, grant is relatively negligible among the much greater sources of error present in heterochromatic photometry.

We have, however, been sufficiently curious to know what results would be obtained with the measuring disc placed 3 cm. in front of the screen to repeat the work represented in the original table for the four highest intensities with this change in the set-up. Differences from the results quoted in the original tables—also, as it happens for the cases tested, the amount of deviation from agreement with the equality of brightness results—ranged from 13.5 to 25 per cent. when the determination was begun with the weaker light, and from 18.6 to 29 per cent. when the determination was begun with the stronger light.[2] These figures indicate that rather than being remarkable for its insensitivity, as is charged by Dr. Johnson on the basis of too narrow a consideration of possibilities and apparently no first-hand knowledge whatever of the facts in question, the method shows by still another test a very high degree of sensitivity.

5. The error in our critic's final conclusion (pp. 395–6) should by this time be so obvious as to need no comment. We will, therefore, rest our case so far as we recognize that a case has existed, until space can be had for a further presentation of results. In this regard it is hardly necessary to mention that we do not consider,

the conditions produced are not compatible with the principles on which the method of making the balance is based. For example, when illuminated directly from the lamp on the bar a sharp shadow is cast by the disc on the screen, which is plainly in the view of the observer at the angle at which the observation is made. This is the equivalent of surrounding the disc with a black band which varies in width as the position of the lamp on the bar is changed. This is obviously not permissible. In fact the error is of a kind which is usually handled in a note of inquiry to the authors.

[1] Also there are, we might mention, a number of witnesses to the set-up of the apparatus used by us in the work on heterochromatic photometry.

[2] On account of the limited space allowed, an explanation of why such excessive deviations are obtained with this incorrect set-up will have to be deferred until later work; also the very obvious explanation of why a greater distance of measuring disc from screen was permissible, in fact of advantage, in the work in which the method was used to detect changes in the diffuse illumination of an optics-room (Psychol. Bull., 1913, 10, p. 371) than when it was applied to the rating of lights on a bar.

as our critic seems to have thought, that a place has been won for our method among those hoary and worn with service on the basis of a single sample table appended to a preliminary description of method and apparatus and representing the results of only one observer for two colors and only six of the possible settings on the photometer bar..

NOTE.—Dr. Johnson mentioned the use of a rotator to equalize the light radiation in different directions; also the deviations found by Wright from Lambert's law of reflection for mat surfaces. Since neither of these points was raised in the original article, it might be inferred that they were not known and taken into account by the authors. It will probably not be prejudicial to either side of the case to mention here that one of the writers supervised the construction of his first lamp rotator for work in photometry in 1901 while a teacher of physics, and is well acquainted with the uses and need of a rotator. Also in 1903 while a graduate student of physics he was assigned a study of the reflection from mat surfaces as a problem for investigation, the object being to continue along the lines mapped out by Wright. Both from his reading and instruction with regard to the work of Wright and others, however, he is totally unable to concur in a single comment that Dr. Johnson has made on the subject of diffuse reflection in the footnote on p. 392. Dr. Johnson says: "Another source of error which the authors appear not to have taken into account may be worthy of mention. The angles at which the light was diffusely reflected into the eye from the stimulus patch and the disc at the fixation point were not the same. The *percentage* of incident light reflected into the eye would have been different, therefore, even if the two surfaces had been of the same material. Furthermore, the difference in *percentage* of incident light reflected in the direction of the eye is not constant for any two positions of the source. Cf. Wright, H. R., 'Photometry of the Diffuse Reflection of Light on Matt Surfaces,' *Philos. Trans.*, 1900, 49, Ser. 5, pp. 199–216." Of the sentences quoted the second is the only one that can be said to be true. The angle of emission e from the stimulus patch in relation to the eye was approximately 0°; while for the measuring disc it was 25°. The reflection, therefore, in the direction of the eye from a given point or unit surface in the area fixated of the measuring disc was less than that from the stimulus patch by an amount equal to the cosine of 25°. Dr. Johnson, however, neglects to take into account in considering the case presented by our method that the observation is not confined to a single point or unit of area and that the area of surface viewed increases as the secant (the reciprocal of the cosine) of the angle at which the surface is viewed measured from the normal. That is, the increase of the area viewed just compensates for the lessened amount of reflection from unit area. Nutting, for example, says: "A red hot metal plate is of the same brightness viewed at any angle since the foreshortening of the area just compensates for the variation in the radiation from a given area. Lambert's law holds for mat surfaces for both emitted and reflected radiation." Even the author referred to by our critic, in discussing the two possible methods of making the photometric determination in his investigation of the reflection from mat surfaces, says in effect the same thing (*cf.* Wright, p. 205), so without exception does every other author after whom we have read. Therefore when two mat surfaces are observed whose areas are not limited, the apparent brightness of these surfaces is the same for different angles of observation provided that the angle of incidence and amount of incident light are the same for both surfaces as was the case for the stimulus patch and measuring disc in our work for any one setting of the light on the bar; for although the reflection from unit area decreases as the cosine of the angle of reflection, the area from which the eye receives its light increases as the secant of the same angle; from which it follows that the amount of light entering or reflected in the direction of the eye is independent of the angle at which the surface is viewed.

It is obvious, then, that Dr. Johnson's statement that the *percentage* of incident light reflected in the direction of the eye would have been different, even if the two surfaces had been of the same material, is not true. From this it is equally obvious that his next statement also is not true, namely, that the *difference* in the *percentage* of incident light reflected in the direction of the eye is not constant for any two positions of the source, for as shown above there is no difference in the *percentage* of incident light reflected to the eye from the two surfaces for any given setting of the light on the bar. In other words, the possible bearing of Lambert's law and Wright's results with regard to this law, is not what Dr. Johnson has stated it to be. Just what this bearing is will be discussed further on in this note. What we wish to do at this point is to show that even if it were true that the percentage of incident light reflected to the eye were different for any one setting of the light on the photometer bar, this would make no difference whatever in the results obtained by our method. That is, if less light were reflected to the eye from the measuring disc than from the stimulus patch for the first light set upon the bar, it would mean merely that the coefficient of reflection of the measuring disc would have to be reduced by a corresponding amount to obtain the match. Then when the comparison light was placed on the bar and its distance adjusted until as much light was given to the screen as was received from the first light, the stimulus patch and measuring disc would again match, for neither the difference in angle of reflection to the eye nor the reflection coefficients would have been changed. Dr. Johnson's point, granting its verity, would have application only if the stimulus patch were illuminated alone by one of the lights and the measuring disc by the other and the method of balancing consisted in bringing these two surfaces to equality—then it would be necessary that each reflect to the eye the same percentage of the light received by it; but the point is clearly quite irrelevant to the method described by us in which the two surfaces are illuminated for each judgment by only one of the lights, and the balance consists in so adjusting the distance of the two lights in the successive judgments that the match for the one based on the amount of induction produced at the stimulus patch holds also for the other. In this case it is important only that the physical situation and other factors be kept the same for both judgments—not that they be equal each to each for the single judgment—for the balance is based on the principle that if all the factors are kept constant the amount of induction at the stimulus patch will always be the same when the same amounts of light are received on the screen. It is obvious also that the same considerations are true with regard to the materials forming the stimulus patch and measuring disc. Moreover, with reference to this point, it may also be said that there was, as a matter of fact, very little difference in the materials forming the two surfaces; for one sector of the measuring disc was identical with the stimulus patch and the other sector was a darker gray of the same series of papers (Hering's series of standard grays).

In concluding our comments on this footnote which has revealed so much of our critic's point of view, we will indicate briefly and only in a general way the relation of Lambert's law of reflection from mat surfaces and Wright's findings with regard to this law to the practical working of our method. As already shown, Dr. Johnson's criticism was based both on an erroneous understanding of this law as applied to the making of the photometric judgment by any method whatsoever and on a wrong conception of the principles of the method criticized. Our actual chance of error in terms of Lambert's law is that the *angle of incidence* (Johnson's 'difference in *angle of reflection*' has nothing whatever to do with photometry from mat surfaces) on the stimulus patch and its surrounding field is different for the light from the standard and comparison lamps when they are of different intensities and a different setting on the bar is required to establish the photometric balance. That is, according to Lambert's law the intensity of the illumination of the stimulus patch and its surrounding field is proportional to the cosine of the angle of incidence (the cosine i).

Now considering for the sake of simplicity the stimulus patch alone, the variation in the cosine of the angle of the incident light for the entire range covered in the work criticized from the least to the greatest distance of the source of light from the screen, falls within 1 per cent. While this would mean only a comparatively slight difference in the induction situation from the lights compared, we have from the beginning in our own thinking frankly faced it as a small source of error in case the reductions of the light on the screen are produced by changing the position of the lamps on the bar. However, it would not enter in at all, as will be readily seen, if the reduction of light is produced by means of a sectored disc or any device: absorbing screen, Nicol's prism, grating, etc., which does not change the distance of the source of light from the screen and, therefore, the angle of incidence of the light on the stimulus patch. In this regard it should be remembered too that our photometer is no more at fault in physical principle than the equality of brightness photometer after Rumford as ordinarily constructed, in which also the angle of incidence is changed with a change of the position of the light on the bar—not so much at fault perhaps, for compensating factors operate in our method of getting the balance which are not present in the Rumford method. The relation of Wright's results to the situation described here is that he found that there are certain small deviations from the law of the cosine i as the angle of incidence is changed. Now just how great the chance of error is in our method from the law of the cosine i considered in relation with the results of Wright it is utterly impossible to estimate with any acceptable degree of precision from the principles involved for the following reasons: (*a*) The surrounding field as well as the stimulus patch must be taken into account in applying the law of cosines. The difference in the angle of incidence for the different points in this field vary for any two positions of the light on the bar—towards zero as a limit, for example, for the points between the stimulus patch and the end of the bar, and differently in other directions. (*b*) The effect is not direct but operates through induction, the quantitative relations of which are not definitely known. And (*c*) Wright apparently considered it worth while to make no change of angle of incidence smaller than 20°, while the entire range of variation of this angle in our work from greatest to least distance of lamp from screen was for the colorless light 2° and for both the colored and colorless lights 5°.

Rather, therefore, than indulge in bootless speculation in regard to the possibilities of error from these sources, it is obviously much more to the point to get some empirical measure of their effective importance. The effective importance of this factor along with others not mentioned by Dr. Johnson may be checked up (*a*) by a comparison of results in the average with those obtained by the equality of brightness method (see table in original article, p. 9); and (*b*) still more definitely and directly by comparing the results obtained by the method when the reductions of the light on the screen are produced by changing the distances of the sources from the screen and when the distance of the source and, therefore, the angle of incidence of the light is kept constant and the reductions are made by means of a sectored disc (see Table I. of this discussion).

Even had these comparisons not been made, the probable relative unimportance of these sources of error as compared with the high variable error obtained for one or any small number of determinations by the equality of brightness method, should, we think, be obvious to all who have a working familiarity with the latter method in heterochromatic photometry. On the point of sureness of principle, moreover, it is instructive to compare the agreements of the induction and equality of brightness methods shown in the tables referred to above with those obtained for the equality of brightness and flicker methods, for example, for lights presenting the same amount of color difference.

Bryn Mawr College,

C. E. Ferree,
Gertrude Rand.

[The above discussion, which exceeds our usual limits, has been accepted by the Editors in order that the authors might have ample opportunity to clear up the points raised in Dr. Johnson's Note. The questions at issue are so specialized and technical that we believe it unprofitable to continue the discussion in the pages of the Review. A committee of experts acceptable to both parties may be suggested as the best means of settling any differences which remain between the writers and their critic.—The Editors.]

THE STANFORD (1915) AND THE VINELAND (1911) REVISIONS OF THE BINET SCALE

A brief analysis of the Stanford and Vineland revisions is here attempted in order to indicate a few of the chief points of difference.

A hasty review of the Stanford revised scale gives one the impression that it is much more difficult throughout.[1] The extension of the scale to age 19½ (superior adult)is certainly a commendable advance.

The scale begins at age three and each age contains six tests, in addition to from one to three alternate tests for each year up to age ten. The placing of six tests in each year permits assigning a two months' value to each test. There are no tests for age 11, the only other ages listed being 12, 14, 16, and 18. Since there are no tests for age 11, the eight tests in age 12 are each made to count toward three months mental age, yielding a total value of two years. By this means tests for 14, 16 and 18, six per year, are made to cover without break the range of mental development from 12 to 19½.

The Vineland revision consists of 49 tests for the ages between 3 and 12. Covering the same period in the Stanford revision they number 56, with 13 additional questions which may be used as alternates.

In the following tables the evolution through which the Vineland revision passes is indicated test for test. Tests not in the Vineland (1911) are printed in italics.

	Vineland (1911)		Stanford (1915) Age—Test
Age III.	Test 1—Pointg. eyes, etc.	remains..becomes	III–1
	" 2—Rpts. 6 syll.	remains..becomes	III–6
	" 3—Rpts. 2 nos.	omitted	
	" 4—Enumer. of pic.	remains..becomes	III–3
	" 5—Knows name	remains..becomes	III–5
		Vineld. IV–1 becomes	III–4
		" IV–2 "	III–2
		" IV–3 "	III–A. 1

[1] See foot-note, p. 179.

Vineland (1911) Stanford (1915)

Age IV. Test 1—Knows sex..shifted to III–4
" 2—Recog. key, etc....... " " III–2
" 3—Rpts. 3 nos.. " " III–A. 1
" 4—Comprs. lines......remains..becomes................IV–1
Klmns. Form Discr.... = IV–2
Vineld. V–4......... = IV–3
" V–2......... = IV–4
Binet Compr. 1 deg.... = IV–5
Stanfd. 4 digits....... = IV–6
Vineld. V–3......... = IV–A. 1

Age V. Test 1—Compares wts.......remains..becomes................V–1
" 2—Copies sq....shifted to IV–4
" 3—Rpts. 11 syll. " " IV–A. 1
" 4—Counts 4c... " " IV–3
" 5—"Patience".........remains..becomes................V–5
Vineld. VII–5........ = V–2
" VI–5......... = V–3
" VI–2......... = V–4
" VI–3......... = V–6
Binet, age........... = V–A. 1

Age VI. Test 1—A. M.–P. M........remains..becomes................VI–A. 1
" 2—Definit'ns, use..shifted to V–4
" 3—3 direct'ns..... " " V–6
" 4—R. hand, L. ear.....remains..becomes................VI–1
" 5—Aesthet. Comp..shifted to V–3
Vineld. VII–3........ = VI–2
" VII–1........ = VI–3
" X–4 (1st ser.). = VI–4
" X–1 (part).... = VI–5
Stanfd. 16–18 syll..... = VI–6

Age VII. Test 1—Counts 13c...shifted to VI–3
" 2—Descr. pic.........remains..becomes............. = VII–2
" 3—Unfin. pic.....shifted to VI–2
" 4—Copies Diamd.....remains..becomes............. = VII–6
" 5—Colors........shifted to V–2
Binet, fingers......... = VII–1
Vineld. VIII–5....... = VII–3
Stanfd. bow-knot...... = VII–4
Vineld. VIII–1....... = VII–5
" VIII–3....... = VII–A. 1
Stanfd. rpt. 3 no. bkwd. = VII–A. 2

Vineland (1911)	Stanford (1915)
Age VIII. Test 1—Differences . shifted to VII–5	
" 2—20-1, bkwds. . . . . . remains . . becomes	VIII–2
" 3—Rpts. days, shifted to VII–A. 1	
" 4—Counts stamps, shifted to IX–A. 2	
" 5—Rpts. 5 nos., shifted to VII–3	
	Stanfd. ball-fld. . . . . = VIII–1
	Vineld. X–4 (2d. sr.) . . = VIII–3
	Binet, similar . . . . = VIII–4
	Vineld. IX–2 . . . . = VIII–5
	Stanfd. vocab. 20 . . . . . . = VIII–6
	Vineld. X–1 (part) . . . . = VIII–A. 1
	Binet, dictation . . . . = VIII–A. 2
Age IX. Test 1—Change 20–4 . . . . remains . . becomes	IX–3
" 2—Superior def. . shifted to VIII–5	
" 3—Date . . . . remains . . becomes	IX–1
" 4—Months . . . . " . . "	IX–A. 1
" 5—Arrange wts. . . . . " . . "	IX–2
	Stan. rpt. 4 no. bkwd. . = IX–4
	Vineld. X–5 . . . . = IX–5
	" XI–4 . . . . = IX–6
	" VIII–4 . . . . = IX–A. 2
Age X. Test 1—Money (part) . . shifted to VI–5	
(part) shifted to VIII–A. 1	
(part) . . . . omitted	
" 2—Design . . . . remains . . becomes	= X–3
" 3—Rpts. 6 nos. . . . . " . . "	= X–A. 1
" 4—Comprh. 1st. ser., shifted to VI–4	
pt. 2nd. ser., shifted to VIII–3	
pt. 2nd. ser. . . . . . . remains . . becomes	= X–5
" 5—Sentence . . . . . shifted to IX–5	
	Stanfd. vocab. 30 . . . . . . = X–1
	Vineld. XI–1 . . . . = X–2
	Binet, 8 memor. . . . . = X–4
	Vineld. XI–3 . . . . = X–6
	" XII–3 . . . . = X–A. 2
	Healy Constr. Puz. . . . . . = X–A. 3
Age XI. Test 1—Absurdity . . . . shifted to X–2	
" 2—Simple sent. . " " IX–5	
" 3—60 wds. . . . . . . " " X–6	No XI
" 4—Rhymes . . . . . " " IX–6	
" 5—Dissected sent. " " XII–4	

Age	Vineland (1911)	Stanford (1915)
Age XII.	Test 1—Rpts. 7 nos., shifted to XIV-A. 1	
	" 2—Abstract def.......remains..becomes	...............XII-2
	" 3—Rpts. 23 syll., shifted to X-A. 2	
	" 4—Line suggest......omitted	
	" 5—Problems...shifted to XIV-4	
		Stanfd. vocab. 40...... = XII-1
		" *ball-fld. sup.* .. = XII-3
		Vineld. XI-5........ = XII-4
		Stanfd. fables........ = XII-5
		" *rpt. 5 no. bk.*.. = XII-6
		Vineld. XV-1........ = XII-7
		Stanfd. sim. 3 thgs..... = XII-8
Age XIV.		*Stanfd. vocab. 50*...... = XIV-1
		" *induct. test*.... = XIV-2
		Vineld. Adult-4...... = XIV-3
	No Tests for XIV	" XII-5....... = XIV-4
		Stanfd. arith. prob..... = XIV-5
		Vineld. XV-2........ = XIV-6
		" XII-1....... = XIV-A. 1
Age XV.	Test 1—Interp. pic. .shifted to XII-7	
	" 2—Clock hands " " XIV-6	
	" 3—Code....... " " XVI-6	No Tests for XV
	" 4—Opposites.........omitted	
Age XVI.	(Average Adult)	*Stanfd. vocab. 65*...... = XVI-1
		" *interp. fables* .. = XVI-2
		Vineld. Adult-3...... = XVI-3
		Stanfd. enclsd. boxs.... = XVI-4
		" *rep. 6 no. bk.*.. = XVI-5
Adult	Test 1—Ctg. paper...shifted to XVIII-2	Vineld. XV-3...... = XVI-6
Adult	" 2—Reversed triang.......omitted	*Stanfd. rep. 28 syll.*. = XVI-A. 1
Adult	" 3—Diff. abstr. wds. shifted to XVI-3	" *comp.phys.rel.* = XVI-A. 2
Adult	" 4—Diff. pres. kg. shifted to XIV-3	
Adult	" 5—Sense select. . " " XVIII-4	
Age XVIII.	(Superior Adult)	*Stanfd. vocab. 75*...... = XVIII-1
		Vineld. Adult-1...... = XVIII-2
		Stanfd. rept. 8 no...... = XVIII-3
		Vineld. Adult-5...... = XVIII-4
		Stanfd. rep. 7 no. bk... = XVIII-5
		" *ingen. test*..... = XVIII-6

Summarizing the above tables:

In age 3, four tests remain and one is omitted.

In age 4, one test remains and three are shifted to an earlier age, being too easy for four-year-olds.

In age 5, two tests remain and three are shifted to age 4, being too easy for children of five.

In age 6, two tests remain and three are shifted to age 5.

In age 7, two tests remain, two are shifted to age 6, and one to age 5.

In age 8, one test remains, three are shifted to age 7, and one to age 9.

In age 9, four tests remain and one is shifted to age 8.

In age 10, two tests and a portion of a third remains, one test is shifted to age 9, one part each of two tests is shifted to age 6, and one part each of two tests is shifted to age 8.

In age 11, all the tests are shifted, there being no corresponding 11-year group in the Stanford revision: two are shifted to age 9, two to age 10, and one to age 12.

In age 12, one test remains, one is omitted, and three are shifted: one to age 10, and two to age 14.

In age 15, one test is omitted and the other three are shifted as follows: one to age 12, one to age 14, and one to age 16.

Of the "Adult" tests, one is omitted, one becomes a test for age 14, one a test for age 16, and two are tests in age 18.

The above changes are indicated in the following table:

TESTS

Age	No Change	Omitted	Shifted						Total
			Earlier				Later		
			1	2	3	4	1	2	
3	4	1							
4	1		3						
5	2		3						
6	2		3						
7	2		2	1					
8	1		3				1		
9	4		1						
10	2⅓	⅓	1	⅔		⅔			
11	–		2	2			1		
12	1	1		1				2	
15	–	1	1		1		1		
Adult[1]	3	1		1					
Total......	22⅓	4⅓	19	5⅔	1	⅔	3	2	58
Percent....	38.5	7.5	32.8	9.8	1.7	1.1	5.2	3.4	100.0

[1] (16–18)?

It will be observed that between 3 and 7 years twelve tests have been removed to earlier years, and no tests to later years.

These changes will dispose of the criticism that the lower end of the scale is too easy. But between 8 and 12 years $14\frac{1}{3}$ tests have been removed to earlier years, and only five tests to later years, thus making the scale still more difficult at its upper end.[1]

38.5 percent of the scale remains unchanged. 7.5 percent of the tests in the Vineland revision are omitted. 45.4 percent of the tests are found too easy for their respective ages and are shifted to earlier years, 32.8 percent being placed in the next earlier age. 7.6 percent of the tests are found too difficult for their respective ages and are consequently placed in later years.

No test is placed more than four years below its original position. No test is placed more than two years above its original position.

It might be well to keep in mind that the tests appearing only in the Stanford revision have not been considered in the above tabulation and in the summarization of the test data. For that reason some of the statements made need not be regarded as seriously critical.

Using the Stanford revision, Terman and his collaborators found that (*a*) by using the intelligence quotient one can transform the 'age grade scale' into a 'point scale' automatically, should one prefer expressing the development of intelligence in that manner. "As such it would seem to be greatly superior to the Yerkes-Bridges scale, for it includes a much larger number of tests and its points have definite meaning and equal value." (*b*) Sex-differences are found to be so small as to be negligible for practical purposes. (*c*) The younger the children the greater the influence of social status on intelligence.

The Stanford revision is to be welcomed in its effort toward a scale free from those objections which are still being quixotically hurled against it.

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[1] The following article, however, "L. M. Terman and H. E. Knollin: Some Problems Relating to the Detection of Borderline Cases of Mental Deficiency" *J. Psycho-Asthen.* 1915, 20: 1–15, coming to the notice of the writer after the above was in type, shows the reverse to be true. Tabulating the reactions of borderline subjects (mental ages by the Stanford Revision between 12 and 14, —104 adults) they found that by the Vineland Revision, weighted for tests above 12, the median age for these subjects was reduced as much as one and one-half years, and with the tests unweighted the reduction was greater, namely two years. It ought also be mentioned, in this connection, that the procedure and scoring of quite a number of tests have been changed in the Stanford Revision. Consequently a *strict* analysis of test displacement must take these facts into consideration. Change of procedure or scoring may so modify the statistical data obtained for a test as to warrant its transfer to some lower year without necessarily increasing the difficulty of the scale at that particular point.

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THE NATURE OF MENTAL PROCESS

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This paper proposes the somewhat unorthodox view that the mental functions with which psychology concerns itself are in reality psychophysical, and at times neural, activities, and that psychology shall study and attempt to comprehend these functions in their entirety. The author adopted this conception of the nature of mental process several years ago and is convinced from his teaching experience that such a mode of treatment possesses certain distinct advantages.

The conception may be contrasted with the more orthodox 'subjective' view which postulates psychophysical parallelism but which confines its efforts exclusively to a comprehension of the conscious or subjective aspect of these psychophysical events. Such a psychology studies color, sound, taste, and pain as experiential results but contends that the neural correlates of these sensory experiences belong to the domain of physiology. An emotion as a psychological phenomenon is described and defined in terms of sensational and affective processes subjectively regarded; the neural events involved in an emotion are relegated to the domain of physiology. The acts of memory, imagination, reasoning and will in so far as psychology is concerned with them consist merely of those aspects which can be immediately experienced; the neural events involved in the acts are quite important but their consideration involves a trespass into domains rightfully belonging to another science. Psychology thus deals exclusively with the purely conscious or psychic as opposed to the nervous and material. Psychology is differentiated from the material sciences in virtue of a peculiar subject matter and a peculiar method of apprehending its data.

Our conception also accepts psychophysical parallelism as a working hypothesis; it contends that psychology shall study psychophysical processes in their entirety, and that it shall include within its domain activities which lie outside the field of consciousness. Psychology will study emotions, and acts of reasoning, memory and will, but it will define and envisage these acts as psychophysical processes, and attempt to comprehend the neural events involved as well as those aspects immediately experienced.

The conception allows a division of the field of organic functions between the sciences of biology, physiology and psychology along natural lines of cleavage based upon differences of interest, training and technical procedure. Irrespective of definitions, psychology has been concerned with an ultimate comprehension of those operations by which an organism in virtue of its previous experience is enabled to adapt itself to a complex and variable environment. Physiology and biology have been interested in other types of functional activity—different in character, evolutionary history, and biological significance. The three sciences represent distinctions which appeal to radically different types of human motive and scientific interest, and which necessitate different sorts of training and technical equipment. This paper is not concerned with the formulation of an exact definition delimiting the boundaries of the three sciences.

The conception is unorthodox only in relation to prevailing *definitions* of psychology. To my mind it is essentially in harmony with the dominant point of view of the science, and it is not wholly inconsistent with much of current practice. Sciences have a way of developing and outgrowing their definitions. Practice and attainment often fail to square with theory and definition. The subjective conception of the nature of mental process (mental as opposed to material) originated from philosophical interests at a time when a dualistic conception of the human organism prevailed. At present the prevailing point of view in the science is biological,

—a view which emphasizes the essentially unitary character of the human organism. The science is interested in certain modes of adjustment, and any adequate understanding of these processes of adjustment must necessitate a comprehension of the acts in their entirety. A division of any act of adjustment into its material and conscious aspects with the consequent treatment of but one component certainly gives a very inadequate comprehension of the phenomenon in question, and introduces a distinction which is not only without value, but which is likely to involve the student in many distracting perplexities. Our proposition, however, will necessitate no radical changes in much of current modes of procedure. As a matter of fact, many psychologies treat mental operations as psychophysical processes with only occasional lapses into a consistency with their subjective definitions of the science. It is our definitions that need revision, a revision in harmony with current tendencies and ideals.

The conception allows of a matter of fact treatment of the cause and effect relations in mental activity. One can assert that behavior is influenced by previous acts of memory or will and mean exactly what we say and what the unsophisticated mind understands by the statement. Psychologists will not be compelled to add for the benefit of the sophisticated qualifying phrases to the effect that although they asserted a causal influence of a mental act, yet they really did not mean it, but were forced to employ such statements by certain inadequacies of language. Any conception which allows of a natural and matter of fact treatment of the causal category in mental operations is at least deserving of respect. The new definition of the mental will permit a restatement and a solution of the mind-body problem more in accordance with common sense. Interactionism is logically possible; in accordance with popular belief we may say that our mind is influenced by bodily conditions and that our mind is also an effective influence upon bodily activities, for mind and body have been so conceived and defined in relation to each other that such statements are no longer logically or factually impossible. With our definition the distinction of mind and body is merely a distinction of two systems of organic function.

Our definition will include within the domain of psychology the non-conscious components of mental life. I refer to such phenomena as retention, memory disintegration, conflict of impulses, Aufgabe, unconscious motivation, the concept of habit, and the wealth of subterranean activities brought to notice by the investigations of abnormal psychology. The subjective psychologies have assumed several attitudes toward these phenomena. Some are logically consistent with their presuppositions and attempt to ignore such intruders. Others admit the significance of these data for their purposes, but consistently remind their readers that after all these phenomena really belong to the domain of physiology. Others include these events within the domain of their science, but feel compelled by motives of consistency to impose upon these processes some sort of a 'conscious label.' Witness such terms as unconscious, subconscious, co-conscious, psychical dispositions, etc. These are not merely negative terms, equivalent to neural; they have a positive significance. Since these activities have a mental significance and since the mental must also be conscious, these acts must be conceived in such a way as to possess certain positive characteristics of conscious process. For my part, I prefer the remaining possible mode of procedure, viz., to revise my definition of the nature of mental process in such a way as to include such data.

The subjective conception of mental process as something immaterial constitutes an inadequate tool for the physician in his attempt to comprehend the nature of the mental, or functional disorders. Watson, in a recent article,[1] has asserted his inability to understand the medical concept of mental disease. He cites a case which was diagnosed as being 'purely mental,' and which was described and defined wholly in conscious terms. Watson gives the impression that the physician was of the opinion that this disorder could not in any manner be stated in neural terms, that it was a disorder exclusively on the conscious plane without neural counterpart. Such a conception of the nature of the so-called mental diseases is of course foreign to current psychological

[1] 'Behavior and the Concept of Mental Disease,' *J. of Phil., Psychol., &c.*, 1916, 13, 589.

doctrines, and I doubt very much that such a view is universally prevalent in the medical profession. I am willing to admit that this and similar crude and preposterous conceptions are to be met with, but, unlike Watson, I am inclined to place the blame for this unfortunate state of affairs upon psychology rather than upon medicine. Medicine has merely adopted current conceptions. Psychology must be held responsible for the fact that better and more adequate conceptions were not available. Given a conception of mind as something non-nervous and non-material, crude notions of the nature of a mental disease must result. The mental will be distinguished from other diseases in terms of immaterial vs. material instead of functional *vs.* organic. The conception needlessly introduces into the discussion of mental disease and its treatment the old philosophical question of the relation of mind and body stated again in terms of the conscious *vs.* the physiological, or neural. Certainly the injection of such philosophical questions in discussions of the treatment of mental disease adds nothing of any positive value, introduces a perplexing distraction, and to my mind is wholly unnecessary. The old philosophical problem vanishes at once if we start out with the assumption that the disordered mental functions are in reality psychophysical events.

The psychophysical conception of mental process offers a mediating point of contact for the two extremes of subjectivism and behaviorism. Such a view permits the widest latitude as to methods of approach; it permits mental processes to be studied from the standpoint of immediate experience, of objective observation, or of clinical data. This suggestion of a common ground for the subjectivists and the objectivists, I am well aware, will evoke no approval from either of the warring camps. This phase of the argument is designed exclusively for the benefit of the neutrals. Our program will differ from that of the subjectivists in allowing an objective mode of approach to the problems of psychology; it will differ from behaviorism in two respects: it admits that the study of conscious data has given us much useful knowledge of the nature of mental operations and that further progress is possible in the future. Behaviorism as defined logically includes the whole field of organic function. Psychology should be content with a more modest program and make a more reasonable division of the field of functional activity among the sciences of psychology, biology, and physiology.

The conception will modify our attitude toward the purposes and methods of comparative psychology. The subjectivist to be consistent must define the object of comparative psychology as a reconstruction of the inner life of an animal. Such a program appeals to some minds; to other minds it is repellent. The latter are impressed with the insuperable difficulties and limitations of the interpretative process, and are inclined to regard any results achieved as somewhat futile even if logically valid. Our conception makes possible for those who prefer it a purely objective or behavioristic science of comparative psychology, and yet permits others to reconstruct the conscious life of organisms if they so desire.

An exclusively subjective psychology is prone to meet certain needless difficulties in its presentation. The psychological discussion of a mental event is usually followed by an explanation in terms of neural mechanisms which are labelled physiological. The psychological process viewed by itself impresses the mind of the student as a peculiarly inert and unimportant thing. When the neural mechanism is now added, the process takes on life and significance; mental processes can now do things and be effective instruments of organic adjustment. Moreover, the neural chain of events is, by hypothesis, more complete than the conscious aspect for not all neural events are represented in consciousness. It is small wonder that many students decide that they must look to physiology for any complete and real explanation of mental life. This sceptical attitude of the student toward psychology is not wholly unjustified, nor is it confined entirely to immature students; as a matter of fact this attitude toward our science is quite prevalent among the physiologists. I do not contend that it is impossible to meet such a position in a satisfactory manner; the significant point is that this attitude of scepticism is wholly unnecessary and exceedingly detrimental. No science can afford to be placed in a defensive and apologetic position. The dual presentation further raises such distracting questions as the relative values of the neural and conscious components in an act of adjustment, their causal interrelations, and the necessity for a double treatment of mental events. All these are valid problems but they are philosophical in character and have no place in an empirical, matter of fact, science. Psychology generally recognizes the philosophical character of these questions and their tendency to distract the mind of the student from the main task at hand, and the usual method of attempting to ignore them is by the adoption of parallelism as a working principle. To my mind this method fails utterly to achieve its purpose. Such philosophical questions must necessarily obtrude with a dualistic mode of presentation; in fact this method was developed at a time when psychology was studied primarily as an introduction to philosophical problems, and a better method for this purpose would be hard to devise. These difficulties are at least minimized if not eliminated by adopting the conception of this paper. Parallelism is adopted as a working hypothesis in a matter of fact way without calling the student's attention to it. The total activity is made the object of study; the dichotomy involved is not one of process, but one of method of approach or apprehension.

In conclusion we may say that there are no fixed and immutable boundary lines between sciences. Any science includes within its domain whatever is pertinent to its primary interest. If mental acts are means of organic adjustment, then these acts must be conceived and studied as such devices. If neural events are essential parts of the act, the conception of the mental must be broadened so as to include them. There is no a priori necessity for defining the mental in nonmaterial terms, except habit and the force of tradition. The concept of the mental must be adapted to further the primary task of psychology as it is now conceived.

A REFORMULATION OF THE LAW OF ASSOCIATION

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I

The reformulation that this paper undertakes is to render explicit certain facts and points of view that the writer finds implicit in current psychological thinking. The 'law of association' as formulated and discussed by British psychologists concerned itself with ideas and sequences of thoughts and not with sequences of conscious states in general (which would have included sequences of sensory data). Such was the case with Aristotle also and with the intervening writers. Observation was warped primarily by the prevailing interests in logic and epistemology and by the tendency to overestimate the importance of vision in consciousness.

Aristotle says, *e. g.:* "Hence, when we are recollecting we keep stimulating certain earlier experiences until we have stimulated one which the one in question is wont to succeed. And just so we hunt through the sequence, thinking along from the present or some other [thought], and from similar or contrasted or contiguous."[1]

In Berkeley's writings much use is made of association, or suggestion as he terms it. And it is particularly to be noted that sequences of sensations as opposed to sequences of ideas are produced by the Deity. In David Hartley there is a thoroughgoing physiological as opposed to a logical associationism. Hartley's classical formulation is as follows: "Any sensations *A*, *B*, *C*, etc., by being associated with one another a sufficient number of times, get such a power over the corresponding ideas *a*, *b*, *c*, etc., that any one of the sensations *A*,

[1] Quoted from H. C. Warren, 'Mental Association from Plato to Hume,' Psychol. Rev., 1916, 23, p. 210.

when impressed alone, shall be able to excite in the mind *b*, *c*, etc., the ideas of the rest."[1] Hartley's physiological interests led him to apply the law specifically and in detail to habit formation. Obviously here the cases described—particularly the speech habit (see below),—must furnish data in harmony with our present contention; but Hartley never realized it. Had he or other writers done so, they must have recast their general law.

The point of view of these writers and even their formulations of the law have persisted into current writing in spite of the fact that a constantly increasing growth in the knowledge of habit and thought has been making the conventional law inadequate. The following quotations from current textbooks will indicate the present general attitude.

In Calkins[2] we find: "Successive association is the sequence of an imagination on a perception (or another imagination), a sequence which is attributed (in after-reflection) to the previous occurrence, simultaneously or in swift succession, of the two experiences." This is repeated in the table on page 359.

Titchener[3] says: "We then find this: that whenever a sensory or imaginal process occurs in consciousness, there are likely to appear with it (*of course in imaginal terms*)[4] all those sensory and imaginal processes which occurred together with it in the earlier conscious present. This we may term the law of association."

Angell[5] formulates the matter as follows: "The law of association asserts that whenever two images, or ideas, have been at any time juxtaposed in the mind, there is a tendency, if the first of them recurs, for the other to come with it." Judd and Thorndike state the law in a very general form which could be interpreted to cover our present point; but the context in each case indicates clearly that it is the old view of the

[1] Priestly, Jos., 'Hartley's Theory of the Human Mind.' London, 1775, p. 14, Prop. 5.

[2] Calkins, M. W., 'First Book in Psychology,' 4th Ed., 1914, p. 116.

[3] Titchener, E. B., 'Textbook of Psychology,' 1910, p. 378.

[4] Italics mine.

[5] Angell, Jas. R., 'Psychology,' 4th ed., 1908, p. 206.

nature of the second term of the association which is in their minds. The present thesis receives no consideration.

Since the Greeks it has been recognized that the principle of association is the principle of habit formation. But the development and interrelations of the two thoughts has ended there because of the interests above mentioned. At the present, behavior studies and the critical literature on thought afford us the ground for and even require a recasting of the doctrine of association. The revised version needs, I believe, but to be stated in order to carry conviction in many minds: *If A and B are experienced together in space or time and if later one is experienced either in sensory or in imaginal form, it tends to arouse the other either in sensory or in imaginal form.* In other words the second member of an association may be and often, if not usually, is a sensory process. By the term sensory process I would include both sensation and perception. The essential point is that the conscious state which forms the second term of the association is often conditioned by a present and on-going peripheral (sensory) activity. It is passing strange that this has not been explicitly stated and systematically incorporated before now. Ever since Ebbinghaus's tests on nonsense syllables 'associations' have been studied which have involved the reproduction or recall of sensory material (auditory-vocal-motor). Even sub-liminal associations have been tested by the saving method and otherwise with the constantly present purpose of making possible the recall in sensory form of certain material. The present formulation does not attempt to explain the fact of association, nor does it attempt to analyze the factors involved in the formation of associations (habits). It proceeds on the assumption that fundamentally 'associationism' cannot be outgrown and left behind any more than neural habits and the setting of synaptic connections can be so treated. It is therefore important that the 'law' be made adequate and general.

II

When psychologists from Hartley to the present time have attempted to sketch the neural side of the 'law of association' they have talked in brain terms and not in terms of the nervous system as a whole. This has followed by virtue of the assumption that the second term of an association must be an image (or a centrally aroused process). Hear James[1] on the neural side: "I shall try to show, in the pages which immediately follow, that there is no other *elementary* causal law of association than the law of neural habit. All the *materials* of our thought are due to the way in which one elementary process of the cerebral hemispheres tends to excite whatever other elementary process it may have excited at some former time. The number of elementary processes at work, however, and the nature of those which at any time are fully effective in rousing the others, determine the character of the total brain action, and, as a consequence of this, they determine the object thought of at the time. According as this resultant object is one thing or another, we call it a product of association by contiguity or of association by similarity, or contrast, or whatever other sorts we may have recognized as ultimate." "Let us then assume as the *basis* of all our subsequent reasoning this law: *When two elementary brain-processes have been active together, or in immediate succession, one of them, on reoccurring, tends to propagate its excitement into the other.*" In the last sentence James indicates that the 'law of neural habit,' essential for association to his mind, is one dealing with central processes.

All that this does is to read over into neural terms the faulty formulation of the law as indicated above. Introspection can at the best only note the sequences of conscious states. If these can be traced to a prior contiguity, they are cases of association. On the basis of the introspective report on the nature of the associated contents, one can then proceed to formulate the neural changes involved. The following illustrations make clear that such neural changes involve far more than the cortex,—that we are dealing with peripherally and not with centrally aroused processes: I see (A) and touch (B) an object which results in my drawing back (C), trembling (D), screaming (E), or swearing (F). Later I see

[1] James, Wm., 'Principles of Psychology, 1890, vol. I., p. 566.

(*A*) or touch (*B*) the object (or do both) and at once *C*, *D*, *E*, or *F* appear. And they may and do appear in sensory form. Here belong the so-called cases of emotional and affective memory which were so much discussed some years ago. I see (*A*) an object and it gets bound up with unpleasantness (*B*). Later when I have *A* or its image *a*, the unpleasantness returns as *B* in the concrete with all its organic and motor changes. Or again I see a word and hear it at the same time, later I see or think of the word and the sound is again produced by my vocal organs. It is hardly necessary to point out how in these cases the nervous impulses shuttle back and forth between the brain and the periphery of the body. Central arousal and images do not monopolize these situations. On the conscious side we have had a succession of sensations whose connection, reflection can trace to a prior contiguity. This is what we mean by "mental" association and this is where the law starts or gets its basis. Neurally we have had widespread activity unconfined to the hemispheres.

Current writing is emphasizing the relation of language and the vocal processes to thought. It seems clearly established that thinking can go on without detectable imagery. The 'meanings' to most psychologists (and I agree) are not 'pure thoughts,' but accompany, among other things, the kinæsthetic sensations (sensory processes) of the vocal organs. These language cases are functionally the most important instances where the second member or succeeding members of an associative train are sensations (as I should say), or peripherally initiated processes (as others would say).

It should not be objected that with the increasing automatism these peripheral processes become short-circuited and drop from consciousness. The same could be said of the imaginal members of the association possibly even more truly. It is doubtlessly the case that with both sorts of material the meaning is usually the clearest and most prominent feature; but that again is a point for any formulation of the law of association and not alone for the present one. The essential thing for us here is that the meaning can be carried, and is carried, as well by sensory as by imaginal processes and that by virtue of the internal control of their stimuli these sensory processes may constitute the second terms of an associative sequence.

Nor should it be objected that the present account fails to consider simultaneous association or persistent association, where one part of an associated complex persists over into the succeeding moment. The explanation of such cases need be no easier with images than with sensory processes. The play of muscles from moment to moment offers perhaps the clearest illustration of this type of event. Muscles 1–10, let us say, are active at a given moment and by their activity lead to the contraction of muscles 8–15. A common element, muscles 8–10, is found in each activity. If we speak from the conscious side, we have a core of kinæsthetic (and cutaneous) sensory experiences persisting through two successive moments of time. Practically this situation is always present and gives us our feeling of bodily presence and of the continuity of the self. Here again our account could be much more complete if we could formulate the causes of the setting of associations (synaptic connections). Such incompleteness, however, does not affect our essential point, nor is it a difficulty peculiar to the present thesis.

III

Sensory processes have their functional significances in the responses that they initiate. And so, stated the other way round, what adaptive movements shall take place depends very largely upon the sensory processes available. Certain of these sensory processes are practically beyond the organism's control. Certain others on the contrary are very much within the control of certain organisms and to a variable degree within the control of all. An animal's control of its own movements or responses is to a large extent dependent upon the sensory processes that it can itself initiate. Self-initiated stimuli in most animals are those of hearing, cutaneous sensitivity (pain excluded?), and kinæsthetic, organic and static sensitivity. Those that are not self-initiated are taste, smell, and vision. The last statement, of course, needs much qualification. Many animals produce odors and others have phosphorescent organs which emit light. The same may be true also of taste, particularly with aquatic animals. In man the qualification that I feel it most necessary to urge is that of visual sensations produced by intention and probably initiated by the efferent optic fibers.

Wherever we find sequences of habits, there we should expect to find stimuli under the organism's control. Otherwise in order to have a habit sequence such as speech, running a maze, or solving the delayed reaction, it would be necessary that some non-controlable stimulus initiate each link in the response. But the contraction of one set of muscles stimulates receptors which arouse the succeeding muscular activity. If this sequence is essentially constant and if the receptors condition consciousness, we have a typical case where an associative sequence is sensory in its entirety. Writers on association at the time the law was fixed upon psychology were too fascinated by vision (and logic)—and vision is a sense whose stimulus (light) is practically non-producible by man's own activity. What more natural then than that in formulating a rule of sequences, they should have said that the second terms of associations could not be sensory but must be imaginal. Indeed the last sentence is misleading. The question was not clearly enough considered for them to use the terms 'could not be' this or 'must be' that. But this error, I believe, proceeded from tacit assumptions as I have indicated. And the same assumptions, tacitly made, still persist and lead to our conventional formulations, although such formulations are as wide of the mark as I have tried to indicate. It is true that where writers come to consider kinæsthetic habits, *e. g.*, they give essentially the account that I have sketched above. But at the same time they forget that so far as the conscious side is concerned they are dealing with genuine cases of the law of association; and when formulating that law, they forget the other situation. Nowhere do they bring the two into working union. Suggestive statements are alone to be found. I cite three instances:

First, Hartley's account[1] of the growth of the speech habit:

[1] Priestley, *op. cit.*, pp. 33-34.

"About the same time . . . the muscles of speech act occasionally in various combinations, according to the associations of the motory vibratiuncles with each other. Suppose now the muscles of speech to act in these combinations at the same time that sound is produced from some agreeable impression, a mere sensation, or a slight associated cause, which must be supposed to be often the case, since it is so observable that young children, when in a state of health and pleasure, exert a variety of actions at the same time. It is evident, that an articulate sound, or one approaching thereto, will sometimes be produced by this conjoint action of the trunk, larynx, tongue, and lips; and that both these articulate sounds, and inarticulate ones, will often recur, from the recurrence of the same accidental causes. After they have recurred a sufficient number of times, the impression which these sounds, articulate and inarticulate, make upon the ear, will become an associated circumstance (for the child always hears himself speak, at the same time that he exerts the action) sufficient to produce a repetition of them. And thus it is that children repeat the same sounds over and over again, for many successions, the impression of the last sound upon the ear exciting a fresh one, and so on, till the organs be tired."

Miss Calkins also comes near making the essential point when she points out that any state of consciousness plus a feeling of generality is a concept. (". . . when I think of fear, the consciousness of generality accompanies a genuine, though certainly faint, experience of that emotion."[1]) Now since thinking consists in a sequence of concepts, or their physiological equivalents, she should have added that it is possible to have a *sequence* of internally controlled but peripherally initiated states of consciousness. And this would have forced her to re-state the law of association whose phrasing I have quoted above.

James, too, came perilously near discovering the point that I am making. There is one sentence in the very chapter on Association that gives a beautiful illustration of the fact that the second term of an association may be sensory.

[1] *Op. cit.*, p. 147.

". . . probably every one who bathes himself in a certain fixed manner is familiar with the fact that each part of his body over which the water is squeezed from the sponge awakens a premonitory tingling consciousness in that portion of skin which is habitually next to be deluged."[1] Surely only the momentum of historical usage and the absence of the new psychology of thought could have prevented James from correlating his discussions of Habit, Association and Will with reference to this point!

I would once more call attention to the semi-behavioristic background of the present discussion. Studies of the delayed reaction in young children and in raccoons (possibly also in dogs[2]) have indicated the functional presence of internal factors which can initiate responses in the absence of the accustomed external stimulus. These internal factors may theoretically be either centrally or peripherally aroused processes which are called up by the sensory stimulation due to operating the release box of the apparatus. Elsewhere[3] I have given reasons for deciding in favor of the peripheral processes. The importance of the appearance in the animal series of this ability to control adaptive muscular response by internal stimuli which need not be constantly present can hardly be overestimated. Man's language sequences are but the development of the animal form of sensory associations—a development which is centralized more and more in the vocal apparatus.

[1] James, *op. cit.*, p. 555.

[2] Walton, A. C., 'The Influence of Distracting Stimuli during Delayed Reaction in Dogs.' *J. of Animal Behav.*, 1915, 5, 259–291.

[3] Hunter, W. S., 'Delayed Reaction in Animals and Children,' *Behav. Mon.*, 1913, 2, No. 1.

THE SCIENTIFIC PRODUCTIVITY OF AMERICAN PROFESSIONAL PSYCHOLOGISTS

BY SHEPHERD IVORY FRANZ

Within the past few years there have appeared reviews of the progress of psychology for different periods of time.[1] That general progress has been made is evident. That in America progress has been made is shown by the inauguration and rapid increase in the membership of the American Psychological Association, by the foundation of journals devoted to the publication of psychological discussions and researches, by the appearance in numbers of psychological books and articles, by the great popular interest which has been aroused, by the birth and growth of numerous laboratories for teaching and investigation, by the creation of special university chairs apart from those for allied disciplines (especially philosophy and education), and by the establishment of special research laboratories for applied psychology. It is also pertinent to remark that during this developing period of psychology there have been begun and continued several lines of investigation which, although not entirely American, have continued in mass and in importance to be due to workers in this country.

Although there is abundant evidence of advance, those historians who have recounted the progress have dealt with their theme in an impersonal way. They have reported the number and the character of the published investigations, the establishment of independent departments, and the number of conferred doctorates. They have not dealt with an equally important subject which is germane to the one regarding which they have written. We have not been informed by whom the psychological advances have been made, or whether or not in view of the increasing number of

[1] See C. A. Ruckmich, 'The Last Decade of Psychology in Review,' Psychol. Bull., 1916, 13, 109–120. This contains references to previous reviews of like character.

professional psychologists there has been a corresponding increase in the number or in the value of the published investigations. In other words, although it is admitted that advance has been made, we are as far from knowing whether or not the advance has been satisfactory and corresponds with the number of psychologists.

The estimation of the value of an individual's contributions has been attempted from time to time and in a variety of ways. There is the well-known attempt to grade psychologists by votes of a few selected individuals. The general results of this grading have been reported.[1] There has also been a negative grading of psychologists in general in the elections to the National Academy of Sciences. Of the five so-called psychologists who have been members, three at the time of their election were professors of philosophy and primarily interested and concerned in the teaching and publication of philosophical (ontological, epistemological, and logical) doctrines. There has also been a continuing selection (and promotion) of men for professorships and other academic positions, which selection acts as a grading of a less obvious kind. The last kind of selection brings about two adverse conditions, one of them being that an individual selected for a position because of time-serving or personal recommendation has automatically acquired a claim to a grade to which his previous position and work did not apparently entitle him, the other being that because of this there has been a considerable amount of time-serving and inbreeding in some of our institutions.[2]

All of these methods of estimating the relative ranks of individuals have obvious and with respect to even their temporary value perhaps vital defects. Much the same may be

[1] 'American Men of Science.' Ed. by J. McK. Cattell. 2d ed. 1910. See especially pp. 537–596. The results have been given in only a general way; the names of the judges are not mentioned, and we must depend upon the editor's word that they were competent.

[2] While it may not apply to psychologists it is not unknown in scientific circles that some individuals, because of personal charm or characteristics such as self-appreciation and a hypercritical attitude toward others, have been able to attract students and have been able to persuade others of their importance and to get a self-enhanced reputation spread.

said regarding methods now in use for the determination of the value of a published research or other contribution. If the different methods were used in combination and subjected to safeguards respecting individual interests they would probably give better estimates than have hitherto been made. To take individuals at a given time and settle upon values or grades may be necessary, but it should be recognized that the grades shift at different times. The same holds true with regard to estimated grades or values of performed work. The supposed values of the nineteenth century do not hold for the twentieth, nor do those of today hold for tomorrow. It is, however, possible to determine whether or not a given individual or a group has carried out some of its functions, and to determine whether or not certain individuals have fallen short or have done more than their normal or expected share. We may assume as proven that in psychology there has been satisfactory advance in general, both in character and in quantity of the work, and we may inquire: "By whom have the advances been made, and in what manner have individuals or groups contributed to the advance?"

It has already been said that if we wish to deal with absolute value it is not possible to make satisfactory judgments, and answers to the questions could not be given. No one psychologist has sufficient knowledge of methods and results in all branches of psychology to be considered a sufficiently expert judge. Nor has any one the confidence of all or of a majority of psychologists. Each judge of values is influenced in making his judgments by considerations of training, of knowledge, and perhaps of special personal interest. There is no absolute impartiality. The introspectionist does not hesitate to say that behavior studies are not truly psychological. The behaviorist may reply that there is no such thing as the introspection that is talked about so much. He who does not hesitate to teach and write about the functions of the parts of the nervous system may never have performed or seen any of the classical cerebral experiments; by some psychologists the realm of the abnormal has never been visited and by them it is known only like uncharted parts in a geography; and the remainder of applied psychology is for many like a prostitute whose acquaintance is not desired and who is to be shunned because of fear of infection.

Notwithstanding the limitations of value of individual opinion, much might be gained by taking a consensus of opinion of those of divergent special interests and of those who are admitted to be fair jurymen.[1] But we can also do something very definite by determining that a certain individual has or has not made any published contribution towards psychological advance.[2] This is a comparatively easy method giving positive results. It admits of little or no discussion of a judge's partiality, it rests solely upon the admission of published material as the facts with which to deal. We may also determine that one has contributed by books, or by the publications of investigations, or by any other means which we may select. And there is also the possibility of answering the question, "Has the progress, as measured by the number of publications, corresponded with the number of individuals who have become professional psychologists?"[3]

[1] Not related academically to the individuals to be judged, supplied with all the data respecting the individuals (not with only a part as was done in Cattell's 'American Men of Science' classification), and of sufficiently diverse individual interests to form a jury representative of all branches of psychology.

[2] There is little possibility of determining the value to psychology of the individual's college or university teaching. Some indication might be obtained by the determination of the academic antecedents of psychologists and of those who have taken advanced courses in psychology but who have not become professional psychologists. This does not mean the determination of the number of conferred doctorates. It is often forgotten that the interest of the individual has been created before he has known of the possibility of advanced work, and the doctorates give in the main only a clue to the institutions which are recommended by those who have created the interest, or which are selected because of material advantages. Another method of determining the value of the individual teacher would be that of estimating the average value and the amount of published work which his students put forth after leaving him. Not all of their subsequent work is due to his influence but an estimate could be made.

[3] The consideration of these matters has been somewhat forced upon me in connection with editorial duties during the past few years, since it was necessary to know what lines of work were being investigated and by whom. During a longer period of time I have not infrequently been asked to offer suggestions regarding possible candidates for positions, and having had very few students fitted for such positions, I have taken the opportunity to recommend those who have exhibited some accomplishment (publication) rather than those who were known to me as individuals of 'promise' in their advanced work. The results of some of these casual inquiries regarding accomplishment were so different from what I had expected that it led to the consideration of a larger group.

We have available for the purpose well-known yearly bibliographies, and the present article has been made possible because of them. The membership list of the American Psychological Association was consulted to obtain a list of our 'professional' psychologists. Since membership in the Association is not limited to those who are instructors or professors of *psychology*, only those whose official positions consisted *solely* in relation to psychology were considered to be the professional psychologists.[1] Since those few who hold only research positions can not be dealt with in the same manner as teachers, I decided to omit their names from the list to be investigated. This left 87 names, of which three were omitted because of the lack of certain data which I thought essential. The 84 individuals represent 48 institutions, 45 as professors, 26 as assistant, associate or adjunct professors, and 13 as instructors. The academic titles mean little but they are mentioned only to indicate the range or the number of departments of psychology. Five individuals were connected with one institution, four with another, there were three in each of nine institutions, there were two in each of eleven, and one in each of twenty-six institutions. Half of the men had received their doctorate during the decade as follows: 1906, 2; 1908, 6; 1909, 6; 1910, 4; 1911, 2; 1912, 10; 1913, 7; 1914, 4; 1915, 1.

Since my immediate interests have been connected with recent publications I selected the past ten years, 1906 to 1915, inclusive, and have tabulated the contributions of each of the 84 individuals for each year. The contributions which have been listed were found in the PSYCHOLOGICAL INDEX.[2]

[1] The 1916 membership list was used. This contains the titles of positions as supplied by the members, and is presumably correct. I am convinced that some of the titles are incorrect, but the mistakes are relatively unimportant. I have not gone beyond the official returns. Some professional psychologists who are not members of the Association are not included. Some of them are known to the writer, but it seemed unfair to include them, since they are not members of the national body of professional psychologists.

[2] This bibliography does not give references to all of the publications of psychologists, some contained in inaccessible periodicals are not given, some which are not psychological are not noted, but the failure to list all the psychological publications of any one individual rests solely with that individual, since the INDEX asks that omissions

The different kinds of contributions have been listed under the following titles: Monographs, Original Articles, Discussions, Books, General Reviews, Reports of Meetings. A few translations of American books into foreign languages were omitted, on the ground that the original authors had already made the contribution and the work of translation was that of another. Translations of foreign books into English were also omitted from consideration. In some cases it is difficult to decide whether a certain publication is a discussion or an original article, in other cases whether a publication be an original article or a monograph, or a monograph or a book. Some periodicals publish long articles which in other series would be published as monographs, but they have been dealt with as they stood. When doubt arose as to the character of the publication the contributor was given the benefit of the doubt.[1] The inclusion of general reviews and reports of meetings may need justification. They have appeared to me to be legitimate methods of advancing psychology because they may create interest to investigate certain matters or give facts to others which would not ordinarily be discovered, etc. Their value, however, must be considerably less than those of publications containing the results of personal investigations. Somewhat similarly with books, since they are largely compilations of the work of others.

Table I. gives the results of the examination of the INDEX for the individuals and the decade under consideration. Here there are shown the numbers of each kind of publication for each year and for the ten-year period. It will be seen that with the exception of a few lean years (1907, 1914) the number

and corrections be supplied. A few mistakes were noted, when they were obvious they were taken account of in the tabulations. The tabulations by years are not always exact on account of the inability of the INDEX compilers to secure all the references for a particular year at the time of publication, but titles omitted in one year are found in the next number.

[1] That is, as will be noted below, an artificial value was assigned to each kind of publication, and when doubt arose the higher value was assigned. At the same time a 'joint' article, etc., was assigned to both individuals, and given in each case its full value. I was at first inclined to reduce the article value for 'minor contributions,' but I concluded to let the above consideration hold. The recent publication of the Proc. Nat. Acad. of Sciences was another difficult matter. The 'articles' in that journal are in reality abstracts or summaries (*Auto-referate*).

TABLE I

KINDS OF PUBLISHED CONTRIBUTIONS OF PROFESSIONAL PSYCHOLOGISTS FOR THE DECADE, 1906–1915, INCLUSIVE

	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915	Totals
Monographs............	1	3	2	2	5	5	5	10	6	6	45
Articles................	46	36	43	49	42	44	59	58	39	76	492
Discussions............	7	4	5	7	2	8	9	12	9	3	66
Books..................	2	1	10	6	3	6	8	5	9	5	55
Reviews................	4	1	4	3	4	29	32	25	26	33	161
Reports................	1	0	4	3	4	6	3	2	4	5	32
Totals................	61	45	68	70	60	98	116	112	93	128	851

of research articles and monographs has not differed very greatly from year to year and there is a fairly gradual increase. There is a slight increase in 1912 and 1913 over the preceding years and a greater increase in 1915 over 1913 and 1912. The notable decrease in 1914 is not explained.[1] It may be that the increase of interest in the subject of tests is responsible, those who had taken up this line of work being occupied in 1913 and 1914 with the accumulation of facts which were published in 1915.[2] When we look at the yearly totals we note that the increase began in 1911, and this was coincident with the inauguration of the series of general reviews in the PSYCHOLOGICAL BULLETIN.

Table II. shows the distribution of the contributors over the ten-year period. It is a remarkable fact that the first five years are almost constant in number of contributors and that the increase in the number of contributors has taken

[1] A writer in the *New York Times* book review supplement has mentioned that in general literature 1914 was a lean year. The reason is not obvious in that case or in the case of our psychological publications, since the European war could not have its effect on production until very late in the year. In connection with psychological journals this effect should have been more noticeable in 1915 if it existed because of the war.

[2] Ruckmich reports over 800 *original* articles for the decade 1905–1914. Probably the number in 1905 was nearer that for 1906 than that for 1915 of the present table. On that assumption we would have about 500 titles of *original* articles and monographs for that decade by professional psychologists. Not all of these have appeared in the magazines listed by Ruckmich, and in comparison with the 'over 800' noted by Ruckmich it is apparent that fully 3/8 of the grand total was contributed by the non-professional psychologists, or by those whose loyalty was divided between psychology and philosophy, education, etc.

Table II

Grouping of Professional Psychologists According to Yearly Number of Publications, for the Decade, 1906–1915, Inclusive

	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915
1 contribution	15	16	12	11	17	14	20	17	19	28
2 contributions	10	7	8	8	8	9	13	13	13	12
3 contributions	3	3	5	6	3	7	5	5	8	6
4 contributions	0	0	1	2	2	0	2	5	2	6
5 contributions	2	0	1	1	2	6	1	3	2	1
6 contributions	0	1	0	2	0	0	2	2	1	2
7 or more contributions	1	0	2	0	0	2	4	1	0	2
Totals	31	27	29	30	32	38	47	46	45	57

place during the past five years. The greatest number of contributors is found in 1915, the year of the greatest number of contributions. This table also shows what may be termed the scientific activities of psychologists for it gives the number of publications of groups of individuals. For the past five years about 30 per cent. of those who contributed published three or more articles, etc., each year. This is, of course, not to be taken to mean that the same individual did this from year to year, although it may be mentioned that the tabulation of the material indicates that the man who does it one year is more apt to repeat with more than a single contribution for the following years.

Since not all of the individuals on our list have been, nor could they be expected to be, active in publication during the whole of the decade it is of interest to compare the figures which are given in Table II. with the examination of the years of the doctorate or other higher degree which is held. There were 42 individuals who had obtained their higher degree anterior to 1906.[1] The other 42 could not be expected to publish work before their doctorate, although some did so. Counting the date of the doctorate as the date when publication might reasonably be expected, the numbers of individuals expected to publish were obtained by adding the new doctors to the original 42 at the beginning of the decade. It has already been mentioned that some published before the doctorate, the number being 14 (out of a total of 42), and a few published more than one article.

[1] Two individuals on the list hold the Master's degree only.

When now we compare the number of expected contributors with the actual number of contributors we find an interesting condition. Table III. gives the data. Although the number of expected contributors increased during the first five years the yearly number of actual contributors did not increase. The increase in contributors came during the second half of the decade, but the percentage of expected contributors who published is the same (68 per cent.) at the beginning and at the end, there being a decrease in the percentage up to the year 1910 and an increase thereafter. If we separate out those contributions which are intended to convey new facts or new interpretations,[1] which would limit us mainly to articles and monographs, we find an even more

TABLE III

THE PUBLICATION ACTIVITIES OF PROFESSIONAL PSYCHOLOGISTS FOR THE DECADE, 1906–1915, INCLUSIVE: THE NUMBERS OF EXPECTED CONTRIBUTORS, OF ACTUAL CONTRIBUTORS, OF THOSE CONTRIBUTING ARTICLES AND MONOGRAPHS, AND THOSE CONTRIBUTING PREVIOUS TO DOCTORATE

	1906	1907	1908	1909	1910	1911	1912	1913	1914	1915
Expected contributors	44	44	50	56	60	62	72	79	83	84
Actual contributors	30	26	26	28	29	37	43	45	45	57
Contributors of articles and monographs	28	24	23	26	28	27	30	34	35	40
Contributors previous to doctorate (additional)	1	1	3	2	3	1	4	1	0	0

interesting comparison. At the beginning of the decade there was a total of 44 individuals expected to contribute. Of this number 28 made contributions of articles and monographs (64 per cent.). The percentage (actual contributors in relation to expected contributors) decreased in the following years as follows: 55, 46, 46, 47, 44, 42, 43, 42, 48. The contributions anterior to the doctorate by those who contributed previous to their doctorate are not included in these calculations. It is to be observed, therefore, that the percentage of original contributors has decreased and the percentage of total contributors at first decreased and later reached its original figure. The great differences in the percentages from 1911 to 1915 inclusive are to be understood

[1] It is not intended to say that books, discussions, reports or reviews do not contain new facts and new explanations, but that they are less apt to do so.

primarily as the result of the publication of general reviews and an apparent satisfaction of the authors of these reviews in their accomplishment.

The subject of books deserves a separate paragraph. These were mainly the work of the group of older men, for of the 55 books which have been published in the decade only five have been written by those whose doctorates were granted in 1906 and subsequently. Two men were responsible for one book each, two for two books each, four for three books each, one for four books, and one for fifteen books. The years of publication of the 55 books are shown in Table I.

Mention has already been made of the increase in the expected contributors owing to the granting of the doctorate. The original 42 names have had added to them an equal number. Of these additional 42, 14 published previous to the doctorate. The first publication after the doctorate, in most cases dissertations or parts of dissertations, of 18 was made in the year of the doctorate, of 10 the first publication was in the year following the doctorate, of 9 in the second year, of 1 in the third year, of 2 in the fourth year, and of 1 in the fifth year after the granting of the doctorate. No publication by one who received the doctorate in 1913 had been made up to and including 1915. These figures show that either our means of publication are insufficient, or that the dissertations presented in partial satisfaction of the doctorate are not nearly ready for (or are not worthy of) publication, or both.[1]

The 42 individuals who had received the doctorate prior to 1906[2] might each have contributed something in each of the ten years under consideration, but only seven did so. Seven others contributed in 9 of the ten years; 4 in 8; 4 in 7; 6 in 6; 1 in 5; 2 in 4; 3 in 3; 2 in 2; 2 in only one of the years; and 4 had no publications. Those who received the doctorate in 1906 and subsequently can not be dealt with in the same man-

[1] It might be well for university authorities to deal a little more strictly with the matter of publication. The publication of a dissertation in part or as a whole is the only evidence to the world outside of the particular university that the individual has shown a capacity for investigation, one of the main doctorate requirements in all institutions of which I have knowledge.

[2] Including the two who have not taken the doctorate.

ner, but the groups in accordance with the percentage of years in which contributions were made, counting the total of years since the doctorate as the expected total, are as follows: 0, 1; from 1 to 10 per cent., 7; 11 to 20 per cent, 5; 21 to 30, 4; 31 to 40, 5; 41 to 50, 13; 51 to 60, 1; 61 to 70, 1; 71 to 80, 3; 100 per cent., 8. The number of older men who averaged at least one contribution for every two years, or more often, is double that of the younger men. The younger men had more than two thirds of their number who did not publish as much as one contribution for every two years. It should further be stated that of the younger men placed in the most regular class (100 per cent.) one contributed original work in only one of 8 years, a second in only two of 6 years, a third in six of 7 years, and a fourth in two of 3 years. The other 4 contributed an article or monograph in each of the expected years. Of the older men in the 100 per cent. class, one failed to report original work in only one of the ten years, one did not report such work for two of the years, and two for three of the years. Three contributed at least one article or monograph in each of the ten years.

Comparing the two groups we find that of the 420 expected individual years of publication of the older group (10 years each for 42 individuals) there were only 257 individual years of publication, a percentage of 61; of the younger group there were 214 expected years and an actual total of 109, a percentage of 51. This difference is entirely accounted for by the long delay in publication after the doctorate, for if the delay periods be subtracted from the total there is a percentage of 61 for the younger men. Although there is not a sufficient number of years to make the conclusion certain, the figures would lead to the belief that when the younger men start there is not a great difference in total productivity between them and the older men. My impression previous to tabulation was the reverse, probably because of some notable examples of productivity of the older men. The latter is counterbalanced by the fact that there are five of our professional psychologists, four of the older group and one of the younger group (the latter case mentioned above), who have not published anything worthy of citation in the PSYCHOLOGICAL INDEX in ten years.

The individual differences which have been mentioned are better shown when we compare the totals and yearly averages for the individuals of the groups. It would not be expedient to mention names of individuals, or to designate them in recognizable terms, so that we must fall back upon generalities of individual differences. Of the older group there are four who did not make a scientific contribution of such a character or in such a journal as to be deemed worthy of mention in the INDEX in the decade; there is only one of the younger group. In addition the contributions of one of the older

TABLE IV

DISTRIBUTION OF PROFESSIONAL PSYCHOLOGISTS ACCORDING TO THE NUMBERS OF CONTRIBUTIONS IN THE DECADE, 1906–1915, INCLUSIVE

Number of Publications	Total Publications		Articles and Monographs	
	Older	Younger	Older	Younger
0	4	1	5	1
1 to 10	14	35	16	38
11 to 20	12	4	16	2
21 to 30	5	1	4	1
31 and over	7	1	1	0

TABLE V

COMPARISON OF TOTALS OF PSYCHOLOGICAL CONTRIBUTIONS BY THE OLDER AND YOUNGER GROUPS OF PROFESSIONAL PSYCHOLOGISTS FOR THE DECADE, 1906–1915, INCLUSIVE

	Articles	Monographs	Books	Discussions	General Reviews	Reports	Totals
Older	369	19	50	55	109	18	620
Younger	123	26	5	11	52	14	231

group consisted exclusively of the class of general reviews and reports. The accompanying tables show the distribution of the men in accordance with their scientific-literary productivity. In Table IV. there are shown the numbers of individuals in each group in relation to the total number of publications and in relation to the publications of articles and monographs. In Table V. there are shown the different kinds of publications (articles, monographs, books, etc.) according to the groups. The average total number of contributions for the decade by the older men is 14.8, for the younger men it is 5.5; the average of articles and monographs by the older men is 9.2, and by the younger men only 3.5. These figures may be somewhat misleading if taken as they stand, on account of the difference in the total number of years that might be expected for scientific publication. The total years for the older group has already been said to be 420 (42 individuals for 10 years), and 214 for the younger group. When the comparison is made of total publications and of original (monograph and article) publications of the older group and of the younger group in relation to the expected number of years it is also found that the older group outranks the younger. Thus the individual yearly average for total contributions for the older group is 1.5, and only 1.1 for the younger group; and the individual yearly averages for articles and monographs are respectively .92 and .69 for the older and the younger men.

It is interesting to speculate on the reasons for these differences. Doubtless in most institutions the younger men are employed a greater part of the time in preparation of materials for the laboratory work of students and in the grading of themes, etc. In the smaller and less well endowed institutions there is less aid for the prosecution of investigations, and if aid can be obtained it takes an exorbitant amount of time to get the administrative machinery in running order. At the same time the younger group has to take considerable time in the preparation of material for their courses of instruction, and they are more frequently called upon to act as subjects or assistants for other research workers. On the whole the older group has the advantage of long established policies, of equipment, and of professional and mechanical assistance. At the same time the labor of teaching is correspondingly less on account of the previous experiences. Perhaps if we should compare the work of the older group, or of as many of them as held positions in the decade of 1891 to 1900, with that of the present younger group we should not find as great differences as now exist.

On account of the differences in time opportunity it is to be expected that the younger group would publish less in total and that the individual total would be less than many of those of the older group. Thus we find that nearly one half (19) of the older group reached or exceeded the average of 1.5 publications per year, and that only one third (12) of the younger group equalled or exceeded the yearly average for that group (.5 publications per year). Table IV. gives the results of the groupings. This shows that more than one half of the older men averaged one publication per year, seven having more than three per year The figures for the younger group must be read in the light of the total yearly expectation (214 instead of 420). Each individual should be considered in relation to the total of his expected years of publication. Thus the six individuals of the younger group who exceeded ten publications had a total publication expectation of 44 years. The total of publications was 124, which gives a yearly average less than 3.

The data with regard to the character of the publications of the two groups are given in Table V. The difference in the number of books has already been mentioned. Monographs are apparently the prerogative of the younger man, the newly created doctor, for he has in proportion to the expected years two and one half times as many as his older colleague. The older men publish 33 per cent. more articles, as has been mentioned, they take part in more controversies as judged by the number of discussions, and they contribute an equal share of the general reviews. With regard to the last the remark may be necessary that the general review is most frequently due to 'request' and not infrequently a request to the elder is declined in favor of a younger colleague.

Since the different kinds of contributions are so varied no direct comparison may be made of individuals except in terms such as have already been used (total number of contributions, number of monographs and articles, and the relation of these to the expected years of publication). An indirect comparison may, however, be made if we assign to the different classes of publications an arbitrary numerical value. This must be very arbitrary on account of the impossibility of making good comparisons of values, as has already been explained. It is also to be kept in mind that no arbitrary value for a class can be defended in particular cases, for if we consider two articles we may immediately note that one deals with an investigation in which some new methods have been used and the analysis of factors appears to be well wrought out, and we may also find that the second consists of a confirmation of previous work by the use of the same methods which had previously been used. Even in two articles which contain new facts or demonstrate new methods we may find similarities and divergencies of completeness or of apparent originality. It favors the majority of low grade (if we may use such a term) publications if we assign the same arbitrary value to all of one class, and this I have done. The values which I have selected for the different classes of publications are as follows: Reports of Meetings, 1; General Reviews, 2; Discussions, 3; Books, 6; Articles, 6; Monographs, 9. No justification of these arbitrary values will be attempted, but the following were in mind when the values were assigned. A discussion is often of no observable value in adding to our knowledge, and scientifically is worse than useless when it takes on the character of a personal attack. On the other hand, it may tend to clear up doubtful points, bring up new ways of viewing a situation, and at the same time by pointing out gaps in our knowledge indicate lines of investigation. In so far as a discussion does any of the latter things it appeared that it has a real value beyond that of a report of a meeting, and since a discussion also tends at times to inter in a suitable manner some supposed facts which very generally, but erroneously, have been accepted it was thought worthy of a greater value than that of a general review. It is assumed that all of our psychological discussions are of the good character mentioned. Differences exist in general reviews. Some are summaries of a few contributions of others. Some give a fairly complete account of current work with an evaluation of the material and thus help others who are not specializing in the subject to obtain a better view than would be obtained by looking over the mass of details which the original sources contain. Some of the general reviews which we are considering here may be worthy of a higher value than some of the discussions, but many might bear a reduction of the general value figure. Here again it became necessary to decide for the majority rather than for the few. Much the same may be said with regard to books. Although the writer does not pretend to have the specialist's critical ability in every branch of psychology he is satisfied that many books published during the period are nothing more than general reviews, and at times poor ones. Some of the books have brought out new facts and explanations, they have added considerably to our psychological advance. But because of their general character it is thought that a value double that of a discussion and triple that of a general review would be an ample average value. The original, or research article (whether experimental or otherwise) was assigned a value equal to that of a book, and the monograph (but only on account of its length and supposed completeness) was assigned a value fifty per cent. higher. The remark previously made regarding monographs, that some monographs would be articles in other publication series, is a point against such a valuation, and the valuation is not insisted upon. The increase in value is on the side of the younger men, who most need the extra count.

Having selected the arbitrary values which have been mentioned it becomes possible to make comparisons of the output of different individuals without making special reference of an identifying character. At the same time it becomes possible to make comparisons of the groups since the heterogeneity of the different kinds of publications has been translated into a homogeneity.

The calculation of the individual values for the ten-year period shows that the range is from zero to 244. This means that some have contributed nothing and that others range from the zero point up to a valuation of 24.4 points per year. It is necessary to translate all the total valuations of the younger group into 'expected' values, or average yearly values in accordance with the number of expected years of publication. When this is done for all individuals we find that there is a general average of 6.7 points per year for the group as a whole, and a median of 4.2. When the five individuals who have not contributed even as much as a report of a meeting during the decade are omitted the average is 7.2 and the median is 4.5. The distribution of the 84 individuals in respect to average yearly values of publications is given in Table VI. Eighteen of the older group and twenty-five of the younger group are below the median of all values; 22 of the older and 30 of the younger groups are below the average of the total. Arranging all in sequence of average yearly values and dividing into four equal parts, each containing 21 men, we find the following distribution of the older and younger respectively in the groups from lowest to highest: older, 10, 7, 11, 14; younger, 11, 14, 10, 7.

TABLE VI

GROUPING OF PROFESSIONAL PSYCHOLOGISTS ACCORDING TO AVERAGE YEARLY "VALUES" OF CONTRIBUTIONS FOR THE DECADE, 1906–1915, INCLUSIVE

Values	1.0 and Under	1.1–2.0	2.1–3.0	3.1–4.0	4.1–5.0	5.1–6.0	6.1–7.0	7.1–8.0	8.1–9.0	9.1–10.0	10.1–15.0	15.1–25.0
Number of men......	9	14	9	9	9	2	0	5	5	4	9	9

The older group on account of time, material equipment, academic relations, and other conditions has advantages which make it of special interest. All of the individuals had attained their higher degrees previous to 1906, about three quarters are heads of departments, and about the same number are connected with well-equipped and long-established laboratories. This group furnished four individuals who did not make a published contribution of any kind to psychological advancement in the decade. Six of the group contributed one original article or monograph during the ten-year period; two contributed two original contributions; two contributed three; and three contributed four. We have in this group, therefore, forty per cent. who have not averaged an original contribution once in two years. Some of these seventeen individuals did contribute in other directions besides monographs and articles, for they published 5 books, 33 general reviews, 2 discussions, and wrote one report of a meeting. Besides the four who did not make any kind of a contribution there were four additional who made no contribution beyond the original articles and monographs published. On the whole the younger group, while not as productive as the older, show better results with respect to the publication of individuals. Only one (duration three years) has not published, as is mentioned above. Ten others have not published as frequently as once in three years; there is a total of sixteen who have not published as frequently as once in two years.

Since the older group had the opportunity to publish for ten years we need not deal with averages entirely, but may consider totals as well, on account of the homogeneity of the series. It is of interest to know that seven of the group (17 per cent.) contributed a total of 159 articles and monographs (41 per cent.); the highest half of the group (21) contributed eighty-seven per cent. of all the articles and monographs of the group (337 articles and monographs). This leaves for fifty per cent. of the older men only thirteen per cent. of the articles and monographs published by the group.

The majority of older men who have contributed little in the way of articles and monographs, and also to the total, have held their present positions for many years, and have apparently 'grown up' in their present locations, they are located in some of our better endowed institutions, and in those with good laboratory facilities, they have colleagues teaching in the same lines, and they occupy what may be properly called positions of prominence in their respective institutions. They have not the apparent disadvantages of isolation, or of having to carry the burden of the psychological world upon their shoulders since there are colleagues to help in teaching and perhaps in research. But these are some of the men who are representing psychology as a science in their respective university niches. Those of the younger group who are least productive, and have given little evidence of interest in psychological advance by publication, are mostly located in the smaller institutions, where there are no colleagues of sufficient training or productiveness to be elected members of the American Psychological Association. There are in the younger group exceptions to this, several notable cases being evident when the list is inspected. Without going into the figures for total or original publications, since the number of men at the different institutions is small, it may be stated that those younger men who have the opportunity to remain at a long-established department have done better than those in the more recently created departments. But this does not hold for particular cases, since there are surprising exceptions of individuals with apparently all the advantages which can be obtained in the better endowed institutions doing little or nothing which is prepared for the edification of their scientific colleagues.

It should not be assumed, and it is here stated to the contrary in order that there may be no misunderstanding, that these men are doing nothing for psychological advance. Some may have editorial duties, some may conceal themselves in the work of their students, and some (like Herbert Spencer) may be reserving their energies for some *magna opera* which will be given to the world in due time. It seems unlikely, however, that as many as 40 per cent. of the older group are engaged in the accumulation of material for the development of a cosmology, or of a system of psychology, or of an exhaustive history of the science, or of other large projects which should not be laid aside in favor of the minor contributions such as articles and monographs. It is apparent that a few of those who hold chairs of psychology are contenting themselves with teaching and the carrying out of the social obligations which fall to the lot of every scientific man, who must meet his colleagues and take part in the life of the university with which he is connected. Whether or not there are more psychologists who are doing these things than scientific men in other lines can not now be determined. And, the other view may be expressed that those who are contributing much do so without proper scientific care and because of reportorial tendencies. The character of the work of many of our most productive men shows that they have not done their work at the expense of care. But, the writer feels that some of the so-called 'professional' psychologists should be classed with dilettantes; they are not scientific professionals in the sense that they are forging ahead and that they are succeeding because of their efforts in scientific work.

Much has been written recently about university positions and university control; the professor always being the oppressed and the university as represented by the president being the oppressor. I have a feeling that part of the dissatisfaction may be due to the 'great promise' for which candidates have been recommended never showing up in practice. A president of one of our leading institutions has been criticized for saying that every man on the faculty may be expected to publish at least one article every two years. It may be that he had had experience with those of 'promise' and not of performance. Much has also been written about academic tenure, as if that was a sacred right (or rite), but the critic might well imagine a better state of affairs to follow in some of our universities if there was a power of recall for those members of the faculty who do not measure up to the expected or to the average amount of performance. If the present methods of filling positions, from the grade of professor down to that of instructor, be continued we shall always have some of little or no accomplishment (the dilettantes) filling important chairs, especially if there be the added conditions that promotions be made as vacancies occur. It is the writer's belief that much of the inconsistencies would be eliminated if every vacancy were advertised as open to application, stating that applicants were expected to send in the accounts of their academic careers, copies of publications, etc., and that these would be graded by a non-interested committee of specialists (advisory board) who would report to the faculty or to trustees the results of their findings. We should probably escape the absurdity of having in one year two young men recommended from one university as 'geniuses of the first water.' This has happened, and the writer is not certain which is to be the more pitied, the university which permitted its 'geniuses' to go to other institutions, or the universities which would accept men who were recommended as such.

Another matter may be worth considering briefly. This is the relation of productivity to the institutions from which the higher degrees were received. In pointing to psychological advances in institutions, we have hitherto been content to mention the number of doctorates which have been granted. But it is obvious that numbers count for very little in progress, unless one is going to play a mass game or overrun a weak nation. What should be considered is the question: What have the doctors done after leaving an institution? And we may also ask: Has the training which is supposed to fit the man for research been effective in stimulating him to efforts in that direction? Part of these questions may be answered by considering the performance of those granted higher degrees by different institutions. Of the 84 men on the present list 17 were scattering with respect to institutions, but the remainder were distributed over seven institutions. Only one institution is mentioned by name (Leipzig), the other individual institutions (American) are represented by letters. The 17 scattering cases are grouped together as 'other foreign' and 'other American.' Table VII.

TABLE VII

COMPARISON OF PUBLICATIONS OF ALL KINDS AND OF ARTICLES AND MONOGRAPHS, BY GROUPS OF PROFESSIONAL PSYCHOLOGISTS, ARRANGED ACCORDING TO INSTITUTIONS CONFERRING DOCTORATES. (TWO INDIVIDUALS WHO HAVE NOT RECEIVED THE DOCTORATE ARE INCLUDED UNDER THE INSTITUTIONS WHICH CONFERRED THEIR HIGHER DEGREES)

Institutions	Leipzig	*A*	*B*	*C*	*D*	*E*	*F*	Other Foreign	Other American
Numbers of men	8	11	7	15	11	8	7	6	11
Total expected years	78	79	54	132	74	58	42	57	60
All publications:									
Total credits	859	674	421	884	381	226	103	422	269
Credits ÷ years	11.0	8.8	7.8	6.7	5.1	3.9	2.5	7.4	4.5
Range of average yearly credits of individuals	3.6–18.6	1.5–19.3	1.1–23.0	0.0–24.4	1.4–14.0	0.6–11.3	0.0–7.5	0.6–9.4	0.0–10.2
Articles and monographs:									
Credits	603	510	360	708	312	198	96	348	222
Credits ÷ years	7.7	6.5	6.7	5.4	4.2	3.4	2.3	6.1	3.7

gives the numbers of men from different institutions, the total years under consideration (the total 'expected' years of all in the special group), the total credits, the relation of credits to the total expected years of publication, and the minimum and maximum average yearly credits by the individuals in the groups.

Probably the total expected years, rather than number of individuals, is a better means of comparison with the totals of publications, and the relation of these two are shown in the fifth line. Here it will be observed that the 8 men from Leipzig far surpass the groups from the other universities. They have the highest minimum, showing that each is publishing fairly regularly, and although they are surpassed in maxima by three other institutions this is largely due to a few very productive men. The men from Leipzig have mostly been of the older group, as indicated by the average of expected years (9.8), but the average is not much greater than that (9.5 years) for the group from Other Foreign Institutions. Leipzig also closely approaches the total of *C*, although the number of men is only slightly over half, and the total number of expected years is about 60 per cent. of this American university. The order of institutions in total credits divided by expected years is as follows: Leipzig, *A*, *B*, Other Foreign, *C*, *D*, Other American, *E*, and *F*. When now we compare the performance of the doctors from the different institutions in relation to publication of articles and monographs we find the results shown in the last two lines of Table VII. Here again Leipzig shows a great superiority. The order of institutions in credits divided by years has not materially changed, there being a reversal of the order for *B* and *A*. The remainder of the table speaks for itself.

One matter remains to be briefly considered. This is the relation of administrative work to the carrying on of scientific production. Our group contains five individuals who have given their occupation as partly that of dean or president. Two of these individuals notwithstanding their arduous administrative duties have managed to carry on investigations and to publish the results of them. At the same time they have both for total and for original publications managed to be in the more productive class. The other three probably have their administrative duties as reasons for their relative non-performance. In the group there are also eleven who have greater or less editorial duties in connection with the publication of periodicals. Of this group two have been below the average in performance. Both of these extra-scientific duties (administration and editing) have not interfered with the production of an average amount of scientific articles by two thirds of the men thus engaged, and it may be that these added functions have been beneficial rather than the reverse. In the estimation of productivity of individuals editorial functions have been omitted and had these been added to the totals in some numerical way it is quite likely that more of the group of editors would have approached the top. As the arrangement now stands, counting only articles, books, reviews, etc., we find that two thirds are in the higher half of the distribution, and of these most are in the higher quarter.

It is pertinent to remark that psychology appears to be getting from those whose chief interests are not in its development (from the non-professional psychologists, in other words) as much as, if not more than, from many of its own men.[1] From some of its own psychology is receiving much less than should be expected, and there is, perhaps, some reason for the assertion that "were it not for an academic title it would be difficult to discover the reason why certain individuals are called psychologists." In conclusion the attention of the reader is called to the consideration of the wisdom of the action of certain scientific societies which require that a member shall retain membership in them only as long as he continues to show an active interest in the advancement of his science by publication, provided continued ill-health or other disabling conditions (old age and the like) do not prevent.[2]

[1] I expect to deal with the psychological contributions of these non-professional members of the Association in another article.

[2] A constitutional amendment of this character was recommended by its council to the American Psychological Association at one of its meetings, and at the subsequent meeting the council reversed its recommendation (see the Proceedings of the Association for the years 1906 and 1907).

THE PSYCHOLOGY OF THINKING IN THE CASE OF READING

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If a person is presented with a paragraph to read and questions about it, his responses provide useful material for studying some of the facts and laws of thinking. Consider, for example, the following task and the following responses made to the first question by pupils of grades 5 and 6:

I

Read this and then write the answers to 1, 2, 3, 4, and 5. Read it again as often as you need to.

Nearly fifteen thousand of the city's workers joined in the parade on September seventh, and passed before two hundred thousand cheering spectators. There were workers of both sexes in the parade, though the men far out-numbered the women.

1. What is said about the number of persons who marched in the parade?

....

2. Which sex was in the majority?

....

3. What did the people who looked at the parade do when it passed by?

....

4. How many people saw the parade?

....

5. On what date did the event described in the paragraph occur?

....

Two hundred people
Three thousand
Thousand
Eighteen thousand
Two thousand
Five thousand
Ninety thousand
Twenty-five thousand
About thirty-five thousand
Nearly twenty thousand
More than ten thousand
There were about 25000
200,000
It was 200,000
About two thousand
Maybe No. 12
About 2700
Two hundred thousand spectators workers in the parade
Two hundred thousand spectators
Two hundred cheering
Nearly 115000 on Sept. of people
Nearly sixteen thousand
Hundred thousand spectators
It is said about the number or group of people
It is said that they are great
A very great deal

A lot of people
Congregation
There were a great lot of men
The men outnumbered the women
The men were more than the women
There were more men
They outnumbered the women
There the par on number the
The men were far ahead of the women
Men and woman
Citizens
They were workers
There were workers of both sexes
Workmen in the parade
Of all the working men
That the city workers joined the parade
Workers joined the parade
That they rejoin in the parade
They were joined
A number of workers joined the parade
Joined the parade
Workers join
They joined
They pass two hundred spectators
Before the spectators
Passed before two hundred thousand spectators
They two hundred thousand cheering spectators
Passed before 200000 and 15000
They passed nearly 5000
Passed before two hundred spectators
They marched before cheering spectators
Three thousand cheering them
People of both sexes cheering them
They are cheered
Parade before two hundred spectators
Parade spectators
They marched nice
They marched very nice
They kept in step
They marched very straight
They did good or bad
They look so nice
They clap their hands when they see the American flag
They keep their step and many others
There character
Honorable and good
The people said the parade large
Most of them were old
They are soldiers and marched
They say halt
The captain says march
There was a lot of floats
The people are killed by the war
The meddles
September seventh
Irish

The variety of responses to this one fairly unambiguous question is a challenge. There is a challenge also in the relative frequency of the different responses.

I shall report here some general facts which are displayed by some hundreds of responses to each of a dozen or more sets of questions upon a paragraph, which I have examined. The first is that:

When a question on a paragraph is answered, any one word may be over-potent in determining the response. As a limit we have the case where a word produces a response due to that word alone irrespective of all else in the situation. Or, more generally, any element in a situation may be over-potent to any degree.

The evidence supporting this claim is the existence of answers or elements in answers which could come as probable results or over-potent action of single words, but whose occurrence otherwise is highly improbable. For example, in the case of the words of the paragraph such influence is seen of:

thousand, in this answer to 3: "The people cheered thousands."
city, in this answer to 2: "City workers."
workers, in these answers to 2: "Workers," "Workers of sex"; and in this answer to 3: "There were workers."

In what follows a number in parentheses preceding a response designates the question as an answer to which the response was given.

joined, in (1) "They joined," (1) "They were joined," (3) "They joined in," (3) "They were joined in the parade," (3) "They cheered then joined in."
parade, in (2) "in the parade," (2) "Both in parade," (2) "Sexes in the parade."
September, in (2) "There were workers of the September."
seventh, in (2) "Seventh," (3) "September seventh," (3) "Seventh Avenue."

Lest the bizarre nature of some of these errors lead the reader to fancy that they are fragments, or answers misplaced a line too high or too low, I may note here that every quotation that has been or will be given in this article is, unless specially noted at the time, a complete answer, as given by some pupils, and undoubtedly intended for the question whose number it bears. Quotations are exact except that the first word is capitalized whether or not this was done by the pupil.

passed, in (3) "Two hundred thousand cheering spectators passed."
spectators in (1) "Parade spectators," (1) "Cheering spectators," (1) "Two hundred thousand spectators workers in the parade," (2) "Spectators and working," (3) "The people who looked cheered the spectators," (3) "The people looked and cheered the spectators," (2) The sex spectators.
cheering, in (2) "Cheering."
both, in (2) "There were both workers," (3) "There were workers in both spectators."
sexes, in (3) "Six in the parade," (3) "Sixes," (3) "Cheered the sex in the parade."

though, in (2) "Though the men marched in the parade," (2) "Though the men far outjoined parade."
men, in (3) "They cheered the men in the parade."
far, in (2) "Far out women."
out, in (3) "They counted out how many women."
numbered, in (3) "The men numbered women," (3) "Numbered the women," (3) "They numbered the people."
out-numbered, in (2) "Sex out numbered of women," (2) "Out-numbered."
women, in (2) "Sexes of women," (2) "Men and women," (3) "The women."

Any phrase or other part of a sentence tends in a similar manner to produce a response due to that phrase per se.

Thus, *fifteen thousand* appears as an answer to questions 2, 3 and 4 and (in "Passed before 200,000 and 15,000") as an answer to question 1. The following are responses probably explainable by the independent action of phrases or other groups of words.

the city's workers, (2) "City workers."
joined the parade, (1) "Joined the parade," and (2) "They joined the parade."
September seventh, (1) "September seventh."
passed before two hundred, (3) "Passed before two hundred."
two hundred, (1) "Passed before 200 spectators," (2) "Two hundred spectators," and (4) "Two hundred."
hundred thousand, (4) "About a hundred thousand."
two thousand, (1) "Two thousand," and (4) "Two thousand."
two hundred thousand, (1) "Two hundred thousand," (2) "Two hundred thousand," and (3) "Two hundred thousand."
There were workers, (2) "There were workers."
of both sexes, (1) "People of both sexes cheering them," and (5) "Sept. 7th both sexes."
There were workers of both sexes, (1) "There were workers of both sexes," and (3) "There were workers of both sexes in the parade."
though the men, (3) "Though the men far out joined parade," and (5) "Thought the man fat out."
outnumbered the women, (1) "They outnumbered the women," and (2) "Sex outnumbered of women."

In the same way it can be shown that every word and every word group in a question tends to produce a response due to that word or word-group *per se*. Consider for example the responses from pupils in grades 7 and 8 to Test *M*.

M

However certain it may seem to be that men work only because they must, and would avoid labor except for the food, clothing and luxuries that are its rewards, the facts may well be to the contrary. It can hardly be the case that men dislike work because they wish to be utterly idle. For mere rest, mere inactivity, is not commonly enjoyed. To have nothing to do is not what men seek. Were that so, we should envy the prisoner shut up in his cell. If men had to choose between a life spent at eight hours of work daily in a factory and a life spent at eight hours of sitting on a throne without moving hand or foot, many of them would, after trying both, choose the former. Activity of body or mind, at which a man can succeed, is, in and of itself, rather enjoyed than disliked.

1. What is it that this paragraph says may seem sure, but probably is false?

....

2. In what respect is a prisoner in his cell like a man with a million dollars?

....

3. If the absence of any activity were what we wished for, what would be our attitude toward a prisoner in his cell?

....

4. What is stated in the paragraph to be really liked and not objected to?

....

5. What choice is described in the paragraph as an argument that work, merely as such, is not always avoided?

....

In question 1 the influence of *Paragraph* is seen in such responses as:

"Work,"
"Labor,"
"Idle men and working men,"
"Idleness and wealth," and
"Men that do not work."

The influence of *Paragraph says* is seen in:

"It says that men should not avoid labor,"
"It says that some men work and some of them would be idle," and
"Some men envy prisoners in a cell rather than work."

The influence of *Sure* is seen in:

"For mere rest mere inactivity is not enjoyed."

Similar evidence in the case of questions 2, 3, 4 and 5 is as follows:

QUESTION 2

prisoner in his cell—"Because he is shut up in a cell," and "A man in prison is sitting 8 hours daily with chains."

QUESTION 3

activity—"Is rather enjoyed than disliked," "For mere rest, mere inactivity is not enjoyed," and "A man succeed or rather enjoyed than disliked."
absence of activity—"Sluggish," "To have nothing to do is what men seek," and "Idleness."
prisoner in his cell—"He would not be there unless do something wrong," and "Because it is not a good thing to be a prisoner."

QUESTION 4

is stated—"Men who work," and "That the American man wants to work."
really liked—"Good clothing and luxuries," and "A hard work man should be liked."
objected to—"Mere rest mere inactivity is not commonly enjoyed," "Inactivity," and "To have nothing to do."

QUESTION 5

choice—"The man who works and the one who does not."
described—"Why men work."
avoided—"They would avoid labor if it was for food."
work avoided—"Idle and stealing."

Consider also the following responses to questions 1, 2, 3 and 4 on paragraph *J*.

J

In Franklin, attendance upon school is required of every child between the ages of seven and fourteen on every day when school is in session unless the child is so ill as to be unable to go to school, or some person in his house is ill with a contagious disease, or the roads are impassable.

1. What is the general topic of the paragraph?

....

2. On what day would a ten-year-old girl not be expected to attend school?

....

3. Between what years is attendance upon school compulsory in Franklin?

....

4. How many causes are stated which make absence excusable?

....

5. What kind of illness may permit a boy to stay away from school, even though he is not sick himself?

....

6. What condition in a pupil would justify his non-attendance?

....

7. At what age may a boy leave school to go to work in Franklin?

....

1. "Every paragraph must have a period"
"A group of complete sentences" } due to *paragraph.*

2. "Monday"
"Wednesday" } due to *day.*
"Friday"

"The ten year old girl will be 5a," due to *ten-year-old girl.*

3. "It was a great inventor"
"Because its a great invention" } due to *Franklin.*

"Because it is a small city," due to *Franklin.*

4. Twenty five
about ninety } due to *How many.*
2000

A boy should bring a note
If you bring a note } due to *absence excusable.*

In the illustrations given so far the action of the element has been accompanied usually by some vague action of the situation as a whole, but cases may be found where this reduces to about as near zero as is possible, provided the pupil writes any answer whatever.

Consider, for example, the following response to questions 1, 2, and 5 on paragraph *I* (on page 220).

QUESTION 1

"Most of them were old," which shows almost no effect of anything save the "*persons.*"

"There were a lot of floats," which shows almost no influence save of "*parade.*"

"Irish," which could fit any paragraph or any question provided *parade* or *persons* or both occurred therein, almost as well as it fits this.

QUESTION 2

"The chief commander of all" and "Captain and lieutenant" seem to show no influence save the *major* of majority plus a faint effect of *parade.*

QUESTION 5

"1492" and "1776" seem the products of *date* in total neglect of all else in the question and paragraph.

Some of the cases above, if taken alone, are perhaps as explainable by other causes as by the tendency of each word to act irrespective of the total of which it is an element, but no one, I think, will assume that the collection can be well explained save by supposing that the single words do have that tendency. We must then think of the pupil who examines the paragraph and the questions as beset by a tendency to answer each question by each word in the paragraph and have each word in the questions evoke a response that is bound to it alone. Most of these tendencies are of so nearly zero strength that they almost never compete successfully with other tendencies; and most of the resulting thoughts are so absurd that if they come to mind they are promptly dismissed. But a sound theory must accept their existence, traces of which we have illustrated in the case of Paragraphs *I*, *J* and *M*. Every element in a situation tends to arouse the response which is connected with it.

Just as any element of the situation may be, relatively to others, far too potent in determining the response, so also it may be not nearly potent enough. Calling Pa the potency of element a and Pb the potency of element b, Pa/Pb may vary between 0 and ∞ as limits.

The following are some cases of under-potency in the case of *I* (page 220):

QUESTION 1

nearly—(Failure to include this in the response to 1 is of course very, very common.)

fifteen—"Thousand"

what is said about the—"Honorable and good," "They march very nice," "They marched very straight," "They did good or bad," and many similar responses.

number of persons—"They were workers," "Men and women," "That they rejoin in the parade," "Passed before cheering spectators."

who marched in the parade—The many responses of "200000," "They cheered them," etc.

All of question 1 except *parade* is under-potent—"There were a lot of floats."

QUESTION 2

the "out" of outnumbered—"A number of women."

which—"Both sexes was in the parade," "There were both sexes there," "Workers of both sexes," "Men and women," "Two sexes," "Two of them."

sex—"City workers," "City workers of N. Y.," "The chief commander of all," "Working," "The front ones," "Spectators," "Cheering," "Fifteen thousand."

in the majority—"Women," "Sex outnumbered of women."

which . . . was in the majority—"The sex spectators," "Sexes," "In the parade," "Sexes in the parade," "There were men of other sex in the parade."

QUESTION 3

what did the . . . do—"They were cheered by the people," "Two hundred."

people—"Tip his hat."

people who looked at—"Passed before two hundred," "Passed before a number of cheering spectators."

when it passed by—"Two hundred thousand cheering spectators passed."

it—"They saluted them," "They cheered them."

cheering—"Inspected the parade," "They were glad to see it," "They talked about it" (and many others).

All save *parade* underpotent—"September seventh," "Seventh Avenue."

QUESTION 4

saw the parade—"The men outnumbered the women," "Far outnumbered."

two—"About 100000," "One hundred thousand," "Three hundred thousand."

hundred—"Two thousand."

thousand—"Two hundred."

two hundred [thousand]—"Fifteen thousand," "Nearly fifteen thousand," "Over 25000," "Over five hundred," "About 10000," "About 5000," "About 1000."

QUESTION 5

what date—"There were workers of both sexes in the parade," "Thought the man fat out," "Described," "Sexes of the parade," "The parade," "And outnumbered women."

event described in paragraph—"March 4, 1915," "March 17," "April 23, 1903," "November 4," "December 4," "On Friday," "March 17," "March 18," "St. Patrick's day," "On the twenty-second of February," "St. Pattac," "1492," "1776," "1820."

Seventh—"September seventeenth" (a common error, often due to misperception or memory, probably) "September."

In any situation we may distinguish in a rough way between words meaning things, qualities and events on the one hand and words meaning relations between them on the other. Thus we may think of question 2 below as:

$$\underbrace{\text{[(Use } \textit{of} \text{ a gas range) } \textit{instead of} \text{ (use } \textit{of} \text{ a coal range)]}}_{\textit{effect upon}}$$

$$\overbrace{\text{temperature of the kitchen}}$$

H

You need a coal range in winter for kitchen warmth and for continuous hot-water supply, but in summer when you want a cool kitchen and less hot water, a gas range is better. The xyz ovens are safe. In the end-ovens there is an extra set of burners for broiling.

1. What two varieties of stoves does the paragraph mention?

....

2. What effect has the use of a gas range instead of a coal range upon the temperature of the kitchen?

....

3. What is needed to provide a supply of hot water all day long?

....

4. For what purpose is the extra set of burners?

....

5. In what part of the stove are they situated?

....

6. During what season of the year is a gas range preferable?

....

These relating elements are subject to over-potency and under-potency like any others, but especially to under-potency. Thus in the question quoted, we have as responses due to under-potency of the *instead of:*

"The stove makes heat" and "You need a coal range in winter and a gas range in summer."

As responses due to under-potency of *effect upon*, we find:

"You can cook better," "Heats quicker," "Because it is summer," and "Because we need no coal."

As responses showing under-potency of both *instead of* and *effect upon*, we have:

"Gas to cook and coal to get warm," and "Gas range in summer."

Elements may act in substantially correct potency but out of their proper relations. Responses to arithmetical problems will illustrate this richly. In reading we have such cases as "A cool kitchen is used for a gas range" in response to *What effect has the use of a gas range instead of a coal range upon the temperature of the kitchen?*

The connections leading from any element or group of elements may be wrong whether the element is under-potent, of correct potency, or over-potent. They may be *wrong in toto* in the sense of leading to unserviceable responses to that element for any purpose, or *wrong for any defined set of purposes*, or *wrong for the test's purpose* in the sense of leading to responses unserviceable for the particular need or problem. Thus "A complete sentence" is wrong for almost all purposes as a response to *paragraph;* "Commas and periods" is perhaps even more universally futile; "A group of sentences" is wrong for the particular purpose of answering the question of *J* 1.

Inadequacy may be considered as a special case of wrongness by lack or insufficient connections, and failure of response as the limiting case of inadequacy.

Incorrect thinking due to wrong or inadequate bonds leading from one or more elements of the situation is a simple consequence of the general facts of connection-forming that does not need demonstration here. We know *a priori* that every element tends to call up what has followed or accompanied it.

I will report only three illustrations. The paragraph being

John had two brothers who were both tall. Their names were Will and Fred. John's sister, who was short, was named Mary. John liked Fred better than either of the others. All of these children except Will had red hair. He had brown hair.

and the questions being

5. Who had brown hair?..................................
6. Who had red hair?.......................................

there are with pupils of all grades from the third to the eighth a large per cent. number of responses of "John" and "Fred" to "*Who had brown hair?*" and 30 per cent. of responses of "Will" to *Who had red hair?* Under-potency of *All the children except* caused the formation of the connection of *Will* with *red hair*. This wrong connection not only produced the large crop of errors to question 6, but also worked back to prevent the correct answer to question 5.

To question 1 on paragraph *J* we find the following answers, all being more or less wrong connections leading from the element *paragraph*.

"The sentence,"
"A sentence that made sense,"
"Period,"
"Capital,"
"A capital letter,"
"To begin with a capital,"
"Subject and predicate,"
"A letter,"
"In every paragraph must have a period,"
"Capital letter in the first letter,"
"Commas and periods,"
"The paragraph man was marsh."

To the question, on paragraph *T*, *What do you think "heaven's azure" means?* we find the following answers from college freshmen, in whom right bonds leading from *azure* are either absent or so weak as to be suppressed by the wrong (for the purpose) bonds leading from *heaven* or from the general sense of glorified moralizing which the passage establishes. Only about one college freshman in three thinks of so plain a thing as the blue sky!

T

But it is to you, ye Workers, who do already work, and are as grown men, noble and honorable in a sort, that the whole world calls for new work and nobleness. Subdue mutiny, discord, widespread despair, by manfulness, justice, mercy and wisdom. Chaos is dark, deep as Hell; let light be, and there is instead a green flowery world. Oh, it is great, and there is no other greatness. To make some nook of God's creation a little fruitfuller, better more worthy of God; to make some human hearts a little wiser, manfuller, happier,—more blessed, less accursed! It is work for a God. Sooty Hell of mutiny and savagery and despair can, by man's energy, be made a kind of Heaven; cleared of its soot, of its mutiny, of its need to mutiny; the ever-lasting arch of Heaven's azure overspanning it too, and its cunning mechanisms and tall chimney-steeples, as a birth of Heaven; God and all men looking on it well pleased.

"The everlasting rainbow,"
'"Peace,"
"Peace, universal,"
'Peace progress and justice one to another,"
"Peace and the world purged of strife,"
"Peace and happiness love and manliness,"
"The peace and love of man's fellow men,"
"The peacefulness one sees in a God-fearing community,"
"Harmony in life,"
"The millenium,"
"Appreciation of the beauty of nature,"
"Wisdom mercy,"
"Happiness,"
"Enjoyment,"
"The happiness and contentment that comes+" (+ here and later means that more was contained in the response; but nothing correct),
"God's love,"
"God's influence,"
"God's approval of work,"
"God's creation,"
"A world in which +,"
"A place where men +,"
"The light of the world,"
"The light of a new method—of freedom and happiness,"
"Man's outlook on life,"
"High ideal +,"
"The clearing of mutiny,"
"Perfect condition of things with every one happy and working for the best,"
"The work that man has accomplished,"
"Eternity,"
"Eternal life,"
"Eternal blessing,"
"Love your neighbor as yourself +,"
"God is love. Do unto others as ye would have others do unto you."

We must then think of a pupil who answers the questions concerning a paragraph as beset by tendencies of each word and word group in the question to assume undue potency, to become dislocated from its proper relations, and to call up its past accompaniments and sequents. For him to answer rightly means that an elaborate hierarchy of bonds is active and that an intricate set of forces maintains a balance of power. One may become directly aware of at least a part of this complex coordination and subordination of tendencies if he will note just what it implies to respond correctly to this question on paragraph *T: What does the author refer to as a "Sooty Hell of mutiny and savagery and despair"?* One may infer it less directly but more adequately by surveying the hundreds of different tendencies to respond which a paragraph and question evokes in a thousand pupils and realizing that almost or quite all of these tendencies were present as truly in any one successful pupil, but were prevented from determining final response by some organization within themselves or by some guiding tendencies from without.

Sometimes the correct balance or organization reduces to the simple case of letting one element be as potent as it may and reducing the potency of all other elements to negligible amounts. We then have the case of reasoning of which James has given the classic description. But the task of thought is, it seems, not usually to choose only one element in the situation for potency, or to accept one only of the facts evoked by that element. Usually there are many elements to be let work together and many evoked facts to be used for the purpose at hand. In our illustration, the "workers," "sooty," "mechanisms" and "tall chimney" all need to be given potency to secure the response of "a region of factories," "a manufacturing community," "a factory town disturbed by labor troubles," or the like, and the "God," "chaos," "Hell" and "Heaven" of Carlyle's grandiloquence all need to be somewhat tempered in their tendencies to call up the world, human nature or other things of vast scope and great moral importance.

Elaborate as are the compositions of forces which give thought its final motion and direction, the forces themselves are of simple nature, being elements in situations and connections leading from these elements to responses which use and (in my opinion) satisfying accompaniments have yoked to each element.

Three simple mechanisms—under-potency and over-potency of elements, dislocation or disrelation of elements, and wrongness or inadequacy of connections—seem to be all that are needed to explain errors in thinking. Conversely, proper balance and organization of elements and right bonds therewith seem to explain correct thinking, no matter how elaborate or subtle. Thinking and reasoning do not seem to be in any useful sense opposites of automatism, custom, or habit, but simply the action of habits in cases where the elements of the situation compete and cooperate notably.

It is of course the case that, along with the balanced action of elements, there goes an inspection and validation of them and the ideas or acts they evoke, whereby each succeeding situation is often amended by increasing or reducing the potency of certain of its elements, and whereby certain futile ideas may be cast away entirely. These welcomings and rejectings, retainings and letting go, are however themselves nothing more than situation-response bonds, where the response is attending to or turning from, cherishing, repeating, saying *no* or *yes* to, or the like. It is also the case that the "set" or adjustment of the organism plays a more striking rôle in reasoning than it does in mere day-dreaming or routine habit-action, but not a different sort of a rôle.

I conclude therefore that the general laws of human behavior which explain why a pupil puts his clothes on or off and eats or leaves uneaten his breakfast explain why he succeeds or fails in making geometrical demonstrations or scientific researches, and that there exists no fundamental physiological contrast between fixed habits and reasoning.

THE SIMILARITY OF BROTHERS AND SISTERS IN MENTAL TRAITS[1]

BY DANIEL STARCH

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In this study, information was sought on two questions: (1) To what extent are adult siblings (children of the same parents) alike in mental characteristics and (2) is the similarity greater in those mental traits which are directly affected by training in school work than it is in those traits which are not directly affected by school work?

Some experimental work has been done on these problems with siblings and twins of various ages below adult life.[2] The purpose of the present study was to investigate the mental resemblance of adult siblings, and with these problems in mind two series of tests were carried out.

I. For measuring mental functions which are directly affected by school work, the following tests were used: Speed and comprehension of reading ability, size of reading vocabulary, speed and quality of hand-writing, and ability in spelling. These tests were made in exact accord with the methods described elsewhere[3] and hence no further details will be given here. Abilities in the four fundamental operations in arithmetic were measured by the Courtis tests, Series *B*.[4] Ability in arithmetical reasoning was measured by the writer's scale ('Educational Measurements,' p. 114).

II. For measuring mental functions which are not directly affected by school work, the following tests were selected: Two perception tests were used: (*a*) The well-known A-test

[1] This investigation was planned by the writer. The tests themselves and the computation of the results were made very carefully by Clara Fuller Taylor.

[2] Thorndike, E. L. 'Measurements of Twins,' *Archiv. of Phil., Psychol., &c.*, No. 1.
Starch, D. 'The Inheritance of Abilities in School Studies,' *School and Soc.*, 2, 608–610.

[3] Starch, D., 'Educational Measurements' (Macmillan), pp. 20, 38, 60, and 89.

[4] Courtis, S. A., 'Manual of Instructions' (Detroit), p. 58.

which determines the number of A's cancelled in one minute, and (*b*) a geometrical form test which determines the number of a certain geometrical figure cancelled in one minute on a page of similar forms. Memory was measured by the number of words that could be recalled after hearing a series of ten monosyllabic nouns read at the rate of one word per second. Motor capacity was tested by the tapping test in which the task consisted in tapping with the right hand, as rapidly as possible for thirty seconds.

Each test was carried out twice on two different days with a group of eighteen pairs of adult siblings who were students in the University of Wisconsin. The ages of these persons ranged from nineteen to thirty-two. The tests were repeated so as to obtain more reliable measurements of the capacities involved than a single test would yield.

The results may best be presented in the accompanying table of coefficients of correlation computed according to the formula

$$r = 1 \frac{6 \text{ sum } d^2}{n(n^2 - 1)}.$$

In arranging the pairs of siblings, the older ones of the different pairs were placed on one side and the corresponding younger members were arranged on the other. The scores made by each person in each test were then tabulated opposite each person's name. The older members of the various pairs were then ranked by themselves in each test and the younger ones were likewise ranked by themselves. On the basis of these ranks the coefficients of correlation were computed and found to be as follows:

Reading—speed	.51
Reading—comprehension	.64
Writing—speed	.72
Writing—quality	.46
Size of reading vocabulary	.07
Spelling	.05
Arithmetical reasoning	.38
Addition—attempts	.71
Addition--rights	.44
Subtraction—attempts	.43
Subtraction—rights	.29

Multiplication—attempts	.37
Multiplication—rights	.25
Division—attempts	.46
Division—rights	.56
Average	.42
Memory	.31
A-Test	.50
Geometrical form test	.07
Tapping	.65
Average	.38
Coefficients based on ranks in all rests combined	.73

Several interesting results appear in this table. (1) The resemblance of siblings is apparently no greater in those mental traits which are directly affected by school work than in those which are not so affected. The average correlation in the former group of tests is .42 and in the latter .38. This seems to indicate that the mental similarities of children of the same parents are due primarily to heredity rather than to similarity of environment since the resemblance is no greater in those traits which are more directly affected by environment.

(2) The resemblance of siblings is approximately as great in mental traits as in physical traits. Pearson found the correlation between brother and brother in height to be .50 and in cephalic index (ratio of length to width of head) .49. These correlations for physical traits are a little larger than the ones found here for mental traits taken separately. The correlation, however, calculated on the basis of a combined rank for each person in all mental tests together was found to be .73. This greater correlation for all tests combined as compared with the correlations for single traits is due partly to the variation of the correlations among the single traits and partly to the imperfections in the separate tests, which are counterbalanced to some extent in a combined ranking.

(3) Apparently the resemblance is greater in some traits than in others. Spelling ability, range of vocabulary, and perception of geometrical forms seem to be correlated very slightly; whereas speed in writing, speed in tapping, and speed in addition seem to be correlated very closely. To what

extent different mental traits are correlated by greater or less amounts can not be stated with confidence on the basis of the present tests. Further measurements are necessary.

The chief significance of the present results consists in further corroborating the notion that the mental make-up of human beings is as much a matter of heredity as their physical make-up and that environment plays a relatively small part in producing the resemblance of closely related individuals.

A METHOD OF RECORDING ERRORS IN FORM BOARD TESTS

BY E. K. STRONG, JR. AND EDWARD P. GILCHRIST

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The length of time required to perform a Form Board Test has been, in most cases, the principal element in measuring the performance. Various efforts have been made to utilize the error-element also. But most of these have not led to anything of real value except to the originator of the scheme, himself. Yet all who have used form board tests have come to realize that they learned as much, if not more, from the way the test was performed as from the time it required. The reason the errors have not been used more has been due to the difficulty of recording them in the few seconds one has at his disposal.

Miss Schmitt[1] has recently very vigorously protested against using time as a measure of efficiency and has substituted in its place an analysis of the 'method of attack.' She reduces the child's method to one of three types: 'planned,' 'trial and error,' and 'chance.' The writers believe that this analysis should lead to some very desirable additions to our present methods of evaluating a child's ability, but they are not prepared as yet, to accept Miss Schmitt's rather arbitrary method of determining what is a 'planned' or 'trial and error' performance. However, her position raises again the great need of devising a method by which one can keep track of the various movements by which a child solves the form board test. The following method has been found useful in this connection and is recorded here as a possible help to others who have been confronted by the same problem.

Suppose we are working with the Healy-Fernald Construction Test *A*. This test can be solved by making five

[1] Schmitt, C., "Standardization of Tests for Defective Children," Psychol. Monog., No. 83, 1915.

moves and in three seconds (our best record): or it may be solved after a great number of moves and after a long period of time. Ordinarily five or ten minutes is considered sufficient time in which to test the child's capacities. Now, if one uses coördinate paper, illustrated in the plate, he may draw a diagonal line upwards and to the right for every 'placement'; a horizontal line to the right for every 'rearrangement'; a diagonal line downward and to the right for every 'removal.' The record illustrated in the plate tells this following story. Three blocks were picked up, one after the other, and placed in the rectangle: two rearrangement moves followed; one block was removed from the frame;

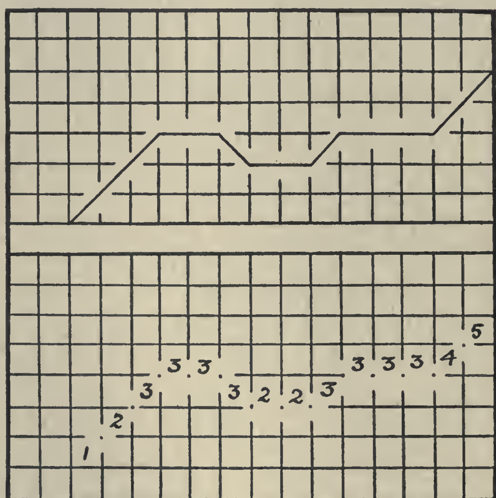

Fig. 1. Showing Method of Recording Movements in Solving Healy-Fernald Construction Test *A*.[1]

two rearrangements of the two remaining blocks followed; another block was placed in the rectangle; three rearrangements followed; the last two blocks were placed in position.

[1] The lower cut should have been so prepared that the two numerals "2" in the center of the cut and the three uumerals "3" near the right of the cut should be placed on their respective horizontal lines instead of above them. If this had been done the distinction between a numeral on a diagonal and one on a horizontal line would appear more clearly to the eye.

This record can be summarized as follows: Placements—6; Removals—1; Rearrangements—7; Total—14.

If one cares to go into further detail in his analysis of the movements, he may write a number from one to five instead of drawing the horizontal or diagonal lines; each number standing for a block. In this way, just what block was involved in the movement may be recorded. Such a record will appear as in the lower part of the figure.

Note: The blocks are numbered in accordance with their length; blocks four and five being, of course, identical. From the record it is clear that block 1 was correctly placed; block 2 was not correctly placed; block 3 was also not correctly placed; it was moved twice and then removed from the rectangle; block 2 was then moved twice, the second time being placed correctly; then block 3 was replaced incorrectly and moved three times, when it was finally correctly placed; then blocks 4 and 5 were placed correctly.

A slight amount of practice enables one to think the correct number when a block is touched; also to write the numbers uphill when they are being placed, downhill when being removed, and on the line when they are being moved about. And all of this can be done while keeping the eyes for the most part upon the subject.

DISCUSSION

INTROSPECTION VERSUS THE SUBCONSCIOUS

Do not the following experiments show that an assumption lies at the base of the conclusions drawn from certain introspective data regarding the mental processes involved:

1. An experienced observer is given a number, say 6, to multiply by 3, and he answers 18, having had no visual nor auditory images nor indeed any thing, so far as he could determine, to give to protocal. But if he is previously told to observe the steps of the process in multiplying 9 by 4 he often reports a visual image $\begin{array}{r} 9 \\ \underline{4} \\ 36 \end{array}$ and perhaps also auditory and other images in connection with the solution of the problem. That is, in conducting the experiment in the second way the steps of the process appear.

2. Something similar is also observed often when an experienced observer is instructed to reverse the hands of a clock, that is, he has nothing to report except the correct answer until he has been told to observe the steps of the process with greater attention.

3. The same thing came out in a striking way when a student investigating recognition once complained to me that an experienced reagent with whom he was working was evidently a very careless observer as he had little or nothing to give to protocal. After the reagent had been told of the complaint and instructed to observe more carefully the experimenter informed me that this reagent's introspections had become more detailed, as regards the steps of the process.

4. In reaction time experiments the introspections are usually much fuller after the observer has been told to observe what occurred in the Vorperiode, etc., that is, when instructions have been given similar to those given by Ach in his reaction time experiments. That is to say, in the above experiments, the steps of the process were observable only after the reagent had been directed to note them, that is, after they had been drawn from under the threshold of consciousness through the method employed.

As to whether the steps of the process as they appear in the second mode of conducting the experiments just mentioned are the same as in the first mode, one may suppose, (1) they are or (2) they are not or (3) may have no definite opinion regarding the matter. If one supposes the processes are the same, he is assuming, is he not, that the processes above and below the threshold of consciousness are the same, that is, that the conscious and the subconscious are fundamentally one; if, on the other hand, he rejects this idea altogether he holds the opposite view regarding the relation between the conscious and the subconscious; if he does not know what conclusion to draw from the data obtained by the two different methods he has no definite opinion regarding the relation of the conscious to the subconscious. It would seem, would it not, from the data given that the opinion one holds regarding the relation between the conscious and subconscious has a somewhat greater significance in introspective work than has been generally supposed? In short, that our opinion of the value of some introspective data depends upon our view of the relation between the conscious and the subconscious, in fact, of the subconscious itself.

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THE MNEMONIC FEAT OF THE 'SHASS POLLAK'

Some years ago, through the kindness of my friend Professor Hollander, of the Johns Hopkins University, my attention was directed to a special achievement in memorizing which I venture to report; since, so far as I know, it has remained unnoticed by psychologists, and yet should be stored among the data long and still richly gathering for the study of extraordinary feats of memory.

The facts of the case I can hardly do better than to allow the witnesses themselves to state. And first the Reverend Dr. David Philipson, of Cincinnati, to whom I was first referred by Professor Hollander.

"The Babylonian Talmud" he has been good enough to write me, "consist of twelve large folio volumes comprising thousands of pages. All the printed editions of the Talmud have exactly the same number of pages and the same words on each page. This must be borne in mind in order to understand the remarkable feat of memory about to be described. There have been, as there undoubtedly still are, men who know the whole text of the Talmud by heart. Some years ago one of these men, a native of Poland, was in this country. I witnessed his remarkable feats of memory. Thus, one of us would throw open one of the volumes of the Talmud, say the tractate Berakhot, at page 10; a pin would be placed on a word, let us say, the fourth word in line eight; the memory sharp would then be asked what word is in this same spot on page thirty-eight or page fifty or any other page; the pin would be pressed through the volume until it reached page thirty eight or page fifty or any other page designated; the memory sharp would then mention the word and it was found invariably correct. He had visualized in his brain the whole Talmud; in other words, the pages of the Talmud were photographed on his brain. It was one of the most stupendous feats of memory I have ever witnessed and there was no fake about it. In the company gathered about the table were a number of Talmudic experts who would readily have discovered fraud had there been any. The technical name which was used by the Jews of aforetimes to designate these memory experts was Shass Pollak; Shass is the abbreviation for the Hebrew terms for the Talmud, and Pollak is Pole; nearly all these memory experts came from Poland; a Shass Pollak then is a Pole who has memorized the entire contents of the Talmud and is able to give exhibitions of his mnemonic powers like those mentioned above."

And next let me quote from Judge Mayer Sulzberger, of Philadelphia, who in answer to my inquiry, wrote as follows:

"I have met but one 'Shass Pollak' in my life. He was brought into my library one evening by a friend. I conversed with him and experimented upon him.

"After he had been introduced as the expert in question I expressed some curiosity with perhaps a mien of incredulity. He was eager for the fray.

"You are of course aware that all (or nearly all) modern editions of the Talmud are paged alike and printed alike, each page beginning and ending with the same word in all the editions.

"I went to the case and took out a volume of the *first* edition which has its own paging *not* followed by the other editions. He made an automatic dive for a word in a particular part of the page, and lo! it was not there.

"Confounded by this unexpected event, he thought at first that this was not a Talmud I was showing him; and when convinced finally that it was, seemed to bear it some resentment for its improper behavior.

"I then brought out the corresponding volume of an ordinary edition and he undoubtedly made good.

"He would take a pencil and merely glancing at the page put it down anywhere and without looking told the word on which his pencil had lighted. This he did over and over again. There is no reasonable ground for the suspicion that he saw the words. I watched him closely and am convinced that he did not. He had, I feel sure, a perfect image of the page and the position of every word on it in his 'head.'"

Finally, let me give the testimony of Dr. Schechter, of New York, the late President of the Jewish Theological Seminary of America—testimony the more interesting in that while it depends upon the recollection of an experience many years ago, yet it is an independent account of the same kind of testing which Dr. Philipson reports—namely, by pricking through the pages—and consequently confirms the opinion of Judge Sulzberger that the success of the 'Shass Pollak' who was tested merely by pencil was not due to a sly catching of the word by eye.

President Schechter stated to me by letter that once he had come across a 'Shass Pollak' but that it was too long a time ago to give an account of him with definiteness. "It is at least forty-five years since the incident occurred," he wrote. "What I remember was that he could tell you the contents of every page of the Talmud by heart. I remember also that the people amused themselves by prying a needle into any volume of the Talmud, and he could tell exactly the word on which the needle touched. But I also recollect distinctly that it was nothing more than a verbal or rather local memory, the students all maintaining that he knew very little about the meaning of the contents, their interpretation and application. I heard afterwards of many similar 'Shass Pollaks,' but it is a fact that none of them ever attained to any prominence in the scholarly world."

This absence of any scholarly grasp of the contents thus memorized, of which President Schechter speaks, also appears in the judgment of Dr. Philipson. "I looked upon his achievement at the time I witnessed it as purely mechanical," he writes. "It is quite likely that he could not interpret the Talmud though he knew its contents by heart." And Judge Sulzberger, when proposing to his 'Shass Pollak' that he use his knowledge to some scientific or literary end, was listened to with respect, but nevertheless received the impression that such proposals were deemed by his man to be nonsensical.

All of which confirms the oft-repeated observation, that such extraordinary powers of memory may exist in a kind of intellectual disproportion where there is no corresponding development of other powers—where, indeed, there may be an actual stunting of other powers and interests; as though the mind had 'run' to memory, and been enlarged here at the expense of other functions.

As to the more precise amount of matter that was memorized, it should be noted that a page of the Babylonian Talmud consists, as my colleague Dr. Popper, has pointed out to me[1], of the text proper, called the *Gemarah*, and printed as a more central portion on the page, and of a commentary printed below and around this text. Upon special inquiry whether the mnemonic feat applied only to the *Gemarah* or included also the Commentary, Dr. Philipson

[1] Professor Popper has also referred me to the articles "Talmud" and "Mnemonics" in *The Jewish Encyclopedia* for evidence that at one period the Talmud was handed down solely by memory. The feat of the Poles here recounted may therefore be regarded perhaps as the survival of a custom among early Jewish students in many and widely-separated communities. The work of Brüll, *Die Mnemotechnik des Talmuds*, Vienna 1864, should also be cited.

states that the test which he witnessed was upon the *Gemarah* only; and Judge Sulzberger is of the opinion that this was also true in the case that came under his observation. Even so, the task must have been a stupendous one; the amount of reading-matter upon each page is still great, and the number of pages is enormous.

In closing may I express my thanks, in which other students of psychology will certainly unite with me, to the gentlemen who have so generously given the facts above recorded.

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The Psychological Review

VARIETIES OF PSYCHOLOGICAL EXPERIENCE[1]

BY JOSEPH JASTROW

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A commemorative occasion justifies a retrospective, though hardly a reminiscent mood. The personal justification lies in the fact that I speak as one of a small group—in this and all countries—who have held a monogamous professorship of psychology for a quarter century. The contrast of then and now stands forth partly as a shift in intellectual temperament, partly as a diverging succession of interests. Both are responsible for the historical moving-picture, which to our near vision still flickers by reason of imperfect fusion.

The dominant interest under which I began to profess psychology was clearly the experimental one; it set a novel and a positive programme. Equally assertive was the physiological plank. The two stamped one's alliance, in a sense made one a partisan. The emblem of the one was the laboratory, of the other an evolutionary faith and a sense of the reality of the body in the affairs of mind. They might have been emblazoned as a Hipp chronoscope rampant, and a copy of Darwin couchant. The bearer of this coat-of-arms was in many quarters under suspicion. He was more than a radical, less than a renegade. By implication he was challenging the accredited "mental science" of the colleges, which was a branch—in some cases a stunted twig—of philosophy, and a perquisite of the president. At educational gatherings—even more inconsequential and vaporous then than now—he was asked to defend the superiority of experi-

[1] Address given on the occasion of the celebration of the twenty-fifth anniversary of the American Psychological Association, New York, December 28, 1916.

mental over rational psychology; also to indicate what was the fate of the soul under the new regime. To the philosophers the psychologists seemed needlessly young and irresponsible; to the men of science they seemed out of focus or tangential. Now that the philosopher and the biologist have become our ready allies, the attitude toward us as militant invaders is almost forgotten, and may well remain so.

The credentials upon which psychology added a star to the united sciences were significant ones; it advanced from territorial governorship to statehood by patent of ancient right reëstablished under successful pioneering. It was not—as in the case of sociology—a squatter settlement in unoccupied land where the adjoining sciences failed to meet, but the declaration of independence of a domain quite too large and distinctive for colonial status. The first insistence was naturally upon technique. The novel sight, which to some was amusing, was not the philosopher descended to earth from his mythical habitat in the clouds, but actually donning overalls and using his hands. The garb seemed strange and lowly, even grimy. But rapidly enough the new psychology—like the new woman—became a more familiar and less forbidding *Erscheinung*, even revealing the eternally human traits. Problems followed the eclectic clue of technique. The early Wundt was compilational rather than systematic, though the Teutonic *Gründlichkeit* extended to architectural plan as well as building material. Wundt set the interests of the first group of American students of modern psychology, in which ancient and honorable body I may claim a place. The rallying point was the Johns Hopkins University—itself as new as psychology—under the leadership of the author of "Aspects of German Culture," G. Stanley Hall. He, however, found a small group of students already waiting; he quickly attracted others, and then (after five years) transferred his influence to Clark University. Stanley Hall's doctorate was the first given in psychology; Harvard University, 1878. My own was the second and the first given at Johns Hopkins specifically in psychology.

In so far as American psychology was a native product, it reflected the pioneering spirit, the spirit of William James. No speaker on a commemorative occasion would forego the privilege of placing a wreath upon his monument. He was the first among us to bear the title professor of psychology; he holds that position not by priority but by preëminence. He stands as the exponent of the value of varieties of psychological experience. He made clear in his own person the intimate dependence of pursuit upon temperament. It was peculiarly fortunate for the recognition of the new psychology that its academic status was assured by the acknowledged leadership of so commanding an academician, so distinguished a scholar, so great a man. It was equally fortunate for the career of psychology in America that the stamp of the Jamesian genius pervaded its progress and directed its unfoldment to the desirability of seeing psychology steadily and seeing it whole.

The development of a new discipline in a new country encounters directly the social situation. Of first consequence was the accredited recognition by the universities and colleges; which meant the establishment of courses that could play a worthy part in the curriculum. In the conflict of classics and science, which in the days I am recalling was reaching its declining and conciliatory stage, psychology occupied an advantageous position, though it was exposed to the danger of falling down between two stools: the unrecognized stool of the laboratory, the exalted throne of philosophy. Yet the trend of opinion was pacifist and not militant. In the readjustment of the curriculum psychology may more frequently have been offered as a compromise than as a solution. But all this was helpful; the method of intelligence would have been better; but the method of trial and error was acceptable. Pragmatically the situation offered positions to those who would undertake the new training. In the words of the college song:

> "One day the summons came out of the West;
> 'Get Ph.D's and come,' rang the request."

I may readily indicate the status of teaching at that time by mentioning that when I responded there was no elementary text available. Carpenter's 'Mental Physiology' both in title and content was about the only widely read book that reflected the new data and the new interpretation. The pioneer contribution was the invaluable work of Professor Ladd; and this celebrated its quarter century a few years ago by the appearance of the Ladd-Woodworth edition. The text implied a more generous course and a better type of preparation than the college curriculum afforded. To the teacher of psychology this monumental work was a daily support, and to the ablest students, often of nearly the same age as the teacher, a guide, philosopher and friend. The work of James appeared three years later. With the laboratory and seminar the lecture and research and the training course, and then the *American Journal of Psychology*, at Johns Hopkins; with the large personal influence and the systematic work at Yale, the decisive prominence of psychology in the graduate work in philosophy at Harvard, American psychology was launched. The auspices could not have been more favorable. The varieties of psychological experience represented by these creative personalities assured the development of psychology in this country a breadth of outlook, a soundness of technique, and a vital contact with the dominant intellectual interests, which have been most important assets. They may well stand central in our commemorative respect, a tribute to Hall, Ladd and James.

For a time the development of courses, the preparation of texts, the establishment of academic positions kept the small band of psychologists busy; also many of them could secure their positions only by a willingness to share the responsibility for allied teaching, mainly in philosophy, logic and ethics. Indeed these disciplines as commonly represented the major preparation, to which psychology was added. But the outward and visible sign of the psychologist was his laboratory; and the growth of these both for instruction and research represents the most distinctive American contribution. There is no parallel to it elsewhere. In a direct sense the laboratory is responsible for the largest increase in varieties of psychological experience. It set the experimental attitude in inquiry, and this of all the several independent factors unmistakably shaped the career which we are passing in review. It has always been true in the history of science that the kind of questions men ask is determined by the facilities which they have acquired in answering them; though it is equally true that the sense of limitation of facilities has been the motive force in extending them to the inclusion of larger interests and newer problems.

I propose to touch only upon the larger varieties of psychological experience thus furthered. They may be summarized under five overlapping waves. The first is the direct exploration of the intimate structure of mental process, the direct analytical interest of the man of the laboratory. It immerses him in technique and device and the envisagement of problems; it immerses him unduly unless it carries with it the poise conferred by interpretation.

The second is the comparative interest, which in recent years has almost entirely reconstructed the view of animal behavior, and has yielded its interpretative product in the "behaviorist" position. Its high-power study of the beginnings of mind gives it the apect of psychological histology; but its far-reaching conclusions extend to the entire evolutionary programme of the mental life. It forces upon psychology the problem of the distinctive nature of the human endowment, and the stages of differentiation in the onward series.

One may place third and thus in the central position the growth, almost the overgrowth, of applied psychology. This reflects the practical stress of the environment and the pragmatic temper of the American *Weltanschauung*. It was born in the laboratory. The term 'mental test' is so distinctive of English, which in this case is American usage, that it has been adopted in German, French and Italian literature. It arose from the consideration that analysis is not only of the factors of a process but of their place in the individual psychology. The work of Galton should be recalled on this occasion for the reason that while the biological aspects of his versatile studies attracted most attention in England, the psychological significance of his methods and results was more influential on this side. He emphasized the application of psychological tests in combination with anthropological traits. He established the first laboratory for such purpose; and it may be recorded that the first installation on this side was in connection with the World's Columbian exposition in Chicago, 1893, where also the visible embodiment of experimental psychology was shown to the public.

In applied psychology two aspects of psychological experience were involved: the first emphasized that the training of mind could proceed wisely only upon a knowledge of mind; and the teacher was referred, not without misgivings, to psychology. The second aspect was focused upon the ability of the tests to reveal individual capacity. Thus were laid the foundations of educational psychology which in the present outlook looms so momentously large that it heralds the dividing line of further specialization. It is making for a competitive share in professional status; the future division of function is indicated, though localization is somewhat uncertain. With the present expansion of departments one exponent is likely to assume responsibility for the analytical, theoretical disciplines, and another for their applications. Upon the warmly disputed question whether pedagogy is a science or an imposition, and its pursuit a profession or a misfortune, we may maintain an impeccably neutral position. We cannot overlook the fact that the psychology of the schooling processes, by sheer force of practical importance, is entitled to a commanding place in the training of teachers; while collaterally the investigations thus resulting give promise of rounding out the analyses of the learning processes to the great benefit of general problems of primary import. Here lie varieties of psychological experience of sterling value. To some the field has the appearance of extensions of city plots in unborn suburbs, neatly staked out in building-lots, with cement sidewalks but no habitations. The promoter in psychology is not unknown; but the new settlement seems less speculative to one who has a retrospective standard.

A distinctly settled section is that of tests of capacity, the

Binet-Simon addition. Applied individually the test invites a diagnostic use. The vocational pressure is intense; it should be encouraged and only its premature and cock-sure decisions resisted. It is characteristic that the sins of commission and omission alike are far more common among the practical cultivator of trees than among the more theoretical conservator of forests. Get-wise-quick methods offer the most lucrative rewards to the needy psychologist. At the moment we are besieged by requests to enlighten men of affairs how to choose employees, how to detect capacities, and by individuals how to increase mental efficiency. Only one who is blind to the lessons of history can fail to see the dangers in this great white way of psychology. One cannot look upon phrenology, physiognomy and the reading of character as merely the slums of psychology. Whatever the rating of the fakir, his customers come from all shades and grades of education. Here may be gathered interesting varieties of psychological experience; and a *candidatus philosophicus* in psychology may find a promising thesis by illuminating the psychology of fraud. Yet psychology will be shirking its social responsibilities if it declines to cross an unsavory threshold. It is not a sign of virtue to fear to tread, just because frauds rush in. Too proud to investigate is not a proper attitude. The problem of vocational and individual fitness is a wholly legitimate and, for the cautious and modest psychologist, an engaging pursuit. The reputation of the psychologist will depend upon the restraint with which he exercises authority, and pronounces judgments.

The practical varieties of psychological experience are worthy of respect in their own right. We all know that the road from theory to practice is the more indirect the more complex the situation. Qualities are far more generic than their applications. We must insist upon the legitimacy of the psychological perspective and decline to assume while yet we respect that of the practical inquirer with a narrower interest. A man will become a persuasive salesman, or a shrewd employer or a good teacher far more regularly upon the basis of a general equipment than of correspondence courses. At all events it is in the interpretation of the underlying qualities of men that psychology, pragmatically disposed, finds its métier, however ready to utilize the trends of employment, and to direct inquiry to practically significant relations. The two perspectives must differ. The variety of experience is valuable; the forms of experience imposed by modern conditions acquire a peculiar importance; but none of these interests should distort the far more significant varieties of an historically larger and intrinsically deeper experience. In precisely the same sense in which the sociologist, fixing his attention upon modern conditions, will be handicapped by a narrow vision if he forgets that what he is studying with a specialized interest is in reality a transferred biological situation to be interpreted under the principles of biological relations, will the applied psychologist become a mere trained craftsman if his sense of design is unilluminated by the interpretative insight conferred by long immersion in the principles of psychology.

I have chosen deliberately to enlarge upon the practical varieties of psychological experience, for the reason that in this vista the retrospective view directs the enterprise which will plan the highways of the future. The attitude of American psychologists toward the possible and desirable applications of their pursuits, even the mode of capitalization of their personal value for public consumption, seems to me so peculiarly important that I have chosen to project an "insert" on a larger scale in the moving picture which I am unreeling. The psychologist, I repeat, must insist upon complete authority as an architect of his science; what consideration shall he give to the expressed needs and wishes of a possible clientele? The practical and the theoretical perspectives are distinct; how shall they be made to converge, and yet retain that singleness of vision which is indispensable to a solid, realistic, stereoscopic effect?

It requires no prophetic but only a presbyopic vision to foresee that the insistent demands of practice will form a league to enforce attention. The psychological practitioner is coming; upon us rests the responsibility that, when he comes he shall be in no measure a quack or an opportunist, neither papal nor encyclopedic, nor pretend to be all things to all men. The training of psychologists cannot undertake the development of geniuses, who as a rule have found the academic environment unstimulating. A scientifically minded sense of proportion is the central equipment; with it must be combined a clinical sense for the recognition of varieties of experience when encountered. I resent the implication that because a large amount of money is spent in advertising, the psychology of advertising thereby gains in significance or importance. I welcome the fact that the actual interest in advertising supplies a variety of psychological experience which we may utilize to the full. Problems that loom large in application may have but slight illumination in principle, and be rather barren in enlightenment. The analysis of a practical occupation into its underlying factors is the legitimate work of the psychological practitioner; advice will gain in value as it strikes root in a soil enriched by scientific cultivation. The tendency which most I deplore is the neglect of the wider and the truer interpretation, because in some of its aspects it is less amenable to the rigid technique of the quantitative method. Falling in love with technique may be a pleasant but is hardly a rational indulgence. It forms one of the temptations, the idols of the practical mind. You see it pitiably at work in the pursuit of efficiency, with the result that there is more attention paid to cost-accounting than to the value of the article when produced. You see its menacing shadow thrown across the academic portals in the impertinent attempt to measure service by unit-hours and neglect quality and all the finer values incommensurate with the crude and irrelevant yardstick. If the method is continued, education will have but one purpose, ambitious and charitable at once: to make people efficient though incompetent. My plea is for the recognition, selection and cultivation of a psychological competency for practitioner and theorist alike; which is a plea, in an expert sense, for the varieties of psychological experience.

I have referred to the danger of mistaking a quantitative result for an important one, of supposing that what is measurable is significant; likewise to the danger of supposing that what is practically demanded is by that right entitled to a large place in a scientific perspective. With these is combined the danger of proceeding to far-flung battle lines of conclusions upon a slender campaign of experimental results. None of these dangers operates simply; they combine subtly and intrude subconsciously, as is the manner of fallacy and her tribe. I can point the moral most quickly by using a tale for adornment, though I run the risk of stepping upon toes, the owners of which I respect. The extent of my hardihood will be clear when I say that I shall illustrate in terms of that vexed question of the mental differences of men and women. On the basis of well-designed experiments one observer concludes that women are less disposed than men to be affected by argument. In deciding whether one aggregation of markings contains more or fewer individual components than another, the women proved more tenacious of their original opinions (whether this is consistency or obstinacy is an unwise question) than did the men. Ergo women are less desirable than men as members of a jury. In citing this bit of evidence I am fortunate in that I agree with the significance of the findings; but I remain wholly unconvinced by the conclusion.

The second illustration finds me in the reverse attitude. On the basis of a painstaking and well-devised series of sensory and mental tests, the convergence of results measuring specific capacities proves to be far more striking than the divergences of men and women. Upon these data is based the conclusion that intellectual distinctions among the sexes do not exist; they are either the result of imposition of masculine dominance upon femine complacency, or the prejudiced views of tradition. The comprehensive evidence of the varieties of psychological experience embodied in the history of culture is cavalierly disregarded. Everything is ruled out of court except the findings of the laboratory in parallel columns of figures. The contributions to the subject are disposed of as belonging either to the literature of fact or the literature of opinion; which to my view is at once a specious and destructive distinction. Your quantitative fact, however exactly determined, must pass the judgment-seat of interpretation, personified in the venerable and wordly-wise figure of experience; then only will its contributive value appear. On the other hand opinion may be as valueless as gossip, and as important as any brand of truth accessible to the present generation. More particularly, the best of opinion is founded upon exactly the same appreciation of precise investigation, is imbued with precisely the same scientific method, reflects a parallel training and allegiance to accredited principles, as that which guides the experimental devotees. To disregard such contributions and to ignore historical experience on the strength of data authentic in their own province but only modestly significant outside of it, is to violate conspicuously the appreciation of varieties of psychological experience, which constitutes the most important equipment for the loyal psychologist.

Not to court misunderstanding, let me explain that I have properly selected instances of unquestioned authority, and also that the questionings which I have raised are directed with unbiased neutrality to point out the prejudice that is invited by generic interpretation, and by the neglect thereof. If we enlarge small findings to large conclusions we exceed our warrant. If the experimentalist insists upon the supreme value of his experience above all other varieties, proposes to disregard scientifically rigorous thinking expended in other problems than his, is convinced that the quantitative pattern is the only authentic one, that amenability to measurement is the indispensable passport for psychological citizenship, the future of psychology faces an undesirable and unnecessary impoverishment. Moreover it is just because the promising growth of the applied field favors so largely the sharply defined and technically interesting varieties of psychological experience, that the pure experimentalist should safeguard the comprehensive and broader aspects of his function.

With pardonable overemphasis I have cited the logical procedures leading to an agreement with Mme. de Stael: *les âmes n'ont pas des sexes*. According to this view minds have no sex;

according to Freud they have little else. And thus we reach a further variety of experience in the abnormal. The ascendency of the Freudian movement occupies the head-line in this section of the *revue;* whether by reputation or by notoriety critics cannot agree. Either view pointedly illustrates the complex significance of varieties of psychological experience and of the attitudes that lead to their favor or disfavor. Freudian psychology must be saved even more persistently from its friends than from its detractors. As I grasp its bearing, it forms an important and essentially true contribution made by the wrong men; its germinal ideas are sound despite the loosely woven evidence. To some the Freudian orchestra makes unseemly noise and nothing else; others hail it as the music of the future. Personally I am convinced that the acceptable Freudian sonata remains to be written; it will be composed by one imbued with the spirit of its method and possessed of a rare sense of the value of phrasing. The sex *motif* will be less insistent and strident, not silenced or ignored but sublimated. I have every sympathy with those who are nauseated by its seemingly cherished obscenity, and irritated by its seemingly malicious slander of the human mind. But my logical conscience rebukes me by a reminder that any such attitude is irrelevant. The Freudian principles and the Freudian mechanisms must be considered for their value as varieties of psychological experience, quite apart from the bad form and bad taste and bad logic of their support and supporters; the merit of the cause or the campaign and of the tactics or munitions must be judged separately. The Freudian reconstruction—for a time obscurely and disparagingly received, and only recently advancing to a conspicuous place—has vitalized a large realm of observation bearing upon the abnormal but equally valid for the normal experience. In every future retrospective view its place is secure. For the moment it may impress us as a capricious, disorderly *vers libre*, a libel upon the fair name of poetry or psychology; when the exotic and chaotic and neurotic elements have given way to fairer expression its contributions will be more fairly judged and seen.

My concern with the abnormal is limited to its extension of the varieties of experience, and their interpretation for normal use. This the Freudian doctrine attempts. It attempts it practically in the method of psychoanalysis, but confines the procedure to the disclosure of entanglements and disqualifications that impede and distress normal functioning. Yet psychoanalysis is a broader practical procedure, related to an applied psychology and the allied interest of diagnosis in all difficult situations. It embraces the experimental methods in its use of the association-material. It brings to the fore the province of the subconscious which constitutes one of the most significant enlargements of psychological experience, but one readily misinterpreted in favor of unassimilated hypothesis. The inclusion of the domain of the subconscious stands as one of the most significant annexations in the retrospect—a distinct settlement of territory wrested by a difficult exploration in an uncertain jungle.

Going back twenty-five years, one comes upon the thick of a fray that almost threatened a world-war in psychology. For the first five or ten years of my professional experience the popular view of a psychologist was a ghost-hunter. Psychical research held the field of popular favor; that a laboratory was anything else than a séance-chamber seemed incredible. The psychologist had to listen to inane stories of coincidences, that spoiled many a promising dinner-party. When he couldn't explain a lame and distorted tale, his inconsequence was made shamelessly and publicly evident. Scepticism was considered a mask for ignorance, and an unpleasant substitute for the lie direct. But the most remarkable aspect of the movement was the hold it gained upon men eminent in science—but not in psychology—through whose advocacy psychical research attained a prestige far beyond that accorded to psychology as we know it. Indeed in many quarters the interest in psychology was prompted by a hope that it would solve questions foremost in the minds of psychic researchers—such as telepathy and the survival of personality. That in some sense significant varieties of psychological experience pervaded that racially old field of belief, that persistent recurrence of reversal of orderly mental sequence, was a position variously defended. All this has in these days the flavor of dried mementos. Yet it was only a half a dozen years ago, in these scientifically dedicated precincts, that a small group of us found it necessary to allay popular and learned unrest in regard to the specific mechanism by which a homemade basswood table defied the law of physics in the presence of a much heralded and sponsored Neapolitan and of a company sufficiently impressed to pay handsomely for each levitation of a skillful foot. The experience was at once humiliating and enlightening. Fraud-hunting as an indoor sport may have some zest if not value; or it may become a social obligation. It has helped to dissipate the surviving myth that the man of the academy is wholly lost in the mazes of a wicked world. Those of us who in the older days protested against the profanation of psychology, and were often snubbed for our insolence, may properly indulge in a moment of congratulation to the younger psychologists who have not this rôle to play. Yet the true interpretation of the phenomena that psychical research attempted to invite to unlawful secession remains a part of the gain of abnormal psychology.

The concluding phase of the composite psychological wave is represented by the psychology of social relations. Its pursuit obviously requires a capacity for broad interpretation, for the analysis of the deepest motives of human conduct, for the envisagement of underlying similarity of situation despite complexity of circumstance, for catching psychology on the hoof, and sensing its living products and pulsating throbs; my cental plea for catholicity of experience receives here its most direct justification. The laboratory with its simplified and scheduled analyses finds its corrective in the intricate worldly composite of conflicting forces. Social psychology represents the most elaborate phases of an applied science. It is applied in the tests of life. It quickens every interest that is entrusted to the psychologist, and justifies as it amplifies his problems. For it makes clear in how many directions psychology has a voice with other concerns, makes it clear that to be a psychologist implies a capacity to seize the psychological value of experience which to other interests presents different aspects and appeals,—to single out the psychological instruments in the orchestration of life. All life is of one living; the psychologist, far less than the devotee of other specialties can afford to diagram or artificialize his *Fach*. He should be in the world and of it, in the best sense an interpreter of human values, capacities, enterprises; for his is the duty of directing and safeguarding the precious mechanisms upon which all living proceeds. Such must be his insight and his training that he may be rewarded by the respect of his fellows in science, his coöperators in practical affairs.

I look to the increasing study of social psychology for a redemption from too restricted specialization, for a balance to the dangers of absorption in technique, for a compensation for the limitations of the quantitative method. A sense of the reality, of the richness and fullness, the complexity and conflict, the growths, changes and transformations of the mind's products permeates this field as no other. In a measure it projects the culmination of psychological experience; it projects culture and the vaster problems of political and social striving as a psychological evolution, testifying to the greatness of the forces of mind. And we of to-day are witnessing the largest and most appalling issues of estrangement in ideals, sentiments, allegiances, that the world has faced; the psychology of war must be considered in the establishment of enduring human relations. The world is going to be wisely ruled, the endeavors of organized men more sanely directed, the errors of the past less disastrously repeated, if a body of men find participation in the direction of affairs possessed of a psychological discernment; for this insight is as indispensable to modern conditions in certain relations as is an economical, a political, or a business sense in others.

Lying close to this domain and making a parallel appeal is a body of knowledge, engaging in itself and vital in its applications, which I select for special consideration. I do so because it sets forth so amply and so pointedly the qualifications of the psychologist; also because it presents manifold relations to all the several divisions of the present review, and thereby points the moral of my thesis. Dessoir in his "History of Psychology" recognizes it as one of the three great interests in the science of mind, with ancient antecedents and constant influence; he calls it psychognosis. In modern relations it may be viewed as an oblique or irregular section through the entirety of interests that form the composite of psychology; the specific disciplines consider the several fields more directly and disinterestedly. The nature of individual differences, the sources of these in human trends, their expression and emphasis in historical circumstance, their liabilities and possibilities in cultivation and decay, their contributions to human institutions—are all involved in psychognosis. The directions of temperament, the foundations of character, the total determinations of capacity and career form its subject matter. The temper of its pursuit is practical, but always with that wider and deeper foundation in varieties of human experience, which sends it back for analysis and authority to one and another discipline of psychology. It develops congenially under the type of interest that supports social psychology, has its strongest affiliations there; but it utilizes the entire range of psychological science and requires a catholic interpretation of the psychologist's function. Its restatement under the tremendous enrichment of the last twenty-five years of psychological investigation is an urgent desideratum of the present; and this obligation will do much to invite psychologists to that attitude toward their province which shall not be provincial but in the worthiest sense cosmopolitan.

The prospective and the prophetic venture is more engaging than the retrospective; commemorative occasions invite a Janus-faced, judicial attitude. The westward course of empire returning upon itself sets the gaze upon the east once more; with the rounding of the circle the conquests of the future turn to an inward advancement, to the perfection of the human equipment that comes from a comprehension of its nature, origin and history. In this vast reconstruction the psychologist comes to his own. He expresses and confers upon his disciples an interpretative sense, which in the bewilderment of change traces the orderly relations of law. Whatever the progress of the future, he recognizes in Kipling's words, that

> We can bring no more to living,
> Than the powers we bring to life.

THE NEED FOR SOCIAL PSYCHOLOGY[1]

BY JOHN DEWEY

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On the surface it is just coincidence that the foundation of this association and the publication of the 'Principles of Psychology' of William James were so nearly contemporaneous, their respective dates being, as you know, 1891 and 1890. In view, however, of the depth and breadth of the influence of James, we who are celebrating to-day our twenty-fifth anniversary are at liberty, I think, to consider the coincidence as more than chronological, and to date back by one year the gestation of our association. At all events, it would be ungrateful to engage in any discussion of the past and future of social psychology without recalling the few rich pages of the 'Principles' which are devoted to the social self, and, in the discussion of instincts, to the native reactions of human beings in the presence of one another. Big books have been written since which are hardly more than an amplification of suggestions found in these few pages. When, for example, a few years later, the *Socius* became the hero of a psychological drama, not many recalled that he had already been introduced under that very name in the pages of James.

Again it is outwardly a mere coincidence that the work of Tarde on the 'Laws of Imitation' was published in the year in which the 'Principles' saw the light of day, and that practically all of Tarde's work fell within the decade lying between 1890 and 1900. But behind the pure coincidence there was the recognition of the need for social ends of a more scientific treatment of collective human nature, and the important rôle of psychology in building up the new social science. While James confined himself to pregnant suggestions concerning the new forms which human experience and

[1] Address given on the occasion of the celebration of the twenty-fifth anniversary of the American Psychological Association, New York, December 28, 1916.

selfhood take on because of the presence of other human selves, Tarde attempted an ambitious interpretation of almost all facts of social organization, progress and degeneration in terms of certain rubrics to which he gave a psychological quality. For more than a decade his work and that of his followers in France and in the United States—among whom we may cite in diverse directions Baldwin and Ross—dominated social psychology and almost sociology. I shall not rehearse the old discussions about Imitation as a psychological fact and a social force. I shall assume with most of contemporary psychological critics that as a descriptive and explanatory conception it misplaced emphasis and tended to distort facts. But nevertheless we cannot minimize the immense power of this stage of social science in popularizing the idea of social psychology, and in bringing into recognition many facts, such as the importance of prestige, fashion, sensitiveness to the beliefs of others, the difficulties which innovation, no matter how reasonable, has to meet, etc., facts which are permanently imbedded in social science. Tarde himself was certainly one of the most stimulating and varied of writers, and I do not think we shall ever outgrow some of his contributions, although to my mind they are found rather in logic than in psychology—such as the necessity for reducing the gross phenomena of social life into minuter events which may then be analyzed one by one. The most fruitful of his psychological conceptions was ahead of his time and went almost unnoted. It was that all psychological phenomena can be divided into the physiological and the social, and that when we have relegated elementary sensation and appetite to the former head, all that is left of our mental life, our beliefs, ideas and desires, falls within the scope of social psychology.

I hope I may find general agreement in pointing to the work of McDougall and Thorndike respectively as indicative of the next great force in social psychology, together with such writings as those, upon the social side, of Graham Wallas. Aside from valuable contributions in detail, the significance of these contributions lies, to my mind, in recalling social psychology from the wrong track in which the Imitation and

Suggestibility schools had set it going. For those schools gave the dawning science a wrong twist in carrying over into science the old popular and practical antithesis of the individual and the social, and thus setting up two independent and even contrary sciences—individual and social psychology. As a concrete illustration of the absurd results to which this antithesis led, it is perhaps sufficient to refer to those bizarre writings on the psychology of the crowd in which it was assumed that the psychology of the individual left to himself is reflective and rational, while man's emotional obsessions and irrationalities are to be accounted for by the psychology of association with others. From the root of all such aberrations we were recalled the very moment the problem was presented not as one of the relationship of a mythical psychology of an isolated individual mind to the even more mythical psychology of a mass or crowd or public mind, but as the problem of the relationship of original or native activities to acquired capacities and habits. Henceforth our social psychology is placed upon the sure ground of observation of instinctive behavior; it can develop upon the basis of fact undistorted by the requirement of meeting preconceived notions imported from without. The whole question of imitation, for example, reduces itself to one of fact: Is imitativeness one of the original tendencies of human nature. If so, what is its intensity and mode of working in conjunction with the other unlearned activities?

The popularizers of science will doubtless remain half a generation behind this as well as other scientific advances, but for those who have learned the lesson of recourse to fundamental responses, the way is opened for emancipation from the greatest foe with which social science has had to contend—which I shall take the liberty of calling the monistic. How often have we been invited to build up our social, political, and ethical explanations in terms of some single and supposedly dominant mental constituent! How often discussions and disputes have been, at bottom, only a question as to which of rival single claimants we shall yield allegiance. Instincts to power, to control of others, fear of authority, sex, love of pleasure, of ease, all have been appealed to, and explanations constructed in terms of one or another exclusively. Henceforth it is, I submit, pure wilfulness if any one pretending to a scientific treatment starts from any other than a pluralistic basis: the complexity and specific variety of the factors of human nature, each operating in response to its own highly specific stimulus, and each subject to almost infinite shadings and modulations as it enters into combination and competition with others. The conception of social psychology resulting from this mode of approach becomes essentially one with that set forth by Professor W. I. Thomas in his paper on the province of social psychology at the St. Louis Congress of Arts and Science in 1904. On the one hand our problem is to know the modifications wrought in the native constitution of man by the fact that the elements of his endowment operate in this or that social medium; on the other hand, we want to know how control of the environment may be better secured by means of the operation of this or that native capacity. Under these general heads are summed up the infinity of special and difficult problems relating to education on the one hand and to constructive modification of our social institutions on the other. To form a mind out of certain native instincts by selecting an environment which evokes them and directs their course; to re-form social institutions by breaking up habits and giving peculiar intensity and scope to some impulse is the problem of social control in its two phases. To describe how such changes take place is the task of social psychology stated in generalized terms.

I hope I do not need to disclaim an attempt to give in even the barest summary the history of social psychology during the past twenty-five years. My object has been quite other. I have only wanted to refer to some salients in the intellectual fortifications constructed during this period for the sake of pointing out, in equally general terms, something of what now confronts us, waiting, nay demanding, to be done. Before passing on to this point, I feel I must avert possible misunderstanding by mentioning two allied factors which have also influenced the development of which I have spoken. One is the application of statistical methods to psychological research; the other, the behavioristic movement. Neither was devised primarily in the interests of social psychology. The requirements of education have, however, been a powerful agency in promoting the former, while education presents, of course, one phase of the problem of social control. Speaking more broadly, social phenomena are of a kind which demand statistical mathematics rather than the type of mathematics which has been evolved especially for use in dealing with physical facts. Condorcet's great essay on 'The Progress of the Human Mind' forecasts a future in which human arrangements would be regulated by science. In dealing with the influence of mathematical science he points to the newly developing theory of probabilities as that branch of mathematics which is fraught with infinite potentiality for control of social progress. I think it is only fair to see in statistical psychology a step forward, short and halting though it be for the immediate present, in the realization of Condorcet's prophecy.

The behavioristic movement inevitably tends to confirm the tendency of which I have already spoken in connection with the writings of James, McDougall, and Thorndike. It transfers attention from vague generalities regarding social consciousness and social mind to the specific processes of interaction which take place among human beings, and to the details of group-behavior. It emphasizes the importance of knowledge of the primary activities of human nature, and of the modifications and reorganizations they undergo in association with the activities of others. It radically simplifies the whole problem by making it clear that social institutions and arrangements, including the whole apparatus of tradition and transmission, represent simply the acquired transformations of original human endowments.

This provides the possibility of a positive method for analyzing social phenomena. I shall avoid engaging in passing in the disputed question of the value of an introspective psychology. But it seems almost self-evident that even if introspection were a valid method in individual psychology, so called, it could not be of use in the investigation of social facts, even though those facts be labelled social mind or consciousness. Yet one has only to look at the writings of the Austrian and German school of "folk-psychologists" (say of Wundt, obviously the most important) to see how this treatment has been affected by an assumed need of making the method and results of social psychology conform to the received categories of introspective psychology. From such deforming of facts the behavioristic outlook immediately redeems us; it represents not an improvement in detail but a different mode of attack. It is not as yet possible to estimate the significance of this alteration. In my opinion, however, the chief cause of the backwardness of social psychology has resided in the artificiality of the endeavor to adapt the rubrics of introspective psychology to the facts of objective associated life. The opening of another road of approach may therefore be expected to emancipate inquiry.

I thus come to the explicit statement of the purpose of my reminiscent sketch. The aim was to justify the presentation of the conviction that the quarter century in which this Association has existed marks just the emancipation of social psychology from influences which prevented its development on its own feet and its own merits, while the work done on lines which (as it seems to me) must be abandoned, have nevertheless done the great service of enforcing the vast field open to a social psychology, and the great need it has to serve. I turn accordingly from the past to the future, or if you will from prophecy taking the guise of history to prophecy frankly avowing itself as such.

I foresee, then, a great reflex wave from social psychology back into general psychology. An important conclusion in the psychology of native activities does not seem to have been drawn as yet by those who would base a scientific psychology upon this foundation. The conclusion seems inevitable that since 'mind' does not appear in the original list of instincts, it represents something acquired. It represents a reorganization of original activities through their operation in a given environment. It is a formation, not a datum; a product, and a cause only after it has been produced. Now theoretically it is possible that the reorganization of native activities which constitute mind may occur through their exercise within a purely physical medium. Empirically, however, this is highly improbable. A consideration of the dependence in infancy of the organization of the native activities into intelligence upon the presence of others, upon sharing in joint activities and upon language, make it obvious that the sort of mind capable of development through the operation of native endowment in a non-social environment is of the moron order, and is practically, if not theoretically, negligible.

The net outcome of the newer type of psychological method is thus an unexpected confirmation of the insight of Tarde that what we call 'mind' means essentially the working of certain beliefs and desires; and that these in the concrete—in the only sense in which mind may be said to *exist*—are functions of associated behavior, varying with the structure and operation of social groups. Speaking in general terms, there is no more a problem of the origin of society than there is of the origin of chemical reactions; things are made that way. But a certain kind of associated or joint life when brought into being has an unexpected by-product—the formation of those peculiar acquired dispositions, sets, attitudes, which are termed mind. This by-product continually gains in relative importance. It increasingly becomes the significant acquisition among all the varied reorganizations of native tendencies. That anything which may properly be called mind or intelligence is not an original possession but is a consequence of the manifestation of instincts under the conditions supplied by associated life in the family, the school, the market place and the forum, is no remote inference from a speculative reconstruction of the mind of primitive man; it is a conclusion confirmed by the development of specific beliefs, ideas and purposes in the life of every infant now observable.

On the face of it, this conclusion has implications only for the theory of psychology. But slight scrutiny makes obvious its consequences for the struggle to gain control of the forces forming society. The ultimate refuge of the standpatter in every field, education, religion, politics, industrial and domestic life, has been the notion of an alleged fixed structure of mind. As long as mind is conceived as an antecedent and ready-made thing, institutions and customs may be regarded as its offspring. By its own nature the ready-made mind works to produce them as they have existed and now exist. There is no use in kicking against necessity. The most powerful apologetics for any arrangement or institution is the conception that it is an inevitable result of fixed conditions of human nature. Consequently, in one disguise or another, directly or by extreme and elaborate indirection, we find the assumed constitution of an antecedently given mind appealed to in justification of the established order as to the family, the school, the government, industry, commerce and every other institution. Our increased knowledge of the past of man has, indeed, given this complacent assumption a certain shock, but it has not as yet seriously modified it. Evolution in the sense of a progressive unfolding of original potencies latent in a ready-made mind has been used to reconcile the conception of mind as an original datum with the historic facts of social change which can no longer be ignored. The effect on the effort at deliberate social control and construction remained the same. All man could do was to wait and watch the panorama of a ready-formed mind unroll. The French school of imitation, and its present successor, the Durkheim school of collective mind, has practically the same outcome as the German school of Volk-geist in this respect. All are engaged in explaining the past and present, and (if they predict at all) in predicting the future on the basis of the past. The new point of view treats social facts as the material of an experimental science, where the problem is that of modifying belief and desire—that is to say mind—by enacting specific changes in the social environment. Until this experimental attitude is established, the historical method, in spite of all the proof of past change which it adduces, will remain in effect a bulwark of conservatism. For, I repeat, it reduces the rôle of mind to that of beholding and recording the operations of man after they have happened. The historic method may give emotional inspiration or consolation in arousing the belief that a lot more changes are still to happen, but it does not show man how his mind is to take part in giving these changes one direction rather than another.

The advent of a type of psychology which builds frankly on the original activities of man and asks how these are altered, requalified and reorganized in consequence of their exercise in specifically different environments brings with itself the experimental attitude, and thereby substitutes the interest in control for the interest in merely recording and what is called 'explaining.' If mind, in any definitely concrete sense of that word, is an offspring of the life of association, intercourse, transmission, and accumulation rather than a ready-made antecedent cause of these things, then the attitude of polite aloofness or condescending justification as to social institutions has its nerve cut, and with this the intellectual resources of sanctified conservatism disappear. Instincts become mind when they are organized and directed with reference to the ends of attention, esteem, and endeavor which are supplied by the shared life of the place and time. The kind of mind they become depends upon the kind of objects of attention and affection which the specific social conditions supply. The task of unravelling the arrangements which exist into elements of native instinct and past acquisitions is indeed an infinitely complex and difficult one; not the less hard and extensive is the job of showing how this and that association with other persons develops this and that intellectual and emotional disposition—or mind—in this and that individual having his own peculiar original endowment. But if the history of human achievement in knowledge proves anything, it is that the all-decisive discovery is that of an effective and fruitful method. When men once hit, after endless awkwardness, upon the right road, the rest takes care of itself. Scientific movement becomes orderly and cumulative in the very process of occurring. Social and mental phenomena become intelligible because they come within the scope of the experimental method of attack. And again the history of science testifies to a conclusion which may also be arrived at theoretically—the introduction of the experimental method is all one with interest in control—in modification of the future.

There is a genuine modesty, and there is a stupid simulation of modesty which is only a mask for lazy complacency. No science has so much cause to be humble about its actual achievements as has social science, including social psychology. But in prospect, in possibility, social science seems to me to stand about where physical science stood three centures ago in the early years of the seventeenth century. There is the same halting and obstructed tendency to move from the attitude of the outside spectator, classifier and justifier of things as they are outwardly given to that of the active participant and modifier, from that of wholesale organization to that of retail reorganization. The experimental method in physical matters brought with it a technique of control—a technique of invention and construction. Specific desired ends can be formulated in specifically analyzed terms; the conditions of their attainment stated; these conditions subdivided into known and unknown factors, and some definite estimate made as to the praticability, at the given time, of attacking the problem. That we are without any such technique in social matters is self-evident. That the attainment within reasonable time of a similar technique stands and falls with the possibility of developing a human psychology which shall be experimentally applicable to the understanding of social affairs is not, however, self-evident, and is my excuse for reiteration.

I venture accordingly to repeat a thought which I had the honor of presenting before this association some years ago. The need of social control is, of course, as old as associated life itself. But the need of that control at the present time is tremendously accentuated by the enormous lack of balance between existing methods of physical and social direction. The utilization of physical energies made possible by the advance of physics and chemistry has enormously complicated the industrial and political problem. The question of the distribution of economic resources, of the relationships of rich and poor was never so acute nor so portentous as it is now; and this state of affairs is as much the result of progress in physical science as is the recognition of the Copernician astronomy. The present war is too vast and too tragic to permit one lightly to summon it as evidence for any merely theoretical thesis. But is it not, I ask, a demonstration made to order of those old words of Thomas Hobbes? "The utility of moral and civil philosophy is to be estimated, not so much by the commodities we have from knowing those sciences as from the calamities we receive from not knowing them." Such a conception is not fashionable just now; it is easier to place blame upon fate or upon the innate wickedness of human nature as seen in this or in that set of human beings. But the ultimate fate is the fatality of ignorance, and the ultimate wickedness is lack of faith in the possibilities of intelligence applied inventively and constructively.

Physical science has got to the point of bringing even the ends of the earth into physical, forceful relations with one another, and to the point of mobilizing all its resources for a contest in aggression and endurance. We are overwhelmed by the consequences of the very sciences into which have gone our best thought and energy for these last few hundred years. We apparently do not control them; they control us and wreak their vengeance upon us. Yet how infantile and pusillanimous are those who talk about the bankruptcy of science and who blame the increase of knowledge for our situation. Physical knowledge, and the consequent technique of control of physical forces, has far out-run social knowledge and its technique. The recourse of a courageous humanity is to press forward in the latter until we have a control of human nature comparable to our control of physical nature.

From the point of view of the psychology of behavior all psychology is either biological or social psychology. And if it still be true that man is not only an animal but a social animal, the two cannot be dissevered when we deal with man. Hence it is that subsequent years have enabled me to find added meaning in words which I spoke before this association years ago, and which in conclusion I venture to repeat. "We are not called upon to be either boasters or sentimentalists regarding the possibilities of our science. . . . But we are entitled in our daily work to be sustained by the conviction that we are not working in indifference to or at cross purposes with the practical strivings of a common humanity. The psychologist in his most remote and technical occupation with mechanism may be contributing his bit to that ordered knowledge which alone enables mankind to secure a larger and to direct a more equal flow of the values of life."

THE CASE OF SELF AGAINST SOUL[1]

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Most contemporary psychology tacitly ignores that concretely real being, the self or I. Psychologists experiment on mental processes of varying sorts, percepts, images, and emotions; they dispute about the attributes of sensation, the occurrence of imageless thought, the dimensions of affection. Rebels from the orthodox school have a new set of abstractions: psychophysical functions and modes of behavior which constitute, in their view, the proper object of psychological investigation. In the meantime, the self which perceives and feels and thinks, which functions and behaves, though so obviously cast by nature for the title rôle, plays no part at all on the stage of most psychological systems. More literally stated: instead of studying concrete wholes—namely, knowing, feeling, willing, functioning, and behaving selves, persons or *I*'s—the psychologist is wont to concern himself with one or another part or aspect of this concrete whole, often with a schematized, artificial construct, invented for purposes of easy description or in order to bring psychological method into artificial correspondence with that of the physical sciences.

The chief reason for this ominous neglect of the self in psychology is unquestionably the fact that I am always conscious of myself and that I am therefore inattentive to this "ubiquitous" self. "Just as, if I were asked to report fully my sensational experience at a given moment, I might well forget to name the sensations of pressure from my clothing simply because they are so constant"[2] so in my introspection

[1] A paper read November 24, 1916, before the Philosophical Union of the University of California. Part IV. has been enlarged and rewritten and the paper has been revised.

[2] Quoted from a paper by the writer, 'The Self in Scientific Psychology,' *Amer. J. of Psychol.*, 1915, 26, p. 521.

I simply forget to name myself. A second explanation of this deplorable tendency to ignore the fundamental fact of psychology is found in the traditional confusion of 'self' with 'soul.' The chief business of this paper is to compare and to distinguish the two concepts.

It is necessary at the outset to agree, roughly at least, on the meaning of our terms. By 'self' I denote the object of the observation expressed in the words 'I am conscious of myself.' Obviously, this statement is no definition, and for the best of reasons: the self, or I, is, in truth, indefinable, since it is *sui generis*, in a class by itself. But though indefinable it is not, therefore, elemental and indescribable.[1] On the contrary, the self is a highly complex being which may be described by an enumeration of its characters. Among these characters of the self the following are surely fundamental: First, the self of each of us to some extent persists: I am in a true sense of the word the 'same' self who cut a philosophy class in her senior days at college in order to take a drive behind a pair of Vermont horses. In the second place, however, the self, with all its persistence, truly changes, develops: though an outgrowth from that frivolous self, and identical with her, I am yet a changed self. Third, and very significantly, I am a unique self: there is only one of me; I am an individual; no one, however, closely she resembles me, *is* I. The possibility of this enumeration shows, in the fourth place, that I am a complex self, a unity of present with past—yes, and with future—self and a totality, also, of many different experiences; I am a perceiving and remembering and thinking and feeling self. These different experiences or aspects of me do not, however, exist apart from me; I obviously am not what Hume called me, a bundle of perceptions, but each of the perceptions or emotions or thoughts is the expression of me who am inclusive of them. Finally, I am a self related to the world in which I seem to myself to be placed; my fundamental relation to the world I call my consciousness

[1] For the unjustified implication that the indefinable must therefore be elemental cf. E. B. Holt 'The Concept of Consciousness,' pp. 73–74: "Either consciousness is a complex entity not fundamental but definable in terms of simple entities that are not consciousness . . . or else consciousness is fundamental and simple."

of it, and within my consciousness I distinguish different forms of relation, as activity and passivity, and different complexes of relation, as perception, emotion, and the rest. All these characters, it must be added, are immediately experienced. The self, thus described, is observed and not merely inferred; is, therefore, a psychological datum which is taken over into philosophy when reflection discloses that it is the unique fact which can neither be denied nor even doubted without being at the same time asserted.

We have next to distinguish the concept of 'soul' from that of the immediately perceived, the persisting, changing, unique, complex and related 'self.' And here we find ourselves involved in great difficulties of interpretation. For, whereas the relatively late term 'self' is primarily psychological, the word 'soul' is common to many philosophies and to prephilosophic levels of civilization; so that the attempt to unravel its meaning is fraught with subtlest difficulties.

Among these varying meanings are three of commanding importance which for convenience we may designate as the biological (or vitalistic), the metaphysical (or immaterialistic), and the psychological conceptions of the soul. The first two are alike in that both define the soul with reference to the body, but they conceive the relation of soul to body in sharply opposed fashions. (1) According to the earlier, the vitalistic, view soul is equivalent to life. Every reader of early literature knows that the soul is often virtually identified, now with the blood, now with one or another of the vital organs, and again with the breath. This conception of the soul as breath differs, it must be noted, from the identification of soul with blood or heart or kidneys. For the breath though it gives life to the body leaves the body at death. In truth, the doctrine of the soul as *animus* or *spiritus* forms a sort of bridge, or transition, between the 'vitalistic' view and the immaterialistic conception of the soul. (2) This 'metaphysical,' or immaterialistic, doctrine regards the soul no longer as in positive relation to the body but rather as negatively related to it, contrasted with it, opposed to it. The early conception of the soul as shade, or ghost, is an approach to this metaphysical doctrine, the Orphic, Platonic doctrine which persists to our own day, that the soul is an immaterial, simple, unchanging being. These characters, it will be noted, are the bare opposites of the materiality, the compositeness and the constant flux of the body so that the soul, thus metaphysically conceived, may well be defined as *not-body*. Along with this theoretical distinction of soul from body there goes a change in valuation. Whereas the Homeric shade is an insubstantial and a pitiable being, the immaterial soul of the philosopher is regarded as immeasurably more worthy than the body. (3) The third conception of the soul is as conscious being, as a something which perceives, feels and thinks. In truth, by this psychological conception soul and self are virtually identified. It is significant, therefore, to find that from earliest days men seem to have held this view of the soul along with one or both of the other conceptions. Thus, the Homeric ψυχή, primarily a vital phenomenon, has sense experiences, and the Homeric θυμός knows, feels and wills; the Platonic soul, though predominantly immaterial, unites sense experiences and apprehends ideas and loves wisdom; and Descartes's very 'metaphysical' soul has the principal 'attribute' of thought, that is, of consciousness. This fusion of concepts occurs, as will later be shown in detail, even when the characters of the soul, for example, changelessness and simplicity, are exact contraries of the observed characters of the self. Our immediate task is to compare and to distinguish these three conceptions as they appear and reappear in the history of thought. It will be necessary to make rigorous selection from a great mass of material; and we shall profitably begin by a study of the soul of Plato's 'Dialogues.'[1]

I

Plato's doctrine of the soul, like all the philosophical and scientific conceptions of the 'Dialogues,' is presented to us not in systematic form as an articulated body of doctrine but almost incidentally. One can hardly re-read the dialogues in

[1] This paper, in its first form one of a series of lectures on Plato, treats his doctrine at what may seem disproportionate length.

any unbiased way without noticing the relative rarity and usual fragmentariness of Plato's references to the soul. We are wont to stress these passages because later discussions have lent them such transcendent significance, but an unprejudiced study of the dialogues always leads us to reaffirm Paul Shorey's assertion that Plato is a dramatic artist and an impassioned moral and religious teacher, not a scientist or a metaphysician. Yet in spite of the incidental character of Plato's philosophical and psychological teaching, we may, I think, gain a fairly accurate notion of his conception of the soul; and we shall find that it contains all three of the factors, already noted, of the traditional soul-doctrine. That is to say: Plato somewhat confusedly combines the vitalistic, the immaterialistic, and the psychological conceptions of the soul. This summary account of his position has now to be justified.

1. The Homeric concept of soul as a bodily principle or activity was, of course, familiar to Plato. He obviously takes over this primitive view when he makes Socrates say, in the 'Cratylus': "I should imagine that those who first used the name *ψυχή* meant to express that the soul when in the body is the source of life, *αἴτιον τοῦ ζῆν*, and gives the power of breath and revival (*ἀναψῦχον*) and that when this reviving power fails then the body perishes and dies."[1] Here we have a conception of the soul as specifically the breath-giver, a notion which is strongly redolent of primitive thought.

Closely connected with this is the repeated teaching that the soul is mover of the body. This, again, is a theory adopted not invented by Plato. Thales, as we learn from Aristotle, endowed magnets with souls on the ground that the magnets occasion the movement of attracted particles; and doubtless the restless motion of fire fits it, in the view of Heracleitus, to define the soul. Thus, Plato is following an accepted tradition by his constant description of the soul as moving principle and—he adds—self-moved. "The soul," Plato says, in the 'Phædrus,' "is ever in motion. The body," he continues, "which is moved from without is soulless but that which is moved from within has a soul."[2] And

[1] 'Cratylus,' 399, *D–E*.

[2] 'Phaedrus,' 245, *E*.

this, it may be noted, is the teaching of the 'Phædrus,' a dialogue which can not well fall later than in the early-middle period of Plato's thought. But proof of the permanence of the conception is not lacking. For it reappears in Plato's latest dialogue, the Laws, in which the Athenian stranger speaks thus to Cleinias:

"Let us assume," he says, "that there is a motion able to move other things but not to move itself—that is one kind; and another kind is that which can move itself as well as other things. . . . And which . . . ought we to prefer as the mightiest?"

"I must say," Cleinias replies, "the motion which is able to move itself is ten thousand times superior to all the others."

"If then," the Athenian resumes, "as most of these philosophers have the audacity to affirm, all things were at rest, which of the . . . principles of motion would first spring up among them?"

"Clearly the self-moving," Cleinias says, "for there could be no change in them arising out of any external cause: the change must first take place in themselves."

"Let us put a question," the Athenian continues, "If we were to see this power existing in any earthy, watery or fiery substance . . . how should we describe it?"

"You mean to ask," Cleinias replies, "whether we should call such a self-moving power life. . . . Indeed, we should."

"And when we see soul in any thing," the Athenian insists, "must we not . . . admit that this is life? . . . The soul," he concludes, "is the first origin and moving power of all that is, or has become or will be."[1]

These last words of the Athenian stranger and their omitted context, as well as that of the passage quoted from the 'Phædrus,' indicate the content of Plato's cosmological theory that the stars and "the whole heavens and all creation" are moved by soul. But our discussion is limited to the study of the human soul and must utterly reject the lure of this reference to "all that is or has become or will be." On the

[1] 'Laws,' X., 894, *B*–895, *C*, 896, *A*.

other hand it must stress the emphatic and repeated identification of 'soul' with 'life' as proof that Plato—here as often the unacknowledged precursor of Aristotle—holds a genuinely vitalistic conception of the soul.

2. Clearly distinguished from the vitalistic concept of the soul as life, and often in the main inconsistent with it, is the doctrine cherished and stressed by Plato, which I am designating as the 'metaphysical' concept of the soul. This is the theory that the soul is utterly unlike the body, incorporeal or immaterial. The soul, Plato always asserts in opposition to the "vain opinion" of the "physical investigators," is not formed of fire and water and earth and air as its first elements; rather the soul is the "first," not last, "and before all bodies."[1]

It follows, Plato always assumes, that the inherently incorporeal soul is, as it were, a prisoner in the body or entombed within it.[2] Evidently, this is a perpetuation of the Pythagorean teaching that "for a punishment the soul is yoked with the body and buried in it as a tomb."[3] And Plato's assertions that the soul is separable from the body are similarly related to that Orphic belief which traces its source to the ecstatic Dionysiac rites in which "the possessed devotee was set free for the moment from the tangled net of daily life, gained for a brief time new and superhuman powers"[4] so that the soul seemed to be loosed from the body. Now the Orphic and Pythagorean doctrine of the soul's immaterial nature formed an inextricable part of the belief in the preëxistence of the soul. Both of these doctrines are enforced by Plato's teaching, but he supplements their early ethical and religious content by more narrowly metaphysical considerations. In the Orphic Fragments the doctrine of the preëxistence and the reincarnation of the soul enforces ethical exhortation by promising a period of reward or punishment in which "they who are righteous beneath the rays of the sun when they die

[1] 'Laws,' 891, *C*–892, *A*.

[2] 'Phaedrus,' 250.

[3] Quoted by Clement of Alexandria, Stromata, III., 17. (Diels, 'Fragmente der Vorsokratiker,' I., p. 245, Philolaos, *B*, 14.)

[4] C. H. Moore, 'Religious Thought of the Greeks,' p. 50.

have a gentler lot in a fair meadow by deep-flowing Acheron. . . . But they who have worked wrong and insolence beneath the rays of the sun are led down beneath Cocytus's watery plain into chill Tartarus."[1] The myths of Plato perpetuate this teaching and stress the individuality of the soul. Stripped of the body which is "interposed as a veil before" his own soul, the judge of the Gorgias "with his naked soul shall pierce the other naked souls" who have left their brave attire strewn upon the earth."[2] Those who have "already," in the words of the 'Phædrus,' "begun the heavenly pilgrimage may not go down again to the darkness."[3] But all the souls suffer for the wrong which they have severally done.

Plato's ethical soul-doctrine, it should be observed, consists not merely in an exhortation to the soul to escape the prison house of the body but in an emphasis of the domination of soul over body. "When the soul and body are united," he says, "then nature orders the soul to rule and govern and the body to serve." The soul is the pilot of the body; is at variance with the affections of the body; "coerces the bodily elements . . . as if talking to a thing which is not herself." But Plato, though he thus emphasizes the ethical aspects of the earlier soul doctrine, strengthens the conviction of the soul's freedom from the body's mortality by a philosophical (in a way a psychological) consideration. We are constantly, he points out, estimating people and things as good or beautiful or equal; and in our estimate we presuppose an absolute standard of goodness, beauty, equality and the like. Yet the objects we meet with in our actual human experience are never more than relatively good, beautiful or equal. It follows, Plato insists, that the soul must have existed without the body,[4] that "every soul of man has in the way of nature beheld true being: this was the condition of her passing into the form of man."[5]

[1] Fragment 154 *A*. (Quoted by C. H. Moore, *op. cit.*, p. 56, from Abel's edition of the Orphic fragments.)

[2] 'Gorgias,' 523, *D–E*.

[3] 'Phædrus,' 256, *D*.

[4] 'Phædo,' 73 ff.

[5] 'Phædrus,' 249, *E*–250, *A*.

So, the Platonic teaching about immortality preserves the ethical and religious coloring of the Orphic and the Pythagorean beliefs, on a background of philosophical conception and argument. Some of the arguments for immortality are, to be sure, drawn from that vitalistic theory of the soul with which, as will later appear, the immaterialistic concept is not actually to be harmonized. Thus, that latest argument of the 'Phædo' which Socrates brought forward like "a general rallying his defeated and broken army" is based on the admitted truth that "whatever the soul possesses, to that she comes, bearing life."[1] And in the 'Phædrus' immortality is argued from the self-motion of the soul: the self-moving soul, Socrates asserts, as true beginner of motion cannot itself be begotten.[2]

For the most part, however, the 'Phædo' argues immortality from the immateriality of the soul, its unlikeness to the body. Thus, when Cebes begs Socrates to argue them out of their fears, to persuade the "child within" them "not to be afraid when he is alone in the dark," Socrates answers thus:[3]

"The composite may be supposed to be naturally capable of being dissolved; but that which is uncompounded and that only must be . . . indissoluble. And the uncompounded may be assumed to be . . . unchanging, whereas the compounded is always changing. Now . . . equality, beauty—are these essences always what they are . . . not admitting of variation at all?"

"They must be always the same," Cebes answers. And Socrates continues:

"Let us suppose that there are two sorts of existences—one seen, the other unseen. The seen is the changing and the unseen is the unchanging. Further, one part of us [is] body and the other soul. And to which class may we say that the body is more alike and akin?"

"Clearly to the seen—no one can doubt that," Cebes replies; and Socrates asks:

[1] 'Phædo,' 105.

[2] 'Phædrus,' 245.

[3] Condensed from the 'Phædo,' 78–80, with the omission of most of the brief replies of Cebes.

"Is the soul seen or not seen?"

"Not by man, Socrates," Cebes answers.

"Then," Socrates says, "the soul when using the body as an instrument of perception is dragged by the body into the region of the changeable. . . . But when returning into herself she reflects, then she passes into the other world, the abode of purity, and eternity, and immortality, and unchangeableness. . . . And consider the matter in another light. When the soul and the body are united, then nature orders the soul to rule and govern and the body to obey and serve. Which of these two functions is akin to the divine? and which to the mortal?"

"The soul,' Cebes agrees, "resembles the divine and the body the mortal." And Socrates concludes:

"Then . . . the soul is in the very likeness of the divine, and immortal, and intellectual, and uniform, and indissoluable, and unchangeable; and the body is in the very likeness of the human, and mortal, and . . . multiform, and dissoluble, and changeable."

We are not at all concerned with the force of the argument nor with the difficult problem of the relation of soul to the essences or ideas. What we have to note are the characters of uncompoundedness and unchangingness here attributed by Plato to the soul—regarded, in metaphysical fashion, as immaterial being—predicates which are sharply opposed, or at least unrelated, to the characters of life and self-motion which are basal to Plato's vitalistic concept of the soul.

3. We have thus summarily outlined two Platonic conceptions of the soul, distinguishable if often uncontrasted: first, his conception of the soul as vital principle of the body, its breath, inciter to its movements, and second, what we have named his metaphysical conception of the soul as a changeless uncompounded individual being, radically different from body and variously thought of as dominating the body or as marred and hampered by communion with it. But Plato conceives, or at any rate treats, the soul in still a third fashion as a conscious being, as subject of experience. To begin with, the soul *recollects*. For Plato's well-known

doctrine of reminiscence is not merely a metaphysical speculation about reincarnation and an epistemological doctrine about absolute knowledge, but a psychological study of recognition and memory. In other words, the soul not only "calls to mind all she ever knew"[1] as the 'Meno' has it, but, in the words of the 'Philebus,' she has "the power . . . of recovering when by herself some feeling which she experienced when in company with the body"[2]—that is to say, the soul remembers sense-experiences. The soul also *conceives* and *compares* and *reflects*. "By a power of her own," Socrates is made to say, in the Theætetus,[3] "the soul contemplates the universals . . . comparing in herself things past and present with the future."[4] And Plato's conception of the conscious soul is not purely intellectualistic. The soul, he teaches, *feels pleasure and pain*, and "pleasure and desire" may "exist in the mind only apart from the body."[5] The soul finally *aspires*[6] and—as figured in all the myths of judgment—makes genuine choice. Moreover, the soul which perceives the physical and understands universals has also, Plato implies, a knowledge of itself. Thought is the conversation of the soul with herself; the knowledge of universals is a recollection of her former experience; her character is, as we have seen, the outcome of her own choice. The soul, finally, through her 'instruments,' the senses, *perceives*. In the words of the 'Philebus' there is in perception "union of soul and body in one feeling or motion";[7] and there are affections (παθήματα) which vibrate through both soul and body and impart a shock to both and each of them.[8] It is significant to find in this account of the perceiving soul a denial of the sharp opposition of soul to body.

The fundamental protest of this paper, it will be remembered, is against the confusion of the concepts of soul and

[1] 'Meno,' 81, *C*.
[2] 'Philebus,' 34, *B*.
[3] 'Theætetus,' 185, *E*.
[4] *Ibid*., 186, *B*.
[5] 'Philebus,' 34, *C*.
[6] 'Theætetus,' 186, *A*.
[7] 'Philebus,' 34, *A*.
[8] *Ibid*., 33, *D*.

self. If, however, philosophical convention is to be trusted, there is no room for the confusion in Plato's teaching since—so we are told—the concept of the self dawns late on the metaphysical horizon, and is descried at the earliest in the speculations of Plotinus. But whatever may be true of the term 'self,' however long the period before the self was formally introduced in metaphysical circles, philosophers, as well as other men, were conscious of themselves though—also like other men—they often forgot to mention the fact precisely because they were so accustomed to it. To return to Plato. Unquestionably, he uses the reflexive pronoun *ἑαυτός-ἑαυτό* to refer to impersonal as well as to personal realities; and certainly he does not explicitly refer to ego or self; but on the other hand he over and over again refers to the man (*ἄνθρωπος*) or to ourselves (*ἡμεῖς*) as being conscious. Thus, in 'Philebus,' 36, *A*, Socrates inquires whether one of us (*τις ἡμῶν*) who is hungry may not at one time have a sure hope of being fed and at another time feel in despair. "Each of us," he says in 'Republic,' III., "has many wants." Indeed, throughout the discussion of the virtues in the 'Republic,' the state is contrasted not with the soul but with the man (*ἀνήρ*) or with the individual (*ἕκαστος*); whereas the soul is referred to only incidentally and conventionally.[1] In these allusions to 'a man' and to 'us' it is at least possible to descry the germ of a psychological concept of the experienced self, no longer wholly overlaid by the conventional soul-concept. And if we are right in this interpretation we have here an early illustration of the greater empirical accuracy of statements about the self. Influenced by his dualistic prepossession to stress the contrast between soul and body, Plato, as we have already noted, scruples to say that 'the soul' perceives, whereas he clearly realizes—as everyone who, simply and without metaphysical bias, observes himself must realize—that 'we' perceive as surely as we think.

The estimate of self and soul doctrines is, however, best postponed to a point further on in our discussion.

[1] Cf. 'Republic,' 435, *C*. *τὸν ἕνα . . . τὰ αὐτὰ εἴδη ἐν τῇ αὐτοῦ ψυχῇ ἔχοντα.*

II

1. The conception of the soul most emphasized by Aristotle is, as everybody knows, the biological or vitalistic. The soul he says is the first entelechy, or realization, of an organic body—in a word it is the body's capacity for life. It follows, Aristotle teaches, that there are animal souls as well as human souls. And by this he does not mean to attribute consciousness to plants and animals but rather to attribute to them life. Like Plato and the earlier Greek thinkers, Aristotle also makes of soul "the source of local movement."[1]

2. Of the traditional immaterialistic conception of the soul Aristotle, on the other hand, shows hardly a trace. "We should no more ask," he observes,[2] "whether soul and body are one than ask whether the wax and the figure on it are one." Yet the conception of soul as life is an integral part of Aristotle's metaphysical system—the theory of the universe as ascending from primitive, unformed matter (pure potentiality) through successive stages (of which each is, viewed from below, realization, or form, and viewed from above itself potentiality) until at the end pure activity is attained: completely realized being, mind in contemplation of itself, mind which is principle of all motion in that it is goal of all desire and ultimate object of all yearning. The discussion of the Aristotelian philosophy, thus baldly suggested, would, of course, lead us far afield. What is important to our purpose is to notice that there is here no hint of the specifically immaterialistic conception of soul—no description of it as simple and uncompounded. And, so far from conceiving the soul as unchanging, Aristotle explicitly defines it as the progressive realization of growing capacities, or possibilities.

3. That Aristotle, beside conceiving the soul in the manner of the vitalists, certainly regards it after the psychologist's fashion, is shown by the slightest examination of the contents of 'De Anima.' In Book II., he distinguishes from the vegetable soul (ἡ θρεπτική) the animal soul which he here forthwith describes as perceptive (αἰσθητική). The remainder

[1] 'De Anima,' II., Chap. 4, 20, 415 *b*.

[2] *Ibid.*, Chap. 1, 7, 412 *b*.

of the treatise is given over to the discussion of the senses, sight, hearing, touch, and the others; of imagination (φαντασία);[1] of conception (ὑπόληψις); thought (νοεῖν); desire (τὸ ὀρεκτικόν).[2] All these are over and over again referred to in terms common to Aristotle's zoological and ethical as well as to his psychological writings as either parts (μόρια) or capacities (δυνάμεις) of the soul. Thus, the discussion of reason (τό νοεῖν) in Book III., Chapter IV., of the 'Psychology' is introduced by the words: "We must next investigate that part (μόριον) of the soul by which it knows and reflects (φρονεῖν)." These capacities or parts are differently enumerated in different passages; those most often named are the perceptive, the thinking (λογιστικόν or νοητικόν or διανοητικόν) and the impulsive or voluntary (τὸ ὀρεκτικόν, or βουλευτικόν or ἐπιθυμητικόν). More than once, indeed, Aristotle says that the soul is defined by thinking, judging, and perceiving, and that both by thought and by sense "the soul distinguishes and knows real things."[3] The simplicity and precision of the words just quoted ("the soul distinguishes," "the soul knows") bring into clear relief the artificiality of Aristotle's more frequent descriptions of conscious experience in terms, very often reproduced, of personified (yet to the end more or less fictitious) 'faculties'— imagination, reason, and the rest. More frequent than Aristotle's simple statements that the soul knows or thinks are his references to the man (ἄνθρωπος) who hears, smells or thinks; and still more frequent are his assertions that 'we' see and touch and perceive;[4] that images present themselves to 'us,' that 'we' judge (κρίνομεν), affirm or deny (ἀληθεύομεν ἢ ψευδόμεθα), feel fear.[5] Here, as in Plato's parallel expressions, we may perhaps fairly conclude that Aristotle is forgetting the supposed necessity to record his experience in terms of his philosophical doctrine and is conceiving a self, rather than a soul, which is conscious.

It should be noted that Aristotle's profound doctrine of

[1] 'De Anima,' III., Chap. 3, 7, 428*a*.
[2] *Ibid.*, III., Chap. 9, 3, 432*b*.
[3] *Ibid.*, III., Chap. 3, 1.
[4] *Ibid.*, III., Chap. 3, 6, 428*a*.
[5] *Ibid.*, 5, 427*b*.

substance, as elaborated in the 'Metaphysics,' plainly connects itself with the conception of the soul. To trace the connection would be to involve ourselves in a long discussion, but we can not avoid the conclusion that Aristotle, for whom the ultimately real was always neither matter nor form but the individual, meant to endow the soul with this character: uniqueness or individuality.

III

By Stoic, Judeo-Alexandrian, Neo-Platonic, Patristic, and Scholastic thinkers the triple conception of the soul has been handed down to modern psychologists and metaphysicians. The soul-doctrine of the pre-Christian schools belonged, as is well known, to a pantheistic philosophy. The soul was regarded by the Stoics as part of the Universal Reason, by the Neo-Platonists as emanation through the world-soul from the One. But all—Hellenic, Jewish, Alexandrian and Christian thinkers—united to emphasize the worthlessness of the body and the supremacy of the 'soul burdened with a corpse.'[1]

It is not necessary to consider in detail most of these theories but it would be impossible to pass over Augustine's teaching. The conception of soul and self are, as everyone knows, central in his philosophy; and he conceives the soul in each one of the three traditional ways. (1) Although he repudiates the essential materialism of the primitive vitalistic conception which identifies the soul with the blood he yet everywhere insists that the conscious soul is a living being. And from the attribute of life he argues, in Plato's fashion, the immortality of the soul. "Those," he says, "who have held [the soul's] substance to be some kind of life . . . have striven also to prove it immortal, since life can not be without life."[2]

(2) More often, however, Augustine sets forth the immaterialistic theory of the soul. His teaching is very close to that of Plato. The soul, he asserts, in words perpetuated for centuries, is a spiritual substance:[3] it is incorporeal—and

[1] Attributed to Marcus Aurelius (quoting Epictetus).

[2] 'De Trinitate,' X., 7.

[3] 'De Trinitate,' XI., 1; 'De Quantitate Animæ,' Chaps. II., III.

he argues this, as Plato has argued it, on the ground that the soul discerns incorporeal things (*cernit incorporea*),[1] such, for example, as mathematical quantities. The incorporeal nature of the soul, Augustine also teaches, implies its unextendedness—a negative character which he stresses very likely in opposition to Tertullian's materialistic view. Over and over again, in 'De Quantitate Animæ'[2] and in 'De Immortalitate,' Augustine repeats the statement that the soul feels throughout the body (*per totum corpus*) but not with the body (*cum corpore*). The reiterated statement that the soul is simple has the same meaning, that the soul acts as a whole (*tota operatur*). And from its unextendedness and its simplicity he deduces, after the traditional fashion, its unchangingness, imperishability and immortality. He argues in detail that though the soul seems to grow, to develop with the body, it is none the less unchanging. *Non mutatur*, he reiterates in passage after passage.[3] Finally, in his account of the relation between soul and body Augustine follows Plato with especial fidelity. The soul, he everywhere asserts and implies, is the ruler of the body (*rationis particeps regendo corpori*).

(3) Up to this point we have nothing radically new. The vitalistic doctrine of the soul is set forth less convincingly by Augustine than by Aristotle; the immaterialistic doctrine in no wise advances upon Plato's. But Augustine's treatment of the soul as subject of consciousness or knowledge is like new wine in new bottles—an outpouring of first-hand observations, a formulation of acute and independent reasonings. The pages in which he discusses loving and knowing, remembering and attending, thinking and willing, seem to me, though written so long before the rise of scientific psychology, comparable only with the introspective passages of William James for their luminousness, their revealing quality, their ability to cleave asunder the most tortuously intertwined human experiences.

The most emphasized and most truly novel contribution

[1] 'De Trinitate,' X., 7; 'De Quantitate Animæ,' Chaps. XIII., XV.

[2] Cf. Chaps. XXIII., XXX.

[3] 'De Immortalitate,' Chap. I. Cf. 'De Quantitate Animæ,' Chaps. II., XV., XVI.

of Augustine to this psychological conception of the soul is his insistence on the fact that the soul knows itself. We are not here concerned with the implications of this fact for philosophy —the significance of the discovery of this one reality, myself, which is self-evident because in doubting or denying it I assert it. What we are here remarking is the enlargement of the concept of knowing soul by the explicit recognition of its self-knowing character. "'Know thyself,'" Augustine asserts, "is not said to the mind as is 'Know the cherubim and the seraphim'; for they are absent, and we believe concerning them. . . . Nor yet as it is said 'Know the will of that man'; for this it is not within our reach to perceive at all, either by sense or understanding, unless by corporeal signs actually set forth; and this in such a way that we rather believe than understand. Nor again as it is said to a man, 'Behold thy own face'; which he can only do in a looking-glass. For even our own face itself is out of the reach of our own seeing it. . . . But when it is said to the mind, Know thyself: then it knows itself by that very stroke by which it understands the word 'thyself'; and this for no other reason than that it is present to itself."[1]

With equal subtlety Augustine discusses all forms of knowledge. At times, but only apparently by accident, or in theological exigency, he follows Aristotle in apotheosizing the abstractions 'memory,' 'thought' and 'will.' Usually, however, he says concretely that the mind remembers, thinks, judges, wills; or else he says even more concretely, *I* remember, *I* will. For the truth is that Augustine uses the *self*-words, 'I,' 'he,' 'self,' 'man,' nearly if not quite as often as he uses the terms 'soul' or 'mind,' and seemingly he employs the two sets of terms interchangeably. An example of this interchange is selected at random from Chapter XI. of Book X. of 'De Trinitate.' Augustine is arguing that though memory, understanding and will are three yet each is contained by each. He begins with the statement that "The mind is certain of these three things concerning itself . . . memory, understanding and will," but ends with the assertion: "What-

[1] 'De Trinitate,' X., 9.

ever I understand I know that I understand and I know that I will whatever I will and whatever I know I remember." Here the term 'I' replaces the term 'mind' within one and the same closely argued proof.

But though Augustine thus identifies soul with self he does not criticize the one conception by the other. He never turns upon the obscure conventional attributes of the soul the searchlight of introspective observation; he never reconciles the divergence between the teaching of Augustine, the observer, "When the human mind knows and loves itself it does not know and love anything unchangeable"[1]—and the dogma which Augustine, the theologian, uncritically takes over from Plato and the Fathers, "The soul is unchanging and simple."

Mediæval philosophers, so far as I know them, neither make important contributions to the conception of soul nor markedly diverge from it. Two instances will suffice for our purposes. Alcuin, as quoted, compresses an assertion of all three of the soul's traditional attributes into a brief sentence: "The soul is a rational spirit ever in motion, ever living, capable of good and evil." St. Thomas also expressly recognizes all three characters of the soul. He describes it as principle of life and of bodily movement—a clearly vitalistic description. He also asserts, in accord with Plato's doctrine, that the soul is simple substance; and he argues elaborately[2] (here, as in his doctrine of the individuality of souls, following Aristotle) that in spite of being the mover of the body, the soul is itself unmoved. Finally, St. Thomas teaches, in the second Article of this same Question 75, that "the soul knows" or rather (he corrects himself) "man knows through the soul." Questions 77 through 80 develop this doctrine of the knowing soul by enumerating and analyzing, somewhat in Aristotelian fashion, the 'powers of the soul,' that is to say, the five senses, the intellect, memory, appetite and will. Like his predecessors, Thomas obviously uses the three time-honored concepts of the soul in unharmonized juxtaposition.

[1] 'De Trinitate,' IX., 6.

[2] 'Summa Theologiæ,' Pars Prima, Question 75, first Article.

IV

The soul makes its way into modern thought encumbered with its three more or less conflicting duties: to be life-giving, to be immaterial, and to be conscious. It is not, however, left undisturbed in the quiet possession of these traditional characters. At the very outset Descartes threatens to deprive it of one of them by destroying its pretension to be the life and mover of the body. For Descartes formulates the mechanistic conception of life and effectively marshals the evidence, from physiological observation and experiment, for this mechanistic theory. Thus, he says that the circulation "follows as necessarily from the very arrangement of the parts of the body and from the heat which may be felt with the fingers and from the nature of the blood . . . as does the motion of a clock from the power, the situation, and shape of its counterweights and wheels." "The body," he repeats in every possible connection, "is nothing more than a machine."[1] Descartes, to be sure, is not daring enough to treat the human body as a perfect mechanism He (1) still conceives the human soul as vitally related to the body though he allows it but a precarious seat in the pineal gland and tries to hold it down to the function of mere direction, not initiation, of the body's motions. In entirely uncritical fashion, (2) in the second place, Descartes adopts the immaterialistic doctrine, asserting after the manner of Plato, Antoninus, Philo, Augustine, and Thomas the simplicity of the immortal soul and its utter unlikeness to the body. But he (3) also stresses the psychological conception of the soul as conscious being; and here, once more in agreement with his predecessors and in particular with Augustine, he uses the term 'soul' as virtual synonym for 'self' or 'I.'[2] Even more often, however, he describes the self, or I, purely as a conscious being, without reference to the traditional characters of the soul. Thus he says, in reply to the question: "What then am I?" "I am a thinking thing . . . that doubts, understands, affirms, denies,

[1] 'Discourse on Method,' V.

[2] 'Meditation,' II., paragraphs 4, 7; *et al.* For Descartes's narrower use of the term 'think' cf. paragraph 5 of '*Meditation*,' II; *et al.*

wills, refuses, that imagines also and perceives."[1] There is here no mention of 'motive power' or simplicity or immateriality: I am conscious self.

This divorce of self from soul, as will presently appear, becomes explicit in the teaching of Locke. (1) Locke follows Henry More[2] in stressing the conception of the soul as "capable of motion," as "changing distance" and as "moving or quieting corporeal motion."[3] He argues simply that "every day's experience clearly furnishes us with" instances of "will or a power of putting body into motion by thought."[4] This is, of course, a mere reassertion of the classic vitalistic doctrine of soul. (2) The immaterialistic soul of the metaphysicians is subjected by Locke to more radical treatment. He resolutely strips it of all the characters of consciousness and then he rightly insists that the "substance of spirits," thus abstractly conceived, "is unknown to us." "If any one," he repeatedly asserts, "will examine himself concerning his idea of 'substance' he will find he has no other idea of it at all but only a supposition of he knows not what support of qualities."[5] "Our idea of substance," he says in another passage, referring definitely to spiritual substance, or soul, "is but a supposed I-know-not-what to support ideas."[6] (3) To the self, or I, Locke now gives over explicitly most or all the concrete characters which soul and self (before his time uncritically identified) once shared between them: consciousness, moral responsibility, and even identity. "The Self,"

[1] Descartes, 'Meditations,' II., paragraph 7.

[2] More breaks sharply with tradition by insisting that the soul, though immaterial, is not therefore unextended. There are two kinds of extension, More teaches: the first, material and impenetrable, the second a penetrable, "subtle and immaterial extension . . . whereof we have an innate ingrafted idea." The soul is, therefore, a thinking, extended substance ('Enchiridion Metaphysicum, Chap. XXVIII., 3 *et al.*) and is "intrinsically endowed with Life and the Faculty of Motion."

[3] 'Essay Concerning Human Understanding,' Book II., Chapter XXIII., paragraphs 20, 19, 15 *et al.*

[4] More, on the contrary, with his conception of spirit as extended and penetrable, finds nothing impossible in the view that a spirit may literally and "easily pass through" and influence "anybody."

[5] 'Essay,' Book II., Chapter XXIII., 2.

[6] *Ibid.*, Chapter XXIII., 15. Locke accords with More in this doctrine. Cf. More's 'The Immortality of the Soul,' Lib. I., Axiom 8: "The subject or naked essence or substance of a thing is utterly inconceivable to any of our faculties."

he declares, is "that conscious, thinking thing (whatever substance made up of, whether spiritual or material it matters not) which is sensible, conscious, capable of happiness or misery, concerned for itself."[1] And by 'person'—a term which he uses as synonym for 'self'—Locke means 'a thinking, intelligent being that . . . can consider itself as the same thinking being in different times and places.'

To speculate on the curious tenacity with which Locke clings to his empty shell of a soul (expressly reduced to an 'I-know-not-what' and replaced by a self to which he attributes persistence, individuality, and consciousness) is irrelevant to our main purpose. Evidently, even Locke's independent spirit could not withstand the force of the traditional doctrine which received from Plato its classic form—the conception of a being endowed with chiefly negative characters, a not-body, uncompounded, unchanging, immortal; and he seems also to have clung to the soul as a resource in the self's frequent lapses of forgetfulness;[2] but modern students, unhampered by the classic convention, have not scrupled to take the step from which Locke instinctively recoiled: they have rejected altogether the merely inferred, 'immaterial,' 'life-giving' soul—but unhappily most of them have failed to take the preliminary precaution of transferring from the soul to the self the actually experienced characters of persistence, individuality, and consciousness.

It is thus evident that our study of the concept of soul has provided us with an answer to the question from which we set out: why do not all psychologists (and philosophers) acknowledge the existence of the self? One answer to this question was suggested in the beginning: psychologists, like other men, are naturally inattentive to the permanent background of all experience; but a second, a historical, explanation of this pertinacious prejudice emerges from our study. From the very start the self has been confused with the soul; that is to say, the soul has been conceived not only as life and as immaterial substance but also as conscious being. Now when modern biologists, following Descartes's lead, hold

[1] 'Essay,' Chapter XXVII., 17.

[2] *Ibid.*, Chapter XXVII., 10.

that organic processes can be mechanically explained—these mechanists in rejecting the concept of soul as life, reject with it the wholly different conception of the conscious being. And when psychologists and philosophers alike discard the partly mythical, partly empty conception of the soul as simple, immaterial substance they too reject also the conscious being. In their justified but undiscriminating attack on vitalism and immaterialism many scientists have thus unwisely rejected the basic fact of psychology: the directly experienced, changing yet persistent, individual, and complex conscious self. And, in similar fashion, the philosophic critics, from Hume down,[1] have been wont to attack not the experienced self but the soul. The remedy is obvious and has already been suggested. The reinstatement, imperatively needed, of the self in psychology requires, first, that the self take over from the soul the experienced characters which comport so oddly with the inferred attributes of soul; and second, that the soul thus despoiled of consciousness, be dismissed from psychology—unless, indeed, the term be retained as mere synonym of conscious self or I.[2] The reasons for this expulsion of the soul from psychology—and indeed from philosophy—need hardly be re-stated. The metaphysical soul or incorporeal, simple, unchanging being is clearly unjustified by experience, an empty abstraction except for the characters—persistence and individuality—which as truly belong to the self. With equal vigor, the vitalist's soul, or guiding entelechy,—opposed by Descartes but warmly upheld in our own day—must be rejected. For some of the facts on which vitalism rests its case, the occurrence, for instance, of life-furthering instincts, may be mechanically conceived; and the rest are not vital phenomena at all but experiences of the conscious self.[3]

[1] Yet Hume distinguishes soul and self. Cf. 'Treatise,' Book I., Part IV., Sections V. and VI.

[2] In this sense, for the most part, Leibniz and Berkeley use the term (cf. the phrase "mind, spirit, soul or myself" in 'Principles of Human Knowledge,' II.) In the doctrines of Wolff, Baumgarten and other rationalists of the eighteenth century the vitalistic and the immaterialistic conceptions of soul recur. Cf. Baumgarten, 'Metaphysica,' III., II, 1, § 750, Wolff, 'Psychologia rationalis,' §§ 48, 49.

[3] It is interesting to find that some of the modern biologists who challenge the extreme claims of mechanism, virtually identify vitalism with idealistic and personal-

There is, in a word, no middle term between the mechanical phenomenon and the self. This study, therefore, culminates in one insistent conclusion: the soul must go. As a historic concept of immense influence it will always retain its prominent place in the history of ideas; as a term of modern psychology it has outlived any use it may once have had and has become a source of mischievous confusion.

istic philosophy. Thus, H. V. Neal writes: "Biologists must . . . accept the idealistic assumption"; and again "the organic individual is in reality spiritual." ('The Basis of Individuality in Organisms; a Defense of Vitalism.' Address before the American Society of Zoologists, *Science*, N. S., XLIV., pp. 82 ff.) Other biologists, of whom J. A. Haldane may serve as type, reject conventional vitalism along with pure mechanism and count themselves with the personalists ('Mechanism and Personality').

RELATION BETWEEN STRUCTURAL AND BEHAVIOR PSYCHOLOGY

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The purpose of this paper is to show that the problems in the study of mind which are ordinarily the problems of the structural psychologist, may be studied from the behavioristic point of view, in accordance with the methods employed in the natural sciences and with a greater degree of simplicity than is possible from the structuralistic point of view. No attempt will be made to determine whether behaviorism is or is not psychology. The title of this paper might also have been 'The Value of the Objective as Compared with the Subjective Method in Psychology' were it not for the fact that the terms 'objective' and 'subjective' because of their uncertain connotation, are in evil repute both in psychology and in all the social sciences.

Problem of Structural Psychology

To state the structuralists' problem is not an easy one if justice is to be done to all the psychologists who are representative of this school. There are, however, certain fundamental conceptions which are characteristic of structuralism that are generally accepted, and it is to these that the discussion will be limited.

By the term structural psychology is meant the type of investigation which assumes that there is an existential datum called mind or consciousness, within the totality of which ultimate differences may be discriminated which are given the names of (1) sensations, (2) images, (3) affections. These are usually regarded as the elements into which consciousness may be analyzed. The aim of the structural psychologist who accepts this analysis is to describe consciousness, in all its complexity, in terms of these three fundamental classes of consciousness. There is, of course, considerable difference of opinion between structuralists as to just how these classes are to be defined, and indeed, whether there are no more and no less than three elementary categories. But for our purpose these details may be disregarded. It is also irrelevant for us whether it is held that consciousness is present in a situation in which sensations, images and affections, as such, are absent. It is sufficient if we recognize clearly that the structuralist aims to describe the structure of the mind or consciousness, under the manifold conditions of present-day life, in terms of whatever elements he may have set up, and that he considers the descriptive phase of his investigation as complete when he has done this. The explanatory phase of the structuralist problem is the determination of the neural correlates of the conscious states which his analysis reveals.

The direct method by which the structuralist analyzes a conscious complex into its elements is that of introspection. This cannot be done satisfactorily under normal conditions. It is therefore customary to create an experimental situation similar to the one to be analyzed, in which the observer is asked to report in language the mental states that were present during the experiment. These reports of the mental states are then treated statistically. The experimenter assumes that the verbal reports of mental states are not the mental states which the observer introspected, but that the reports are merely expressions which describe the character of these mental states.

There is considerable difference of opinion among structuralists themselves as to whether introspection gives equally valid results in all cases where it may be used. Some maintain that even the most complex mental states may be analyzed by the introspective method, while others maintain that introspection modifies complex conscious experiences so that the elements which it reveals may not be present in the actual experience which is being investigated.

Origin and Problem of Behavior Psychology

As the applications of structural psychology became more numerous in the various applied fields of psychology such as education and medicine, the question as to the relation between conscious states and action arose. An increasing number of psychologists believed that sensations, images, memories, thought, emotion, affection, etc., were the causes or invariable antecedents of certain forms of human behavior. This naturally led to the question as to how consciousness was related to changes in the form of human behavior. Now, since no one denied that, in order to modify behavior, a corresponding change in the neural correlates of consciousness must take place, this inquiry necessarily came to be stated as follows: How can consciousness change the direction of a nervous process? In its most general form this question is an inquiry into the relation between mind and body. In animal psychology the mind-body problem is not an acute one for the zoölogist-psychologist who studies animal behavior; and the success which these investigators have had in explaining some of the most complex forms of animal behavior in purely neural terms (tropisms, reflexes, instincts) has led some of the psychologists to ask whether human behavior could not be placed in the same category with animal behavior and explained without the introduction of a mental or conscious factor.

The attempt to answer this question by the application of the behaviorist point of view to such relatively simple forms of human behavior as reflexes and automatic action was crowned with considerable success, but for the more complex behavior of learning the strictly neural explanation is not readily accepted by psychologists. It will not be necessary at this stage to state the behaviorist problem in detail except to indicate that the behaviorist is concerned with determining the properties and laws of the neuro-muscular system, of which the introspective reaction is a part.

Briefly, we may state the problem of the structuralist as an attempt to answer the question: *What are our mental states* *and how do they come about?* while the behaviorist problem is: *What are our actions and how do they come about?*

The following paragraphs are devoted to a discussion, from the behaviorist point of view, of those factors of structural psychology which give it its distinctive character: namely, the relation between mind and body or the character of consciousness; and the character of introspection.

Behaviorism and the Mind-body Problem

Discussion by psychologists of the mind-body problem has led to the formulation of the three following positions:

1. Consciousness does not enter as a causal agent into such actions as reflexes and instincts, but it does function in what is usually known as intelligent or voluntary action.

2. Consciousness cannot be regarded as the invariable antecedent to any kind of action whatsoever.

The first position is the one usually taken by popular psychology and the group of psychologists known as "functionalists," while the second position is the one maintained by behaviorists. The position of the structuralists is usually known as the double-aspect view, in which there is,

3. Parallelism between conscious processes and neural processes without a causal relationship between them.

This position is best described by the following quotation:

> "Our own position has been that mind and body . . . are simply two aspects of the same world of experience. They can not influence each other because they are not separate or independent things. For the same reason, however, whenever the two aspects appear, any change which occurs in the one, will be accompanied by a corresponding change in the other."[1]

The third view may be regarded as an intermediate position between 1 and 2. That is to say a functional relationship does exist between mind and body but only in the mathematical sense of the term function, as when we speak of the volume of a sphere as a function of its radius.

If this functional relationship between consciousness and behavior is a simple one it would be useful for the behaviorist, because consciousness could then be used as a measure of

[1] Titchener, E. B., 'A Text-Book of Psychology,' 1912, p. 9.

behavior, no matter what the ultimate connection between mind and body might be.

Setting aside all speculation about this connection we find that the facts in the case may be stated in the following propositions:

1. Consciousness (the totality of our sensations, images and affections) is a purely personal experience and has no scientific value or validity unless it is *expressed* in some form of behavior, such as speech or other form of representation.

2. Many forms of behavior (reflexes, automatic action) are not accompanied by consciousness which can be unambiguously analyzed.

3. The consciousness which does accompany a given form of behavior varies from one observer to another, and at different times for the same observer.

4. Complex mental processes, such as reasoning and invention, are more than the mental states into which they may be analyzed. They have a social reference which no amount of introspection will reveal. This social reference is measured by the comparison of the individual's behavior, either with his ordinary behavior or with that of other individuals.

From the above propositions the behaviorist feels justified in making certain deductions:

From proposition 1: If consciousness must be expressed in some form of behavior before it becomes a scientific datum, then consciousness is in the last analysis a classification and a study of behavior.

From proposition 2: If there are many forms of behavior (reflexes, automatic action) which are important from a social or individual standpoint, which are not accompanied by consciousness, then consciousness is not a function (mathematical) of *all* forms of behavior.

From proposition 3: If the consciousness which does accompany a given form of behavior, varies from one person to another, or for the same person at different times, then consciousness is either an independent variable of the behavior, or a dependent variable of highly complex formula. In neither case is it any better measure of behavior than the behavior itself, and consciousness then becomes superfluous as a means for predicting behavior.

From proposition 4: If complex mental processes such as reasoning or invention are more than the mental states into which they may be analyzed, then in this case too the analysis of consciousness does not make it possible to predict the individual's behavior.

In view of the above implications, the very simplest assumptions as to the quantitative relations between sensations, images and affections which characterize an individual's conscious complexes, it is impossible to predict what his mental states will be after an hour. An individual's behavior, however, can be predicted with much greater certainty than can the character of the conscious processes which go with it. A daily photographic record, for example, will show greater uniformities in the behavior of a given individual so far as the socially significant factors are concerned, than will the daily introspective descriptions of the consciousness which accompanies his behavior.

It would seem then that the sensations, images and affections which one experiences are more variable than is behavior, since by the term behavior we usually refer to only the grosser bodily movements and to the speech reactions. The finer changes in muscle contractions, as in respiration, digestion, vascular and glandular changes, are ordinarily not considered as socially significant.

For the behaviorist, then, the double-aspect view of the structuralist is of no value, since the function (consciousness) of the behavior is more variable than the behavior itself.

Consciousness an Inference Derived from the Introspective Reaction

The structuralist is usually disposed to consider the investigations of the behaviorist as quite outside of his field, because consciousness or mind is something entirely distinct from the contraction of a system of muscles. In the last analysis, however, the consciousness of his observer which the structuralist regards as the unique phase of his investigation, is only an inference from the verbo-motor behavior which is called introspection. Structuralists' experiments are controlled reactions to stimuli just as in any behavior experiment. The sensations, images and feeling that are described, are not experienced by the structuralist. They are inferred from the behavior of his observers.

When for instance, an introspective reaction reads, "My attention fell upon the central projection, and vocal-motor imagery of 'Gosh, that's ugly' occurred. I was aware of contraction of my brows and unpleasantness." The structuralist infers that these speech reactions are descriptions of the consciousness that he has when he makes the same speech reactions. To every introspective report of his observer, the structuralist adds, at least implicitly, the speech reaction, namely: "The introspective report of my observer completely describes certain conscious processes of which I am myself conscious or aware, when I make the same speech reactions."

In other words, psychologists have agreed among themselves that an introspective report is more than a reaction; it is a verbal reaction plus some kind of a conscious process, which may be either sensorial, imaginal or affective. The behaviorist may well ask of what scientific value is this habit of supplementing, in a methodological sense, the observer's reaction by another reaction of an entirely different type (consciousness) which the observer did not report. By inferring a conscious correlate to the introspective report—the experimenter is not able to derive anything more from the report than is expressed in it. The manual and introspective reactions of the observer in a psychological experiment can be classified just as minutely and scientifically without inferring a conscious correlate, as they can with it.

Behaviorism and Introspection

From the behavioristic standpoint, introspection may be regarded as an example of habit formation or learning. An untrained observer cannot make these introspective speech reactions in a manner which is constant and uniform enough to permit of statistically treating the results. In order that these supplementary speech reactions may become uniform enough to admit of statistical analysis, two methods of simplifying the results are usually employed:

1. The observer passes through a preliminary period of training in which the reaction time of relevant speech reactions (report of imagery, kinesthesia, etc.) is reduced to the experimental requirements, and those speech reactions which are irrelevant are given an opportunity to disappear. The relevancy of a given speech reaction is usually determined beforehand according to the specific aim of the experiment.

2. The experimental situation is so modified by supplementary situations in the form of instructions, questions or other controls that the required uniformity of the introspective reactions is secured.

The first method is the one with which we will especially concern ourselves in the following discussion.

As already indicated, the total reaction of an organism to even the simplest situation is very complex. Respiration, secretion, circulatory effects, incipient and minute bodily and visceral movements, are always added to the particular reaction (the discrimination of two points, for instance) which might be called the major reaction of the experiment. The minor reactions by themselves would escape notice, but the speech mechanism is a rather sensitive index as to the character of these minor reactions. Permitting a free verbal report after each trial or series of trials in an experiment, reveals more as to the character of the *total* reaction than can be learned from the major reaction alone.

Organic and kinesthetic reactions which would escape observation entirely are thus easily revealed by the speech reaction. If, for instance, an observer who has been trained according to method (1) reports a 'kinesthetic sensation of movement of the neck' that report from the behaviorist point of view would mean, 'an incipient or slight movement of the head,' provided his methods of measuring such movements were accurate enough. In this way the verbal reaction 'kinesthetic sensation of neck movement' really indicates that the receptors in this case are located in the neck muscles.

The speech reaction may therefore be regarded as a function of particular kinesthetic and organic reactions which ordinarily would escape observation.

To report kinesthetic and organic conditions through speech is really a highly specialized example of habit formation. Indeed introspection, whatever its kind, from this point of view may be regarded as a continual process of training in the formation of habits. For this reason many years of training are necessary even for those who have special aptitude in this direction, and many persons never learn to introspect well.

The training of an individual in introspection is, therefore, a process of:

1. Training in *discrimination* so that weak stimuli in obscure receptors lead to verbal reactions.

2. *Substituting* for the usual anatomical and physiological descriptions (head, neck, arms, muscles, viscera, etc.) the special terminology of the structuralist (sensations, images and affections).

The relation of these obscure receptors to the major reaction of the experiment may be of a highly adventitious character. A trained observer does not react exclusively to the experimental situation—one is almost tempted to say that he reacts to everything but the experimental situation. An experiment with trained observers measures principally the training of the observers, it does not necessarily reveal what the normal individual could be expected to do under similar situations or conditions.

From the behavioristic standpoint, then, introspection may be regarded as a greatly augmented reaction to a given situation. The observer not only reacts to the situation, for example, by pressing a key, but he is also asked to react by speech to the stimulation of many secondary and obscure receptors, to which, under normal conditions, no such reaction is made.

Heuristic Value of Introspection

When the structuralist experiment is stripped of its conscious reference and regarded from the standpoint of objective science, its twofold character immediately comes to light as may be seen from the following quotation which is part of the conclusion of a long and painstaking series of experiments "On the Analysis of a Phase of the Process of Classifying."

"The essence of the process of classifying, as this process occurred in our experiments, consisted in the manner of our observer's perceiving the object which he had been instructed to classify. This manner of perceiving consisted in the fact that the region of essential group features—were stressed in consciousness, and these regions behaved in consciousness in a fashion which depended upon their resemblance or lack of resemblance to the corresponding features in the group members. In the former event, the regions in question passed in and out of consciousness in rapid and ready fashion, without retarding the course of attention. In the latter case, on the other hand, the course of attention was arrested sharply; these regions often persisted in consciousness, and they were frequently accompanied sooner or later by more or less focal and intensive kinesthetic, organic and affective contents which functioned in their conscious settings as definite rejectings of the figure."[1]

Will the above conclusions, we may ask, enable a botanist or zoologist to classify his specimens more effectively, or will the scientist know any more about classifying than he did before he read the article? The question may even be asked, whether this particular experiment is really an investigation of the process of classifying. From the behavior point of view at least, the investigation would be regarded as an experiment in discrimination and habit formation, carried out under the following conditions:

An experimental situation was created which approached the conditions under which people make classifications, except that the classifications are rather more than ordinarily difficult. A number of observers who had been carefully trained to respond verbally to the weak stimulation of obscure receptors, were asked to perform a double task; to classify the cards, and also to react to the obscure receptors by verbal

[1] Fisher, Sara C., 'An Analysis of a Phase of the Process of Classifying,' Am. J. of Psychol., 1917, 28, p. 115.

I have chosen this particular illustration because I regard it as one of the best structuralist experiments that has appeared for some time and I need hardly say that no criticism is intended either of it or the method by which the investigator carried it to completion.

reactions which conformed to a more or less uniform terminology. An analysis of the author's conclusion quoted above, in behaviorist terms, may be stated in the following propositions:

1. The observers were able to classify the cards.

2. The observers reacted either to the similar or to the dissimilar features of the cards. The reactions to the similar features were relatively simple; the reactions to the dissimilar features were supplemented by verbal reactions to obscure kinesthetic and organic receptors.

The whole emphasis of the experiment is placed upon what the behaviorist would call the reactions to obscure receptors. The actual process of classifying the cards was merely a device to obtain some degree of uniformity in the verbal reactions to these obscure receptors. From these considerations we can see why the experiment has so little significance for the scientist who really wishes to increase his classifying efficiency. The development of the ability to react to the weak stimulation of obscure receptors may be of value in the above experiment as a means of determining when the investigation of the classifying process was being supplanted by the minor reactions which have no direct bearing on the process of classifying.

That is to say, the presence of sensations, images and affections, indicate that there is something wrong with the experiment.

In ordinary scientific observation, the aim is to eliminate the unessential or obscure so that the effect of the major condition can be observed in isolation. Under behavioristic methods, however, if it is desired to investigate the minor reactions independently, then every effort is made to isolate them and they of course then become the major reactions. But the introspective reactions in this particular experiment would be regarded by the behaviorist as a disturbing factor.

In our effective adjustments to our environment, we do not have sensations, images or affections. When we stop to introspect as to the character of our consciousness in a given situation, our reactions to that situation become to this extent irrelevant. The writer has frequent occasions to design modifications in apparatus used by the students working on original problems. Having also some epistemological interest, it often happens that he begins to introspect on the method by which the designing process is going on. When this happens, he might as well stop. Introspection seriously interferes with the designing activity. The reactions, incipient though they be, which are called introspections, change the character of the designing activity to such an extent that it can no longer be called the designing activity. This is gone; he is merely reacting to a new situation by making aimless sketches and many incipient verbomotor processes which if recorded might read, "Bronze contact, visual image of tuning fork, kinesthetic sensations of neck muscles, slight unpleasantness, image of Mr. M. before chronoscope, visual image of Prof. X. rushing upstairs, etc."

None of these reactions would have occurred had he actually completed the original design. This is, of course, not a unique situation. Under the ordinary conditions, sensations, images and affections are absent. The average man probably never has them; he must be trained to have them in the same way that we must be trained to read and write. Introspection is only one of the ways by which we may react to a situation. How the reactions themselves originate, or what the conditions are under which they become modified, is not revealed by introspection.

Tautological Character of Introspection

By selecting a somewhat simpler activity than that of "classifying" it can be shown that the introspective records are merely special terms for particular classes of reactions. Suppose we take the process of recognition from the structuralist standpoint. We may assume that the recognitive consciousness is made up of a characteristic pattern of sensations, images and affections. Suppose the experiment is one in which we are to select a card (*A*) which has been previously examined, from a series of similar cards which have not been previously examined. We wish to know whether the card (*A*) has been recognized. To do this, the behaviorist would inspect the introspective reactions of the observer. Suppose the verbal reactions to the card (*A*) to be the following. "I have seen it before; I have a visual image of it as it appeared when I saw it for the first time; I have the feeling of familiarity; I have organic and kinesthetic sensations (images?), which mean that I am handling it in a more energetic and discriminative fashion than the other cards; etc." To the new cards the verbal reactions might be as follows: "I have never seen these cards before; the kinesthetic and organic sensations are different than for card (*A*); they do not carry the meaning of familiarity; the visual and auditory imagery is different than for card (*A*); etc." It is not supposed that this is the complete introspective record, and we disregard for the present such objective measures as the reaction time, or of the bodily movements which might have been made.

If we regard the introspections from the purely objective side as being muscle contractions of the speech mechanism which produce the various sounds which we call words or speech, we can immediately see that the observer has reacted differently to the card (*A*) than to the other cards. It is not necessary to interpolate a conscious process (of recognition) in order to see this. The fact is, that if the observer has used the identical words to describe both the (*A*) and the new cards, we should be obliged to conclude that there was no difference in the observer's consciousness of the (*A*) and the other cards. That is to say, a difference in consciousness can only be inferred from a difference in behavior. Moreover, the interpolation of consciousness does not explain how it came about that the card (*A*) was reacted to in a manner different from that of the other cards. The introspective record merely reiterates the fact that the observer *did* react differently. This could have been seen directly by regarding the report of the introspective reactions as what, in fact, they are—objective reactions of the speech type. Because the observer reacted differently to the (*A*) than to the other cards, the experimenter says he has given a recognizing reaction. We cannot say he reacted differently because he recognized, but that he recognized because he reacted differently. In this sense the inference of the conscious process of recognition is tautological.

Structuralism a Phase of Behaviorism

The structuralist point of view can, of course, be consistently maintained. There is a justification for inferring the existence of a conscious correlate for at least some of our actions, but the heuristic value of this assumption seems doubtful when it is shown that behaviorism is not less discriminative or descriptive than structural psychology, and in addition it has the promise at least of a system whose parts are causally related to each other in the sense that a temporal description of a given form of behavior is more uniform than the temporal description of the conscious states which are said to go with it. If this is true then the law of Parsimony will operate to eliminate the unnecessary assumption of a conscious correlate for behavior (implied by introspection). When we recall how we are being exhorted to recognize the unconscious, subconscious, higher thought processes, unanschaulichen Bewusstseinsinhalt, one wonders whether this process of elimination has not gone further than we suspect.

For the behaviorist the structuralist's classifications do not lead to a solution of the problem, What are our actions and how do they come about? or How do the manifold social adjustments of the adult grow out of the relatively simple reactions of the child? Even if the objective side of the structuralist problem be further developed, the neural correlate of conscious processes will only be a very special problem in behavior. Sensations, images, affections, emotions, will, the self, recognition, etc., so far as science is concerned are only special instances of receptor-effector activity. While this is of course very generally recognized by psychologists, the further assumption that these forms of receptor-effector activity are highly significant for our understanding how man is able to make his effective adjustment to his environment, is not warranted, because the analysis and isolation of such a neural correlate does not indicate how the neural correlate itself came to have the configuration that it has.

This, of course, can only be done by a genetic study of the particular type of reaction. For example, when the structuralist has determined the neural correlate for the process called "recognition," his explanation is complete. For the behaviorist this is merely a type of reaction whose effector-receptor phase is described by the proposition: To objects to which we have reacted once, we do not react in identically the same way the next time they are presented, even though the stimulus conditions approach identity for both occasions. This is merely a statement of fact which becomes valuable only when stated quantitatively.

The more important scientific question: *How does this action come about*, can only be answered by increasing our knowledge of neural *function*.

It is in the above sense that we may regard the introspective reaction of the structuralist as only a part of the total problem of determining how man makes his manifold adjustments to his environment.

Basis of Behaviorism

In order to state the position of the behaviorist more concretely, the following assumptions as to the explanation of human behavior are presented.

1. The resistance of neurons varies with function.

2. Every receptor is directly connected by a neural chain with a restricted effector system, and through varying degrees of indirectness to many other effector systems.

3. One nervous process will modify the character of other nervous processes that occur together with it.

4. Certain configurations of neural connections between receptors and effectors are inherited, some are acquired.

These assumptions are not essentially different than those formulated by Max F. Meyer.[1]

From these four laws or assumptions it is evident that there is a possibility of greater variations in the response of an

[1] 'The Fundamental Laws of Human Behavior,' 1911.

organism than is usually included under what the structuralists call the study of mind.

Each of the four propositions represents a series of problems which may be experimentally handled in the same manner that an experiment in any other science would be conducted. There need be no quarreling about consciousness or as to whether there is or is not such a thing, since it will make no difference in the effectiveness of the work of the behaviorist or any scientist how the question may finally be settled. In their present form the propositions represent little more than a program for future work. Each proposition will be divided and subdivided and coefficients will be supplied to the various derived propositions. How fruitful the analysis will be cannot of course be predicted, but if structural psychology, with all the painstaking and careful work that has been done on sensorial and imaginal processes, has not yet developed a definition of sensation upon which two psychologists will agree, then behaviorism can expect a considerable degree of tolerant good will.

Summary

1. The close academic relationship between structural psychology and behaviorism is due to the popular belief that mind and body are related in such a way that mind produces or modifies behavior.

2. A conscious state which is not expressed in some form of behavior is, so far as science is concerned, non-existent. The inference of consciousness is, therefore, unnecessary, since in the last analysis behavior is the only thing that can be classified.

3. For the behaviorist the introspective reaction is only the habit of being able to react by speech, more or less adventitiously, to the weak stimulation of obscure receptors.

4. Even if it is admitted that the special introspective habits are correlated with conscious processes, the latter are so variable that they cannot be used as an index from which to predict behavior which is socially or scientifically significant.

5. The method of introspection favors the reactions to obscure stimuli and in a corresponding degree decreases the

reliability of the major reaction for the investigation of which the experiment is designed.

6. Behaviorism presents as manifold possibilities of analysis and classification as does structuralism, and has the added advantage that its phenomena can be represented as a causal series in the same sense as we now speak of causal relationship in the natural sciences.

7. When the behaviorist solves the problem: How do our actions come about? all the problems of the structuralist will also be solved.

8. The relation between structuralism and behaviorism is such that they may exist side by side. This will occur only when the fundamental conceptions underlying both methods are not very closely scrutinized or where the individual is not particularly interested in the theoretical implications of either the one or the other method.

DISCUSSION

MEANING AND IMAGERY

In the discussion of imageless thought one must bear in mind the fact that there is more than one way in which it might be held that thought is imageless.

1. One might for instance say that thought is not only not identical with imagery, but that it is always and under all circumstances independent thereof, that imagery is in all subjects and at all times subsequent to thought and never helps in the solution of problems that can be thought about. This would be an "out and out imageless position," but one which, so far as I know, no one has yet taken, though some approach it. It would also contradict various pieces of experimental work.[1]

2. One might hold that thought is imageless in the sense that it is a mental process *sui generis*, distinct from imagery, but never imageless in the sense that it is ever present in the mind unaccompanied by imagery. One taking this stand might even maintain that the thought process is dependent on previous mental imagery—in the sense that the image is the source from which the thought process is derived.

Such seems actually to have been the theory of Aristotle who, while distinguishing between and even contrasting *ἐπιστήμη* and *αἴσθησις*, maintained that the mind never thinks without *φαντάσματα*. In fact whenever the mind sees, it is necessary for it to behold imagery—*ὅταν τε θεωρῇ ἀνάγκη ἅμα φάντασμά τι θεωρεῖν*. The next sentence shows that he is really speaking of imagery rather than the forms of sense perception: *τὰ γὰρ φαντάσματα ὥσπερ αἰσθήματα ἐστι πλὴν ἄνευ ὕλης*.

So also St. Thomas maintains that the human intellect cannot actually understand anything without recourse to images. It is interesting to note that this position was taken on the following empirical grounds:

(*a*) When cerebral lesions are sufficiently serious to affect mem-

[1] Cf., for example, George Herbert Betts, 'The Distribution and Functions of Mental Imagery,' Teachers College, Columbia University, New York, 1909.

[2] *περὶ ψυχῆς*, III., viii, § 3.

ory and imagination they also interfere with the orderly course of thought.

(*b*) An appeal to introspection—"quia hoc quilibet in seipso experiri potest"—viz., "when one attempts to understand something he forms to himself certain images by way of example." Likewise when we want to explain a matter to someone else we give examples from which he can derive images that will lead to understanding.[1]

3. Without adopting either of these positions, one might say that while the image is frequently very useful, nevertheless, a thought process can be present to the mind without simultaneous imagery and that it is not necessary for us to draw all our meanings and thoughts from the contemplation of imagery. Thus thought would be imageless in the sense that it is distinct from the *φαντάσματα*. It would often be imageless, too, in the sense that it would not be dependent on the *φαντάσματα* or preceded or accompanied by them.

That the extent to which one makes use of images, and the rapidity with which they come, should be subject to individual variations is certainly to be expected.

In an article on the 'Temporal Relations of Meaning and Imagery,'[2] I pointed out that with eight out of nine subjects I found that meaning preceded visual and kinæsthetic imagery in the perception of printed words, but that in memory, on the contrary, imagery precedes meaning. I was thus led to adopt the last of the three positions I have above outlined.

I did not maintain that this relationship of meaning and imagery in perception was universal. I was nevertheless inclined and still am inclined to believe, mainly on empirical grounds, that it will be very rare that individuals will be found who present genuine exceptions. The one subject who proved an exception in my experiments, did not learn to refrain from reacting to nonsense words, so that one could not be sure of just what he was reacting to, when supposed to be responding to the task meaning or imagery.

I remember talking the matter over with Bühler, who thought that exceptions would prove more frequent than I supposed. Since then, I have often wondered what experiments on a larger number of subjects would reveal.

In the March issue of the Psychological Review, Edward C.

[1] Cf. 'Summa Theologica,' I., Q. lxxxiv, § 7.

[2] Psychol. Review, May, 1915.

Tolman has undertaken such an investigation on 49 subjects. He attempts a preliminary sorting out by having his subjects react to words representing black or white objects: (*a*) when they knew whether it was black or white irrespective of the way in which this was known; (*b*) when they could see it from their visual image. All but fourteen of the subjects gave shorter times for knowing than for visualization. I regret that this modification was made use of, because it changes the whole situation. It introduces a new problem and one in which visual imagery might be very useful and would, therefore, be more likely to be used than in the former situation. I am inclined to believe that I would be ranged among the visualizers in attempting its solution.

Unfortunately this preliminary sorting cannot be compared to the word perception test and we thus still lack evidence of the relative frequency of the cases in which imagery precedes meaning in the process of perceiving printed words.

Having selected fourteen subjects by this preliminary experiment as likely to prove an exception to the usual order he subjected them to the same type of experiment that I used in Munich. He finds, nevertheless, that three of these gave the same order of sequence that I found in Munich. Three subjects gave doubtful results. Eight gave results distinctly(?) pointing to the opposite sequence, so that with them reaction time to meaning was longer than to imagery. In the last group he finds two subdivisions. The first comprises those who as a rule tend to distinguish between meaning and imagery.

Let us consider this group a moment and see why their reaction times to meaning are longer than to imagery. In every one of the introspections given (pp. 133–134) the meaning is analyzed and more or less roughly defined and does not correspond to the 'simple meaning' spoken of in my paper. Sometimes the meaning is evidently a 'concept of purpose' for which I also found much longer reactions. I have not time here to discuss 'analyzed' and 'unanalyzed' meanings, but hope to do so soon in a monograph on 'Perception and Memory' now almost ready for publication.

Meaning is a process which undergoes development. It is not surprising that some subjects wait until it has developed and they feel sure of a definite analyzed meaning before reacting. In these subjects one is likely to get reaction times that are longer than those for visual imagery. This expectation seems to be realized in the reaction times of this set of Dr. Tolman's subjects and confirmed by their introspections.

The second subgroup of subjects did not distinguish between meaning and imagery and their introspections show that when reacting to what they called 'meaning' they were as a matter of fact reacting to images. In only one member of this group is the difference between the averages greater than the mean variations. In none of the others is it even half as great. It might be safer, therefore, to place all but one of these subjects in the doubtful group. Whether the task is meaning or imagery they react to one and the same thing and naturally give in either case about the same results. The general slight tendency to a longer reaction time with meaning and the single pronounced tendency in that way with one subject might readily be accounted for by their being called upon to pick out what they have not been accustomed to label.

Dr. Tolman's assumption that with them meaning *is* imagery supposes too great a distinction of types. It is possible, but scarcely probable, that one set of normal men understand and visualize, whereas the other visualize but never understand. I do not doubt that other experiments would show that these subjects were not wholly devoid of understanding as something distinct from imagining.

Dr. Tolman's study does little more than raise the question: Are there really any genuine exceptions to the hitherto observed relation in the sequence of meaning and imagery? But what would be the import of finding exceptions—and, let us say, even numerous exceptions to the rule? I do not see that the demonstration of even a large number of exceptions would weaken the argument, drawn from temporal sequence, to establish a distinction between meaning and imagery.

Let us suppose that two events moved usually with the same velocity, so that we could scarcely ever detect one without the other. If, however, under certain circumstances one moved more slowly or more rapidly than the other, so that we could observe one without the other these cases alone would enable us to say that the two things are not really identical.

If meaning is identical with imagery then the two events must *always* come together. If it is not then you will find some subjects in whom their rate of development is not identical and one will come before or after the other.

If meaning is dependent on imagery, *e. g.*, in the Aristotelian sense, then imagery will always precede meaning.

We find on the contrary that meaning as a rule precedes imagery.

It is, therefore, not identical with it and as a rule does not depend upon it in the process of the perception of printed words representing objects easily visualized.

Dr. Tolman's paper confirms this very important fact. At the same time it presents no absolutely conclusive proof of subjects who constitute an exception to the rule. It is quite possible, however, that some subjects will be found capable of distinguishing between meaning and imagery, in whom the development of images is so rapid that their mental pictures will usually precede their simple unanalyzed meanings. Such a fact would be significant in the study of types of individuals, but would have little bearing on the more theoretical problem of the existence of imageless thought.

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SOME EXPERIMENTS IN MOTOR REPRODUCTION OF VISUALLY PERCEIVED FORMS.

Some little time ago I had occasion to observe experimentally certain cursive figures of an unusual type while engaged in an experiment along lines quite different from that of the experiment here reported. These figures were presented by means of the Whipple tachistoscope. They were to be learned by means of frequent repetitions, so that they could be reproduced with pencil on paper. The experiment had to do with the relative advantages of learning figures by means of copies in strong relief traced by the fingers of blindfolded subjects, and by means of copies of similar figures exposed to visual perception by the tachistoscope. I acted as a subject for both forms of learning, and had opportunities to observe closely other subjects at work.

Early in the course of this experiment I became interested in the fact that the behavior of the subject at the time of attempting to reproduce the figure was an important factor in determining the accuracy of the reproduction. I found it to be true of myself, and it seemed to be true of other subjects, that if one watched what he was doing while attempting to make the reproduction it seldom happened that the result was as satisfactory as if the reproducing was done with eyes shut or averted. After casual inspection I found that practically all the subjects concerned were far more satisfied that their reproductions resembled the model closely when these reproductions were made without the subjects seeing what they were doing. The point seemed to me to be of sufficient interest to warrant some investigation.

One may have a pretty definite idea of just what he wishes to draw, and may take his pencil in hand with absolute certainty that he will draw it quickly and easily. But as he draws it his eyes follow the outline being reproduced. Does the sight of what he is drawing aid him in completing the figure accurately? Or do the visual perceptions interfere with and destroy the guiding memory images? It is to be understood that the figure which serves as model is not in sight during the reproducing process.

The problem finally assumed the following form: "If one is reproducing on paper an outline which he has just memorized what effect does the visual perception given by the figure in process of being reproduced exert on the visual or kinæsthetic images which are directing the process?"

A series of figures was made which could be presented on the tachistoscope. It was not particularly easy to find figures which were sufficiently new to really require memorization. It would not do to have the figures so made that one could easily describe them to himself in terms of association with other forms or objects. We had recourse to the Arabic alphabet, and, by adaptation and combination, constructed therefrom several figures suitable for our use. There were ten of these figures finally selected, and they were arranged in two groups of five each. In each group these figures were arranged in order of difficulty; or rather, we attempted so to arrange them. We did not discover any principles which would give an adequate *measure* of difficulty, and were forced to be content with arranging them according to the general complexity of the figures, their number of curves, loops and line crossings. We were careful to vary the order of presentation of the figures so that if any were markedly harder than others that fact would not influence the results in one direction more than another.

There were two methods of reproducing the figures after they were learned. In the first method, hereafter called the "sight" method, the subject would watch what he was doing while drawing the figure. In the second or "blind" method a screen was interposed between the eyes of the subject and the paper so that he would not see what he was doing while trying to draw the remembered figure.

The following directions were given to subjects before reacting according to the sight method:

Fixate cross. Observe the figure which appears, and after the smallest possible number of exposures draw it on the paper in front of you. Try to get both direction of turns and general proportions of figure correct.

Subjects about to react according to the blind method were instructed as follows:

Fixate cross. Observe the figure which appears, and after the smallest possible number of exposures draw it on the paper in front of you, but keep eyes on the screen between you and the paper while you are drawing it. Try to get both direction of turns, and general proportions of figure correct.

One half of the subjects followed first the sight method on one set of figures, and then the blind method on the other set of figures. The other half of the subjects used the blind method first and then the sight method. In half the cases the figures used for the sight process were those of Set I., and in the other cases the figures were those of Set II. So that it is true that the same figures were sometimes used for reproduction with sight and sometimes without sight. Also, the same set was at times presented first, and sometimes second. It does not seem that the results could have been influenced by any possible differences in difficulty between individual figures, nor that any possible practice effect was exerted on one reproducing process more than another.

The exposure time averaged 65/100[1] of a second in length. The subject was told that he could have as many exposures as he wished, but was urged to request as few as possible. It was made very plain that he must make every effort to keep the number of exposures few in number.

There were twenty subjects. All but one were students in Oberlin College, the exception being an instructor in economics. All but three were taking courses in the psychological laboratory. The exceptions were the instructor just mentioned, and two students who had had a good deal of training in art. The fact that they had done practical work in drawing suggested that their inclusion in the list of subjects might furnish some interesting variations in results. In point of fact their reactions were in no respect different from those of the other subjects.

[1] The working time of the Whipple tachistoscope varies quite a good deal, but this was the average time. In the tachistoscope in the Oberlin laboratory we have replaced the usual cardboard discs with light metal (ordinary sheet tin) discs, and we time the length of exposure by means of two make-and-break contacts, so arranged that when the disc revolves a circuit is broken at the point at which the exposure begins. This circuit is remade when the disc reaches the place at which the exposure is completed. A magnetic marker is arranged in series with these contacts. This method may leave something to be desired from the physical standpoint, but, after all, it furnishes one with some real information concerning the exposure time.

Eleven subjects used the sight method of reproduction for the figures of Set I., and the blind method for Set II. Nine subjects used Set II. for the sight method, and Set I. for the blind method. It was merely accidental that there were not ten subjects in each group.

When the time came to sum up the result of the trials, it was necessary to find some method of determining the relative accuracy of the figures as reproduced. No completely satisfactory method of measuring occurred to me and I finally arranged arbitrary principles of scoring, and applied them to all the figures alike. The rules which governed the scoring were as follows:

1. Score 1 for each exposure required for learning.
2. Score 1 for a curve in the wrong direction.
3. Score 1 for very great error in the proportion of parts.
4. Score 1 for the insertion of any marked alteration.
5. Score 1 for omission of an essential part of figure.

(Do not score 2 if an essential omission has been part of the result of a curve in the wrong direction.)

It can be seen that on this scheme the lowest score is the best, that there can be no score lower than one, and that there is no maximum score. It should also be noted that the number of exposures required plays an important part in the final score.

The total score made by the twenty subjects when seeing what they were drawing was 381. This gives an average of 19.05 for each person, or of 38.1 for each figure.

The total score made by the twenty subjects when drawing without seeing what they were drawing was 326, an average of 16.3 for each person, and 32.6 for each figure.

It may be objected that the number of exposures should not have counted in the summing up of the results. Some of the subjects may have asked for more exposures than they needed, or might have made equally good reproductions with fewer exposures than they actually requested. I feel that the number of exposures must have had some effect upon the accuracy of reproduction, though I am not quite satisfied with the particular method used for exposing the figures, that is, by means of the tachistoscope. But if we sum up the results and neglect the number of repetitions entirely, the relations of the figures for sight and blind methods is not thereby changed, although the values of the figures alter somewhat.

The total score for the sight method, not taking the number of exposures into account, is 130, and the total for the blind method, under the same conditions, is 120. The average in the first case is

6.5, and in the second case 6. The difference between the two methods is not so great as when the number of exposures is included in the score, but in both cases the score for reproducing the figures without sight is lower, that is, is better than when sight is used. That is to say, *the scoring methods used show, beyond doubt, a somewhat marked difference between the accuracy of reproductions made with vision and those made without vision, in favor of the latter.*

Furthermore, the opinions of the subjects, recorded at the time of the experiment, largely agree that, on the whole, it is easier to record the remembered figures without vision than with vision. This agreement was recorded in spite of the fact that many of the subjects were sure that the records made without vision were far less accurate than those made with vision, although the final results did not bear out this conclusion at all.

Thirteen subjects agree that the method of reproducing without sight is easier than with sight, and a small minority of them feel that it is the more accurate method. Sometimes subjects would try to explain the greater ease of one method, usually the blind method, by saying that one set of figures was easier than another. But it is interesting to note that in just one half of the cases where this statement was made one set was found to be harder, and in the other half of the cases it was the other set which was found to be more difficult. Evidently it was not the figures which were difficult, but the method of reproduction.

Seven subjects found that reproducing the figure with vision was easier than without. Several of these subjects referred particularly to a difficulty of getting the proportions of the figure correct when they could not see what they were drawing. It is probably true that it is easier to get proportions correct by the sight method than by the blind method.

But of the seven subjects who felt that the easier method was the sight method, two essentially modified their statements, if they did not actually contradict them, as follows: One said, "The sight method is easier than the blind method, because it is easier to visualize when you can see where the hand is going." But during the course of the experiment he had been heard to say in slight exasperation, while working according to the blind method, "Pshaw, I looked underneath it (*i. e.*, the screen) and lost it."

Another subject summarizes the situation by saying: "It is easier to see what one is doing, one gets an idea of where one has gone. But in the blind method I did it quicker, and drawing with sight the distraction caused the image to be lost." It is significant that "distraction" should be the word used to describe the effect of seeing the figure he is reproducing while the reproducing is in progress.

In its results the experiment seemed to indicate that in reproducing such cursive forms as I used, to see what one is doing is a hindrance, rather than a help. This conclusion is established in several ways. Scoring the results in terms of their accuracy shows an undoubted greater accuracy in the cases of the reproductions made without sight. This would be true with any fair scoring method, I think, but it becomes extremely evident if the number of exposures of the figures used as models be taken into account as well as the accuracy of reproduction. Further, the large majority of the subjects, after working with both methods, definitely prefer the method without sight.

The observations here reported are part of a more extensive study being made in the Oberlin laboratory of the comparative ease and accuracy of learning cursive forms by visual and by kinæsthetic perception. This experiment, still in progress, seems to show that ease and accuracy of learning stand in nearly direct ratio to the amount of movement which is involved in the learning process. This may mean that kinæsthetic imagery has peculiar advantages as regards retention over visual or other imagery, at least for many people.

If this should prove true, it is further true in all probability that the distracting effect of observing the result of one's effort while trying to reproduce a figure recently seen is due to the substitution of visual for kinæsthetic guidance.

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THE PSYCHOLOGICAL REVIEW

AN ATTEMPTED FORMULATION OF THE SCOPE OF BEHAVIOR PSYCHOLOGY[1]

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COMMON SENSE PROCEDURE

Psychology a Science of Behavior.—Psychology is a division of science which deals with the functions underlying human activity and conduct. It attempts to formulate through systematic observation and experimentation a series of principles or laws which will enable it to tell with some degree of accuracy how an individual or group of individuals will adjust themselves to the daily situations of life as well as to the uncommon and unusual situations which may confront them. It is equally a part of the function of psychology to establish laws or *principles for the control of human action* so that it can aid organized society in its endeavors to prevent failures in such adjustments. It should be able to guide society as to the ways in which the environment may be modified to suit the group or individual's way of acting; or when the environment cannot be modified, to show how the individual may be moulded (forced to put on new habits) to fit the environment. It must be understood at the outset, though, that psychology at present has little to do with the setting of social standards of action and nothing to do with moral standards. It does lie within her province to tell

[1] The material presented in this article is essentially that which will be used in the first chapter of my forthcoming book, 'Human Psychology.' It is published now in the hope that helpful criticism will be furnished the writer both as regards the level of presentation, that is, its textual fitness, and as regards the completeness with which the scope, as here outlined, touches the main points of interest in modern psychology.

whether the individual can act in accordance with such standards and how we may control him or lead him to act in harmony with them. These laws of control or training must be general and comprehensive since social standards are constantly changing.[1]

Psychology when looked at in this way is seen to be something which everyone has been using more or less all his life without calling it psychology. We learn by failures and successes how to run our business, how to get along with our colleagues and associates. We teach our children to act in certain ways. They must eat with a fork, learn to dress themselves, to treat their playmates as they themselves like to be treated, to master the three R's, and then later a trade or profession. We skillfully or bunglingly steer them on their course from infancy until they no longer need our guidance.

The Ancient Origin of Psychology.—Indeed a glance at the mythology, folk-lore, or history of any given race will show that the practical psychology of control began as soon as there were two individuals on the earth living near enough together for the behavior of one to influence the behavior of the other. The serpent controlled Eve's behavior by offering her the delectable apple. Eve learned her lesson quickly and tempted Adam in the same way. Atalanta, the swiftest of runners, was beaten not through the superior agility of Hippomenes but by the fact that she could not resist the temptation to stop and pick up the golden apples thrown by her suitor. The taboo system, the initiation ceremonies and the machinations of the medicine men all serve to illustrate progress towards the control of group and individual behavior.

Common Sense Procedure in Securing the Control of Behavior.—Long before the dawn of modern scientific psychology society found that by roundabout, hit-and-miss methods she had secured a fairly serviceable body of data as to what man can do—his complement of acts; the appropriate situation for calling out any given act; and crude

[1] For a more scientifically worded discussion of the province of psychology see p. 336.

training methods whereby the repertoire of the acts themselves might be enlarged. A brief glance at the practical procedure in securing control of individuals and groups may serve to illustrate both how such data are obtained and how they are used.

The situations or devices for drawing crowds for whatever purpose show the greatest development of skill in practical psychology. If one or two individuals happen by chance to gather around a patent medicine vendor the vendor's momentary success is assured. The small crowd is an irresistible stimulus and soon gathers unto itself a larger crowd. For this reason initial buyers, listeners, applauders, and, yes, even sometimes suitors and mourners are provided and paid for before the public is allowed to act. The advertisement of a 'fire sale' is likewise an ancient and honorable device for gathering a crowd. The announcement at Jamestown of a shipload of virtuous women to be sold as wives apparently, if historical report can be trusted, brought out the whole of the unmarried able-bodied male population of Virginia.

After the crowd has collected, devices for controlling the individual are numerous. Chief among these we find the various lottery schemes; many individuals will not give twenty-five cents for a charitable purpose, but at any charity gathering they will eagerly take one of a dozen twenty-five-cent chances on almost any object the total value of which need not be greater than the cost of a single chance. So uniform is the response to lottery schemes that they have oftentimes become national mediums for raising government funds.

Organized society displays much ingenuity in devising situations for the control of reaction: the clergymen, having found empty pews under an orthodox type of routine, and that the young people were not engaging in church activities, began to try out in a similar way what could be done by altering the old austere situation and by creating for the church entirely new situations. The exteriors of the churches were vastly modified, the interiors decorated, rest and play rooms added as well as gymnasia and playgrounds. A certain type of action was expected and the church arranged a situation to bring it about. We see the same attempt at control illustrated in governing bodies: state legislatures, depressed by the prevalence of drunkenness and crime, establish a new situation by prohibiting the sale of drugs and spirituous liquors in the hope that legislation will prevent such conduct. Finally, mention may be made of recent changes in prison methods. The more advanced prisons, becoming dissatisfied with the amount of insubordination, vice, sloth, and ignorance of all forms of government among the inmates, are trying a new situation with good results, viz., that of letting the inmates establish a miniature republic. This republic makes its own laws and metes out its own punishments. Society has developed a *system of standards of action but it does not know just what situations will produce the needed responses.* The situations are set up in the social field by trial and error; they are modified, changed, etc., until the desired reactions take place, or until they are despaired of.

Watching the Act to Obtain Data on the Situation.—We thus, as we see, get a part of our knowledge of the factors underlying behavior by the trial and error method of manipulating the situation and noting the reactions that take place. This gives us a body of usable data on what to expect of men when they are placed in certain situations. We deal here with situations of our own contrivance. Equally serviceable results are obtained by taking an individual who is performing some act (action not regulated by the observer) and examining immediately into the situation which led to that act—the act is known, the situation which led to it must be investigated. This method is of course supplementary to the first. It extends our knowledge of situations and what to expect from them, and at the same time adds to our knowledge of man's repertoire of acts. Illustrations of the results of this type of practical procedure are numerous; it is hard to convince a mother that she cannot predict[1] what

[1] By prediction we mean nothing but the common-sense, scientific, and logical use of material gathered from observation and experimentation. The planet Neptune was predicted, Mendelian ratios are predictable, coat color or eye color of animals can, within certain limits, be predicted before the birth of the animal.

the situation is which leads her baby to give a certain cry. Depending upon the variations in the cry she will say that the 'baby is hungry, wet, or has colic' (her conclusions are often wrong, be it said). Advancing somewhat in the age scale we see, in passing through a forest, a youth trained to hunt firing upward into a tree, and we note that the dog he has with him has 'treed.' An observer responds to this picture by telling his companion that the boy is hunting *squirrels*. But if he sees the boy fire in another way, say horizontally, and sees a dog in the act of pointing, he will state that the lad is hunting *quail*. If the boy fires towards the ground and has a hound with him, he is shooting *rabbits*. Finally, if he is seen in the hunting fields on horseback, with no gun but accompanied by a pack of hounds, in full cry, our observer remarks that the boy is hunting a *fox*. Watching his actions and taking note of all attendant circumstances enables anyone to predict with some degree of probability the immediate situation leading to the boy's actions. Our ability to observe an act and predict the possible or probable stimulus to that act depends upon the fact that often during our past life, when we have seen individuals doing certain things, we have immediately investigated the situations which led to the acts.

Need of Acquaintanceship with an Individual's Past.— When we come to deal practically or scientifically with individuals too much emphasis cannot be laid upon the extent to which acquaintanceship with their past life will gradually afford the basis for making serviceable predictions as to their probable ways of acting and as to the situations which will call out any given act. This can possibly be most easily illustrated in the animal world. We soon come to the prediction stage with our horses and dogs and can map out with some certainty what they would do under the various situations which might confront them. After watching two monkeys for several years I found after repeated observation that *B* would not touch food until *J* had finished and left the dish, and that *J* would eat and stuff three bananas into his cheek pouches and drag off another with his right forefoot;

that *J* would attack a problem box rapidly and in a rough and harum scarum way—pulling the box towards him, turning it over and maltreating it generally; that *B* would approach cautiously, moving the parts slowly and with no violence; but as a rule would solve the problem before *J*.

But with an organism so highly developed as man's, the prediction of his actions is not always so easy. An individual's actions in everyday situations depend upon such complex factors as his heredity, his past success in adjustments and his failures of adjustment, the responses he has just had to make, as well as upon the permanent and temporary conditions of his organic mechanisms (digestion, circulation, sleep, etc.). A badly cooked dinner, an insufficient amount of food (as in the case of a person who is dieting), extreme heat, etc., may so change the state of the organism that the response reasonably expected is not forthcoming. A forthcoming marriage, graduation, separation, offer situations which, by their involvement of emotional factors, may almost completely disrupt for the time being the everyday systems of responses which are customary with a given individual. In such a complex setting a man may show a temporary breakdown; he may make blunders or show a general inability to go through with his ordinary routine. A bad dream or a slight rebuff at the hands of a friend likewise may upset a man's reactions for a whole day. Further on we shall see that many of the habits formed in childhood and in adolescence, now long since discarded, such, *e. g.*, as attachments to early playmates, to members of the family, early love affairs, may have had a prepotent influence in shaping the whole course of adult acquisitions.

If we are called upon to predict what a stranger of twenty-five years of age will do when confronted in a dark alley by a burglar, we are almost, but not quite, helpless so far as prediction is concerned. Now give us an opportunity of systematically studying the make-up of the man, of knowing something of his reactions in past dangerous situations, the stability of his emotional tendencies, and we shall be able at least to make a crude but serviceable prediction, viz., that he will quietly throw up his hands and let the burglar go through his pockets. He will neither become hysterical, attempt to attack the burglar, nor will he suffer any severe after-effects by reason of his experience. The chances are good that he will report his mishap to the police, confess to his wife or friends that he has been held up, and then will cease to be further troubled by the experience. In another type of individual, whose heredity is questionable, whose reactions are unstable, who is generally excitable and liable to over-reaction, we venture the prediction that even if he does throw up his hands (which he does not always do) and allow himself to be robbed, he will go to pieces after the experience and may suffer some serious and lasting consequences.

Common Sense a Crude but Genuine Psychology.—Most of our illustrations have involved little or no technical psychology and yet they do illustrate a genuine psychological procedure. The business man, the artist, and the artisan have built for themselves rather definite rules of psychological procedure without ever calling it psychology. The church and the theater illustrate this equally well. It is possibly even a debatable question whether common sense has not kept closer to the fundamental truth underlying the psychology of reaction than has the too detached psychology of the laboratory. But even those who are the best practical psychologists realize that common sense methods can never produce universal or widespread progress in psychology. Our great military leaders, our great religious leaders, the demagogues and the politicians have accomplished their results by their very wide acquaintanceship with the reaction tendencies in man and by their happy accidents in creating the situations which will call out such reactions. By reason of the fact that occasional success has been obtained by crude methods and happy accidents, we must not conclude that psychology should not attempt to discover and analyze and bring under scientific control the factors which have occasionally made such successes possible. Because there has been an occasional business leader who knew how to pick out and keep good men, we are offered no reason why we should not seek to understand and control the processes involved in picking and keeping good men. The same may be said of the factors involved in keeping men out of crime, keeping them honest and sane, and their ethical and social life upon a high and well-regulated plane.

This brief summary of the everyday uses of psychology should convince us of two things; first, that common sense, while a reasonable method so far as it goes, does not go far enough and never can; and secondly, that in order to make progress, the phenomena of human behavior must be made an object of scientific study. We shall attempt next, then, to gain some impression of this systematic psychological procedure.

Scientific Procedure

The Detailed Subject Matter of Scientific Psychology.—As a science psychology puts before herself the task of unravelling the complex factors involved in the development and regulation of human behavior from infancy through old age. At first sight it may seem that this program leaves out many of the factors with which psychology ought to be concerned. Historically considered this is true, but when we are confronted both with the practical and scientific needs of life we are ready to admit that after all what we seek to have psychology busy herself with is just this matter of *environmental adjustment;* what can man do apart from his training; what can he be trained to do, and what are the best methods for training; and finally, how, when the varied systems of instincts and habits have sufficiently developed, can we arrange the conditions for calling out appropriate action upon demand? To answer such questions we must necessarily study the simple and complex things which call out action in man; how early in life he can react to the various simple and complex sense stimuli; at what age he can put on the various instincts and what are the situations which call them out. Just what are the patterns of his instinctive acts, that is, does the human being, apart from training, do any complex acts instinctively as do the lower animals? If so, what is man's full equipment of instincts? When does emotional activity manifest itself and what are the situations which call it out, and what special acts can be observed in emotional behavior? How soon can we observe the beginnings of habit in infants? What special methods can we develop for rapidly and securely implanting and retaining the body and speech habits which society demands? Do we find special and individual equipments in infants and do these develop and later form the basis for their entering one kind of vocation or another, or developing into one or another type of personality? Are there such factors as habit and instinct conflicts, distortion of habits and emotions? How do they manifest themselves, and is it possible to develop methods for shaping the environment of the individual so that such conflicts will not arise?

Stimulus and Response.—This general description of the subject matter of psychology helps us very little as regards the analysis of particular problems in conduct and behavior. In order to plan an experimental attack upon any problem in psychology we must first reduce it to its simplest terms. If we look over the above list of subject matter and at our practical examples we see that there are common factors running through all forms of human acts. In each adjustment there is always both a *reaction or response* and a *stimulus or situation* which calls out that response. Without going too far beyond our facts it seems possible to say that the stimulus is always provided by the environment, external to the body, or by the movements of man's own muscles and the secretions of his glands: finally, that the responses always follow relatively immediately upon the presentation or incidence of the stimulus. These are really assumptions, but they seem to be basal ones for psychology. Before we finally accept or reject them we shall have to examine both the nature of the stimulus or situation, and of response. If we provisionally accept them we may say that the goal of psychological study is the *ascertaining of such data and laws that, given the stimulus, psychology can predict what the response will be; or, on the other hand, given the response, it can predict the nature of the effective stimulus.*

Use of the Term Stimulus.—We use the term *stimulus* in psychology as it is used in physiology. Only in psychology we have to extend somewhat the usage of the term. In the psychological laboratory when we are dealing with relatively simple factors such as the effect of ether waves of different lengths, the effect of sound-waves, etc., and are attempting to isolate their effects upon the adjustments of men, we speak of stimuli. On the other hand, when the factors leading to reaction are more complex, as, for example, in the social world, we speak of *situations*. A situation is, of course, upon final analysis, resolvable into a complex group of stimuli. As examples of stimuli we may name such things as rays of light of different wave-lengths; sound-waves differing in amplitude, length, phase and combination; gaseous particles given off in such small diameters that they affect the membrane of the nose; solutions which contain particles of matter of such size that the taste buds are thrown into action; solid objects which affect the skin and mucous membrane; radiant stimuli which call out temperature response; noxious stimuli such as cutting, pricking, and those injuring tissue generally. Finally, movements of the muscles and activity in the glands themselves serve as stimuli by acting upon the afferent nerve endings in the moving muscles (p. 341).

It must be emphasized here that only under the rarest experimental conditions can we stimulate the organism with a single stimulus. Life presents stimuli in confusing combinations. As you write you are stimulated by a complex system —perspiration pours from your brow, the pen has a tendency to slip from your grasp. The rays of light reflected from the paper focus the physical image of the words upon your retinæ. The chair offers stimulation—the noises from the street, etc. But far more important, delicate instruments would show that though you are not speaking aloud your vocal mechanisms—tongue, laryngeal muscles, etc., are in constant motion: moving in habitual trains, these laryngeal movements serve largely as the stimuli for releasing the writing movements of the hands. The fact that you are here in the lecture room facing your instructor and surrounded by your classmates is still another very important element. The world of stimulation is thus seen to be exceedingly complex. It is convenient to speak of a total mass of stimulating factors, which lead man to react as a whole, as a situation. Situations can be of the simplest kind or of the greatest complexity. It should be noted here finally that there are many forms of physical energy which do not directly affect our sense organs. As examples we may cite the facts that ether waves longer than 760 $\mu\mu$ or shorter than 440 $\mu\mu$ do not lead to visual reactions, and that many of the wave motions in the air are of such length or amplitude that they do not produce auditory stimulation. The inability of the human organism to respond to many possible forms of stimulation will be discussed later.

The General Nature of Response.—In a similar way we employ in psychology the physiological term response, but again we must slightly extend its use. The movements which result from a tap on the patellar tendon, or from stroking the soles of the feet are 'simple' responses which are studied both in physiology and in medicine. In psychology our study too is sometimes concerned with simple responses of these types, but more often with several complex responses taking place simultaneously. In the latter case we sometimes use the popular term 'act' or adjustment, meaning by that that the whole group of responses is integrated in such a way (instinct or habit) that the individual does something which we have a name for, that is, 'takes food,' 'builds a house,' 'swims,' 'writes a letter,' 'talks,' etc.[1] In working over the distinctions among the various types of acts the speculative psychologists have introduced many needless technicalities and metaphysical concepts, such as purpose, end, etc. Psychology is not concerned

[1] But it should be well understood that whatever the man does under stimulation is a response or adjustment—blushing, increased heart beat, change in respiration, etc., are definite adjustments. We have names for only a few thousands of the total possible number of such adjustments. The term adjustment is used by most writers to refer to the doing of one of these *named acts*. In this volume the terms adjustment, response, reaction, etc., are used almost interchangeably.

with these distinctions. Because a man fails by his separate acts to get his food, to build his house, to work out his mathematical problem, or to live in harmony with his wife, is no reason for rejecting him as a psychological subject. We study him for his *reaction possibilities and without prejudice:* the discovery of the fact that he will make only abortive attempts to meet and control certain aspects of his environment is an important part of our task; just as important as being able to state that he can make certain other types of adjustment. 'Successful' adjustments, 'good' acts, 'bad' acts, are terms really which society uses. Every social age sets up certain standards of action, but these standards change from cultural epoch to cultural epoch. Hence they are not psychological standards. Reaction possibilities, however, on the average probably remain about the same from eon to eon. It lies well within the bounds of probability that if we were able to obtain a newborn baby belonging to the dynasty of the Pharaohs and were to bring him up along with other lads in Boston, he would develop into the same kind of college youth that we find among the other Harvard students. His chances for success in life would probably not be at all different from those of his classmates. The results obtained from the scientific analysis of reaction in the human being should fit any cultural age. It is part of the function of the psychologist to tell whether a given individual has the reaction possibilities within him to meet the standards of that cultural age, and the most rapid way of bringing him to act in accordance with them. The fact that social values (group *mores*) change puts ever new burdens upon the psychologist because every change in the *mores* means a different situation to which man has to respond by a different combination of acts, and any new set of acts must be incorporated into and integrated with the rest of the action systems of the individual. The problems put up to psychology are those of deciding whether the individual can meet the new standards and for determining and developing methods of instructing him.

Motor and Glandular Indicators of Response.—What is it that the psychologist can observe? *Behavior* of course. But behavior on analysis is the separate systems of reactions that the individual makes to his environment. When we come to study the mechanics of such adjustments we find that they depend upon the integrations existing among the receptors and the muscles and glands.

The unicellular organisms have no separate muscular or nervous systems. Yet a part of their one cell must be specialized in a motor as well as in a sensory way, since these organisms do move in response to stimuli—to light, gravity, heat, cold, electricity, etc. As you pass higher in the scale special sense organ tissues (receptors) develop and along with them both motor or effective organs, and neurones connecting receptors and effectors. Action in such cases becomes sharper, more localized, more immediate, and at the same time more sustained. Furthermore, as we pass still further up the scale, glands begin to develop. Glands like muscles are responsive organs and special glandular action takes place whenever motor action takes place. The activity of the glands in turn reacts back upon the muscular system and affects its functioning (p. 338). Furthermore, there are two kinds of muscles, striped and unstriped. The striped muscles move the arms, legs, trunk, tongue, larynx, etc. The unstriped muscles control largely the blood vessels, intestines, lungs, etc. Usually when we speak of response we mean that the organism goes forward to right or left, or retracts as a whole, that it eats, drinks, fights, builds houses, or engages in trade. But these patent and easily observable changes do not exhaust the term response, as we pointed out on p. 339. We should mean by response the total striped and unstriped muscular and glandular changes which follow upon a given stimulation. Our problem of the moment determines which movement shall be studied in relative isolation; in man, though, interest has been largely centered in the integration of separate responses; in getting him to form some habit—that is, to do something with arms or legs or vocal cords. It is important to get at the outset a comprehensive notion of response. A child or animal may stand stock still under stimulation, but we should not say that there was no response. Close observation shows that there are changes in the tension of the muscles, in respiration, in circulation, and in secretion.

General Classification of Responses.—The various possibilities of reaction are thus seen to be vast; so vast indeed that it would seem at first sight as though any classification would be impossible. We can at least find a convenient grouping which will serve us well both for discussion and for setting experimental problems. Most reactions may be looked upon as falling into one of four main classes:

1. Explicit habit responses: as examples we cite unlocking a door, tennis playing, violin playing, building houses, talking easily to people, staying on good terms with the members of your own and the opposite sex.

2. Implicit habit responses: 'thinking,' *by which we mean subvocal talking*, general body language habits, bodily sets or attitudes which are not easily observable without instrumentation or experimental aid; the system of conditioned reflexes in the various glands and unstriped muscular mechanisms, as, for example, conditioned salivary reflexes.

3. Explicit instinctive responses: including man's observable instinctive and emotional reactions as seen, for example, in grasping, sneezing, crawling, walking, etc., and in fear, rage, love.

4. Implicit instinctive responses: this includes of course the whole system of endocrine secretions, changes in circulation, etc., so largely studied by physiology. Here again instrumentation or experimental aid is necessary before observation can be made.

These various types of response will be studied in detail in later chapters. The classification as a whole should be clear with the possible exception of 2, implicit habit responses. This group is so important and so generally neglected in discussion that we shall single it out here for brief mention in advance of the chapter in which it is entered into with some care.

What Man is Doing when Not Overtly Acting.—With a highly specialized organism like man even careful observation often fails to show any overt response. A man may sit motionless at his desk with pen in hand and paper before him. In popular parlance we may say he is idle or 'thinking,' but our *assumption* is that his muscles are really as active and possibly more active than if he were playing tennis. But what muscles? Those muscles which have been trained to act when he is in such a situation, his laryngeal, tongue, and speech muscles generally.[1] Those muscles are as active and are carrying out as orderly a system of movements as if he were executing a sonata on the piano—they are doing it well or ill depending upon the training he has had along the particular lines which engage him. While we cannot at present watch the play of this implicit stream of words there is no reason for hypothecating a mystery about them. Could we bring 'thinking' out for observation as readily as we can tennis playing or rowing, the need of 'explaining' it would disappear. We shall see later that efforts have been made to bring such responses under experimental control. But entirely apart from our present unreadiness to make observation on implicit habits, we find a certain way of arriving indirectly at the same end: *implicit language habits*, by methods which we shall study, come to issue finally in overt action. By watching the easily observable explicit habits and instincts of an individual keenly enough, and for a sufficient stretch of time, and under varying enough conditions, we can obtain the necessary data for most psychological requirements.

Scientific Methods Contrasted with Practical Procedure.—Having now examined at some length into the general nature of both stimulus and response, we should be prepared to understand the object of a psychological experiment and to contrast the scientific procedure with the common sense or practical procedure which we discussed at the beginning of the chapter. We shall take up almost at random some definite illustrative psychological problems and the methods of solving them. Our first problem is *to find out what the*

[1] Indeed the whole glandular and muscular systems are contributory.

reactions of a six-months-old infant are to living furry animals. We first arrange the situation (complex group of stimuli). The infant is held by its mother in a well-lighted room. We observe first that the infant is smiling and comfortably disposed. Then one after another we present a white rat, a dog, a cat, a white rabbit, beetles, and a snake. We next record accurately and separately the responses to these objects. The infant, which has only learned to reach out for objects a short time before, slowly puts out first one hand and then the other. The smile leaves his face but no crying or withdrawing of the hands or external secretions follow. These are only the more easily observed responses. Other changes take place undoubtedly, in the internal glands, circulation, respiration, etc. It depends upon our immediate problem as to where the emphasis in observation shall fall in our record of reaction changes. In this case our problem was to determine whether there were any overt instinctive tendencies on the baby's part to react against or withdraw the hands or whole body from live animals. Our problem might very well have led us into observing the changes in the eyes, respiration, blood pressure, salivation, or in the endocrine glands, or in several of these at once. Again it should be noted that our problem is not so simple as it seems at first sight. Suppose we had found that the baby did withdraw from the objects, began to cry, void urine, or attempt to hide behind the mother's clothing—could we have concluded that there was an instinctive reaction against live furry animals? *Not without delving into the baby's past.* If we had had the child under constant observation and found no record of previous acquaintanceship with live animals, our answer would be that the observable responses were probably instinctive. But if on the other hand we found that the child had been severely bitten by a cat only two days before our test, our conclusions would have to wait upon more extended observation. Nor can we, from the behavior of this one child, draw any conclusions as to what other children of the same age will do, or what this child might do at a slightly different age or when tested under different conditions; before generalizations can be made many children should be brought under systematic observation.

As another example of a somewhat more restricted type, let us take the case of a man whose everyday behavior has led us to suspect the normality of his responses to monochromatic (colored) light. Common sense has nothing to say; it can give no adequate report upon him. His mistakes may be due to one or many things. We take him into the laboratory where monochromatic light is under control and we put him in situations where he has to react to the lights in pairs, and where each one of the lights can be widely varied in energy. In the course of the investigation we find that when there is a certain energy relation obtaining between the red and the green lights he can no longer react to them differentially (that is to say, they do not offer different stimulating values). We note further that we can find a white light of a certain intensity to which he reacts as he does to either of the monochromatic lights. But at no energy relation between any other two colors can we break down his differential responses. We conclude after this careful study that the man is red-green blind, that is, that he reacts to red and green as he does to certain intensities of white light.[1] Let us take another example, and this time from the field of vocational psychology. Suppose that the telephone directory of a large city is getting entirely too bulky and complex for men to handle easily. What is the best method for obviating this? The telephone people and the psychologists work together. The psychologist may suggest printing in smaller type and four columns to the page instead of three. These and many possible suggestions may lead to a solution of the problem. But the matter has to be put under severe trial both before individuals trained to look up names in a directory and before individuals having no more training than has the general public. *Syste-*

[1] If we find by repeated tests that the anomaly is more than temporary, we are right in advising this man that he will be handicapped if he enters certain occupations, *e. g.*, locomotive and marine engineering, geology, advertising, etc. In other words, the results of psychological experimentation are as immediately practicable as are results in any other scientific field.

matic trial and error is the procedure here with statistical treatment of the results. In the end it is found that a four-column page with a certain amount of spacing between the lines of print makes the directory not only 20 per cent. less bulky but also one in which the subscribers can find names 10 per cent. more rapidly.

The Divisions of Psychology and the Relation of Psychology to the Other Sciences

The Various Fields of Psychology.—It is just as difficult to draw a hard and fast line between the different branches of psychology as between the different branches of biology and physics. Practical and theoretical interests determine where a man will throw the emphasis of his observation. All scientific psychology is experimental, or is at least carried out under such conditions that rigid and controlled observation is possible. All psychology is 'genetic' in the sense that we have to go back to the child and contrast it with animals in order to determine what native systems of integrations belong peculiarly to man. For purposes of specialization we speak of human psychology as being made up of *individual*, *vocational*, *child*, *folk*, *educational*, *legal*, *pathological*, and *social* psychology. For our purposes we need not enter into a separate characterization of these special branches. The remaining chapters in this book attempt to deal generally with the simpler results, problems, and methods in common use in psychology. We shall not emphasize, except here and there, the particular branch to which such material belongs.

Relation of Psychology to Physics.—Both physiology and psychology are dependent (as is every other science at bottom) upon physics for the control of apparatus and of stimulus. It is essential for a research student in psychology now to know the general facts about wave motion; as, for example, heat, sound, and light. It is important to know how to install and use simple electrical instruments, galvanometers, thermal couples, and photometers.

Relation to Neurology.—It might be supposed that psychology would lean most heavily upon neurology. Indeed this has been the general assumption in the past. Psychological texts have been overburdened with cuts and descriptions of the nervous system and we have many works which claim on their title pages to be physiological psychology. Gradually we are gaining the point of view that the psychological laboratories cannot teach both psychology and neurology. Where a neurological laboratory is at hand training in neurology should certainly be included, but it is doubtful if much can be gained by a psychological student from merely looking over cuts and listening to lectures on the subject. Some notion of the elements involved in reflex arcs is certainly essential—the way sense organs are connected with the central nervous system and the central nervous system with the muscular and glandular systems. In a later Chapter we touch upon some of the more elementary features connected with the arrangement and functioning of reflex pathways.

Relation of Psychology to Physiology.—It has been claimed by some that psychology is really physiology. That this is not the case appears from even a casual examination of the respective scopes of the two provinces. Physiology teaches us concerning the functions of the special organs. For purposes of experimentation and exposition the heart, liver, lungs, circulation, respiration, etc. are isolated, or are at least discussed as isolated functions. All of the functions of the bodily organs are gone over in this way. Muscle-nerve preparations are taken out and their properties investigated. It is not meant to assume that physiologists deal wholly with organs in isolation. Certain combined processes are studied, such as metabolism, digestion, effects of poisons, etc., but nowhere in physiology do we get the organism, as it were, put back together again and tested in relation to its environment as a whole.

From our discussion of the scope of psychology we are now prepared to see what when the physiologist has learned all that he can about the functioning of the separate organs of the body of man, he has encroached upon our field only in a very slight degree. Our task begins only when the physiologist puts the separate organs together again and turns the whole (man) over to us. The physiologist *qua* physiologist knows nothing of the total situations in the daily life of an individual that shape his action and conduct. He may teach us all there is to know about the mechanism of stepping, but it is not his task to determine whether man walks before he crawls, the age at which walking begins, whether walking begins earlier in boys than in girls, or whether defective children walk at a later age than normal children. Again, he may teach us a great deal about the functions of the kidneys, the bladder, and of the sphincter control of the latter; but of the special situations (outside of disease entities) which may lead to incontinence in children, his science teaches him nothing, nor of methods of controlling this mal-adjustment. In studying psychological functions, for example, the emotions, it does not help him very much to try to picture what chemical and neural processes go on in the brain. It has often been asserted, *e. g.*, that the thalamus is operative in emotional disturbances. We do not get very far, though, by trying to picture such activities, or by speaking of what goes on in the individual neurones. We get a very incomplete but a somewhat better view if we consider what goes on in glandular action during emotional states. But even glandular action is not easily observed by methods which are known today. We can, however, study the reaction states we popularly call sadness, elation, moroseness, rage, fear, love, etc., from the standpoint of what the organism can do in these states and as to whether the smooth running of the general system of organized habits is facilitated or disturbed by the presence of emotional activity. We can, further, often determine by a study of the life history of the individual how frequently such disturbances come about and can trace out the causes or factors leading to their onset. Physiology has nothing to tell us of the character and personality of different individuals nor of their emotional stability or lack of emotional control, nor as to what extent their present place in life is dependent upon their upbringing. Physiology tells us nothing of man's capacity to form and retain habits nor of the complexity of man's habit organization. Hence if we wish to predict whether an individual is capable of rising above the environment to which he is not adjusted, we should have to go to psychology and not to physiology for our answer. In thus emphasizing the entire theoretical independence of the two fields let us not set up a false impression of antagonism. Physiology is psychology's closest friend among the biological sciences. We can hardly move a step in psychology without using physiological data. But in this we are not different from the other biological sciences, or indeed from medicine itself.

Overlapping of the Two Fields.—Occasionally we find physiologists who have dealt with functions which overlap the field of human behavior. As examples, we cite the work of Cannon on the bodily effect of violent emotional disturbances, and of Carlson and others on the question of the reactions which are present in the stomach in the absence of food. Where the two fields overlap most, however, is probably in the study of the nervous, muscular, and glandular systems, and in the realm of sensory physiology. This latter topic no longer seems seriously to interest the physiologists, and where they have shown interest in it, in this country at least, it has been mainly pedagogical. Most of the work in sensory physiology has been done by psychologists. Until the recent work of Pavlow and Bechterew and their students physiologists have shown little interest in the study of habit formation, which general topic is one of our central ones. In general it may be said that there is some overlapping in the two fields, but that this does not keep them from being separate disciplines. In cases where there is an overlapping the methods and points of view of the two sciences in no wise differ.

Relation of Psychology to Medicine.—Up to the present time psychology has been of only slight service to psychiatry and medicine generally. It should form a background for the whole field of medicine. But it has dealt hitherto so largely with speculations and with philosophical considerations that its usefulness for this purpose has been seriously restricted.

The physician, whether medical specialist or general practitioner, would like to know something about the method of approaching and handling his patients. He must encounter—and he must be prepared to encounter—such things as stubbornness and unyieldingness in his human subjects, and he must learn to study his patients in relation to their present environment, and to go back into their life history for an understanding and explanation of such attitudes. He must learn how to size up his patients and to get at the details of their individuality and characteristics. He must be able to tell whether the patient can do what he is told to do, and whether he has sufficient assets to meet the environment in which he has to live, and whether he has sufficient assets to rise out of the environment which is unsatisfactory to him. These facts on character adaptation cannot be expressed in any other terms than behavior terms. These are, to be sure, factors which concern everyone who has to deal with his fellow-man, but on account of the intimate relationship existing between the patient and his physician they are of especial importance to the latter. The psychiatrist has not neglected these factors; indeed, it has been due to him that they have been emphasized at all, and it is largely through his efforts that we have a well-developed and systematic technique for isolating the factors of importance in the life history of the patient. In so far as psychiatry is concerned I think we can say that the psychology the psychiatrist uses is not different from the psychology we are trying to study. The psychiatrist has to be both a physician with a specially developed therapeutic technique, and a psychologist with special interests in certain divisions of psychology. Psychiatry has no special need for detailed studies on reactions to sensory stimuli. Much of the detailed work on habit formation and on the separate analysis of instincts is not of special use to him. On the other hand, any of the material which the psychologist may offer on the subjects of attachment and detachment of the emotions, on the genesis of instincts and habits and their interrelations, on the effect of age, drugs, etc., on habit formation and retention, upon false reactions and failures in reactions, on the effect of lesions of the central nervous system in trained animals and the resultant success that comes from retraining them, can be utilized by the psychiatrist at once, both in a specific way and by reason of its value in helping him to size up his patients. Most psychiatrists will admit that when the proper kind of psychology is developed they can utilize directly a large part of both of our methods and of our materials. This appears clearly when we examine the various tests which have been devised by psychologists for evaluating the general behavior levels of individuals. Such tests in one or another form are in common use in every psychiatric clinic. Topics such as 'general behavior,' 'stream of talk,' 'attitude,' 'orientation,' 'retention' of recent and past happenings, 'general information,' the emotional level at which acts can be carried out, etc., are discussed in relation to every patient admitted to a psychiatric clinic.

Preparation for Psychology.—In dealing with the native equipment of man the student of human psychology will find a background of study of animal behavior a helpful one. As a further preparation for this part of his work he will find that he needs some equipment in physiology and experimental zoölogy. His work in habit formation leads him again into physiology and pharmacology for such factors as the effect of age, drugs, etc., upon the human organism. The consideration of habit and instinct conflicts, abortive reactions and failures of adjustments generally which we see so well emphasized in tics, sympathetic chorea, hysteria, obsessions, etc., leads the psychologist into the psychiatric clinic if he wishes to prepare himself to the fullest extent. Business and law are making ever and ever larger demands upon him. Some familiarity with legal and business problems is almost essential. Finally, in order to handle adequately experimental data some training in the use of statistical methods is needed. If a start is made early enough by the student who is preparing for psychology he can obtain the above related branches before he begins his special study of psychology. While today is a day of specialists it should not be a day of narrow specialists. The tendency to have information about one small corner of psychology should not be encouraged. It leads to such anomalies as pure "mental testers," psychotechnicians, and the like.

RELATION BETWEEN FUNCTIONAL AND BEHAVIOR PSYCHOLOGY

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The concept of evolution in biology supplemented the purely descriptive and systematic accounts of plants and animals by the introduction of a genetic and developmental factor. So fruitful did this new outlook prove to be and with such audacity and effectiveness did it probe into the cherished beliefs of orthodoxy that even man's mind became the object of prying research. Psychology was no longer content to study the structure of mental states, but its interests expanded so as to include the development and genesis of mind. As experimental methods became more prevalent, the practical needs of pedagogy stimulated interest in the relationship between mind and action, and this in turn resulted in a more critical analysis of behavior or conduct. Finally the axiomatic character of the proposition 'Mind controls action' was challenged and this introduces one of the controversial points of modern psychology.

Problem of Functional Psychology

The causes which led to the point of view called functional psychology were the need for a more dynamic principle in the explanation of human behavior than was offered by the descriptive and systematic accounts of the nature of mind as given by the structural psychologists. That the study of the mind was of great value in understanding human behavior had never been questioned, but the lack of uniformity in terminology and the controversies on methodology made it practically impossible to formulate the problem of the relationship between mind and action in such a way that it could be critically discussed by any considerable number of psychologists. As the investigations of the sense organs and of the nervous system increased the body of fact available for the development of hypotheses, the original definition of psychology as the science of consciousness was extended, in spirit at least, to include the neural correlates of conscious processes.

William James[1] was one of the earlier psychologists in the country who clearly foresaw that the investigations into the relationship between the psychological faculties and human conduct had been neglected. The shift of the emphasis from the systematic treatment of consciousness to the investigations of the *conditions* under which consciousness manifested itself, may be regarded as the starting point of the *functional* point of view.

Briefly, the problem of functional psychology may be expressed in the question: How does consciousness function in human behavior or conduct?

Problem of Behavior Psychology

The origin of behavior psychology was largely due to the fact that functionalism failed to indicate the manner in which a conscious process could be regarded as controlling behavior. Neither the parallelism nor the interaction of conscious processes and physiological processes did more than indicate a possible relation. These theories did not show how behavior was actually modified. The question, whether an idea or conscious state could bring about action appropriate to the idea was denied by many. Some psychologists even concluded that no form of consciousness whatever could alter the direction of a neural flux, and consequently mere consciousness was not a factor in behavior or conduct. This inevitably led to the question as to how human behavior did actually come about and those psychologists who questioned the causal effectiveness of consciousness, formulated their problem in the following manner: How may the behavior of man (or animals) be described as *solely* due to receptor-effector processes in the neuro-muscular system.

[1] 'The Principles of Psychology,' 1890, Vol. 2, p. 1.

"They (sensations and perceptions) are therefore names of different cognitive *functions* not for different sorts of mental *fact*."

Conscious Processes as Metaphors

The lack of precision and accuracy in the terminology of the functionalists was one of the contributing causes in differentiating behaviorism from functionalism. The *function* of consciousness in behavior was accepted as a self-evident fact that needed no proof. "That they (mental phenomena) lead to acts is of course the most familiar of truths, etc."[1] This was, of course, merely a modified form of faculty psychology in which the term mind or consciousness was substituted for the term faculty. The following quotation is representative of the attitude of many functionalists so far as the practical applications of psychology are concerned.

"The formation of the elements of the process of knowledge and the inauguration of the control of our movements in accordance with the mandates of experience—these are the two great functions of perception."[2]

If this quotation is taken literally, then "perceptions" must be regarded as entities which inaugurate and control our movements. A faculty of 'perception' is, however, just as unscientific as were the entities of reasoning, poetry, foresight, etc., of the faculty psychologists.

We are warned, however, that the quotation is not to be taken literally but that the faculty implication is merely assumed to avoid an involved and cumbersome terminology.

"Let it be understood once and for all that wherever we speak, as occasionally we do, as though the mind might in a wholly unique manner step in and bring about changes in the action of the nervous system, we are employing a convenient abbreviation of expression which harmonizes with ordinary everyday methods of thinking and speaking about these relations. The real fact appears to be, that whenever we have mental activity we also have neural activity in the cerebral cortex. The basal distinction in the two kinds of nervous action to which we are referring in this chapter (mind, neural action and habit) is, therefore, not primarily

[1] James, Wm., 'Principles of Psychology,' 1890, Vol. 1, p. 5.

[2] Angell, J. R., 'Psychology,' New York, 1908, p. 171.

between a form in which the mind suddenly produces changes in the nerves as against one in which it does not, but rather a distinction between certain kinds of neural activity overtly involving consciousness, *e. g.*, cortical activity of the cerebrum, and certain other kinds not overtly involving it, *e. g.*, spinal reflexes. To use on every occasion the long modifying phrases necessary to precise accuracy on this matter would evidently be unduly cumbrous, and so the commoner modes of expression are employed, but the fundamental facts which lie behind these convenient metaphors must not be forgotten."[1]

One might very well condone a lapse from the "long modifying phrases necessary to precise accuracy" into the "occasional use of convenient metaphors," but when the precise accuracy is restricted to a few paragraphs and the occasional metaphors make up the body of the book, one is led to wonder whether the principle of faculty psychology may be considered repudiated.

Passing aside the question as to whether a textbook written in metaphors can be said to present its subject matter scientifically, the more important question as to what are the principles which underlie the functional point of view as implied in the preceding quotation, may be formulated as two propositions:

1. There are certain neural processes which overtly involve consciousness; *e. g.*, cortical activity in the cerebrum.

2. There are certain neural processes which do not overtly involve consciousness; *e. g.*, spinal cord reflexes.

It is difficult to see how these two propositions may be used to convert the metaphors of the following quotation into the precise accuracy that is necessary for scientific understanding.

"Perception enables its possessor to register in consciousness the particular object momentarily presented to the senses. But if consciousness never advanced beyond the merely perceptual stage it is apparent that we could never develop any highly systematized and intelligent movements of response to environmental demands and opportunities.

[1] Angell, J. R., 'Psychology,' New York, 1908, p. 59.

Intelligent deliberation would be impossible. We should always live in the immediate present and our minds could consciously look neither backward nor forward. Now it is in the image with its ability to carry such prospective and retrospective meanings that we find the psychical mechanism for accomplishing both these highly important functions."[1]

The writer does not wish to imply that the functionalists are the only group of psychologists who write essays rather than scientific expositions. On the contrary the preceding quotation on the function of perception is considerably clearer than many of the explanations of the functions of mental activity which are to be found in the literature of educational and applied psychology. Nevertheless, just as long as we excuse ourselves on the plea that others do it, and persist in substituting rhetoric for science, we should not complain if our work is regarded with suspicion by the biologists.

Consciousness and Behavior

Returning again to the fundamental principle underlying functional psychology, namely, 'that it is mental activity rather than mental structure that is of immediate significance for thought and conduct' we will make an attempt to illustrate just what sort of a relationship must exist between consciousness and behavior if the claims of the functionalists are to be substantiated by science.

The form of representation in the following figure is that usually used to illustrate the theory of psychophysical parallelism in which action is the result of the stimulation of a receptor (*S*) of some sort. The nervous excitation which results is transmitted over neurons indicated by solid lines in the direction of the arrows, to effectors (*M*) which may be either muscles or glands. When the nervous excitation reaches these effectors contraction occurs in the case of a muscle, and secretion in the case of a gland. Consciousness in the form of sensations, images, affections, volitions, emotions, etc., are indicated by the dotted lines. This consciousness is thought of as accompanying the neural processes.

[1] *Op. cit.*, p. 215.

The illustration thus represents the neural processes and the conscious processes which are said to be correlated with them.

The diagram below represents the neural and conscious conditions in the following situation: The subject is passing along a familiar road and suddenly comes upon a loose wire dangling from a telegraph pole. Let the solid lines between *Sa–Ma* represent the neurons which connect the eyes with the muscles that bring about the activity of walking. The conscious processes which may occur with the walking are represented by the dotted line parallel with *Sa–Ma*. If the

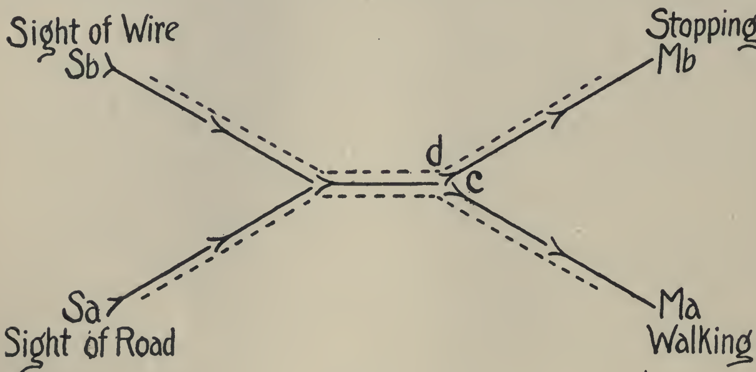

Fig. 1

activity of walking is completely habituated there may be no conscious processes which can be readily analyzed. Let the solid lines *Sb–Mb* represent the neural processes from the eyes to the muscles which result in stopping, supposing this is what happens as soon as the wire is seen. These muscles may of course be partly the same as those used in walking, but since walking and stopping represent different forms of behavior, they have been differentiated in the diagram. The dotted line parallel to *Sb–Mb* again represents the conscious processes which are correlated with the neural processes. For any given individual the elementary constituents (sensations, images and affections) of the consciousness will vary but by referring to it as the 'idea of danger' (from an electric shock) its characteristics are sufficiently described for our purpose. Let this 'idea' be represented by (*d*) on the diagram.

If consciousness controls behavior the functionalist must show how the idea (*d*) acts on the neural processes *Sa–Ma* and *Sb–Mb* so that *most* of this neural flux will go to *Mb* instead of *Ma* as was the case before the 'idea' appeared in consciousness. To do this (*d*) must be regarded as acting on the neural processes at some point, say at (*c*). The problem is simply this: What is the character of the psychical mechanism which will enable us to understand how a psychical process (the idea) can influence or change a neural process.

The functionalist cannot consistently accept the principle of psychophysical parallelism in which the conscious process merely accompanies a neural process without acting on it, since this would repudiate the possibility of consciousness influencing action. Some of the functionalists accept the principle of psychophysical interaction and believe that the 'idea' does in some way 'switch' the neural processes, though they frankly confess that they do not know how it is done.

It might be well to call attention to the fact that the diagram is general in character and that (*d*) may represent any conscious process whatever. In the illustration (*d*) is regarded as a perceptual process, but some functionalists maintain that the affective processes (satisfaction and annoyance) are the only ones which modify behavior; others would ascribe this modifying capacity to the emotions (fear, rage, sentiments, etc.); while still others substitute volitional processes (will, desire, wishes). The problem, however, to show just in what way these conscious processes act upon the neural processes is the same for all and for us it is only necessary to call attention to the solidarity with which they one and all agree that they do not know how this interaction takes place.

The most comprehensive attempt to show that conscious processes should be regarded as having the power to modify or control neural activity has been made by Wm. McDougall, who finally reached the conclusion that "Of the limits of the power of mental control over organic processes of the body we are altogether ignorant, and new evidence, much of it ill reported and therefore valueless, but much of it above suspicion, repeatedly warns us against setting up any arbitrary limit as to what may be effected in this way."[1]

Most psychologists would be content to determine the limits of the power of mental control over organic processes after it had been demonstrated that such control was an actual fact. It seems strange that the functionalists have never recognized clearly that they cannot expect any considerable degree of scientific recognition until they have developed a working hypothesis which will enable them to analyze any experience into those mental and physical components which are the *essential* condition of the experience.

This has been a rather difficult task for those who have tried it and a few functionalists have gone so far as to repudiate psychophysical interaction. For the latter only two alternatives remain: First, the investigation of conscious processes as such, without reference to behavior; or the point of view of structuralism. Second, an investigation of behavior independently of consciousness; or the viewpoint of behaviorism. Functionalism can only claim an independent point of view when it has shown how a mental process may act upon a neural process.

The Neural Correlates of Consciousness and Behavior

The relationship between consciousness and action toward which functionalism seems to be moving regards the *neural correlate* of consciousness, rather than consciousness as such, as the factor that modifies action. In other words, when the functionalist states that the function of perception is to control action, this means that it is the *neural correlate* of perception that controls action.

Referring to the diagram of Fig. 1, this would mean that the individual does not stop because the 'idea of danger' (*d*) acts on the neural processes so that most of the flux reaches *Mb* (stopping) but that the 'idea of danger' had as

[1] McDougall, Wm., 'Body and Mind,' New York, 1911, p. 375.

its neural correlate the processes *Sb–Mb* and it is this neural process (not the idea) which brought about the stopping. This is more clearly illustrated in that phase of functionalism in which the affective processes[1] (satisfaction and annoyance) rather than the perceptual functions are regarded as significant for behavior. The neural correlate for satisfaction is held to be the 'readiness of a neuron to conduct' and the degree of conductivity is measured by the "relief of interference with the life processes of the neurons concerned."[2] This theory seems to imply rather clearly that it is the neural correlate of satisfaction (readiness of the neuron to conduct a nervous process) rather than the subjective satisfaction that modifies the behavior.

It is rather difficult to represent this view on a diagram such as Fig. 1. The conscious process of satisfaction may be represented by (*d*) and the 'readiness to conduct' would then refer to the reduced resistance over the path *Sb–Mb*. However, that there is a "relief of interference with the life processes of the neurons" can only be maintained when it has been demonstrated that the reaction *Mb* does actually occur. Since we have no direct way of predicting the metabolic conditions of the neurons, to *infer* from the reactions that there has been a "relief of interference with the life processes of the neurons" does not help us to understand how the increased 'readiness to conduct' has been brought about. This, of course, is the problem.

Whether we regard the neural correlate of either cognition or affection as significant for behavior, the fact remains that the conscious processes themselves cannot be regarded as significant, because they cannot occur until the neural conditions have been prepared. That is, at best the conscious

[1] Thorndike, E. L., 'Animal Intelligence,' New York, 1911, pp. 244.

This quotation has been selected because it represents a point of view which ascribes causal effectiveness to the conscious processes of pleasantness and unpleasantness rather than to perceptual processes. The actual working hypothesis of Thorndike is however much better expressed by the following quotation, which is however behavioristic rather than functional in principle:

"Every response or change in response of an animal is then the result of the interaction of its original knowable nature and the environment," *op. cit.*, p. 242.

[2] Thorndike, E. L., 'Original Nature of Man,' 1913, p. 225.

processes merely indicate that the neural conditions for action have already been established. The mental processes themselves are no assurance that the appropriate action will actually take place nor do they indicate how the neural conditions for the action which actually does take place have been prepared.

In other words, the conscious processes *follow* the neural processes, they do not *lead* them.

To learn how the neural correlates of conscious processes come to have the configuration which they do, can only be done by considering the properties of the neuro-muscular system.

Some of the functionalists recognize this clearly enough as a general principle, but in the actual development of their subject matter they drop into the terminology of psycho-physical interaction so naturally and with such abandon that one cannot help but feel that the introductory emphasis on neural function is a protective measure against criticism rather than a working hypothesis.

The Neural Correlate of Consciousness and the Neural Correlate of Behavior

The element of greatest confusion in the relationship between consciousness and action is probably the fact that consciousness has always been regarded as an existential datum entirely distinct from action or behavior. Whatever may be the metaphysical principles involved in the mind-body problem, from the standpoint of science, consciousness must be regarded as a reaction. When an observer reports that he is conscious of 'danger' this only means that the muscles of his speech mechanism have contracted in such a way that sounds which we call words and sentences are produced. If the observer does not react in some way, we cannot infer that he is conscious. That is, unless the mental process is expressed by speech (or some equivalent action) this mental process can never become available for science.

Consciousness, as a scientific concept, may be regarded as merely a supplementary reaction of a specific type to a given situation. If for instance I am asked "What is seven times sixteen?" I may, after some hesitation, simply pronounce the words "One hundred twelve." If then I am asked to give my introspections, I may add: "I had a visual image of the figures 7 × 10 followed by the visual image of 70 written on a blackboard; this was followed by the auditory imagery—seven times six . . . forty two . . . ; then I had auditory images with kinesthesis of the speech mechanism—seventy . . . forty . . . hundred ten . . . two . . . hundred twelve . . . visual image of 112 written on the blackboard."

We must note that there are two sets of stimuli: (1) What is seven times sixteen, (2) Give your introspections. We should therefore expect two different reactions. Why should we say that the second reaction (introspection) is of an altogether different type than the first? Every one will immediately admit that it is absurd to say that the response to the second stimulus controlled the reaction to the first. The facts in the case are more scientifically stated when we merely regard the introspective reaction as only one of the responses which might be expected in a situation of this kind. It no more controls or determines the many other

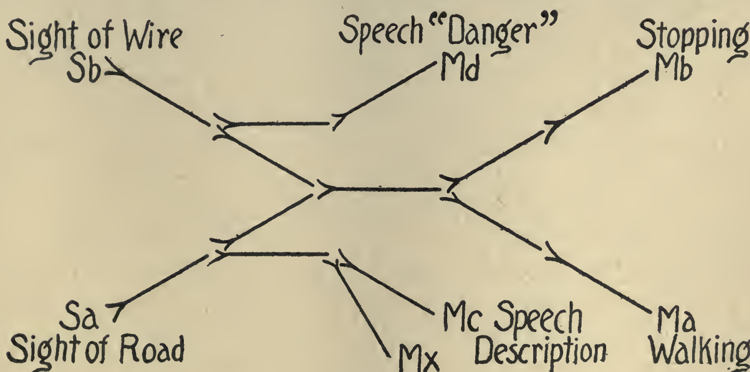

FIG. 2

responses that might have been made, than they control it.

The above diagram (Fig. 2) shows the road-wire situation illustrated in Fig. 1, but drawn as a number of reactions without any hypothetical conscious processes.

When the stimulus is *Sb* the observer not only stops walking (*Mb*) but he may also say: "That wire looks dangerous, I have had one electric shock; last summer Mr. X was killed in this way;" as indicated by the speech reaction *Md*. It is not necessary to conclude that being able also to react by speech (introspection) has anything to do with stopping (*Mb*). The only essential condition for stopping is the stimulus *Sb*. The speech reaction *Md* has nothing to do with it.

Before showing the relationship between the neural correlate of consciousness and the neural correlate of behavior we will consider the terms 'major' and 'minor' reaction. We will assume that every stimulus or situation to which an organism adjusts itself, may result in more than one reaction.

Major reaction: In the first place we have the reaction which is regarded as the appropriate reaction or adjustment to the particular situation. In our illustration this would be 'stopping' (*Mb*) when the wire is seen.

Minor reaction: Secondly, there are also other reactions such as vasomotor; respiratory; changes in the accommodation of the sense organs; speech reactions, both actual and incipient; etc. In our illustration these minor reactions may be a verbal exclamation or introspection (*Md*); decrease in respiration rate; increase in pulse rate; or any of the many bodily and visceral changes which might be described.

Simply stated the major reaction is the one which is significant for behavior, while the minor reactions are those which are usually disregarded. In this sense the introspective report would be called a minor reaction. In the diagram Fig. 2, *Ma* and *Mb* are major reactions; while *Mx*, *Mc*, *Md*, are minor reactions. The difference between Fig. 1 and Fig. 2 lies in the fact that the dotted lines in Fig. 1 which are supposed to represent hypothetical conscious processes have been replaced by neural processes. In Fig. 2 the conscious process of the 'idea of danger' (*d*, Fig. 1) is represented as the speech reaction *Sb–Md*. That it must be some kind of a reaction if it is to be regarded from the scientific standpoint seems clear when we reflect that consciousness which is not *expressed* in some form of reaction, can never become available for science. It is absurd to say that we may have conscious states to which we do not react. The statement itself is a reaction. If we admit this, then every conscious state or process is a reaction. The statement that I have the 'idea of danger' or am 'conscious of danger' only means that in addition to reacting to the sight of the wire by stopping I also react to it by saying, "I must be careful; that wire may be charged; etc."

That this speech may not actually take place is due to the fact that under ordinary conditions we do not react by speech when we are alone. If we have some one with us, a child perhaps, the reaction would take place as a warning or protective movement of some sort. The minor speech reaction *Sb–Md* (which is the only way in which the term consciousness can have a scientific meaning) need have no control or influence over the major reaction *Sb–Mb*. In fact either *Md* or *Mb* might very well occur independently. They are related only through the receptor *Sb*.

From the preceding we can conclude that every stimulus which effects the organism results in more than one reaction. Only one group, however, is usually regarded as socially significant (behavior) and we have called it the major reaction. Along with this major reaction there are many minor reactions and those of the speech type are very numerous. There is no need, however, to believe that these minor reactions are necessary for the adequate functioning of the major reaction.

Consciousness and Introspection

From what has preceded, introspection is merely a name for a group of speech reactions which conform to a particular terminology. While we are introspecting we not only react to a given situation in a manner appropriate to the situation (pressing a key for instance) but we also react by speech in the terminology of psychology. The functionalists have not recognized that the introspective reaction is a minor reaction. The social significance of the major reaction has obscured the fact that introspection itself is only one of the reactions to the particular situation. They have assumed that in some way it reveals what is taking place in the neural correlate of the major reaction. The development of this introspective reaction is a process of habit formation just as any other variation of response. It is not the expression of a metaphysical entity (consciousness) that has been added to the major reaction.

The only difference between psychological observation and observation in the natural sciences lies in the fact that in psychology the introspective reaction is regarded as the major reaction, while in the natural sciences it is regarded as a minor reaction or ignored entirely. However, neither the major nor the minor reactions can be said to control or modify each other and in this sense there is no mind-body problem.

Functionalism Obscures the Nature of the Psychological Problem

It is not supposed that the nature of the preceding discussion will be considered especially new by the functionalists. Their excellent experimental work in pedagogy and the emphasis they place upon habit formation irrespective of any subjective implication, indicates that practically at least they are following behavioristic methods.

The importance for general psychological theory as to whether human behavior is regarded as the result of the interaction between a hypothetical consciousness and neural processes or as the result of neural mechanism only, lies primarily in the fact that the experimental program will reflect which of these points of view is adopted.

If, as the functionalists assume, consciousness can modify behavior, then to bring about socially acceptable behavior in the child, for instance, it is only necessary to bring about those forms of consciousness which are effective. The problem then becomes one of teaching 'ideals.' From the pedagogical standpoint an ideal must be regarded as a plan of action or conduct. If the expressions of consciousness are used as a test as to whether a given set of ideals has been established there will be a tendency to place undue emphasis upon verbal expression, since where a serious attempt is made to determine the character of consciousness introspection must play an important part. The problem of teaching ideals then becomes one primarily of establishing formal speech reactions to given situations. While for many situations in life a speech reaction is the adequate reaction there are, however, many other situations in which speech alone is inadequate, as for instance in many ethical relations. In such instances the verbal expressions of the ideals are no assurance that the socially valuable reaction has been learned. On the other hand the socially desirable reaction cannot be learned by merely establishing the speech reaction which *describes* the appropriate behavior. The behaviorist maintains that it is better to disregard the concept of consciousness altogether and the pedagogical problem then becomes one of determining exactly how socially acceptable behavior is developed directly from the properties of the neuromuscular system. If a verbal reaction is part of the appropriate adjustment, well and good; it must be learned. However, after it has been learned there is no advantage in assuming that this verbal reaction is the function of a hypothetical psychical process.

Behaviorist Program

Perhaps the distinguishing difference between the functionalist and the behaviorist lies in the fact that the behaviorist disregards the entity which the functionalist calls consciousness. This does not mean that the behaviorist ignores those problems in behavior with which the concept of consciousness is usually associated. On the contrary by regarding man as an organism he believes that even the most complex problems can be described and explained without assuming the existence of any causes which are not already accepted by science in general. The behaviorist's principal interest will be the movements of man whether these movements are of the skeletal muscles which carry his body from place to place or the movements which result in the composition of a symphony. He regards the neuro-muscular system as the means by which the organism adjusts itself to its environment, just as the heart, the lungs, digestive tract, are means to keep the organism alive. To withdraw the hand from the scorching candle is a movement of only less complexity than the movements of the pen that signs a treaty between nations. The real object of worth for the behaviorist is not the thrilly, fascinating, esoteric, pseudo-problems of the mystic, but the permanent, measured and describable adjustments of the race.

Summary

1. The axiomatic character of the statement 'Mind Controls Action' is questioned by the behaviorists.

2. The metaphorical explanations of the functionalists obscure the 'faculty' character of their 'mental activity.'

3. The functionalists have never shown how mental activity may control action.

4. The conscious processes of the functionalists actually follow the conditions which bring about a modification in behavior and hence cannot be said to control behavior.

5. Consciousness and introspection to have any scientific value must be regarded as speech reactions which are restricted to the terms used in psychology. The presence or absence of these classes of verbal reactions in no way influence the socially significant reactions.

6. The difference between the natural sciences and psychology lies in the fact that psychology restricts its observations to those special speech reactions (introspections) that conform to the terminology of psychology. In the natural sciences this class of verbal reactions is ignored.

7. The concept of a hypothetical conscious process obscures the real problems of psychology.

8. Behaviorism regards the introspective reaction as only one of the ways in which an individual may react to a situation.

THE RELATION BETWEEN EMOTION AND ITS EXPRESSION

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This paper proposes a conception of the nature of an emotion in relation to its expression which constitutes somewhat of a compromise between the theory of James and the older view which it displaced.

The popular view assumed that some inner or central emotional experience follows the act of perception, and that this emotion is succeeded by a complex series of organic disturbances. The inner activity is the emotion and the resulting organic change is the expression of that emotion.

James denied the existence of any centrally conditioned process intervening between the perception and the organic activities which may properly be termed an emotion. James asserted that the terms emotion and expression must refer to distinctions within the series of organic activities aroused by the perception, and that these activities are sensory and peripheral in character. As is well known, James dichotomized these psychophysical activities into their conscious and their material aspects. The experiential or non-material aspect is termed the emotion, while the material or physiological aspect is the expression of that emotion. As a consequence of this usage of terms, and of the acceptance of the prevalent doctrine as to the relation of consciousness to afferent and efferent nervous impulses, James was forced to the paradoxical conclusion that the emotion is not the cause but the result of its expression.

Our view agrees with that of James in maintaining that the terms emotion and expression must refer to distinctions within the total series of organic activities, and that these processes are peripheral and sensory in character. We shall, however, adopt a radically different mode of division from that employed by James.

These organic activities may first be divided into three sets of psychophysical processes—the act, the emotion, and incidental by-products of the emotion. In anger, the term act is applied to those activities immediately concerned in combat—the fighting activities. Flight or running away is the act in fear. The term emotion refers to all those prior and accompanying organic processes whose function it is to render the act more efficient. The emotion and the act are to some extent independent variables; theoretically one can fight without being mad, and one can become angry without fighting. Likewise one can run away without being afraid, and fear without indulging in flight. The nature of the emotion and its functional relation to the act have been well depicted by Cannon. In general the emotion consists of those processes by means of which the total energy of the organism is mobilized and concentrated for the service of the act. The function of anger is to increase the efficiency of the fighting activities. A cause and effect relation obtains between the emotion and the act. The increased efficiency and sometimes the initiation of the act are thus a result of the emotion. The total series of processes involved in the organic disturbance may also contain other components which are to be regarded as the incidental but necessary by-products of the emotion or the act, but which contribute in no way to the efficiency of either. The trembling of anger or certain digestive and nutritive disturbances incident to the vasomotor shift may be adduced as hypothetical examples.

These three groups of activities—the act, the emotion, and their by-products—constitute the whole of the organic processes. There is no fourth class which can be termed the act of expression. The term expression implies a dichotomy of the same organic activities from another standpoint, viz., their relation to some observer. An emotion can express itself only by producing some effect upon an observer. Without an observer the term expression is without meaning.

The emotion can express or manifest itself to an observer in three ways: (*a*) It expresses itself indirectly through its observable effects upon the act. Such characteristics as the determination, vigor, and persistence of the fighting act are observed and become the sign and symbol of the emotion of anger which produced them. The increased efficiency of the act thus constitutes both a result and an expression of the emotion. (*b*) Any of the observable by-products of the emotional situation also constitute a mode of expression. To an observer they may symbolize the existence of the inner emotional disturbance of which they are a result. (*c*) The emotion also manifests itself to an observer in a more direct fashion. Certain essential components of the emotional processes, such as the flushed face, the frown, and the deeper breathing in anger, are directly observed and constitute another mode of expression. The emotional process as a whole is not observed; only certain surface aspects of the total process are perceived. These perceived aspects are interpreted by an observer in terms of his experience and knowledge and hence become the visible symbols or manifestations of the emotion as a whole. A part thus becomes the symbol of the whole, and a symbol is a mode of expression. Good usage, I think, will justify this meaning of the term. We may thus legitimately assert that the emotional activity manifests or expresses its nature to an observer by means of these surface or observable features.

Our conception may now be compared with that of James. Both are actuated by the same purpose. Both attempt an expository definition of the popular meaning of two terms. Both attempt a definition of emotion and expression in descriptive and empirical terms. Both agree that emotion and expression must refer to certain aspects of the organic activities involved in the emotional situation. The two views ascribe radically different contents to these terms. James's analysis was dominated by the subjective conception of the province of psychology which prevailed at that time. An emotion as a psychological phenomenon must be defined in purely conscious terms; the physiological aspect of the process must be discarded. This subjective emotional experience can naturally express itself to an observer only through behavior or physical means; the behavior, material, or physiological aspect of the process must then constitute the only avenue of expression. With this conception, emotion and expression must refer to the psychic and the physical aspects respectively of the organic reaction to the emotional stimulus, and in virtue of these definitions the emotion must be a result and not a causal antecedent of its expression. Our hypothesis rejects the purely subjective point of view in psychology, and consequently discards this psychophysical dichotomy of James. Both emotion and expression are regarded as psychophysical processes, or rather they are regarded as real functional activities of a human organism irrespective of the fact whether they do or do not contain a conscious component. According to our analysis, these terms have been so defined in relation to each other that one can say that the expression is a result of the emotion.

James's theory of emotion contains two more or less distinct doctrines whose validity must be separately estimated. One of these, to my mind, is correct, and the other fallacious. The first doctrine asserts that the term emotion refers to certain aspects of the organic activities and that these activities are essentially sensory in character. We have subscribed to this feature of the theory, and we believe that it constituted a genuine and important contribution to the psychological thought of the time. This aspect of the theory, it is well to note, is open to empirical verification; James's various factual proofs and the recent experimental attempts at a disproof are relevant to this aspect of the Jamesian doctrine. Needless to say, we believe that the factual evidence at the present confirms James's contention.

The second aspect of James's doctrine consists of the following features: (1) the assumption that emotion and expression refer to the psychic and the physiological components respectively, (2) the acceptance of the prevalent assumption as to the relation of the psychic to its physiological correlates, and (3) the final conclusion that the emotion is the result of its expression.

It must be at once admitted that this conclusion is *logically valid and unassailable* from the standpoint of the prior assumptions. Neither is the proposition susceptible to experimental proof or disproof. James's formidable list of factual proofs is not relevant to this phase of the argument. His conclusion is logically implicit in his assumptions; it represents merely the result of a deductive analysis of what was contained in his premises. James assumed the truth of this conclusion when he made his preliminary definitions. A rejection of one of these assumptions constitutes the only avenue of escape for those who dislike the final conclusion.

This aspect of James's theory contains, to my mind, two essential defects. (1) The conclusion contradicts common sense, and this contradiction is due to the fact that James ascribes to the term 'expression' a meaning which is directly antagonistic to the significance usually attached to it. As previously noted, the term expression popularly signifies some effect of the emotion upon an observer, and James has arbitrarily so defined the term as to reverse this causal relation. (2) James's analysis and conclusion are also lacking in pragmatic value. His mode of treatment gives us no analytical comprehension of the functional interrelations between the various constituents of the organic activities, nor of their nature and significance in relation to mental life and conduct. The conclusion resulting from James's mode of analysis is logically true and valid, but the knowledge it represents lacks genuine significance and worth.

In spite of varied criticism, James's theory has enjoyed an enviable reputation for many years. Several factors have probably contributed to this result. (1) Unless the two phases of the argument are differentiated, one is compelled either to adopt or reject the theory *in toto*, and undoubtedly to many minds the advantages of the theory outweigh its deficiencies. (2) As we have noted, James was driven to his psychophysical distinction by the adoption of the subjective conception of the province of psychology. Likewise, James's theory will of necessity make a strong appeal to those whose thought is dominated by this attitude of mind, and the conventional definitions of the subject matter of psychology have, until recent years at least, been couched in subjective terms. If emotion is a psychological phenomenon, it must be defined in conscious terms. Expression, on the other hand, must be conceived in behavioristic or physiological terms, because expression must refer to some effect upon an observer. (3) The *paradoxical* character of the conclusion is a third factor. The popular mind is somewhat prone to judge the value and worth of a science upon the basis of the novelty and startling character of its discoveries. The wonders of a science are paraded in proof of its amazing progress in attaining its ends. These discoveries are frequently wonderful and startling simply because of their novelty and unexpectedness, —because they contradict or modify prevailing conceptions and opinions. Psychology as a young and growing science must also produce its miracles in order to secure popular acclaim to its worth and greatness, and what can be more wonderful and miraculous than the discovery and labored empirical demonstration of a truth which directly contradicts the common sense opinion of mankind? As psychologists, I fear, we have been somewhat susceptible to this influence, and this suggestible attitude of mind has probably been strengthened to some extent by our experience in the classroom. I know of no doctrine in psychology which is comparable with that of James's theory of emotion from the standpoint of inculcating in the mind of the average undergraduate a wholesome awe and respect for the achievements of our science. This chapter of James almost invariably makes a profound impression upon the student mind, and the reason is not far to seek, for it consists of a very clever and brilliant exposition, and a persuasive logical and empirical demonstration of the truth of a proposition which many students accept with some degree of mental reservation.

In place of James's psychophysical analysis, this paper suggests a threefold division of the organic activities on the basis of their *causal interrelations*. The act refers to those processes of adaptation to the objective situation. The emotion refers to those activities which increase the effectiveness of the act. The remaining processes consist of incidental by-products of the emotion or the act. There is no coördinate fourth group of processes which can be termed the expressive activities. The term expression implies a dichotomy of these same activities from a different standpoint—their relation to an observer. The larger and more important portion of the emotional group of processes can not be directly observed; their nature and existence must be inferred from those aspects of the organic activities which are susceptible to immediate observation. Certain aspects of all three of the previously enumerated classes constitute a sign or symbol of the existence of these hidden operations, and consequently become the means by which these latter manifest or express themselves to an observer.

THE THEORY OF THE SOCIAL FORCES

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It has been said that "the corner stone of sociology must be a sound doctrine of the social forces."[1] With certain limitations this statement may be allowed to stand. In the first place, pure science, as such, does not admit of the use of the term 'force' in the sense of the sufficient cause of any phenomena, physical or social.[2] Taken to mean, however, only an active factor in a given situation, 'force' is in common usage in the physical sciences and has been borrowed by the social sciences for want of a better term. At best it is only a helpful analogy. In the second place, a 'sound doctrine' of the social forces must not be taken to mean or imply an 'exhaustive, logical or psychological classification' of the factors active in social life (as has generally been done by the older sociologists), for the progress of scientific knowledge of human society is not dependent upon success in any such classification. Indeed, as Dr. Bernard has pointed out, "the most accurate possible classifications [of social forces] mark only the most elementary stage in the analysis of social phenomena."[3] Professor Ross's statement, therefore, interpreted or limited in these respects, comes to mean simply that an understanding of what are the active factors in social life is necessarily basic in any scientific study or discussion of social situations and institutions.

What are the active factors in social life? Some sociologists would limit them to forces which are social in their origin; others would include all factors, whatever their origin, which are socializing in their effects. Professor Baldwin

[1] Ross, E. A., 'Foundations of Sociology,' p. 181.

[2] See article by Prof. E. C. Hayes, 'The Social Forces Error,' in *Amer. J. of Soc.*, 16, 613–625.

[3] Bernard, L. L., 'The Transition to an Objective Standard of Social Control,' p. 74.

limits his definition to 'only those psychical products, called desires, which influence individuals in their social relations';[1] Professor Ellwood, much more sanely, uses the term to include 'every factor which has some degree of active influence in shaping and molding the forms of association and the interaction of individuals.'[2] Accepting this latter definition, as the more adequate of the two, we are forced to include among the social forces such physical factors as climate, soil, and other geographical conditions. To primitive men these environmental conditions were much more important than they are today, for civilized man has brought his environment largely under his control.[3] Heredity and variation must also be included as active factors. True, they are not direct factors, and, assuming a strict psychological interpretation of society, they become factors in the organization of society only by setting up conditions or limits within which, and only within which, the more strictly social forces may act.[4] At any one moment, perhaps, the forms of social life seem to depend much more upon mental elements than upon physical factors, but when one surveys human groups over long periods of time the influence of physical factors is more apparent. The environment has acted indirectly, selectively, upon man's hereditary equipment, but also directly, through such agencies as temperature for example, to modify instinctive and habitual responses of masses of individuals and the interaction between individuals.

Coming now specifically to a discussion of the psychological factors or forces in association, we may agree at once with Dr. Ellwood that these consist of man's innate impulses (instincts), his feeling states, and his cognitive or intellectual processes.[5] This assumes, apparently, the existence of a

[1] Baldwin, J. M., 'Social and Ethical Interpretations,' p. 484.

[2] Ellwood, C. A., 'Sociology in its Psychological Aspects,' p. 278.

[3] Thomas, W. I., 'Social Origins,' pp. 130 ff.

[4] See Baldwin, *op. cit.*, Appendix H v, p. 573.

[5] *Op. cit.*, p. 282. The feelings and intellectual processes may enter, however, only in their physical aspects as attitudes called out by stimuli and thereby influencing other responses in the same individual and in others. They cannot function in this connection purely as feelings and as awareness. We can know of psychic states in others only by the behavior of those individuals. These facts will be borne out later.

'social mind' and therefore 'social feeling' and 'social thought' which have the same functional relationship to social activity which individual feeling and thought have to individual activity—since social activity is due simply to the interaction and coördination of individual activities. Necessarily, therefore, we must decide first what is the functional relation of feeling and thought to individual activity. First of all, however, we must assign to instinct its proper rôle in determining activity.[1] Here there is more or less agreement. The instincts are of first importance because they are primary in man and serve in the initiation of action. Human conduct can never exceed the limits of these native proclivities which assign for man the ends of action and alone make any action worth while.[2] Man's native dispositions to activity, however, do not remain long unmodified, but, through interaction with the environment and by training, become overlaid with a mass of habits which come to function, under proper stimulation, as readily and as actively as did the original propensities to activity.

The moot point of the whole theory of social forces enters, however, at this point of the discussion. The issue centers around the part which feeling plays in determining the direction and extent of the modification of man's instinctive impulses to activity. The older view, held by the hedonists from the time ot Hobbes to the present, assigns to feeling the function of a primary force, as lying behind these instinctive activities. All action, individual and social, was explained on a basis of pleasure and pain, as the springs of activity were to be found in calculations of agreeable or disagreeable sensations.[3] The late Professor Ward, the father of Ameri-

[1] By instinct is meant the inherited disposition to respond in certain ways when appropriately stimulated. This definition is used so that instinct may be taken as synonymous with the whole "original nature of man," and, for the purpose in hand, is sufficiently exact.

[2] See Veblen, T. B., 'The Instinct of Workmanship,' Introduction.

[3] See Hobbes, 'Leviathan,' Works, III., p. 42; Locke, 'Essay Concerning Human Understanding,' Book II., Ch. XXI., Sec. 33, 41; Bentham, 'Principles of Morals and Legislation,' Chap. I., Sec. I.; Spencer, 'Principles of Psychology,' II., p. 541; Baldwin, 'Handbook of Psychology,' pp. 301–303; Angell, 'Psychology,' p. 273; Patten, 'Theory of the Social Forces,' Chap. I., Sec. I.; Ward, 'Psychic Factors in Civilization,' pp. 52, 54, 126, and 'Pure Sociology,' p. 132.

can sociology, was particularly at fault in this regard. To quote short passages from his works: "The dynamic agent consists wholly of feeling;"[1] "Feeling is a true cosmic force . . . and constitutes the propelling agent in man and animals."[2] "In the associated state of men, it (feeling) is the true social force."[3] "The thinking faculty is not a force; but feeling is a true force and its various manifestations constitute the social forces."[4] True, Ward speaks of the desires also as a true social force, but he uses 'desire' in the 'feeling' sense,[5] saying that desire is a form of pain.

There are two other types of writers, only partially or not at all hedonistic, but individualistic for the most part, who treat of social forces as causes of activity in one way or another. One class regards feeling as one, but only one, of the determining factors in activity. Those of the other type hold that feeling can never be such a cause.[6]

What then is the actual part which feeling plays in initiating activity? The answer must first of all settle the question of what part consciousness plays, if any, in the process. The better psychology at present holds that mental processes are conditioned by changes within the organism, notably within the nervous system.[7] The more immediate physical qualities of mind lie within the brain and are determined (1) by stimulus and (2) by disposition or tendency, which latter indicates that the "neural functions are determined by the residues of earlier function (impressional, associative, determining and habitual tendencies, and general cortical set)."[8] The facts of perception are mainly to be

[1] 'Pure Sociology,' p. 256.

[2] *Ibid.*, p. 39.

[3] *Ibid.*, p. 99.

[4] *Ibid.*, p. 101.

[5] 'Psychic Factors in Civilization,' pp. 53–54.

[6] Among writers of the first class are Titchener (see his 'Outlines of Psychology,' p. 250), Thorndike (see his 'Elements of Psychology,' p. 284). Among writers of the second type are James (see his 'Psychology,' II., pp. 559, 580), Dewey (see Dewey and Tufts, 'Ethics,' p. 270), McDougall (see his 'Introduction to Social Psychology,' p. 43).

[7] Bentley, Madison, 'A Preface to Social Psychology,' in one of a series of articles entitled 'Studies in Social Psychology,' in Psychol. Monog. No. 92, June, 1916, pp. 10 ff.

[8] *Ibid.*, p. 10.

explained by stimulus and associative tendency; passive memory and imagination by associative and impressional tendencies; emotion and action by stimulus and determining tendency; skillful performance by habitual tendency; and thought by dispositions of the determining sort. In view of these facts we cannot admit that imagery is necessary, in any causal way, even to voluntary activity. Neither can we regard it as a superfluous or parallel process. Rather, imagery is the evidence of associations and neural activity between stimulus and response. It is these processes self-aware in a fashion. The complete determinant of voluntary activity is nothing more or less than the total set of the nervous system of the moment, plus the stimuli. The total cause of any act is certainly more than the conscious part of it. A percept or image, coming about when the neural pathway or the act is forced by interference to run probably through the more complex channels in the cortex, is not the cause of the act but only the sign of the whole act of which it is a part.

The same is true of feeling—it is not the cause of the act but only the sign of the whole act of which it is a part. Says Dr. Bernard: "Feeling modes are resultants of internal neural adjustments, . . . which correlation probably is made in the cortex only when feeling is experienced. It is absurd to speak of these feeling modes as the cause of such neural correlations."[1] Again: "Feeling . . . is the result of the correlation, that is, the supplementation or interference, of nervous processes in such a way as to increase the neural activity along a . . . given pathway. Where a nervous process is augmented, pleasantness is experienced, and where a nervous process is weakened or diminished, there is unpleasantness."[2]

Professor Ellwood professes to be in sympathy with these views as being simply more exact statements than the crude evolutionary view of feeling which he advances, and recon-

[1] *Op. cit.*, p. 37.

[2] *Ibid.*, p. 18. This theory is only a more accurate statement of the theory of feeling given by Max Meyer in his articles on 'The Nervous Correlate of Pleasantness and Unpleasantness,' in Psychol. Rev., 1908, 15.

cilable with it.[1] Feeling to him is however 'an organic valuation of our activities.'[2] He says: "Society is made up of biological and psychological individuals, and these individuals are thinking, feeling men whose actions are mediated, guided and *controlled* by feelings and ideas."[3] Again: "Feelings and ideas are not coëxtensive with activity," but 'relatively new and independent elements' which 'appear within physiological activities at certain points to evaluate them, mediate and control them.'[4] Finally: ". . . social phenomena are in the nature of responses to stimuli, and these responses are modified, in the mature individual at least, by complex series of feelings and ideas."[5]

To such statements must be raised the fundamental objection that they do not in any tangible way explain how feeling operates to accomplish its evaluating function. Why, for example, does the pleasurable act survive over the other acts? Can a successful result act backwards and strengthen the impulses leading up to it and stamp out the unsuccessful impulses? Hardly. Rather, it happens that "by the actual overlapping of many tendencies to respond in diverse ways the erroneous tendencies are directed into the successful ones and the latter are strengthened by reinforcement. Without such overlapping of various impulses in the same general response, the inhibiting effects of the successful upon the unsuccessful or irrelevant tendencies are incomprehensible."[6] The pleasure accompanying the successful act as a complete response is not itself a cause or natural antecedent of the surviving act but only the inner or 'felt' aspect of it and therefore valueless in explanation. The selectiveness of the organism, which Ellwood would call 'feeling control,' is simply its more easy adaptation to certain direct and indirect stimuli than to others, due to inherited and acquired nervous correlations. The pleasurable tone accompanying certain

[1] *Op. cit.*, footnote on pp. 112–13.

[2] *Op. cit.*, Chapter X., p. 247.

[3] *Op. cit.*, p. 250.

[4] *Op. cit.*, p. 250.

[5] *Ibid.*, p. 251.

[6] Peterson, J., 'Completeness of Response as an Explanation Principle in Learning,' Psychol. Rev., 1916, **23**, 153–162.

activities is only a subjective indication that the response, up to a certain limit, follows the line of least resistance. Certain acts are 'chosen,' because they are on the whole the most natural to the organism under the circumstances, not because they are pleasant. That they *are* pleasant, in the main, indicates subjectively that the response is relatively 'complete' and in harmony with one's inherited and acquired organization.

As has been shown experimentally, some acts may be both pleasureable and painful, painful alone, or without conscious results. We may, again, experience pleasantness from higher sensory or ideational processes at the same time with pains from lower neural processes. Finally, any act may be made pleasant or unpleasant through habit. Consequently feeling modes cannot be effective guides to individual or social adjustment and control. Ideas, images or other subjectivistic criteria are not always valid, says Bernard, and are only dependable when checked up by objective reference. "Feeling," he states, "as the conscious part of mere correlation, *i. e.*, supplementation and interference of neural processes, is the least able to be so checked up and is consequently the least reliable of all subjective criteria or evaluations of action in an objective and social world."[1] Feeling is a purely personal and individualistic phenomenon.

A similar problem to that of determining the function of feeling in connection with the forms of association and individual interactions is presented in establishing the rôle which the intellect or the cognitive elements of mind play in these processes. Professor Ellwood holds that "the distinctive character of our social life is due to the modifying influence of intellectual elements,"[2] and he maintains that the intellect plays a decisive rôle not only in adapting the individual organism, in man at least, to his environment, but also in bringing about those 'higher adaptations which characterize civilized societies.'[3] Again, he says that the intellect, the

[1] *Op. cit.*, p. 28. Practically the same view is set forth by Dr. A. F. Bentley. See his 'The Process of Government,' Chapters I. and II.

[2] *Op. cit.*, p. 261.

[3] *Op. cit.*, p. 261.

cognitive, objective side of mind, 'evaluates activities with reference to the environment and functions to mediate and control them with reference to environmental factors.'[1] And finally: "While the intellect seems to have been developed chiefly as an aid in carrying out the instincts and in satisfying the demands of feeling, in its higher reaches it can and does *act more or less independently* of them."[2] By this last statement he means that the intellect modifies instincts, substituting habits for them which become as strong as the original instincts.[3]

Such passages seem, almost, to set up the human mind as a separate, supernatural entity which acts as a sort of dictator to instinct and feeling and to motor responses. This idea is brought out more forcibly when he says that "more and more the process of living together needs the interference of reason."[4] What Professor Ellwood means, however, seems to be only that ideas have come largely to be the stimuli to activity among civilized peoples. Reflective thought, which played an insignificant rôle in primitive society, is now, he thinks, the decisive element because "upon it depends the control, not only of the forces of physical nature but also, . . . of the feelings and impulses of human nature."[5] For this reason, and in this sense, he argues that ideas are entitled to be called forces, "since they at any rate become active factors in the later stages of social evolution and absolutely decisive, . . . in the making of the more complex adjustments."[6] These ideas, in civilized man at least, come in time to constitute for the individual and society a sort of 'subjective environment' and to this environment "the mass of individuals respond quite as they do to stimuli in the objective environment."[7] Professor

[1] *Op. cit.*, p. 263.

[2] *Op. cit.*, p. 263.

[3] Practically the same view of the function of the intellect was held by Ward. See 'Pure Sociology,' Chapter XVI.

[4] *Op. cit.*, p. 264.

[5] *Op. cit.*, p. 264.

[6] *Ibid.*, p. 277.

[7] *Ibid.*, p. 265. The same position is argued at length in his recent article: 'Objectivism in Sociology,' in the *Amer. J. of Soc.*, December, 1916.

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If the mind is conditioned, as was shown above (p. 5), by stimulus and disposition or tendency, the use of such expressions in connection with social activity as: "mind 'influenced' by another mind," "Man is 'suggestible' or 'imitative,'" "one mind 'rules' or 'dominates' and another 'acquiesces,'" etc., is wholly outside the plane of scientific explanation. A stimulus, as said before, is a physical agent and sets up a series of concrete organic processes. 'Suggestion,' 'imitation' cannot be accounted such agents when used to explain the destructive activities of a mob, nor can 'domination' or 'sense of power' be given as the cause of the acts of a railway trainmen's union. Used in such a fashion such terms are wild abstractions used as forces,[1] and are closely akin to the 'faculties' of two centuries back.

'Faculties,' of course, is unacceptable for the purposes of scientific explanation, but we also lack a concrete meaning for 'mental dependence' or 'mental interaction' in such a statement as "individuals tend to believe and to think and to feel, etc., in mutual dependence." "It is obvious," says Bentley, "that the mind of my neighbor is not to be added, as a condition of my mental processes, to the sober and authenticated facts of stimulus and disposition. If my neighbor speaks with the voice of authority and decision and so convinces me that I should attend the meeting of the Municipal League, my mental processes are set up, after all, just as they would be if I found a blight upon my fruit trees and decided to destroy the orchard. Auditory or visual stimuli and associative tendencies account for the perceptual part of either experience, and determining and habitual tendencies for the performance.

"The only thing that is unique about the conditioning factors in social or mental dependence is the fact that the presence of other persons . . . or the assumption of them . . . touches off certain dispositions or neural tendencies, giving to our 'social experiences' a certain kind of significance. The sight of the blighted fruit trees and the sight and sound of my persuasive neighbor are psychological

[1] Illustrations of this are to be found in Ellwood, *op. cit.*, pp. 283 ff., 288; Ward, 'Pure Sociology,' pp. 256 ff., 457 ff.; Ross, 'Social Psychology,' p. 13.

events of the same order. There is not, in the one instance, the mere apprehension of an object; in the other, the operation of a subtle and mysterious force through the agency of which my mind is wrought upon by my neighbor's. Because of my constitution and my history the two things are differently apprehended, have different significance, and lead to unlike performances."[1]

Classifications of the social forces have been attempted by sociologists from Hobbes, Fourier and Spencer to many present-day writers, and the majority unite in placing emphasis upon the psychic factors or forces, though the later writers reject the old hedonistic criteria and adopt the functional view. However, with only one important exception,[2] all classifications have been subjective in that the content is lodged in the individual consciousness as the source of activities. What is needed is a classification of social forces which does not stop with consciousness, real or imaginary, and, in so doing, cover up the real and objective sources of stimulation to activity.

The most complete, accurate, and objective classifications of the social forces must constantly aim to point out, according to Bernard, (1) how the individual acts or behaves, the organs he uses and how he uses them when stimulated in known or unknown ways, and (2) how a group acts or behaves, the types of control which are exercised over individual activities or behaviors, in known or unknown ways. In the individual these may be instinctive or acquired (habitual) tendencies; in the group they may have grown up unconsciously through custom, or they may have been consciously legislated into existence, or taken on through the pressure of public opinion, or as a result of scientific investigation.[3] The 'causes' of activity under (1) have been called 'social

[1] Bentley, *op. cit.*, pp. 11–12.

[2] Recently a very valuable and suggestive inventory or classification of the responses of the original tendencies in man to various classes or types of stimuli has been made by Professor E. L. Thorndike in 'The Original Nature of Man,' 1914. He regards this, however, as only a beginning—as indicating the direction which further inquiry must take and the subject-matter with which truly objective classifications will have to deal.

[3] See his discussion, *op. cit.*, p. 73 ff.

forces' and the subjectivists have traced them back to the individual consciousness and lodged them there, 'because the individual is usually conscious of his socially most conspicuous acts, and when he is not thus conscious, consciousness is . . . assumed.'[1] Thus the early sociologists stopped at the sacred threshold of consciousness and ended their search for social forces in the *forms* of consciousness. Only recently has psychology pointed out that consciousness is not ultimate, but is caused, and is only *one* factor in adjustment. The social behaviors under (2) have also, by analogy, and with the same subjective emphasis, been called 'social forces,' being abstracted for this purpose from the unified social situation and made the product of individual activity. The distinction was not made, between 'interests' and 'desires,' nor was it recognized that the latter type of behaviors is to be termed 'social forces' with more reason than the former, since they necessarily go behind the individual consciousness to some extent.

The passage from an introspective to an experimental and biological psychology, with its analysis of the conditions of consciousness and its functional activities in making adjustments to the environment, has caused us—forced us—to look back of the mere forms of consciousness in studying social causation. Our search for 'social forces' must undertake to account objectively for the activities of the individual and of the social group. In actual practice we have adopted this method. Thus, for example, we have 'ceased treating disease on a demonistic basis, or attempting to cure national ills by public prayer.' We still, however, practice retaliatory methods in our criminological procedures, and limit morality in general to the scope of consciousness or intention.[2]

The old classificationists present the subjective social forces as only forms of consciousness by which the conscious person is made aware of his own activities, while their formulations of objective social forces are only abstractions for presenting to ourselves the social processes. Says Bernard,

[1] *Ibid.*, p. 74.

[2] For a critical discussion of this point see Bernard, *op. cit.*, Introduction.

in speaking of these abstractions: "They are not forces; at the most they are partial indices of social 'forces' or processes. Nor have they constant equivalents; for conscious processes and our statements of social processes have at different times different activity equivalents. . . . They are qualitative rather than quantitative indices. They merely invite to always further analysis and re-analysis of the objective social situation; and it is on the bases of these analyses that all our problems are to be comprehended and effectively solved. When a situation is once adequately analyzed, when the forces lying back of the forms of consciousness or the abstracted and generalized types of social and individual activity are understood, the method of the solution of the problem is simply that of the application of common sense. The only mystery that there is about the treatment of social problems is that which we make by being content to stop with the forms of consciousness in our analysis. We talk about the riddle of personality as an impregnable barrier to an adequate understanding of social conditions, because we are attempting to work out a logic of forces and activities from the kaleidoscopic presentations of our conscious processes."[1]

The problem ahead of the sociologists, then, according to Bernard, "is to push farther back the analysis of objective phenomena." Until sociology abandons its subjective criteria it cannot attain to true scientific efficiency. "As psychology retreats from its introspective analysis of the solipsistic self, and as ethics gives up mere intention as the criterion of morality, so sociology must turn from a subjective classification of 'social forces' and study the functioning of objective social processes as they operate in individuals and groups."[2]

Summary

Sociology, ethics, and the other social sciences are in need of more tangible explanations of individual and social action than are commonly given, and they have a right to look to psychology for a true statement of the facts so that

[1] *Op. cit.*, pp. 75–6.

[2] *Ibid.*, p. 76.

all can work on the same plane. The terminology now in common use implies or postulates the possibility of stimulation by such subjective factors (frequently referred to as 'forces') as 'feelings' and 'ideas,' though the way in which they become stimuli has never been satisfactorily explained. This conception seems to have failed in the explanation of behavior as much as the 'faculties' now supposed to be discarded from psychology.

All tangible stimuli must be of such a nature as to set up nerve impulses. They may come from within or from without the individual. He may be stimulated by external things—by other persons, by spoken or written words of his language, by books, etc.,—or by organic processes and muscular changes within his own body. Organized stimulus-response systems as developed by habits are important in determining the direction and the extent of his responses. It is inconceivable, therefore, how conscious states can serve as stimuli; for all conscious states are but imperfect, subjective representations of stimuli in the objective world. The customary usage thus involves one in a vicious circle.

In group relations, therefore, explanations must hark back to stimulus, disposition, and response instead of halting upon the plane of 'mental interaction,' 'suggestion,' 'social consciousness' and the like. The uniform action of individuals in society is due to common sources of stimulation, to common associative tendencies, and to common habits, overlapping in the generations. Divergent reactions are due to differences in organic set or total nervous organization, which are the products of more or less unlike heredity and experiences. Finally, the 'intellect' can not be set up as a separate entity operating as a force or cause in inducing action or in mediating activity. As an aspect of the sum total of nervous correlations in the higher brain centers, the intellect can not be considered as a causative agent, nor can an idea, which is always the outcome, though remotely so, of sensory stimulation, be used as the cause of individual or group activity.

THE MENTAL WORK CURVE

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Practically all studies in the field of mental work and fatigue have employed, in one form or another, one of two general methods of investigation. According to one method the subject is required to perform certain tests at the beginning and again at the close of a period of work, or at intervals during its progress. The differences between the successive applications of the tests are regarded as measures of the effect of the intervening work. According to the second method instead of making comparisons of the results of short tests at the beginning and at the end of the period of work, a study is made of the progress of the work itself, and the factors which affect its progress.

Instead of regarding these two methods as different ways of dealing with the same general problem, they should rather be regarded as two distinct problems, since they differ not only in procedure, but also in the result sought. The former tries to secure results that shall be free from the influence of such factors as practice, "warming up," and variation in attention and interest and to obtain an index of fatigue only as it is developed in the course of the work. The latter, instead of trying to eliminate the influence of these factors, tries to take particular account of them and to assign to each its specific value in the work curve.

It is very essential in studies of the latter type that the task done be of uniform difficulty throughout the entire period of work. Otherwise we shall not know whether the variations in the curve are due to conditions in the work or in the worker. It is likewise very desirable to employ a form of activity which involves a minimum of physical effort and sensory strain, so that the mental factors may be isolated as fully as possible. The kind of work which has proved most satisfactory, and which has been most extensively used, is some form of mental computation.

The Experiments.—In a recent study of mental fatigue[1] results were secured from forty-five hours of mental calculation done under uniform experimental conditions and carefully checked both as to the number of errors made and the amount of work done in each successive half-minute. Twenty-three persons took part in the experiment. The work was done in forty-one periods varying in length from thirty minutes to two hours and a half. The type of work done was that devised by one of the writers and described elsewhere.[2] This method was essentially as follows:

The subject was seated comfortably in a quiet room. The experimenter began by giving him a number consisting of two digits. The subject added mentally, six to this number, then seven to the sum thus secured, then eight to this last sum, and then nine to this result, and then again six, seven, eight, and nine in rotation, adding in every case to the sum resulting from the previous addition. The subject spoke his answers aloud. At the end of thirty seconds the experimenter announced a new number and the subject immediately dropped the series on which he was working and began with this new number by adding six, seven, eight, and nine in rotation as before, for another thirty seconds when another starting number was given, and so on without interruption for the entire period of work. The advantages of this type of addition are that it reduces all physical and sensory elements to a minimum, that it taxes mental effort to its full extent, as was witnessed by every person who took part in the experiment, that it is almost perfectly continuous, giving no opportunity for relaxation, and that it is composed of small and fairly uniform units of work in terms of which the results can be measured accurately. Even adding columns on paper involves more muscular and sensory activity than the present type of addition involves. The subjects were usually allowed

[1] Ash, I. E., 'Fatigue and Its Effects upon Control,' *Archives of Psychology*. Vol. V., No. 31. The results here discussed were not presented in that monograph.

[2] Starch, D., 'Experiments in Educational Psychology,' pp. 172–181.

to make two or three practice series of the additions before the regular experiment was begun.

When the sum of successive additions had reached one hundred or more the hundreds digit was dropped and the additions continued with the units and tens digits. Thus, if the number first given was 72, then the successive sums should be 78, 85, 93, 102, 108, 115, etc., but instead of giving these last sums as 102, 108, 115, they were given simply as 2, 8, 15, etc.

To enable the experimenter to keep the results accurately mimeographed sheets of paper were prepared in advance which contained all the numbers used as starters[1] and, in columns directly under them, the correct sums of twenty additions. Whenever the subject gave an incorrect sum, the experimenter wrote it down beside the correct sum. A line was drawn under the sum resulting from the last addition in each thirty-second period, so that an exact record was kept of the amount and accuracy of the work done. During the experiments, the subjects either closed their eyes or sat in such a position as to be unable to see the experimenter in order to avoid distractions and to concentrate to the fullest extent upon the task of the experiment. Neither subject nor experimenter spoke a word during the experiment except the "starting numbers" which were given by the latter, and the results of the successive additions by the former. No time was allowed to elapse between series.

Results.—The data afforded by these experiments may be treated from several different angles, and will throw light on a number of important phases of the problem of mental work and fatigue. The first questions to suggest themselves very naturally related to the general form of the curve for work periods of varying lengths. How soon after beginning work does one reach his maximum efficiency, and how long will he be able to maintain it? How closely will the curve of work for one day represent that of another? How widely

[1] The numbers used as starters comprised all the numbers between 10 and 100 except those whose right-hand digit is 0 or which will produce sums whose right-hand digit is 0. All these were omitted since the additions in such cases are decidedly less difficult.

do different persons vary with respect to the form of their work curves? How greatly and in what general respects do the work curves of those who work slowly differ from the curves of others who work rapidly?

The results are shown graphically in Figs. 1 to 4. The points in the curves represent the average number of additions made in each five successive thirty-second series. The curves for the errors are shown in the lower part of each figure. Fig. 1 shows the composite curves for three two-hour records obtained from two subjects. Two records were obtained from one subject. Fig. 2 gives the results for nine one-and-a-half hour records obtained from eight subjects. Fig. 3 represents twenty-one one-hour records obtained from fourteen subjects. Fig. 4 represents eight half-hour records obtained from four subjects.

These figures show that the number of additions per unit of time gradually increases during the first twenty-five to thirty-five minutes, when the maximum speed is attained. This maximum speed is maintained for another thirty or thirty-five minutes, at which time the number of additions begins gradually to decrease and continues to do so until the work ceases. The curve of errors takes, on the whole, the opposite direction to that of the work curve. That is, the number of errors is the largest when the number of additions is the smallest. There is greater "zigzagging" or irregularity in the work curves when the rate of addition is highest. The initial period of increase in the rate of additions is not found in Fig. 1. That is, the rate at the beginning of the experiment is as high as at any time during its progress. This is probably due to the fact that the subjects were quite familiar with the work of the experiment since they had made shorter records before.

While the curves in Figs. 1 to 4 show clearly that there are fewer additions made in those periods in which the number of errors is greatest, they do not show the full difference in the number of additions made in those series in which errors occurred and those in which no errors occurred. In order to determine just how great this difference was, the

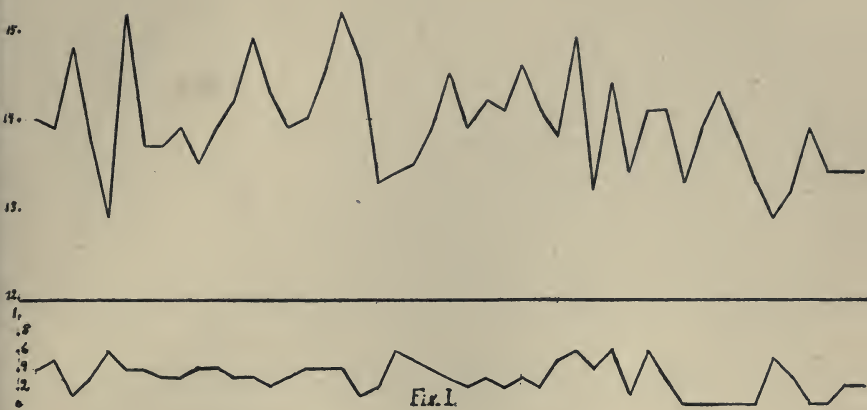

Fig. I.

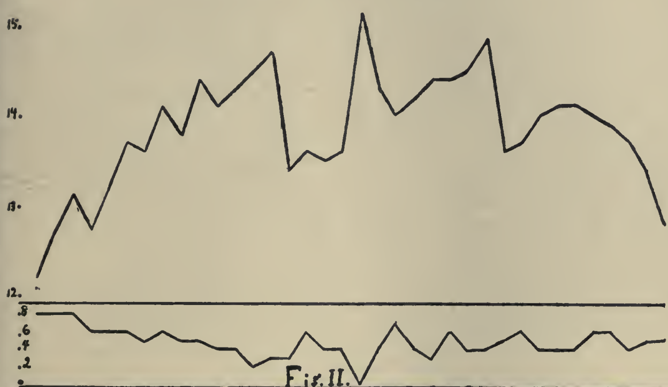

Fig. II.

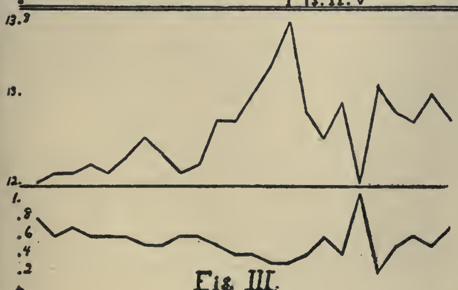

Fig. III.

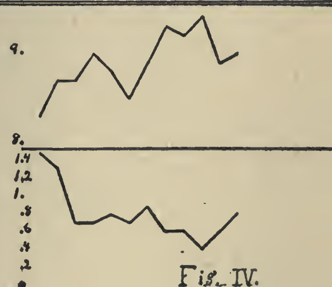

Fig. IV.

number of additions in those series which contained errors, and in those which contained no errors was counted.

Before doing this, however, the data of all the experiments were divided into two groups. Group I. included those experiments in which the subjects averaged more than twelve additions in each half-minute period or series. Group II. included all those records in which the average was less than twelve. The experiments of Group I. covered 25 hours of work and therefore included 3,000 half-minute series of additions. In 2,333 of these series, no errors were made; while in the other 667 series 920 errors were made. The average number of additions per half-minute period in the series in which no errors were made was 17.2. The average for the 667 series in which errors were made was 11.8. In other words, an average of 46.2 per cent. more additions were made in those series in which no errors occurred than in those in which errors did occur.

The experiments of Group II. covered 20 hours of work and included 2,400 half-minute series of additions. In 1,441 of these series, no errors occurred. In the remaining 959 series, 1,784 errors occurred. In this group, the average number of additions per thirty-second period for the series which contained no errors was 9.71 and for those in which errors did occur the average was 7.8. In this group there were, on an average, only 23.8 per cent. more additions made in those series in which no errors occurred than in those in which errors occurred, as against 46.2 per cent. in Group I. These results indicate that those who worked rapidly worked more accurately than those who worked slowly. The more significant fact, however, brought out in these results is that it requires a longer time for any one, whether he work rapidly or slowly, to make an incorrect addition than a correct one. This fact, overlooked by many investigators, obscures the signs of fatigue, which have been sought by the method of continuous work. Fatigue is unquestionably developed in all such experiments but it is not shown in the actual output of work.

These figures, however, do not express the real, or full differences between the number of correct additions made in any unit of time and the number of incorrect additions for the same time; or the effects which an error, or those factors which cause one to make an error, have upon one's speed in mental work. In order to show the full significance of the errors, or of the factors which caused them, in slowing up the work, a computation was made of the number of additions in those half-minute series which contained errors, before any errors were made, and also of the number including and following the first error in each of the different series. In all of the 667 series of Group I. which contained errors, the total number of additions before the errors occurred was 5,714. The total number of those including and following the errors for all the series was 2,177. We may reasonably assume that the rate for those additions which were made before any errors occurred would be the same as for those series which contained no errors. If this assumption be correct, then the amount of time required to make these 5,714 additions which were made before the errors took place would be about equal to 331 thirty-second periods, leaving 336 periods to make the 2,177 additions which included and came subsequent to the errors. This would make an average for these additions of 6.5 per thirty-second period, or slightly more than one third the rate when no errors occurred. In the 959 series of Group II. which contained errors, 3,846 additions were made before any errors occurred, while the number including and following the errors was 3,665. Treating these numbers as we did those in the preceding group we find the average rate for the incorrect additions and those subsequent to them in the series in which they occur to be 6.5 per thirty-second period, or about two thirds the rate for those series in which no errors occurred.

That the assumption is substantially correct is indicated by a special record made on one subject in which the exact time was noted when the first error in each series occurred. In this record the time per addition after the error occurred was 3.78 seconds, or 31 per cent. longer.

Still another factor in the distribution of errors and the effect of this distribution on the rate of additions is the relative number of series containing errors at different stages in the experiment and the number of errors in each of these different series. This distribution is shown in the following tables. (Table I. giving the results of Group I.; Table II. those of Group II.) The top horizontal row gives the suc-

TABLE I

GROUP I

1...	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
2...	20	20	20	20	20	20	7	7	7	2	2	2	1	1	1
3...	400	400	400	400	400	40	140	140	140	40	40	40	20	20	20
4...	156	129	116	100	84	60	8	6	5	0	1	1	0	1	0
5...	159	129	127	129	124	115	40	36	39	0	6	6	0	7	0
6...	1.0+	1.0	1.1−	1.3	1.5−	1.9+	5.0	6.0	7.8	0.0	6.0	6.0	0.0	7.0	0.0
7...	39	32	29	25	21	15	5.7	4.3	3.6	0.0	2.5	2.5	0.0	5	0.0

TABLE II

GROUP II

1........	1	2	3	4	5	6	7	8	9	10	11	12
2........	21	21	21	13	13	13	5	5	5	1	1	1
3........	420	420	420	260	260	260	100	100	100	20	20	20
4........	202	176	170	106	105	110	34	33	17	3	1	2
5........	342	324	347	165	168	194	61	73	60	11	8	11
6........	1.7−	1.8+	2.0+	1.6−	1.6−	1.8−	1.8−	2.2+	3.5+	3.7−	8	5.5
7........	48	42	40+	41−	40+	42−	34	33	17	15	5	10

cessive ten-minute work periods. The second row gives the number of records concerned in the experiment; the third row gives the total number of half-minute addition series made in the successive ten-minute periods; the fourth row gives the number of half-minute series in which error occurred; the fifth row gives the total number of errors; the sixth row gives the average number of errors in those series in which errors occurred; and the seventh row gives the percentage of series containing errors out of the total number of series made.

The reason why the numbers in the second row decrease from left to right is that not all records were of equal length. Twenty records in Group I. extended through the first six ten-minute periods, seven through the next three, etc.

It will be seen from the foregoing tables that not only does the number of errors decrease as the work proceeds, row 5, but more particularly does the number of the series which contain errors decrease, rows 4 and 7, while the number of errors in those series increases, row 6.

These facts very naturally raise the question: What causes these errors in the additions, and why should they become fewer as the work proceeds? It is clearly evident that these errors did not occur because the subjects did not know the sums of certain numbers to be added. The errors undoubtedly resulted from a kind of interruption. Some other thought, coming into the focus of consciousness, momentarily crowded out the number to be added, or the previous sum to which this number was to be added.

Now if we take mental fatigue to mean (as we have shown in the article previously referred to)[1] a loss of control over the direction which any particular nervous excitation within the brain shall take, and a growing inability to inhibit or repress irrelevant or obtruding ideas and suggestions, and analyze the work from that point of view, we may see how fatigue can be developing all the time during the work and yet the gross results show little or no effects of it. At the beginning of the work all one's faculties are alert and ready to respond to the slightest suggestions, or the intrusion of any idea or impression. But as the work proceeds the faculties become, as it were, insulated to extraneous suggestions and intruding impressions. While from two fifths to one half the series of additions at the beginning of the work contained errors, at the end of an hour and a half or two hours only about one twentieth to one sixth contained errors. At the beginning of the work we are required not only to make the additions but also to combat the host of intruding ideas which are striving for a place in the focus of consciousness. Every one who engages extensively in mental work, especially if it be varied in character, knows that a certain amount of time is required to "get settled" to any particular kind of work. We say we can not concentrate at

[1] 'Archives of Psychology,' No. 31.

the beginning of work, which is only another way of saying that we cannot successfully combat irrelevant ideas which are seeking to intrude themselves into consciousness.

To attempt to measure mental fatigue by such experiments as are usually employed in those studies which employ the methods of continuous work is like having the subject do a number of things at the same time and then measure his efforts by what he accomplishes in one of them. Such a method would not be very far wrong if the relative difficulty of the different activities remained constant throughout the work period. But if the unmeasured activities can be shown to grow constantly less difficult as the work progresses, then it is evident that more could be accomplished of that which is being measured without, on the whole, the expenditure of greater effort or more energy, or the same could be accomplished with the same expenditure of effort.

It has been shown in muscular work and fatigue that, as the muscles become fatigued, there is developed a resistance to motor impulses in the nerve tracts leading to the fatigued muscles. This resistance protects the muscles from complete exhaustion which would occur if every motor impulse reached the muscles without having any of its force or strength neutralized by this nervous resistance which arises as fatigue of the muscles develops. Just so in mental fatigue. As the mind becomes fatigued by mental work its "receptive" faculties become less responsive. Fewer impressions and suggestions enter the mind, and as a result fewer demands are made upon it as fatigue develops, and more of our mental energies can be devoted to dealing with those ideas and impressions which we are consciously and purposely introducing.

The question may have arisen: Why is it that there are fewer errors at the close of a period of work than at its beginning, if fatigue means the loss of control over the processes of mental associations and mental elaborations? Adding two numbers is simply making an association between two numbers to be added and a third which is their sum. The answer is that this loss of control affects first the most recent and hence the least familiar forms of mental associations. In the matter of adding simple numbers the associations are old and well formed. The association is so completely formed that the stimulus of the former will naturally bring forth the response of the latter or the sum, unless that stimulus be confused by the presence of another idea or impression.

That these irrelevant thoughts and impressions are less easily controlled when the mind is fatigued, if they succeed in entering the focus of consciousness, is shown by the fact that near the end of our experiments, when an error did occur in an addition series, it was usually followed by four or five more. In other words, when the intruding impression got into the focus of consciousness and hence crowded out the numbers to be added, it required a longer time and more effort to suppress the former and recall the latter.

Summary

1. The number of additions per unit of time gradually increases during the first twenty-five to thirty-five minutes of work. This maximum is maintained for approximately thirty to thirty-five minutes. Then the number of additions gradually decreases until work ceases.

2. The curve representing the errors has in general the opposite course. The number of errors decreases as the speed of addition increases, and then increases again as the speed decreases. An incorrect addition occupies considerably more time than a correct addition does.

3. The half-minute periods in which errors occur becomes considerably less numerous during the progress of work even toward the end of long work periods, when the speed of adding again decreases.

4. But when an error does occur it is followed immediately by other errors more and more frequently as the period of work continues.

5. The explanation offered for this fact is that as work continues the mind and the neural processes involved become more and more insulated against distracting stimuli accompanied by a decrease in controlling the direction of mental

energy. The decrease in the occasion of errors would seem to indicate the former and the increase in the number of errors in immediate succession would seem to indicate the latter.

6. Slow workers make relatively more errors than rapid workers make. The difference is considerable.

7. In some respects the most striking fact brought out by these experiments is that mental work, even of a difficult nature, and when continued without interruption for as long as two and a half hours, seems to produce a much smaller lowering in speed or accuracy than is commonly supposed.

INDIVIDUAL DIFFERENCES IN A NORMAL SCHOOL CLASS

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The data which form the basis of this article were obtained in connection with an advanced course in educational psychology given at the Bowling Green, Ohio, State Normal College during the summer session, 1916.

TABLE I

PHYSICAL DATA

No. Pupil	Age	Sex	Weight	Vital Capacity	Vital Index	Height Standing	Height Sitting	Relation
Ave....	....	....	60.8	3,600	59.2	165.6	87.7	52.3
1	28	M	59.0	4,700	80	178	93	52
2	30	F	57.7	3,600	62	163	89	54
3	39	M	75.5	4,800	64	169	93	55
4	32	M	70.5	4,800	65	172	88	51
5	25	F	52.5	3,000	57	153	83	54
6	28	F	45.2	3,000	66	159	82	52
7	31	F	52.7	2,600	50	161	83	52
8	28	F	59.5	3,400	58	161	88	54
9	19	F	69.6	3,800	55	171	89	52
10	29	F	56.7	3,200	56	171	93	54
11	35	F	62.2	3,100	50	156	84	47
12	25	M	63.0	3,900	63	170	90	52
13	31	F	66.0	2,900	44	169	87	51

No. Pupil	Head Girth	Head Length	Head Width	Cephalic Index	Grip R. H.	Grip L. H.	Relation	Strength Pull
Ave....	55.6	18.5	14.8	80.2	89	81.8	81.6	60.8
1	54.0	18.5	14.0	76	100	100	100	70
2	57.0	19.0	15.0	79	70	66	94	55
3	56.0	18.5	15.0	82	148	133	90	110
4	56.5	18.5	15.0	81	120	100	83	62
5	55.0	18.0	15.0	83	67	58	87	58
6	54.5	18.5	14.5	78	70	65	93	40
7	56.5	19.0	15.0	80	80	65	75	53
8	55.5	19.0	14.0	73	100	80	80	58
9	58.5	19.5	15.5	79	85	88	103	60
10	55.0	18.0	15.0	83	70	65	92	50
11	55.5	18.5	14.5	78	72	75	103	58
12	54.5	17.5	16.0	91	100	100	100	70
13	54.5	17.5	14.0	80	75	68	91	48

The great variation among the members of this class with respect to age, experience, physical inheritance, etc., offered an exceptional opportunity to illustrate some of the more essential facts of individual differences by the measurement of the class itself.

Then, too, it was desired to demonstrate to the class the value of the experimental method of teaching a subject like psychology.

Table II
Psychological Data

No. Pupil	Log. Mem.	Rote Mem.	Digit-Symbol	Symbol-Digit	Free Asso.	Oppo-sites	Add. O. I	Add. O. II	Add. O. III	Genus-Species
Ave.	38.3	93.0	29.2	29.9	24.2	26.0	27.7	27.7	14	15.8
1	25	89	28	27	23	24	26	26	12	16
2	40	91	31	42	24	28	28	20	16	14
3	44	85	21	23	23	26	20	28	22	22
4	25	82	24	32	24	22	20	26	14	14
5	28	91	27	24	24	26	28	36	4	10
6	43	93	38	33	27	30	30	32	18	22
7	44	112	31	30	30	34	34	32	20	22
8	30	83	32	35	17	30	34	30	8	4
9	43	101	27	28	20	28	30	28	18	20
10	49	102	31	32	28	18	24	28	10	18
11	39	108	30	32	30	26	30	26	10	10
12	30	62	24	24	20	18	22	20	12	14
13	58	109	36	27	25	28	34	28	18	20
Av. men . .	38.3	86.6	29.3	33	42.2	22.1	. . . . . .	. . . . . .	. . . . . .	15.1
Av. wom. .	40.1	87.4	32.2	31.3	38.3	22.4	. . . . . .	. . . . . .	. . . . . .	15.5

(These averages taken from data of Professor Pyle.)

No. Pupil	Add. G.-S. I	Part-Whole	Add. P.-W. I	Ink Blot	Cancel-lation	Canc. Accur.	Word-Build.	Add. W.-B. I	Puzzle Box
Ave. . . .	21.8	19.5	26.3	11.7	21.4	93	17.2	15.2	5–07
1	24	16	22	10	26	99	14	15	5–00
2	24	20	28	11	32	92	19	10	3–19
3	16	14	32	12	20	100	19	21	2–00
4	26	26	24	16	13	94	13	13	5–06
5	20	26	26	8	15	92	18	16	2–45
6	28	24	32	11	29	98	17	19	14–54
7	26	30	30	17	29	98	22	18	6–35
8	14	14	22	4	19	87	17	10	14–00
9	26	24	28	14	23	98	10	12	3–06
10	22	18	26	19	23	86	20	15	1–07
11	16	14	28	10	23	92	18	16	1–25
12	14	14	18	5	7	74	15	15	2–10
13	28	24	26	15	19	98	21	18	5–00
Av. men	. . . .	18.5	. . . .	10.6	22.2	. . . .	18.6	22.7	. . . .
Av.wom.	. . . .	19.7	. . . .	9.8	23.0	. . . .	21.1	22.0	. . . .

(These averages taken from data of Professor Pyle.)

Table I. exhibits the physical data for the 13 members of the class through the traits named, Table II. gives the psychological data, while Table III. gives the data for an experiment in the learning process and the transfer of training with the same class.

Table III

Showing Data for Initial and Final Tests in Addition and Division, with the Averages for the Class. The Amount of Gain is Also Shown

No. Pupil	Speed				Accuracy			
	Addition		Division		Addition		Division	
	Init. Test	Final Test	Init. Test	Final Test	Init. Test	Final Test	Init. Test	Final Test
1	53	71	108	137	81	91	96	98
2	30	47	91	89	89	100	99	95
3	32	41	65	91	84	87	96	97
4	37	48	97	126	82	90	98	99
5	25	24	44	78	83	72	100	100
6	42	61	82	88	92	95	97	98
7	45	62	122	127	98	100	100	100
8	20	27	32	49	76	93	99	100
9	39	43	114	115	88	93	95	95
10	38	44	77	92	90	88	95	99
11	31	41	51	66	94	95	96	97
12	21	26	45	56	43	54	75	93
13	70	88	90	115	100	96	100	100
Av.	37.2	48.0	78.3	94.5	84.6	88.8	95.8	98.0
Gross gain	....	10.8	....	16.2	....	4.2	....	2.2
Per cent	....	29.0	....	22.0	....	....	....	....

Inasmuch as all the measurements and tests are described in detail in various publications, it is not deemed necessary to offer here more than a brief explanatory sentence concerning each.

Age—Recorded to the nearest birthday.

Sex—'F' represents female and "M" represents male.

Weight—Taken in the metric system and recorded to the nearest tenth of a kilogram.

Vital Capacity—Taken with a wet spirometer and recorded to the nearest hundred cubic centimeters.

Vital Index—Computed as the ratio between the vital capacity and the weight.

Height Standing—Taken in the metric system and recorded to the nearest centimeter.

Height Sitting—Taken and recorded the same as height standing.

Relation—The ratio of the sitting height to the standing height, recorded in terms of per cent.

Head Girth
Head Length —All taken in the metric system and recorded to the nearest
Head Width one half centimeter.

Cephalic Index—The ratio of the width of the head to the length, expressed in terms of per cent.

Grip, Right Hand —Taken in the English system and recorded to the
Grip, Left Hand nearest pound.

Relation—The ratio of the grip of the left hand to that of the right, expressed in terms of per cent.

Strength, Pull—The number of pounds the subject can pull with both hands, the dynamometer being held near the chest, but not touching the body.

Logical Memory—The material used was "The Marble Statue."

Rote Memory—The material used was that given in Professor Pyle's manual. The score is the average for the concrete and the abstract lists of words taken together.

Digit-Symbol, Symbol-Digit, Free Association, Opposites, with the additional tests I, II and III, Genus-Species, with additional test I, Part-Whole, with additional test I, are all described in Pyle's manual.

These tests were all given according to the instructions given in the manual, the score recorded being the average for one minute.

Ink Blot—The material used was the set of 20 ink blots by Whipple. The score recorded being the number of suggestions written down in two minutes.

Cancellation—The material used was the standard test beginning with the letters h, p, l, g. The score recorded being the number of a's cancelled in one minute, with the per cent. of accuracy recorded in the next column.

Word-Building—The material used was composed of the letters a, e, o, b, m, t, with the letters e, a, i, r, l, p, for the additional test. The score recorded is the number of words made in 2½ minutes.

Puzzle Box—The Healy puzzle box was used in this test. None of the members of the class had ever seen the box. Each one was allowed two minutes to examine the box and was then given the button hook and directed to open the box as quickly as possible. The time recorded is the number of minutes and seconds required to perform the task.

In Table I. the averages at the top are for the entire class, men and women taken together.

In Table II., in addition to our own averages, the averages found by Professor Pyle for adults are also included at the bottom, insofar as these are comparable.

The slight discrepancy between the adult averages given by Pyle and those obtained from our class in the case of free association was noted by the class at the time and this test was re-checked, but with no better results. It will be noted that the class showed no better ability upon the whole in writing words from free association than was shown in writing easy opposites. This fact may be explained upon the ground that in either case the test was not a test of ability to associate ideas so much as it was a test of ability to write down words, *i. e.*, a test in motor speed.

In the experiment in the learning process the materials used were the Thorndike addition sheets (single column addition examples of ten digits each, no zeros or ones included) and the division sheets of Thorndike, which were used by Kirby in his experiment with pupils in the Children's Aid Schools of New York.

These examples consist of columns of division problems arranged as follows: "48 equals . . . 5s and . . . remainder," the task being to fill in the blanks with the proper numbers, in this case the numbers being 9 and 3.

In the initial test 10 minutes were allowed for addition and 5 minutes for division, the score being the number of examples done correctly in the time given, which is called the speed. The score for accuracy represents the per cent. of the total number of examples done that was done correctly.

The final tests were given in the same way as the initial tests, but after an intervening daily practice of 5 minutes at addition for eight successive days, Sundays being omitted. There was no practice in the case of division.

Results of the Experiment

The class, composed of 4 men and 9 women, showing an average age of 29.2 years to the nearest birthday, showed an average initial ability of 37.2 columns added correctly in 10 minutes, with an average accuracy of 84.6 per cent.

As a result of 40 minutes of practice distributed over 8 days the class showed a gross gain in speed of 10.8 columns added correctly in 10 minutes, or a percentile gain of 29.0 per cent., with a gain of 4.2 per cent. in accuracy.

The average ability of the class in division in the initial test was 78.3 examples done correctly in 5 minutes, with an average accuracy of 95.8 per cent.

The final test, with no intervening practice in division, but with 40 minutes of practice in addition, showed a gross gain in speed of 16.2 examples done correctly in 5 minutes, or a percentile gain of 22.0 per cent., with a gain of 2.2 per cent. in accuracy.

Summary and Conclusions

1. Individual differences due to heredity are best illustrated in a study like this by such traits as height, weight and other traits not here recorded, as for example, color of hair, color of eyes, shape of features, complexion of skin, etc.

2. Individual differences due to sex are best shown by such traits as vital capacity, strength of grip, strength of pull, together with certain tests of motor capacity such as tapping, not here recorded.

3. Individual differences between persons of the same sex are often greater than the difference between groups of opposite sex. This appears in the case of imagination as tested with the ink blots, hard opposites, and the genus-species tests.

4. Individual differences due to school training are well illustrated by the addition and division tests, as it was evident that certain members of the class had mastered these combinations, while others evidently had not done so. In a general way it may be said that equal amounts of school training will produce somewhat the same results in a number of individuals, as for example, three years of school training will enable the majority of a class to read with the same general ability from, say the third reader, yet if the same class were tested with psychological tests it might be found that individual differences had been augmented by the three years of school training and experience.

5. Individual differences due to other causes are not so well shown in this particular class, since they were all of one race, one occupation, one nationality, and none was possessed with any defect of body or mind due to accident.

6. One of the most striking and perhaps the most significant examples of individual differences, from the standpoint of the teacher, was brought out in the puzzle box test.

The puzzle box designed and used by Dr. Healy in connection with his work at the Psychopathic Institute in Chicago, was the one used in this experiment. The origin, improvement, construction and use of this puzzle box are set forth in detail by the above author in an article appearing in Vol. XIII., No. 2 of the Psychological Monographs.

The puzzle box is about six inches square by five inches deep. The lid is glazed and is hinged and fastened with a hasp, which in turn is held in place with a bolt hook and this is made fast with a ring attached to a string, and so on with a series of strings and rings fastened to metal pins within the box. By proper manipulation the fastenings may all be removed by means of a button hook and thus the box may be opened without the necessity of forcing at any point.

The following paragraph from the monograph of Dr. Healy will give a fair understanding of the operation of opening the box: "The color of the strings is, of course, arbitrary and is made different in order to facilitate the tracing the sequence of events necessary in opening the box. One removes first the ring over the post *K* and pulls out the staple from its holes in the back of the box, releasing the attached ring. Next the ring over the post *G* is lifted off, which loosens the short orange colored string so that the ring on the arm of post *D* can be readily removed. This then so loosens the blue string that the final ring can be pushed over the curved arm of the bolt hook and the latter may be withdrawn, the hasp lifted and the box opened."

With regard to the purpose and the significance of this test, Dr. Healy says: "The purpose of the test is obvious. It may bring out abilities or defects in the manipulative powers, in the ability to analyze a slightly complicated situation, in powers of attention and continuity of effort. . . . It is obvious that the general results obtained from this test must vary greatly, but there seem to be three main types of approach to the problem: first, random trials; second, intelligent profiting by the experiences of trials and successes or failures; third, conscious analysis of the puzzle as a whole with recognition of the relation of the parts. Of course, on account of the differences in strength and manipulative power there would, other things being equal, be considerable difference in the times taken by the subjects. Indeed, altogether it has seemed to us that the method employed by the subject is of more significance than the time. Most of our twelve-year-old subjects have opened the box in from one and a half to nine minutes, but a certain number have finally failed."

While the above test was designed especially for use in the classification of juvenile delinquents, yet it seems evident that it tests the same abilities in any one who might undertake to open it. The individual differences brought out in our class of thirteen adults were vastly more significant than might be inferred from the examination of the data presented in the table accompanying this article, since we have here only recorded the time that was required for each person to open the box.

Observation of the efforts made by the different members of the class in trying to open the box showed that no one of the thirteen worked out the solution of the problem by the use of reason before attempting to open it. Most of them began by random trials. Two or three of them made definite moves which, as they thought, were correct, although most of these efforts were useless.

One significant fact was that most of the class failed to profit by mistakes made until the same mistake had been made over a number of times, in one case as high as seven times. The one step in the solution of the puzzle that caused most trouble was the removing of the staple at the back of the box after the first ring had been removed. In their own words, they "did not think that the staple would come out."

The experiment was carried a little further and each person was asked to retrace all the steps and thus lock up the box, after having succeeded in opening it. In almost every instance an effort was made to replace some of the rings or other fastenings *without first closing the lid!* Now no one but a child would fall into the error of locking a trunk, for example, before closing the lid, and when this fact was called to their attention, then each one recoiled with chagrin that he had not been able to think of that himself.

One member even replaced the bolt hook and all the rings and fastenings complete and failed to notice that the hasp had not been placed over the staple! Subsequent experiments upon a number of adults at random have brought similar results in almost every instance.

7. Insofar as comparisons will admit between our data and that reported by Professor Pyle, there appears to be a very close correspondence, with the exception of the free association test.

An irregularity, however, in our data which could hardly be due to the small number of subjects, is that our subjects make a better showing in writing the additional tests in restricted association and invention than in the first tests given. The additional tests are supposed to be more difficult, hence one would naturally expect the record to be lower. Our explanation of this reversal of expectation is that the influence of practice in the first tests more than offsets the difference in the difficulty of the tests.

8. The outcome of this experiment tends to confirm the writer's contention that the experimental method of teaching psychology is the best method, even for short term courses. Of course it is understood that the regular textbook and reference reading, together with systematic notebook work were all carried on by the class.

9. In the experiment in the learning process and the transfer of training the improvement shown by the class was about the same as that reported by other investigators and corresponds very closely to unpublished results obtained by the writer from a group of 157 adults.

10. The apparent transfer of ability from the function of adding to that of dividing is partly explained by the fact that the division examples were arranged in a novel way which of course means a lower initial score and a higher score in the final test, due largely no doubt to the influence of the practice in the tests themselves.

On the other hand, a part of the gain in division may be accounted for by reference to improved habits of work brought about by the practice in addition, since none of the members of the class had been practicing with such material just in this way.

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THE PSYCHOLOGICAL REVIEW

ADVANCE ADAPTATION IN BEHAVIOR

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In the study of behavior we find that the animals that have the most highly organized nervous systems are the best adapted to changes in their environment. When we observe behavior that is adapted in advance to changes in the environment, we call it purposive behavior. It is thought that from the behaviorist point of view it would be more consistent to call it 'advance adaptation,' but purpose and purposive are very convenient expressions. We shall therefore use these terms in this discussion. It is to be understood, however, that this does not mean that we recognize consciousness as a factor in the operations.

A study of the broader aspects of purpose in behavior has recently been presented in a series of articles by Prof. Howard C. Warren.[1] The following extracts from them will serve in some measure as a basis for our arguments: "For some biologists purposive activity remains an insuperable obstacle to the complete acceptance of the mechanistic standpoint." "The vitalists . . . assert that a nonmechanistic factor . . . is needed . . . in purposive phenomena." "It remains to be seen whether the mechanistic interpretation is *adequate* to account for the purposive character of behavior."[2]

Let us consider now what part of purposive behavior can be traced to nervous mechanisms. Recent writers on behavior have shown how we may account for such behavior

[1] Howard C. Warren, 'A Study of Purpose,' *J. of Phil., Psychol., &c.*, 1916, 13, Nos. 1, 2 and 3.

[2] *Loc. cit.*, pp. 31 and 32.

as performing a definite series of movements. They have shown that each movement of the series excites kinesthetic impulses which follow certain paths in the nervous system because those paths have been opened up, as it were, by frequent or recent use. In this way these impulses excite the next movement of the series.[1]

In order to provide a good foundation for a discussion of the subject before us, let us try to get a further understanding of such behavior as performing a definite series of movements and of the nerve mechanisms that account for such behavior. As already stated, movements make impulses that excite other movements. Such impulses play a most important part. In many cases impulses from the environment assist in exciting the particular movement. When a man takes his accustomed morning bath, he makes movements in a definite order, each being prompted by the joint action of the environment and the preceding movements through the medium of the nerve paths.

When the movements are repeatedly made in a certain order, the lot may be termed a memorized series. The nerve paths or fibers that determine the movements may be distinguished as association fibers. They are developed by practice. Movement *A* may send kinesthetic impulses through association fibers to the muscles that cause movement *B*, or movement *A* may make a change in the environment and the change may affect some receptor which sends impulses that provoke movement *B*. In either case we may say that a counter signal from movement *A* excites movement *B*.

Each movement of a series is excited by a counter-signal and each produces counter-signals for movements to follow. That is not, however, the whole story. Let us suppose that a soldier has been trained to make a series of movements which we will call *A B C D E F G*, each letter representing a movement. Counter-signals from movement *A* will directly cause movement *B*. Moreover, counter-signals from *A* will tend to excite *C* and more faintly *D* and still more faintly *E*,

[1] Watson, John B., 'Behavior,' New York, 1914, p. 275.

and so on. In the same way each movement will partially excite every movement that follows in its train. Now when, for example, a counter-signal from *A* reaches the nerve path leading to the motor ending for *D*, it in some way facilitates the later impulse from *C* to *D*. It opens the common path or lowers the resistance so that movement *D* is thereby insured.

To explain further, we will assume that there is a dual common path that is open to impulses from *A* and *C* only and which leads to the motor ending for movement *D*. This path has been developed further each time an impulse from *C* has followed in the wake of an impulse from *A*. As a result of this path-making, an impulse from *A* leaves the path so wide open for a time that an impulse from *C* can effectively reach the motor ending for movement *D*. This explains why movement *D* follows after *A* and *C*. In like manner each other movement makes counter-signals that facilitate each movement that follows in its train.[1]

To express it another way, each movement makes a certain impression on or change in the nervous system which takes part in determining what movements shall follow. This impression must last a considerable time, but gradually diminishes. Let us now suppose that these impressions on the nerve fibers remain after the whole performance is completed and last until the performance is repeated. We see that new impressions will be imposed on old ones.

In passing on these theories of nervous operations it is well to remember that the simplest movement is caused by the contraction of one or more muscles and that each muscle is a group or community of muscle fibers. Leading to these muscle fibers is a nerve made up of nerve fibers. Many of these fibers are motor fibers. A single motor fiber may branch to a number of muscle fibers. We know, of course, that a muscle contracts with varying strength when stimulated by impulses coming through the nerve. On the other hand, physiologists have made experiments in recent years

[1] A discussion of this theory is given by the writer in an article on 'Compound Substitution in Behavior,' Psychol. Rev., 1917, 24, 62–73.

which indicate that in an individual nerve fiber the size of the propagated disturbance does not vary with the strength of the stimulus.[1] In this connection, the great number of nerve fibers that may be concerned in each operation has an important bearing.

Modern behaviorist theories of nervous mechanisms rest on the principle that recency and frequency of previous excitations determine the extent that the resistance is lowered in a given nerve path, to the passage of an impulse. It may be noted here that a memorized series is learned by one lesson after another. In the first lesson the situation is more or less new or unfamiliar. With each additional lesson, of course, the situation becomes more familiar. In childhood man learns a vast number of movement series, *i. e.*, he acquires numerous habits. In maturity his behavior is largely governed by habits. Language habits and other social habits are important factors in man's daily life. In new situations behavior may be largely instinctive, in familiar situations habit governs. Let us put it down as an established fact that such behavior as performing a memorized series of movements can be satisfactorily explained in terms of nervous mechanisms made up of association nerve fibers.

The object of this article is to show that purposive behavior can just as well be explained in the same terms. At least, we can show that some forms of behavior that would be considered purposive can be so explained. We shall begin our demonstration by pointing out what behavior may be termed purposive. We shall then call attention briefly to the correspondence in the growth of purposive behavior with the growth of habits and language movements, and movements associated with measurement of time. The part played by teaching will be pointed out. The effect of reward upon actions will be discussed. Recognition of purpose will be considered. We shall then take up the development of forethought, so to speak. It will be pointed out that memorized movement series and series of incipient movements may be combined so as to provide for and give rise to purposive behavior.

[1] Adrian, *J. of Physiol.*, 1914, 47, 460–474.

In looking back over a series of behavior phenomena, we see that some of the changes in the environment were provided for in advance just as "Coming events cast their shadows before." For example, we see that Mr. Gray, who went out for a walk at three o'clock, at starting borrowed an umbrella. At four he returned in the rain, sheltering himself with the umbrella. We see that the act of borrowing the umbrella at three was adapted to the need for shelter at four. It is therefore advance adaptation or in common terms purposive behavior. Let us take another case. We see that Dick said to Ralph, "I want to get a canoe and go paddling. Will you come too?" Ralph replied, "Yes, I will go with you," and later on we find them on the water in their canoe. The question and reply, we find, were adapted to later conditions when they arrived at the dock. So this, too, is purposive behavior. Let us take a third case. A boy saw a strange dog and stooped and picked up a small stone. The dog saw the movement and dashed around the corner of the house so that he escaped the flying missile. We see that the dog's action was adapted in advance to the boy's act of throwing. This, too, is then purposive behavior. To compare with this, let us think of a boy that slipped and fell on a briar and hurriedly got up. This is not purposive behavior, as there is no anticipation in it.

We may here observe that the acts of an educated man are usually compounded of purposive and nonpurposive elements. It is in the educated man that we find advance adaptation the most highly developed. The best measure of the degree of advance adaptation, it is thought, will be found in the precision and definiteness of the correspondence of the behavior with the ensuing changes in the environment. Let us call this, for convenience, the degree of purpose. As an illustration, a man who composes the score of a grand opera is giving a good example of behavior having a high degree of purpose. No argument is needed to show that in the human species, the degree of purpose develops side by side with the accumulation of knowledge, *i. e.*, with the formation of memory associations. The same rule seems to hold with the brutes. The fox that has been hunted before is harder to capture because, as we say, he is more wary. On consideration, we find good reason to believe that in race development and in individual development, the growth of purposive behavior has been in proportion to the growth of habits.

Let us now enquire briefly into the effect of language upon purposive behavior. In considering adaptation in animals we must take account of their social environment as well as of their physical environment. The individual must adjust his behavior to meet the actions of others of his species and also those of his natural enemies and it may be those of the creatures he preys upon. If the animal has even the most primitive nervous system, it will be affected by the movements of other animals. Natural selection brings about the evolution of language habits in many species. The sense of hearing must have been developed mainly by the demands of social environment. By means of sound waves there is communication between animals so that the movements of one individual will excite a response in another individual. When a quail is flushed, the sound of his wings provokes the flight of the rest of the covey. The senses of touch, smell and sight are also used for communication. It is probable that sign language is much used in many species. Dogs and cats show by the movement of their tails what their feelings are and no doubt their companions are observant of the movements and are guided accordingly. When a big dog growls at little dogs, he says in dog language, "Keep away, or I will bite you," and when the little dogs retreat, they say with their tails, "Come away or he will hurt us." On consideration, we find that the evolution of language habits must have proceeded side by side with the evolution and differentiation of species and with their social evolution. In the individual we find that language habits develop from birth up. In this connection let us note that nerve paths are lines of communication from sense organs to muscles and from muscles and joints to other muscles. Let us note furthermore that by means of distance receptors the lines of communication are extended into the environment. Language movements serve for communicating with other individuals. Hence the excitation of a sense organ of one individual is conveyed to another one and the latter responds accordingly. This control of one creature by another must have been, from the beginning of its evolution, concerned with advance adaptation.

In the human species we find that the degree of purpose keeps step with the increasing use of language. The naval architect making plans and specifications for a submarine boat, is employing language movements and, at the same time, is behaving with high-degree purpose. In fact high-degree purpose seems almost dependent on language. Let us remember that language movements in man are the result of training, *i. e.*, of the development of habit mechanisms in the nervous system.

For high-degree purpose, a knowledge of the meaning of time is necessary. Until a child has learned the meaning of time, his actions show little definite purpose. He must be taught to note the passage of time. He must learn that it takes a minute to walk to the barn and it takes an hour to walk to the railroad station. He is taught to count the days of the week and the weeks before vacation. Let it be remembered that such knowledge of time is largely due to the development of association or habit mechanisms in the nervous system. By these mechanisms, certain movements come to be associated with the measurement of time.

When the individual has acquired language habits and time measuring habits so to speak, he begins to take part in the joint activities of the family and other social groups. More purposive behavior in the individual is favorable to greater adaptation of the social group to its environment. It follows that natural selection, acting through the group, brings about the evolution of purposive behavior. We can say then that man owes his purposive powers to his social position.

In highly cultured communities, the children are trained up to purposive behavior. This is done by means of language mechanisms and habit mechanisms. A child is made to tell what he is going to do and what good will come of it. He is trained to plant the seed so as to get the flowers or fruits in due season. As a result the individual not only comes to be highly purposeful but he also comes to think of human behavior as being largely purposeful. Hence he is apt to think that he himself or another man is guided by purpose when he is really guided by habit. He is apt to think also that the purpose that guides his movements is created spontaneously within him, quite independently of his organs of sense or movement.

Let us now go back to more primitive behavior. The animal trainer rewards his dog with a tempting bit of food. Every time the dog performs well, he gets the reward. So the dog associates the reward with the movements that make up the trick. Hence we may say that this behavior is purposive in a sense. He responds to the trainer's signal so as to secure the reward. Now it can be shown that association mechanisms will account for such behavior. Experiments by Pawlow's[1] method prove that after proper experience, a dog's mouth can be made to water at the sight of a green light or other stimulus. Such a response is known as a conditioned reflex. It is plainly a case of association mechanism. The sight of food by a hungry dog has a decidedly animating effect. The effect on his nervous system is evidently widespread. By means of association mechanisms, other kinds of stimulus may be substituted. An animal will run to the feed box when it hears the keeper coming. Any stimulus that frequently occurs a short time before the getting of food will soon establish a sort of conditioned reflex, so that the stimulus will have a marked animating effect on the animal. So when the trainer gives the signal for a trick, it has much the same effect on the trained animal as if food were shown it. The animal is alert and responds readily to the accustomed signals according to the established habit. It then receives a bit of food as its accustomed reward.

[1] J. B. Watson, 'Behavior,' Holt, 1914, p. 65.

Now it may be while the brute is performing that he has something in mind like an image of the food that is to come after the trick, but, whether he has or not, we see that the association nerve paths provide for the proper responses. We see then that although the animal's behavior is in some degree purposive, there is no active power that comes from within, as it were. There is nothing beyond the impulses aroused by the environment and by the trainer's signal. The association nerve paths govern the behavior. The trained animal is then only a piece of machinery, although we must admit that it is in advance of the machinery built by man, for as yet no machine has been constructed by art, even in appearance, that will be influenced by a reward to follow its operations. It is probable that the most intelligent behavior of animals is governed by past experience and present environment and not by what is going to be. So far as animals go then, purpose is, in one sense, only a name, as it were. And yet the dog that goes after a stray sheep and drives it back to the flock seems to have as much purpose as there is in most actions of a human child.

Let us now observe that purposive behavior in animals includes what we may call the faculty of recognition of purpose in others. When you pick up a stone to throw at a dog, he appears to know what you are about to do and slinks off. People used to say that such behavior was due to association of ideas. There is no way to tell when the response is due to association and when it is due to conscious recognition of purpose. When a hunter takes down his gun from the rack, his bird dog shows by her demonstration of joy that she knows her master is preparing to take her out for a hunt in the field. A critical view, however, indicates that it is another case of association mechanisms. Just as the sight of food can cause increase of animation, so can the sight of her master's preparations, by virtue of conditioned reflexes, do the same. The dog wags her tail, leaps and perhaps barks, thus saying in dog language that she is delighted at the prospect.

Let us now review briefly what progress we have made.

We have considered the effect of language habits and time-measuring habits upon purposive behavior. We have found that men are taught to have purpose. We have considered the way reward acts in advance adaptation. In all these items, we have found that associative memory is a most important factor. In view of these relations, we may allow that purposive behavior is underpinned on all sides by association nerve mechanisms. Let us now enquire how the central foundation of such behavior is constituted.

When an animal or child has learned to go through a certain series of movements in a given order, the performance is, as before stated, a memorized series. We have already established that such a series is the operation of nerve mechanisms and that kinesthetic impulses link the movements together. Each one of the movements in turn is provoked by a volley of effective nervous impulses coming by way of converging association fibers, located, we will say, in the cerebral cortex. These fibers have been recently prepared by scout impulses, as it were, that came from the receptors in the muscles that made the movements which went before in the series. Each association fiber received a stimulus or scout impulse from a certain movement which lowered its resistance for a time. It was thus prepared for the effective impulse that came soon after. In this way, we see how each movement in the memorized series is determined by the movements that go before, by means of association fibers which have been developed by previous training. Let us now think of the series so modified that each movement is incipient only, but that it causes afferent impulses that excite other incipient movements in order, so that the incipient movements correspond to the actual movements in the primary series. Let us for convenience call this modified form a secondary series. We see that a secondary series is something like a train of thought.

As above stated, we must presuppose that an incipient movement can in some way provoke an appropriate afferent nerve fiber, so that we shall have an impulse that corresponds to a kinesthetic impulse provoked by an actual movement.

To explain more definitely what takes place, we will recognize two alternative assumptions. The first one is that an impulse is conducted by a motor nerve, although it is too weak to cause contraction of the muscle and is then conducted slowly through the muscle to a sensory terminal of an afferent nerve fiber. Hence a faint discharge from the brain to a muscle is followed by a counter-signal from the muscle to the brain. As an alternative the assumption is that at some nerve junction or synapse there is a short circuit from the motor nerve to an afferent nerve fiber and that the impulse is delayed appreciably at this junction. Hence a faint motor discharge is followed by a counter-signal that reaches the cortical centers and serves in place of a kinesthetic impulse from an actual movement. With either assumption, we see how one incipient movement will provoke another one and there will be a series of impulses running back and forth, from and towards the cerebral cortex. As in the case of the primary series, the counter-signals will follow the common nerve paths where the resistance has been lowered by recent or frequent previous impulses.

We now have a fair understanding of both the memorized series and the secondary series and the relation between them in behavior. Let us therefore attack the main question before us and determine whether such behavior mechanism operations will account for responses that are adapted in advance to changes in the environment.

Let us take a definite memorized series made up of movements which we will call *A B C D E F G H I*, and suppose that it is sometimes replaced by a secondary series, *a b c d e f g h i*. When a child has learned these series, if he is induced to make the movements *A B C*, the other movements *D E F G H I* will be made from habit and if he is induced to make the incipient movements *a b c*, the other incipient movements *d e f g h i* will tend to follow. We may believe that the same association mechanisms are concerned in both series. When the primary series is followed by the secondary series, we may term it a recall. On consideration we see that it may sometimes happen that the secondary series will come in between the movements of the primary series like this:

A B C D E a b c d e f g h i F G H I.

We see too that here we have something resembling forethought, for the incipient movements *f g h i* show a correspondence with the actual movements *F G H I*, made afterwards. In this way we find that an elementary case of forethought can be accounted for by simple association mechanisms. It is the first step that counts, so it is not an unfair presumption to claim that we could advance by degrees and account for all purposive behavior by association nerve mechanisms. It will be seen that where there is a series of incipient movements that corresponds with a series of actual movements to be made later, a foundation is provided for advance adaptation.

The intelligent animal knows the future by the past. What was the future yesterday morning is now the past. Looking back one sees what he might have looked forward to yesterday morning. From this he may conclude what he may look forward to this morning. Hence the experience of yesterday becomes the forethought of today. The entrance to the path that led to food yesterday will suggest the best path to take today to the hungry animal. If he takes the path and it leads to food again we have a case of advance adaptation. In considering the effect of reward, we saw that such behavior as that is explained by association mechanisms.

Let us now think of a child that makes the incipient movements *f g h i*, as a result of previous operations. Suppose that the afferent impulses from these movements provoke associated language movements as in uttering the words, "I am going upstairs." Then the actual movements *F G H I* of the memorized series are made, such as proceeding upstairs. We see that the uttered words exhibit advance adaptation. Let us note that this demonstrates how a secondary series together with association accounts for purposive behavior. In a similar manner it is thought that behavior equally purposive but not including language movements can be accounted for. For example, taking a key out of your pocket to unlock a trunk. Of course the necessary common nerve paths must be there.

We may conclude from this demonstration that the secondary series provides a foundation for advance adaptation. It is obvious then that purposive behavior can be explained as due to association nerve mechanisms. On further examination we shall find in very many cases, especially with man, that the secondary series is made up of incipient language movements. A man who is given to talking will hold a silent conversation with himself, before acting in a situation that is not quite familiar. From our point of view, it is all due to nervous impulses that follow the paths most open to them as determined by previous *nerve muscle operations*. We shall also find in human behavior many cases where movements or incipient movements that are associated with measurements of time, constitute the secondary series that anticipates the future situation, so to speak. One can as easily plan for next Christmas as for his next meal. The same kind of nerve mechanisms are employed in either case, and in both the responses are determined by one's past experience. It should be remembered, however, that thanks to the advantages of spoken and written language and to education, a man can make the experience of others serve as his own. That is where the social environment comes in.

The more highly developed the nervous system and the greater the number of association nerve fibers, the more precise and definite will be the anticipation of future needs. An old fox is warier than a young one. An educated man is more purposeful than a savage.

To sum up our conclusions briefly, if habit forming can be explained as due to association nerve mechanisms, advance adaptation can be accounted for in the same way. If our view is the right one, mechanistic interpretations are adequate to account for the purposive character of behavior.

RELEVANT AND IRRELEVANT SPEECH INSTINCTS AND HABITS

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Introduction

The audible vocal responses of certain birds and of the human being will be the matter of chief concern in this paper; but in order to discuss these responses intelligently, and in order to include all the movements which may be used as means of social communication, it will be necessary to investigate a number of other bodily movements. Excepting the vocal responses, the most important ones for speech are the mouth movements which modify the vocal responses directly. Movements of minor significance are those of the hands, feet, head, eyes, wings, tail, and the like, which may influence in any way the vocal responses, or which may themselves serve as gestures.

All of these responses are instinctive, and all of them may be at the same time habitual; a habit is an instinct which occurs more frequently than originally. The frequency of occurrence of an instinct is necessarily increased if it is associated with, and is regularly conditioned by, at least one other response of the same individual.

Relevant speech instincts and habits are those responses of the individual which serve as stimuli to call forth predictable responses in other individuals. Some speech responses are at the outset relevant, while others are made so through a process of training of the individuals who are to produce, perceive, and respond to them. In other words, many irrelevant speech responses must become conventionalized, if they are not to remain superfluous expressions in ordinary speech.

For the sake of simplicity in dealing with vocal responses, I shall make use of the vowel names, *a*, *e*, *i*, *o*, and *u*, as abbreviated means of speaking of the very large number of vowel sounds which an organism in question may utter. The qualitative nature of any given vocal sound could be indicated by marking diacritically one of these five letters, but it would be unnecessary and perhaps confusing if the attempt should be made in the following pages. It will be necessary, however, to indicate in a limited number of cases the specific qualitative nature, *e. g.*, of an *a*; but in general the reader will be privileged to image or utter any *a* sound which occurs to him at the time.

In discussing the consonants, I shall also speak primarily of some of the simpler consonant names of the alphabet as the most familiar representatives of the very large number of consonant sounds which the organisms utter.

Experiment and Discussion

To understand conventional speech, it is advisable to study carefully some simple forms of speaking organisms. I shall for this purpose begin with the *barbet*, or, as it is usually called, the 'bearded bird.' It frequently speaks, or more strictly sings the letter *a* somewhat as in *art*. When the bird is not molested it utters a succession of *a*'s approximately in the tempo of 0.2 sec. These sounds form a great number of *a*-groups, the largest one of which contains not less than five hundred *a*'s as its elements. When the group is very long an observer can become aware of a progressive, qualitative change of the *a*'s. The change is usually so gradual that it is not apparent in the shorter groups. Often the mouth closes at the end of a group in such a way that the final *a* is converted into a sound resembling the English *r*. When an *a*-group is thus terminated, I shall speak of an *ar-group-compound*, or simply of an *ar*-compound. The *a-group-complex*, or the *a*-complex, which is the summation of the quantitatively identical and different *a*-groups, is the most essential one which the vocal apparatus of this bird manifests.

The mouth movement which occurs regularly at the end of a given *a*-group and converts the final *a* into an *r* sound, is a specific activity which at some previous time interrupted a series of *a*'s and became permanently associated with the final *a* of the *a*-group which it isolated from the longer series. This mouth movement became the permanent conclusion or final accent of the isolated *a*-group. The following experiment makes this view plausible.

Observing the bird carefully, I struck, whenever possible, the glass of its cage at the fourteenth *a*. This stimulus called forth the mouth movement which interrupted the series of *a*'s and served to convert the fourteenth *a* into an *r*. It became eventually so well associated with the final element of the 14-*a*-group that I no longer had to apply any stimulus to the bird at this point of the series. The *ar*-compound which thus came to be uttered as regularly as the 14-*a*-group occurred, consisted in all of fifteen perceptible elements: first, thirteen *stutters*, that is, the *a*'s which were unnecessary for the perfect pronunciation of the *r*; secondly, the significant *a*, the only one necessary for the *r*; and, thirdly, the mouth movement which made the audible difference between the fourteenth *a* and the preceding ones. The bird was also taught by the same method to utter an *ar*-compound consisting of thirty-three stutters and the *r* which was a modified *a*. No other habits were acquired by this animal under experimental conditions.

The particular training to which the bird was subjected habitualized the mouth movement; that is, it caused this movement to occur more frequently than originally.[1] After the training, the mouth closed not only when the glass of the bird's cage was struck, but also whenever the final *a* of either the 14- or the 34-*a*-group occurred. No portion of an *a*-series was caused to occur more frequently than originally, and consequently not even the 14- and 34-*a*-groups can be called habits. This training, at least, did not make them habits.

[1] For a more detailed discussion of instinct and habit, see my article, 'Ueber einfache Bewegungsinstinkte und deren künstliche Beeinflussung,' *Z. f. Sinnesphysiol.*, Bd. 49, 1915. See especially pp. 247–248 for my definitions of instinct and habit.

The training did, however, cause certain *a*'s to occur less frequently than originally. For example, in the case of the series from which the 14-*a*-group became isolated, the fourteenth *a* conditioned the mouth movement instead of the fifteenth *a*; and with the mouth closed the bird could not continue the series. This does not mean that the latter part of the original series vanished completely, but only that the fourteenth and fifteenth *a*'s became dissociated. The most significant result of this complete dissociation was the addition of one more group to the *a*-group-complex.

The *cockatoo* is a bird whose speech instincts outnumber those of the barbet. Its vocal apparatus functions in various tempi, as is indicated by the differences in pitch of the many tones; and the mouth moves in various tempi, directions, and amplitudes. A large number of group-complexes is manifested by the vocal apparatus and mouth parts. The cockatoo, like the barbet, stutters; but the succession of the elements of an audible series is usually so rapid that only a continuous tone is perceived. It is sometimes a whistling tone. The mode of whistling is, however, quite different from that of the human being; for, while the human being whistles mainly with his lips, the cockatoo, which is almost void of soft lips, produces these tones primarily with its vocal cords.

When any one of the vocal responses of the cockatoo is interrupted by a mouth movement a compound almost invariably results which might well be used in conventional speech. Many mouth movements are of such slight amplitudes that a vocal series which is in progress at the time may not be interrupted completely, but only slightly modified by them at the points of the series where they occur. Further, these movements are often of considerable duration. I learned that I could call forth a large number of these movements in a desired order, and took advantage of the situation to teach a *yellow crested cockatoo* to whistle recognizably a certain simple portion of 'The Wearing of the Green.' The cockatoo had never heard this melody until it itself produced it. The stimulus I used to modify the monotones which the bird for some unknown reason frequently whistled, was a burning candle which I moved in various directions and with various speeds before the animal. In whatever direction the candle was moved, the cockatoo threw its head or even its entire body toward the flame. For example, when the candle was moved to the left, the bird often raised only the left foot and thrust its body in that direction. Many of these thrust-like movements were accompanied by alterations in the size of the mouth cavity. Thus the tone could be modified successively in various ways.[1] The method used to cause the cockatoo's tones to become melodious, also caused it to dance to the movements of the flame. At other times, *i. e.*, in the absence of the candle, it danced as if to the tune it produced. At these odd times the thrusts of the head or entire body were not quite so pronounced and sudden as when I called them forth with the flame, and the dancing was accordingly more graceful and the whistling more melodious.

I also used various other stimuli which called forth the desired responses when properly moved before the bird, but the candle was the most useful one I found. Some of the others were a piece of ermine fur, a bundle of feathers, a frog, and a dead mouse. The burning candle was especially adapted for this investigation, not merely because the flame was a stimulus which the bird's eyes generally followed, but because its shape and size could be varied with the velocity of movement. When moved rapidly, the flame became very small and then flared when the movement ceased. I could make the flare of the flame so disturbing to the bird that the tone would be interrupted completely, which event I avoided in this particular experiment.

There are some very brief mouth movements manifested by the cockatoo which do not interrupt a vocal series com-

[1] This particular bird was the most suitable subject I found for this experiment. Other cockatoos responded to the flame in a similar way, but their movements toward it were not so pronounced, and the tones were not altered in the same degree. I also worked in the same way with a small owl, whose vocal utterances were more similar to those of the barbet, and determined that its vocal responses became slightly modified at many turns of the head.

pletely, but only alter the tempo slightly and become inserted between two of the elements. Such a compound would be *aaapaaaa*, or *eeetee*.[1] Any such mouth movement which can be caused to occur once in a vocal series, may be caused to occur twice, and if the bird in a given environment is stimulated to insert a *p* in an *a*-series, it will very probably be stimulated in the same way soon again. This is a form of natural training which is evidently responsible for many of the compounds spoken by wild cockatoos. Some of the more frequent of these are: *aaapaapaa*, *aaaamaaamaa*, *aakaaakaaa*, *uutuuutu*, *eeemeemeee*.[2] These activities are not necessarily rhythmical, because the mouth movements in any case do not necessarily occur at absolutely regular intervals.[3]

It is not necessarily the case that a vocal response becomes thus slightly modified two or more times by the same mouth movement. The mouth is capable of executing a great variety of movements, and it is scarcely conceivable that the stimulus for only one of these should be present in any given environment. Further, if the bird should take a step or perhaps turn the head after the mouth has moved the first time, and while the vocal response is still in progress, such an act might create a sufficiently new environment for the animal that it would now be exposed to a stimulus for a decidedly different mouth movement. Some of the resulting compounds might be: *aaaapaaataaaaa*, *aaaaaapaaateeeeeee*,

[1] Any series of like letters not separated by commas are meant to indicate elements of a vocal response which is only apparently continuous.

[2] Perhaps Binet and Simon had such compounds in mind while investigating the child's language. Alfred Binet et Th. Simon, 'Langage et Pensée,' *L'année Psychologique*, 1908, p. 310: "Comment pourrait-il prononcer des mots qu'il n'a jamais entendu, et si on ne les lui a pas appris par une autre voie? Il existe donc, au moment de l'élaboration des fonctions, des relations nombreuses entre les fonctions naissants."

[3] Wundt calls special attention to the fact of the repetitions in the child's utterances, but he seems to think the phenomenon is due to its appreciation of rhythm. Wilhelm Wundt, 'Die Sprache und das Denken,' *Essays*, Leipzig, 1906, 2. Aufl., S. 281: "Eine primitivste Äusserung des Gefallens am Rhythmus ist wohl auch die Neigung zur Wiederholung der Laute, durch die beinahe alle Wörter der Kindersprache zu Verdoppelungsformen geworden sind. Ursprünglich sind aber die Wiederholungen fast immer mehrfache, und erst allmählich sind diese zu den geläufigen Verdoppelungen, Papa, Mama, Wau-wau, u. dergl., verkürzt worden."

eeeteeeepaaa, *aaaaakaaakaaaatuuuu*, and the like. In the last instance, *e. g.*, the *u*'s follow the *t* because the initial *u* of the *u*-group became at some previous time associated in this order with the mouth movement responsible for the *t;* and if the bird does utter the *u*'s, it cannot utter the *a*'s at the same time. The *tu*-compound, or whatever is responsible for it, which in this case inhibited a number of *a*'s, served the same purpose as the mouth movement of the barbet: the barbet was unable to pronounce the *a*'s while the mouth was closed, and the cockatoo was unable to pronounce the *a*'s while uttering the *u*'s.

One reason why so many of these words more often start with a consonant than with a vowel, is that the very movement which opens a closed mouth to utter a vowel group is quite frequently one which is indispensable for many of the consonants. If the bird should be stimulated to utter the series *uuuu* at a time when the mouth is not already in the proper position for this act, it might be opened suddenly and thus permit the cockatoo to say *tuuuu*, *puuuu*, or some other consonant. We should look upon the last three of the *u*'s in either of these cases as stutters, because they are not necessary for the perfect pronunciation of the *t* or *p*. The letter *t*, as it appears on paper, is only a symbol for some one of the many forms of a vowel which is modified by a mouth movement. We can write the letter *t* without the vowel of which it is a modification, but while this exists as a visual stimulus, it is an impossibility in audible speech. The written *t* of the alphabet signifies the *te*-compound. Another reason for the fact that vocal responses so frequently start with a consonant is that the brief mouth movements essential for the pronunciation of many of the consonants occur much more frequently than the vocal responses; no animal speaks every time it opens its mouth. This means that the proper mouth movement for a given consonant often occurs and modifies a vowel sound, provided that upon the opening of the mouth there occurs a vocal response to be thus modified.

Many speech compounds, however, *must* start with a vocal response. For instance, any one of the consonants of our alphabet, which is not one or another of the vowel forms initiated by and modified by at least one mouth movement, is initiated by some vocal response, the last element of which is modified by the mouth movement. This means that some of our consonants would be impossible in verbal speech if certain vocal responses could not be in progress when the significant mouth movements occur. These consonants of the alphabet are the *ef*-, *ah*-, *el*-, *em*-, *en*-, *ar*-, *es*-, and *ex*-compounds. The existence of such compounds as these and many other similar ones which do not appear in the alphabet, illustrate clearly the impossibility of starting a rather large number of our conventional speech compounds with mouth movements, for, in these cases, the mouth merely moves and modifies vocal responses already in progress.

The vocal and mouth movements of the cockatoo can be associated under controlled conditions to produce not only the various consonant names, but also great numbers of our monosyllables, and these can in turn be associated with one another to form larger compounds such as our polysyllabic words. A monosyllable is for the speaking organism just as simple as, or at least not much more complex than, most of our consonant names. A monosyllable in speech is merely a more or less compound utterance that maintains its identity when associated with other speech compounds, more often than do our consonants, which are also compounds. In other words, a monosyllable is ordinarily not as variable as a consonant name, and is usually just as easily spoken. The consonant names consist of *a*-, *e*-, *i*-, *o*-, or *u*-groups which are either preceded or succeeded by a mouth movement that modifies at least one element of the group. This mouth movement is of such magnitude and brevity that the resulting audible effect is usually an explosive one. The monosyllables consist of like vowel groups, which *may*, however, be preceded and also succeeded by the distinct mouth movements.

The vowels, too, are groups which are often modified by mouth movements; the mouth frequently moves before as well as after each vowel group, but these movements do not produce such explosive effects as in the cases of the consonants and monosyllables. Various tendencies which manifest themselves when we attempt to pronounce the vowels, deserve emphasis. In uttering *a* or *i*, the *a*-group or *i*-group is frequently terminated in each case by an *e*-group, while the *u*-group, on the other hand, instead of being terminated by an *e*-group, is frequently preceded by it. In the face of these facts we are not justified in writing for the vowels only the *a*-, *e*-, *i*-, *o*-, and *u*-groups, but we must add the *ae*-, *ie*-, and *eu*-compounds, as well as others. These compounds represent only three of the so-called diphthongs of speech; and even they, depending in general on whether they occur at the beginning, in the middle, or at the end of our larger speech compounds called words, are under conditions differently pronounced.

While the majority of the mouth movements are such as can modify vocal responses, it is to be borne in mind that there are a great number of other bodily activities which neither completely interrupt nor modify in any observable way the audible vocal responses. Some of these are the movements of the tail, wings, hands, feet, head, and eyes. True enough, these may carry the body through space and cause the speaking organism to become affected by a number of different stimuli which call forth various mouth movements. An eye movement may sometimes appear to alter a vocal response which is in progress when the eye moves, but a safer supposition is that it conditions a mouth movement which interrupts the vocal response or modifies it in one of the other ways previously discussed. Any movement of the body can become associated either directly or indirectly with a vocal response and condition it, be conditioned by it, or merely accompany it; but the mouth movements alone can interrupt it.[1] Many of the larger movements of the body which are associated with vocal responses, and which appeal primarily to the visual sense, are called gestures; but

[1] For reasons that should be obvious, I am considering only those vocal responses which occur during the period of exhalation.

there is really no fundamental distinction to be made between these and the movements of the vocal cords, or of the very slight movements of the various parts of the mouth. Any given perceptible response of an animal organism becomes a means of social communication when it stimulates other beings to behave in a predictable way. Some of these movements may be perceived with the ear, some with the eye, and some with both eye and ear. It is a matter of insignificance what sense organ they affect. Those perceptible movements which do not call forth predictable responses in other individuals, since they have not become conventionalized, are nonsense expressions. The process of conventionalizing responses for means of communication is nothing more nor less than a process of training the individuals who are to utilize them to stimulate one another for calling forth predictable responses.[1]

The human being often stutters in a manner similar to the barbet, but, as does the cockatoo, in such a very rapid tempo that for the hearing organism an apparently continuous tone results. He stutters thus either before the interrupting mouth movement occurs or after it. The tendency of the human being to stutter in this way is indeed very great, as is illustrated by the manner in which the infant 'coos' and by the way it later utters such words as

m
maaaaaaaaaa, or *paaaapaaaaaaaaa*. In the earliest stages of infancy the child does not often begin these words with *m* or *p*, but, as the cockatoo often does, with *a*. They should accord-

m *m*
ingly be written somewhat as follows: *aaaaaaaaaaaaaaa*, *aaaaaapaaaaapaaaaaaaa*. The mouth movement of the child which is responsible for the *m* sound within an *a*-series

[1] While this point seems obvious, it has not been given due attention by the investigators of speech. Wundt, for example, in his 'Physiologische Psychologie,' III., S. 285, says: "Alle Ausdrucksbewegungen geschehen selbst beim Menschen im Anfang des Lebens unwillkürlich; sie sind teils Triebhandlungen, teils reflectorische Bewegungen. Allmählich erst werden einzelne willkürlich gehemmt, andere hervorgebracht, und es entstehen auf diese Weise willkürliche Ausdrucksformen;" and also, S. 452: "Auch die *Sprache* ist in gewissem Sinn eine form der Geberde. Sie entwickelt sich wahrscheinlich teils als affectartige, teils als nachahmende Bewegung."

merely occurs simultaneously with one or more *a*'s and does not even alter the tempo of the series as does that movement responsible for the *p*. When the *m* is uttered the *a*-series continues as a nasal sound, which does not occur with the cockatoo.

Other prevalent compounds spoken by the child are:
m
aaaapaaakeeeee, *aaaaaaameeeee*, *aaapaapeeee*, *aadaaadeeee*, *aaaabaabeee*, *aaaaapaaapuuus*, and the like. And in reading, the child says "Whaat iiis thiis? Iiit iiis aaaaa hooorse aaaand aaaaa cooolt. Caaaan youuu riiide theeee cooolt? Oooh, no! Iiit iiis toooo liittle." Stimulate the child by telling it to read more rapidly, and it will very often pronounce the words so indistinctly that they can be understood only with difficulty. One child which was thus stimulated seemed to read the words as follows: "Wat is sis? It is a hog and a shoat. Can you wide de shoat? Oh, no! It is too witte."

There is moreover a second variety of stuttering which is quite closely related to stammering. An adult once asked: "I, i, i, i, is ti, ti, ti, this the ti, ti, train for A, A, A, A, A, A, A, Aurora?"[1] If one tries to stutter in this manner in pronouncing *Aurora*, one easily observes that these are not the same, but qualitatively and quantitatively different *A*'s which succeed one another; each of these is in reality an *A*-group, seven of which are nonsense or superfluous expressions. The eight different *A*'s may be indicated as follows: *AA*, *AAAAA*, *A*, *AA*, *AAAA*, *AAAAAA*, *A*, *AAA*urora, or, as follows: 2-*A*-, 5-*A*-, 1-*A*-, 2-*A*-, 4-*A*-, 6-*A*-, 1-*A*-, 3-*A*-group. Although each group has its accent which is either a mouth movement or a movement of the respiratory muscles, the final accent of the 3-*A*-group is most easily observed. The speech compound represented by the letters *ro* was not associated with the 2-, 5-, 1-, 4-, or 6-, but with the final element of the 3-*A*-group, and other *A*-groups merely preceded this one. The expectation arising from this view that the individual would occasionally not

[1] As the word *Aurora* was spoken the *u* was silent, and the initial *A* was pronounced in the same way as the *a* of the alphabet.

stutter in the indicated fashion in speaking *Aurora*, is realized: he does not stutter thus when the 3-*A*-group is the first one of the possible *A*-groups to occur.

The same fundamental statements can be made concerning the *ti*'s that were uttered before the word *this* was pronounced. In this case the mouth movements which resulted in modifying the initial element of each of the *i*-groups was more pronounced than any of those which accompanied the *A*-groups. When the compounds are as distinct and as explosive as these *ti*'s, I shall call them stammers. There is not the *ti*-compound in the word *this*, and the stammerer who has an image of the word simply has to wait until he is properly stimulated to utter the compound which begins with the very peculiar mouth movement responsible for the *th* sound. He would have no trouble in speaking the word if he would in the attempt either sneeze or imitate a sneeze. All of the various compounds which appear as stammers, involve at least one vocal response and at least one distinct mouth movement. The failure to recognize the fact that even the simplest stammers involve in each case at least one form of a vowel group which is modified by a mouth movement, has led investigators of speech to very serious errors.[1]

Many stammers are conventionalized compounds which either merely occur at inappropriate times, or which are unnecessarily repeated. They are accordingly not only

[1] The following quotation taken from C. S. Bluemel's 'Stammering and Cognate Speech Defects,' I., p. 187, is one of many cases which I could choose to illustrate this point. "*The stammerer's difficulty is transient auditory amnesia:* he is unable to recall the sound image of the vowel that he wishes to enunciate. This then is the thesis of the present monograph. . . . His futile struggles with the initial consonant are directed solely by his kinæsthetic imagery, but he cannot pass to the vowel because he cannot recall its sound, its peculiar or characteristic quality—in short, the *vowel-color.* When he attempts to speak the word *ten,* he produces the *t* entirely by feeling; but he cannot mentally hear the sound *ĕ*, and is hence unable to proceed." My criticism of the thesis of the monograph is that the stammerer utters the vowel whether or not he recalls its sound; for, in his attempt to pronounce the word *ten,* his stutters are generally a number of *tĕ*-compounds. The *t* which the author has written, is only a visual stimulus symbolizing the vowel which is modified by a mouth movement. The compound *tĕ* can be spoken, as the stutterer does, but we cannot utter a *t* apart from the *e*, *ĕ*, or some other vocal sound.

often superfluous, but even harmful in speech, in so far as they distort the compounds which the stammerer is attempting to use to call forth predictable responses in other individuals. Take, for example, the compound 'you know.' When one says, "Do you know the name of that mountain?" or, "You know what I mean," there is no violation of a conventionality of speech; but, when one says, "Did you ever, *you know*, smoke a cigar in the wind?" the superfluous 'you know' is a non-conventionalized or stammering expression. Other common superfluous utterances are 'Well,' 'Oh,' 'Now look,' 'Now listen,' 'Now,' 'Don't you think?' 'Don't you know?' 'Isn't it?' 'Yes sir!' and so forth. We often hear such meaningless expressions before or after a sentence, or indeed often after mere phrases of it.

The simple fact that a speech compound is at one time a nonsense and at another time a sense expression, means that our training methods are not adequate to enable us to use all the speech manifestations of the organism in all the combinations they so frequently help form. It is important that any nonsense expression can become conventionalized by training the individuals of the social group to respond in a definite way when stimulated by the expression. Our conventional speech is accordingly reduced to a form of stuttering and stammering, the stammers being the more pronounced elements. Some stutters and stammers are common to a greater number of individuals than are others and are consequently more highly conventionalized. Generally, only the more idiosyncratic utterances are superfluous in ordinary speech.

A *bald-eyed cockatoo* was taught to stutter. It was taught to repeat after me the word 'Kakadu,' the German for cockatoo; but it did not say 'Kakadu' immediately: first it said 'What,' 'Jako,' 'Kak,' 'Kak,' and then 'Kakadu.' While the bird frequently uttered various compounds before saying 'Kakadu,' the training to which I subjected it established this particular order. It was also trained to stammer in certain very definite, but silent ways after the word 'Kakadu' was spoken. My primary interest was in the speaking of the word 'Kakadu,' and I accordingly called all those compounds which were uttered or produced in any other way before and after this word, stammers. For a second observer the word 'Kakadu' might have been only one of the stammers which regularly occurred after the word 'Jako.' A wild cockatoo may utter a series of compounds which become established through self-training; but any one of these may be of the same interest to us as any other one. If we should become able, by training ourselves for the purpose, to respond in a definite way, or, in other words, to attribute a definite meaning to the whole of such a series of compounds, we should be inclined to consider no element of it a stammer. Also, if the cockatoo should be taught a conventional sentence with a subject, a predicate, and so forth, tradition would scarcely permit us to speak of stammering activities, in spite of the fact that the cockatoo may not have been taught to use the sentence in the conventional way.

It is a noteworthy fact that well-established, conventionalized sentences of the human being become frequently distorted with stutters and stammers. It is peculiar, but interesting, that students of ancient languages, who use their knowledge of these to speak a more correct English, do not as a rule speak fluently; they often pause in the middle of a sentence and insert a few stammering expressions before going further. These stammers are activities which interrupt the established series of conventionalized compounds and become associated with some of their elements just as the mouth activity of the barbet interrupted a long series of innately associated *a*'s and became associated with the fourteenth or final *a* of the 14-*a*-group. Those Greek and Latin scholars who, according to their introspections, do not attempt to use their knowledge of these languages while speaking their mother tongue, may speak as fluently as other people. The fact that our Greek and Latin scholars may and usually do speak, provided we neglect their non-conventionalized stammers, a more grammatical English, and also that they may possess an extraordinarily large and varied vocabulary, has nothing to do with our present problem. A student of logic may make fewer fallacies in his reasoning; that is, his responses may conform more with convention, by having studied logic; but a logician who attempts to apply his principles of logic and to speak fluently at the same time will acquire habits of stammering. Stimuli which may cause a person to stammer thus, may have many sources.

A non-excited person may say *Aurora*, but when in a state of excitement he may say *A*, *A*, *A*, *A*, *A*, *A*, *A*, *Aurora*. These *A*'s represent eight qualitatively different *A*-groups which became isolated from different parts of an *A*-series which can be represented as follows with the potentially adequate stimulus for each *A* underneath it: $\underset{a^1}{A}\ \underset{a^2}{A}\ \underset{a^3}{A}\ \underset{a^4}{A}\ \underset{a^5}{A}\ \underset{a^6}{A}\ \underset{a^7}{A}\ \underset{a^8}{A}\ \underset{a^9}{A}\ \underset{a^{10}}{A}\ \underset{a^{11}}{A}\ \underset{a^{12}}{A}\ \underset{a^{13}}{A}\ \underset{a^{14}}{A}\ \underset{a^{15}}{A}\ \underset{a^{16}}{A}\ \underset{a^{17}}{A}\ \underset{a^{18}}{A}\ \underset{a^{19}}{A}\ \underset{a^{20}}{A}\ \underset{a^{21}}{A}\ \underset{a^{22}}{A}\ \underset{a^{23}}{A}\ \underset{a^{24}}{A}$. When the stimulus a^1 is presented the first *A* is conditioned by it, the second *A* is conditioned by the first, the third by the second, and so on. If a^1 should be removed as soon as the first *A* is conditioned, the after-response would consist of the remaining twenty-three *A*'s. If a^5 were the stimulus presented, the fifth *A* would be conditioned by it, and the after-response would consist of the last nineteen *A*'s. There are experimental and also theoretical reasons for asserting that a^5 can be presented repeatedly without bringing about a dissociation of the fourth and fifth *A*'s, but in this discussion we do not need to consider why it should be the case. If the 24-*A*-series appears more frequently than any of its parts, this means either that a^1 is a stimulus which affects the organism more frequently than does any one of the other stimuli, or that the first *A* is conditioned not only by a^1, but also by one or more frequently occurring responses which have become associated with and precede the first *A*. It may well be the case that of the potentially adequate stimuli here represented for the different *A*'s, a^1 is the only one present in a given environment. If then, while the series is in progress, a mouth movement should occur and isolate the first three *A*'s from the remainder of the series, the 3-*A*-group only will occur as long as the person remains in the given environment.

If the individual should shift to a new environment and become affected by a^4, the 21-*A*-series will occur and become habitualized, because the initial *A* of this series will become associated with and later be conditioned by the response which happened to occur immediately before it. This accidental response will now serve as a substitute for the 3-*A*-group which became previously isolated from the original series. We could proceed in the same way until we get the series broken up into a number of fragments or unitary groups each of which maintains its identity in many different environments by occurring more frequently as a whole than in parts. These unitary fragments of the original series may be the 3-*A*-, 5-*A*-, 1-*A*-, 2-*A*-, 4-*A*-, 6-*A*-, 2-*A*-, and 1-*A*-groups. If it should be such a compound activity as *rora* (I omit the *u* because it is silent in the spoken word Aurora), which isolated the 3-*A*-group from the original series and became associated with the final element of this group, the individual would be able to pronounce the word Aurora only when the 3-*A*-group occurs. If it should happen that while he has an image of the word he should be stimulated to produce the 5-*A*-group, which is not associated with the 3-*A*-group, at least one more effort would have to be made before the word could be pronounced.

Let us suppose that the individual happens to be in an environment in which the stimulus for only the 3-*A*-group is present. He would succeed at the first effort to pronounce the word. If he should pass to a new environment which contains the stimuli for all the groups, he might by chance have to make many efforts. Now it does not matter whether he actually passes into a new environment or whether these stimuli are introduced into the old one, leaving it otherwise unchanged. It would be the same if they should be secreted by his own body. Perhaps they are under certain conditions secreted by some of the ductless glands of his body. Perhaps some of the hormones secreted under conditions of excitement are adequate stimuli to condition a number of the superfluous *A*-groups while he has an image of the word Aurora. It is thinkable that the hormone secretion could be of such a nature that if it should not call forth all the *A*-groups simultaneously, it might cause them to occur in very rapid succession. In the first case a 'boisterous' *A* sound of relatively short duration would result, and, if the final accents of the superfluous *A*-groups should be relatively insignificant for the hearing organism, it might seem in the second case as if one long *A* were being uttered before the word Aurora is pronounced. A hormone which is, under certain conditions of stimulation, thrown into the blood, may be analogous to a response which a training in logic, Greek, or Latin has caused to occur quite frequently and necessarily interrupt at times either an instinctive vocal series or an habitualized series of speech compounds.

Out of the previous discussions arises the practical problem as to the surest and most economical means of breaking an individual of stuttering or stammering. In order to solve this problem we should become thoroughly acquainted with the process of teaching a subject to stammer. I noticed that when I stepped noiselessly to the bald-eyed cockatoo's room and, before opening the door, said 'Kakadu,' the bird said 'What,' and when I said 'What,' the bird said 'Kakadu.' When I rattled the door knob or opened the door without showing myself, the cockatoo said 'Jako,' when I jerked its cage with a string it said 'Kak,' and when I suddenly appeared in the room before the bird, it said 'Kakadu,' after which it began beating a horizontal bar of the cage with its beak, becoming then active in a number of other ways. In the course of time I found many stimuli which called forth the mentioned responses; but I made use only of the particular ones here given. Before the bird was aware of my presence outside its door I called out 'Kakadu!,' opened the door without showing myself, jerked the cage twice in succession, and then entered the room suddenly. After each stimulus the bird made a verbal response, the entire series of which was 'What, Jako, Kak, Kak, Kakadu,' after which it beat the horizontal bar with its beak. It was necessary for me to carry out this program several times each day for about three weeks before the bird pronounced the series regularly without error when I spoke the word 'Kakadu.' I discovered that after the series was well established I no longer had to go through with the ordeal of stimulating the bird by calling out the word 'Kakadu' while still outside the door; I could remain in the bird's room for some time and then utter the word 'Kakadu,' and the series would be spoken just as perfectly as when I called through the door.

At the end of two months I employed the following means to break up completely this series of associated speech compounds. I said 'Kakadu' repeatedly and as rapidly as I could for several minutes during each period, and after about three days this stimulus no longer served to instigate the series. However, when I rattled the door knob, the cockatoo omitted 'What' and began the series with 'Jako.' I then presented this stimulus repeatedly without long pauses until it likewise became ineffective. As soon as this task was accomplished, I repeatedly jerked the cage with the string until this stimulus was no longer effective. Of the three tasks, the last one was the most difficult to accomplish. When the series was thus completely broken up and I spoke the word 'Kakadu,' the bird generally said something, but not always 'What,' as it did before the training, and it said 'Kakadu' no more frequently than it did 'Jako,' 'Kak,' 'Ohoh,' 'Adieu,' or almost any of the many other sense or nonsense words which it could utter. Now, when it said anything other than 'Kakadu,' I repeated the stimulus for some time, and as soon as the bird said 'Kakadu,' I made a long pause before restimulating it in the same way. This procedure was necessary in order to make the cockatoo act as if it were imitating me. Before I could bring about this apparent imitation it was necessary for me to cause the rather complex stimulus used to become ineffective for a considerable number of stammers. I have applied this method of dissociation with equal success in various other fields of behavior.

If we now desire to apply the method to the human stutterer or stammerer, we should proceed somewhat as follows. Paradoxical as it may sound, we should present in rapid succession the adequate stimuli for the undesired activities. If we cannot find or cannot control the most adequate ones for these responses, we should use those less adequate ones which we do have at our command. For example, when a person, attempting to speak a word, stutters or stammers, we should simply stimulate him by telling him to do that same thing again and again, perhaps a hundred or a thousand times in rapid succession. But care must be taken that he does the *same* thing and not something which may be only very similar to the particular undesired act. After he has repeated the act a great number of times, he should then be stimulated to pronounce the word which was previously preceded by the undesired activity. It is essential that he should make a pause of at least a minute each time before he repeats the word; otherwise, this word, too, will appear less frequently and will not always be recalled at the significant places for it in ordinary speech. This process of dissociation will not effect an absolute forgetting of the undesired responses, but will cause them to occur less frequently at the inappropriate places.

It is to be emphasized that this dissociation does not mean an absolute forgetting of any activity. In the case of the cockatoo, for instance, each of the relatively forgotten or dissociated elements of the series of speech compounds, was later spoken at times just as perfectly as previously. Excepting the word 'Kakadu,' each of the compounds occurred less frequently than before the dissociations were accomplished. This relative forgetting brought about by continued stimulation is sometimes spoken of as 'forgetting with experience.'[1] We have no reason for supposing that any of the organic structures involved in the so-called forgotten responses became annihilated. Owing to the fact that the recuperation pauses were made as short as possible, certain structures involved became at least temporarily simplified to such an extent that they later functioned only upon the presentation of very particular, and, naturally, a smaller number of stimuli; the structures became 'immuned'

[1] See for example, H. Piéron, 'L'oubli chez la Limnée,' *Archives de Psychologie*, No. 33, Tome IX., 1909.

to certain stimuli. The simple fact that a person can stutter and stammer when properly stimulated to do so is significant in this connection. The fact that an expert rope walker returns to an amateur level of performance the very moment he, as we ordinarily say, 'gives serious thought to what he is doing,' is a phenomenon of the same category. The simplification of certain of the organic structures involved in a given, desired response to bring about its dissociation from other undesired responses, is, I believe, the fundamental principle underlying the facts of dissociation or relative forgetting, the automaticity of certain responses, and a few other related phenomena; but it would not be appropriate to attempt the explanation in this paper. I have merely inserted this view as a suggestion that my method of breaking up such undesired activities as those of stuttering or stammering can be explained without assuming the annihilation or permanent exhaustion of any of the structures involved.

Conclusion

Speech instincts and habits are the perceptible responses manifested by an individual which serve primarily as auditory and visual stimuli to call forth responses in other individuals. These stimuli are vocal utterances, mouth movements which modify audibly the vocal responses, and the larger bodily movements which affect primarily the visual sense.

A vocal instinct is a series of innately associated elements which presents a gradual, qualitative change from the initial to the final element. Any vocal instinct may be called a vowel series. There exist besides the *a*-, *e*-, *i*-, *o*-, and *u*-series, a number of vocal instincts which may be indicated by appropriate diacritical markings of the vowels of the alphabet. But a marking which designates the sound of the first few elements of a series is often quite inappropriate for the last few elements of the same series. For this reason, diacritical markings should also be employed to symbolize, *e. g.*, the different *a* sounds which can frequently be detected between the initial and final elements of a given *a*-series. Theoretically, if the anatomy were such that an *a*-series would not be prematurely interrupted by exhalations, there should occur within this series all the *a* sounds which the organism can utter. The gradual, qualitative changes of the *a*'s within the longest *a*-series of the barbet support this supposition.

A vocal response is a habit, as well as an instinct, when it is caused to occur more frequently than originally. The process of habitualizing a vocal response is a process of training which causes the instinct to become the final accent or conclusion of one or more responses which originally did not regularly precede and did not condition the initial element of the vocal instinct. The whole or any fragment of an instinct is not necessarily a habit, but a habit is invariably an instinct.

Vocal responses are essential for audible speech and are accordingly speech instincts and habits.

Mouth movements serve to interrupt vocal instincts and habits and isolate fragments or unitary groups of elements from them. A given mouth movement which isolates a unitary group from a longer series of *a*'s becomes at the same time associated with the final *a* of the isolated group and is later conditioned by it. This instinctive mouth movement which becomes the final accent or conclusion of the isolated *a*-group, is thus caused to occur more frequently than originally and is accordingly a habit as well as an instinct.

There are many mouth movements of such slight amplitude and of such brief duration that they may not completely interrupt a vocal response, but merely modify it slightly. Such mouth movements may only modify slightly the tempo of certain elements of a vocal series and become inserted between two of them, as in *aaaaaapaaaaa*, or they may occur simultaneously with one or more of the elements, as in

m
aaaaaaaaaaaaaa.

Mouth movements are as essential for audible speech compounds as are the vocal responses and should likewise be reckoned among the speech instincts and habits.

Larger movements of the hands, feet, head, and body in general, serve to carry the organism through space and cause it to become affected by a variety of stimuli which may not occur in any given environment. Many of these stimuli call forth mouth movements and vocal responses which become the final accents or conclusions of the movements of transportation and are later conditioned by them. Further, the less perceptible movements of the eyes serve the same purpose.

All of the bodily movements which cause mouth movements and vocal responses to occur more frequently, should also be called speech instincts and habits.

Any instinctive or habitual response of an organism is a means of social communication if it is used to stimulate individuals of known training to behave in a way which can be predicted from the nature of the stimulus. In discussing speech instincts and habits, it was necessary to discuss some problems of stuttering and stammering, because our conventional speech is only a form of behavior of which stutters, stammers, and larger bodily movements are the elements. When these elements of speech are conventionalized, they are ordinarily called letters, monosyllables, words, sentences, and gestures. A series of innately associated stutters which is modified by one or more mouth movements to produce such a speech compound as a consonant or a monosyllable, is a stammer. A number of like or unlike stammers may be combined through association to form larger speech compounds, such as polysyllabic words and series of words; but these are merely stammers of a higher order. The process of conventionalizing a stutter, a stammer, a number of associated stammers, or any other simple or compound response, is a process of training the individuals who are to utilize these to call forth predictable responses in one another.

Any artificially combined series of speech compounds can be interrupted in the same way as can an innately associated series of simple elements; and a response which either completely interrupts a conventional series or merely becomes inserted between two of its compound elements, is analogous to a mouth movement which either completely interrupts or merely changes slightly the tempo of movement of a vocal instinct. Those responses which occur most frequently are most likely to interrupt the conventional series. A training in ancient languages or in logic may cause them to occur frequently. A hormone secretion may affect the speaking organism in a similar way.

It seemed appropriate to present a method to diminish the frequency of superfluous utterances, which are undesired stutters and stammers. This method consists in presenting appropriate stimuli to cause the patient to produce a given nonsense or superfluous expression repeatedly without the intervention of long pauses until at least a large number of the stimuli for the superfluous act cease to call it forth. This results not in an absolute, but only in a relative forgetting of the undesired response. It was finally suggested that the fact of relative forgetting of the superfluous response is due to a dissociation of certain organic structures involved; that this dissociation is in turn a result of a simplification or purification of the structures; and that these phenomena can be consequently explained without assuming the annihilation or even the permanent exhaustion of any of the structures which function to produce the nonsense expression.

A PRELIMINARY REPORT ON 'WORK WITH KNOWLEDGE VERSUS WORK WITHOUT KNOWLEDGE OF RESULTS'

BY GEORGE F. ARPS

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The primary purpose of this study is to secure a quantitative statement of the influence of awareness as a factor in work.[1] To this end three sets of experiments were conducted by means of the Bergström ergograph. In each of the sets the observer at one time remains in comparative ignorance (complete so far as it is possible to make it so) of the amount and character of the work he is doing; at another time he is given every opportunity to observe his work as it proceeds and to study the results ad libitum. Each set of experiments is therefore divided into two series, designated 'unknown' and 'known' respectively. A series is made up of work periods.

The mental complexes, antecedent to the work response in the various series of experiments, are sufficiently diverse to prompt an inquiry concerning the meaning of this diversity in terms of work units.

Three observers, *M*, *J* and *W*, functioned in the experiments; in the case of *M*, experimentation continued over the greater part of three years. The method common to ergographic work prevailed. In our case the hand and arm of the observer were strapped into the machine in such a way as to secure maximum freedom for the middle finger (the work-finger of our experiments) of the right hand.

The instructions given each observer were simple. In the first work period he was instructed to pull the ergographic load on every beat of the metronome, sixty to the minute. This load was 4 kilograms for the first two sets of experiments

[1] The study will in no wise concern itself with the theoretical aspects of the controversial question of the relation of the psychical to the physical.

and 6 kilograms for the third set. The observer was repeatedly instructed to make each lift represent his maximal pull and to continue the lifting until he was no longer able to budge the load. The work period closed when the observer failed to move the load in two successive attempts. In the second work period, forty-eight hours later, the observer was instructed to lift the load for ten successive beats of the metronome and then to rest for one second (one beat) then to lift again for ten beats followed by another rest of one second. This process was repeated until the observer, as in the first work period, was unable to budge the load. The third work period of this series differed from that of the second in that the rest period was increased by one second (one beat). In each successive work period of this series the rest increased by one second. There are eleven work periods in each series. This procedure prevailed as well for the work periods in which the observer was relatively unaware of his accomplishments as for those periods during which he was fully aware. The procedure for any ascending series may be algebraically represented by the following formula:

$$\frac{P}{W+R_0}, \quad \frac{P}{W+R_1}, \quad \frac{P}{W+R_2}, \quad \cdots, \quad \frac{P}{W+R_{10}},$$

in which P represents ten ergographic lifts; $W + R_0$, a work period without rest; $W + R_1$, one second rest, etc., until the number of lifts and the number of rests are equal.

A 'set' of experiments is made up of (*a*) an ascending series of work periods in which the periods differ increasingly by one second of rest up to ten as a maximum, and (*b*) a descending series in which the periods differ decreasingly by one second until there are no rests. Graphically they may be represented as follows:

$\rightarrow$ Ascending Series, Known.
Rests = 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.
$\rightarrow$ Ascending Series, Unknown.
$\rightarrow$ Descending Series, Unknown.
Rests = 10, 9, 8, 7, 6, 5, 4, 3, 2, 1, 0.
$\rightarrow$ Descending Series, Known.

Care was taken to distribute as equitably as possible the effect of practice. The disadvantage, for example, accruing to the *known* in the ascending series of the first set was practically neutralized by beginning the second set of experiments with an *unknown* ascending series. Other advantages and disadvantages are similarly neutralized, as, for example, the disadvantage of the so-called phenomenon of 'breaks' which occurs between the conclusion of certain of the series and the beginning of certain other series. It will be seen from the above graphically represented series that the concluding work period of the ascending known series contains an equal number of ergographic pulls and rests. The beginning work period of the immediately succeeding series contains *no* rests. This transition from a work period containing ten rests in each of its subdivisions to a period containing no rests is termed a 'break.' Obviously it is important to distribute such breaks equally to the known and unknown series.

In the three sets of experiments there are six *unknown* and six *known* series, which together constitute 112 work periods. In the third set of experiments the number of work periods in each series is six. Each of these periods has an *absolute* and a *unit* efficiency value. Both values are explained under the tables given below.

Tables I. and II. set forth the two values of the twenty-two work periods of two of the series and other data which may serve in presenting a survey of this study.

Some of the important results are as follows:

1. The final average of the *absolute* efficiency values of all the known series of the first and second sets of experiments exceeds that of the unknown by .2 per cent.

2. The final average of the *unit* efficiency values of all the known series, excepting the third set of experiments, excels that of the unknown by 7 per cent. In the known work periods the observers work at a higher rate of speed.

3. The average absolute efficiency value of all the known series exceeds that of the unknown by 10 per cent.

4. The average absolute efficiency value of the known

series of the first and second set of experiments exceeds that of the corresponding unknown by 10 per cent.

5. The average absolute efficiency value of the known series of the third set of experiments exceeds that of the corresponding unknown by 10 per cent. (The ergograph load in this set is 6 kilograms.)

Tables I. and II.

First Ascending Known. Load 4 Kilograms (Observer *M*)

B	*C* Min.	*C* Sec.	*I*	III Min.	III Sec.	*C*+III Min.	*C*+III Sec.	*D*	*W*	$\frac{W}{C}$	$\frac{W}{C+\text{III}}$	*MWJ*	*WP*
1	1	57	0	0	0	1	57	1.665	6.66	.0569	.0569	.0595	.0595
12	1	53	1	0	11	2	4	2.073	8.29	.0733	.0669	.0805	.0737
11	1	46	2	0	20	2	6	2.330	9.32	.0879	.0740	.0878	.0735
12	1	56	3	0	33	2	29	2.276	9.10	.0785	.0610	.0854	.0338
21	3	37	4	1	24	5	1	4.877	19.51	.0899	.0649	.0828	.0605
24	3	59	5	1	55	5	54	5.710	22.84	.0954	.0646	.0883	.0614
23	3	49	6	2	12	6	1	5.447	21.79	.0949	.0603	.0859	.0549
32	5	16	7	3	37	8	53	6.144	24.57	.0776	.0461	.0789	.0481
32	5	27	8	4	8	9	35	8.482	33.93	.1008	.0589	.0908	.0627
54	8	59	9	7	57	16	56	13.921	55.68	.1003	.0548	.0917	.0497
55	9	17	10	9	0	18	17	13.693	54.77	.0985	.0500	.0917	.0465

First Ascending Unknown. Load 4 Kilograms

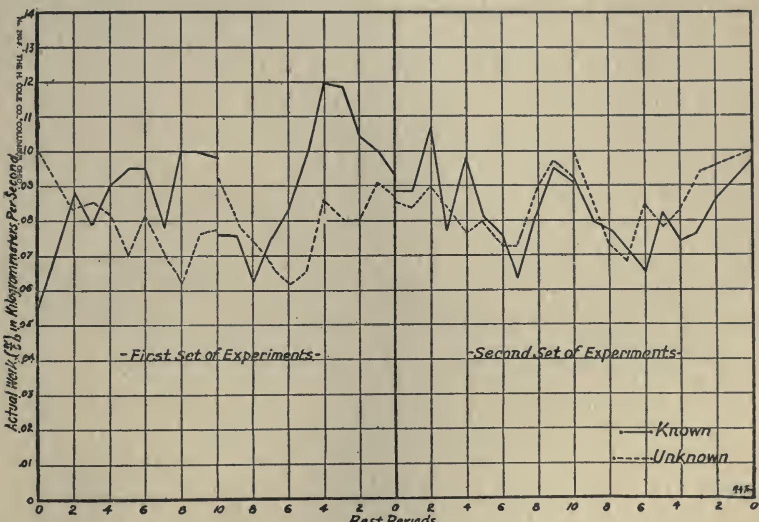
B	*C* Min.	*C* Sec.	*I*	III Min.	III Sec.	*C*+III Min.	*C*+III Sec.	*D*	*W*	$\frac{W}{C}$	$\frac{W}{C+\text{III}}$	*MWJ*	*WP*
1	1	20	0	0	0	1	20	2.162	8.65	.1008	.1008	.0705	.0725
11	1	54	1	0	10	2	4	2.642	10.57	.0926	.0851	.0893	.0788
13	2	18	2	0	24	2	42	2.905	11.62	.0843	.0717	.0876	.0793
15	2	25	3	0	42	3	7	3.101	12.40	.0856	.0664	.0747	.0595
21	3	35	4	1	20	4	55	4.382	17.53	.0816	.0593	.0707	.0579
33	5	26	5	2	40	8	6	5.734	22.94	.0703	.0471	.0714	.0479
21	3	35	6	2	0	5	35	4.339	17.36	.0806	.0518	.0708	.0454
40	6	37	7	4	33	11	10	6.746	26.98	.0679	.0402	.0677	.0407
64	10	40	8	8	24	19	4	9.781	39.12	.0612	.0342	.0624	.0350
101	16	52	9	15	0	31	52	19.528	78.11	.0771	.0408	.0658	.0350
90	15	0	10	15	0	30	0	17.584	70.34	.0781	.0407	.0692	.0354

B = Number of subdivisions of work periods.
C = Total time at work.
I = Length of rest period in seconds and number of the period.
III = Total rest time.
C + III = Total time of work and rest.
D = Distance load travels in meters.
W = Work done in kilogram meters.
W/*C* = Unit value per second of work done each period.
$\frac{W}{C + \text{III}}$ = Rate of work including the rest time.
MWJ = Average unit value (*W*/*C*) of all observers.
WP = Average rate of work per second for each period including the rest time.

6. The average *unit* value of the known series of all sets of experiments shows a higher rate of work by 18 per cent. over the corresponding value of the unknown series.

7. The average *unit* value of the known series of the first and second sets of experiments shows a higher rate of work (speed) by 5 per cent. over its corresponding value of the unknown series.

8. The average *unit* value of the known series of the third set of experiments shows a higher rate of work by 35 per cent. over the corresponding value of the unknown series.



Involuntary Substitution of Imagery for Perceptual Contents

The curious appearance of imagery during the work periods of certain of the unknown series is an interesting phenomenon in that it clearly indicates the observer's tendency to avoid working blindly. The point of definite appearance of imagery may be seen by reference to the curve given above.

The relation of the unit values of the known and unknown second ascending series of the second set of experiments is especially interesting in that the fact of overlapping of the curves is referred to the apparent functioning of imagination. From the introspections it appears that the condition of 'ignorance of results' prevailed more completely in the *first set* of experiments than in the *second set.*

The unknown series of work periods of the second set of experiments approach that of the known through the employment, without set purpose on the part of the observer, of visual and kinesthetic imagery reproductive of the perceptual and kinesthetic experiences of the work periods of the preceding known series. In the third work period of the second set of experiments (see curve), the following introspections are recorded: "The task of this period proceeded with a fair degree of definiteness, with a comfortable degree of orientation hardly comparable to any of the preceding unknown periods. Certain individual lifts I pictured vividly; in certain other cases I compared successive lifts. The comparisons were especially pronounced when the first evidence of fatigue appeared." In the sixth period of the same series the observer remarked that he "seemed to sense the efficiency of the entire period in perspective more or less tangibly."

Imagery is also involved in the more or less abrupt closing of the unknown period as contrasted with the gradual tapering off closing of the known periods. On this point one of the observers remarks as follows: "In closing an unknown period I seem to let down suddenly in spite of all efforts to avoid it. Short lifts have little meaning in that I fail to image the pen marks which support me in the long lifts."

In all these cases evidently imagery of the sort here described acts as an incentive to work. The presence of imagery was first detected in the first work period of the second unknown ascending series of the second set of experiments. It is probable that the overlapping of the curves of this set of experiments finds an explanation in the definite presence of imagery. For, as the imagery content of consciousness peculiar to the work periods of the unknown series approximates that of the perceptual content characteristic of the work periods of the known series, the differences in the awareness of results diminish. It is probable that the more essential features of the perceptual experiences acquired in the first set of experiments function in imagination in the work periods of the unknown series of the second set of experiments. If the mental complex operative in the second set of experiments forms a close resemblance to that complex functioning in the first set, and, if the efficiency differences of the known and unknown work periods are primarily due to the degree of 'knowledge of results,' then we have an explanation for the crossing back and forth of those parts of the curve representing the unit values of the work periods of the second set of experiments. This means that we have here phases of the process of habituation in which the established habits of response under perceptual conditions tend to subordinate responses operating under imaginal conditions. That the organism tends to gravitate towards its more accustomed channels of behavior is clearly indicated in succeeding work periods. Each succeeding set of experiments shows an increasing amount of crossing of the 'unknown' and 'known' curves (see graph).

From the general character of the curves and the introspective statements it appears that work carried on under conditions of partial awareness of results loses in efficiency and that such conditions are extremely difficult, if not impossible, to maintain when such work is followed or preceded by, work of identical character under conditions of complete awareness. Without set purpose the observers employ certain mental complexes during work under the former conditions, which parallel in function certain essential features of the mental complexes operative during work under the latter conditions.

THE BEHAVIOR OF THE HUMAN INFANT DURING THE FIRST THIRTY DAYS OF LIFE[1]

BY MARGARET GRAY BLANTON

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In view of the fact that a large amount of experimental work is being done by the psychological laboratory upon the reflex and instinctive equipment of the babies in the maternity ward of the Johns Hopkins Hospital, it seemed worth while to make a preliminary observational study of them under the conditions actually obtaining in the nursery. Most of the studies now in the literature are of this character and the present one has also the object of checking up others of the same character. The present observations have been made upon a very large number of infants and a thoroughly objective viewpoint has been maintained throughout.

Buccopharyngeal

1. *Sneezing.*—The earliest reflex noted. It was present on one occasion while the infant was being lifted from the mother. Subject L. sneezed before the birth cry appeared, and Subject G. at 5 hours, from which time on it is common. It was frequently noted on taking the babies into an overheated room where observations were made. It was not noticeable on bringing them again into a cooler atmosphere.

Preyer[2] says: "It demonstrated the existence of a very firm connection, long hereditary, of the nasal branches of the trigeminus with the motor expiratory nerves." He also

[1] This work was undertaken under the direction of Professor John B. Watson, director of the psychological laboratory of Johns Hopkins University. Through the courtesy of Dr. J. Whitridge Williams and Dr. Daniel Davis, the material in the nursery of the maternity ward was placed at our disposal. I am also much indebted to Miss Liphart and to Drs. McKee, Harris, and Sperry for many privileges on the ward.

[2] Preyer, 'The Mind of the Child,' p. 214.

quotes Darwin as saying that many healthy children at their coming into the world do not cry, but sneeze.

2. *Hiccoughing.*—Subject Sp. at 6 hours hiccoughed for three minutes. Subjects 7, 10, 11, and 14 days old hiccoughed after feeding. This list might be continued, for over 50 cases were noted. It seems so well developed at 6 hours that there is little reason to believe that the possibility of it does not exist at birth did conditions call it out. The condition most commonly calling it out is a full stomach (producing pressure on the diaphragm).

3. *Yawning.*—I have noted yawning twice within 5 minutes after birth and relatively few times during the first month. Subject S. yawned at 5 hours, eyes tightly shut. Subject F., 11 days, yawned 6 times in 1 hour; her eyes were squeezed tight and her chin trembled as she shut her mouth. Subject R., 5 days, yawned so like a sigh that the movement barely fell within the definition of a yawn. It was the only occasion on which the remotest resemblance to a sigh was observed.

4. *The Beginning of Speech and Emotional Reactions.*—Crying, less than any other manifestation of a baby's existence, can be considered separately. During the birth processes the connection between the mother and infant are made less efficient by the gradual displacement of the placenta. The concentration of the blood increases, which stimulates the respiratory centers and causes the intake of air to the lungs. The birth cry takes place at this point. Occasionally the nerve endings of the mucous membrane, affected by the unaccustomed stimulation of the air, may cause a light breath to be taken in and expelled in what we know as a sneeze. In four observed cases in which hot and cold plunges were necessary to stimulate breathing there was also a sharp movement of the arms. The cry observed came on the plunge into icy water and was sharp and short and on an inspiration as in an adult under similar conditions. The birth cry is rarely spontaneous, as the technique of the delivery is to stimulate it at once by vigorous rubbing and slapping on the back and buttocks. This cry, as I observed

it, differed in no way in timbre, pitch, etc., from other cries of the first few days. The birth cries of different infants were not alike, ranging from simple *a* (as in at) to *u* (as in cut). Most of them were compound *u* (as in cut) followed by *wah* (as in at); *uh* (cut); *nga* (at); and variations just as in the later cries of infancy. This was true also of the three spontaneous cries which I heard.[1]

Major[2] says: "The beginnings of language are usually traced to the reflex crying of the newly born babe." Both Major and Preyer assert that "they are produced as well by a child without a cerebrum as a child with one." I was not present at the birth of Subject B., mentioned elsewhere, but I was told by the surgeon that this infant did not cry out. Only 4 cries were noted during its entire 10 days of life, all of them taking place during a deep examination to determine the extent of displacement of the bones of the skull. The birth cry which is followed by an interval of quiet is rarely followed by an interval of stillness.

Crying has been observed under the following conditions: (1) hunger; (2) in response to noxious stimuli (including rough handling, circumcision, lancing and care of boils, sores, etc.), and (3) possibly from fatigue or lack of exercise. In the subjects with which I worked I did not find the cries of hunger, to noxious stimuli, to fatigue, and so forth, uniform. There were differences of vowels and consonants, of timbre and degree, but no one was used as response to one set of circumstances that was not at some time used to others. For example the cry in response to noxious stimuli was in many cases an exaggeration of the cry usual when hunger was present.

The cry of colic was the one exception. Starting abruptly

[1] The dramatic element of the first cry has led to a most amusing amount of poetic license. Kant's famous saying that "the outcry that is heard from a child scarcely born has not the tone of lamentation but aroused wrath," has been subjected to many and various comments. Preyer in quoting him says, "Kant wrote without having himself observed children and animals just born." Dr. William A. White quotes Adler as saying: "It is an expression of its overwhelming sense of inferiority on thus suddenly being confronted by reality, without ever having had to deal with its problems."

[2] Major, 'First Steps in Mental Growth,' p. 282.

about 3 to 5 octaves above the adult female voice it slid through a modified chromatic scale to within the range of the middle octave. It was made with an accompanying rigidity of the abdominal walls and thus of necessity varied from the cry which included the activity of these muscles. Subject L., 15 days, often started with a few short rapid breaths, a few of which sounded like an 'affected' cough and ended with a two syllable cry, *a* (at), and *yow* (owl), long and on a falling inflection. The whole is much like the word *meow* with the *m* replaced by *a*. The cries did not vary so much with the ages of the infants, during the first five or six days, as with the weight. A baby of 2,300 grams at one day, and 2,300 grams at 5 days will show little difference. However, a baby of 4,000 grams first day and 4,000 grams fifth day would show more difference, probably because 4,000 grams weight would indicate a better physical condition. In such a large group of babies any vowel or consonant can at some time or other be heard on the first day of life that will be heard on any other day during the first month. *A* (what) will be heard in a first syllable, but I have not heard it in a second syllable excepting in a series of short grunts. This I suppose is because a maximum of activity is reached on the second syllable, and the maximum activity in the infant is accompanied with an open mouth and a contraction of those muscles which draw the inferior maxillary inward and downward. This, it is seen, precludes the making of the *ah* sound.

The crying of one baby can be distinguished with some practice from the cries of another even in a nursery of 25, the overtones varying just as in older people. Subject M., first day, *u* (cut), *nah* (at) accent on last syllable, *u* (cut), *wah* (at), *wuh* (cut), *ha* (at). The 'hunger cry' has generally a well marked rhythm, the first syllable of preliminary sound coming on the first part of the first beat, the second or accented syllable on the second part of the first beat and a quick intake of breath as the third beat. This measure is most often repeated in groups of 5 or 6, each slightly more forceful than the preceding ones until the fourth or fifth, the last one being softer. Thus also will the groups be repeated. Each measure is also a trifle higher in pitch than the one preceding.

Sounds Heard during the First 30 Days.—Consonant sounds commonly heard are *m* in conjunction with *a* as *ma* (at), *n* as *nga* (nat), *g* as in *gah*, *h* as in *ha* (at), *w* in *wah* (at), *r* as in *rah* (at), *r* as in *burr*, very slight sound, and *y* as in *yah* (at).

Vowel sounds are *o* as in owl, *e* as in feel, *oo* as in pool, *a* as in an, and *a* as in father (relatively rare).

Of interest was the variety of animal cries simulated in the nursery. The 'pot-rack' of the quail, the cry of the goat, the whine of the young pig, and the wail of the wild cat, each had a close imitation.

Pelvic

5. *Erection of Penis.*—Subjects Le S. and R. had an approximate erection of 50° at birth, Subject F. 80°, and Subject Sch. at birth, a complete erection against the stomach wall. These babies were, of course, lying on their backs. Subject M. at 4 days, also lying on back, had complete erection.

6. *Voiding of Urine.*—This occurs at birth in about 30 per cent. of the male infants observed.

7. *Defecation.*—This of course occurs often before birth, as the condition of the amniotic fluid shows. It is also, possibly, an occasional cause of suffocation, which would seem to indicate that it occurs some time before birth. In Subjects S. and R. it was persistent and profuse immediately after birth. The passage is of course the usual meconium. If it does not occur then the usual time is from 18 to 24 hours. No norms were kept, however. When the temperature thermometer is pressed into the anus there is very often a resultant passage of feces. On five sets of observations the results were: (*a*) 7 positive, 8 negative, (*b*) 4 positive, 15 negative, (*c*) 13 positive, 11 negative, (*d*) 5 positive, 4 negative, (*e*) 8 positive, 16 negative, making out of 91 observations a *total* of 37 positive and 54 negative.

Ocular

8. *Eye-movements.*—While an inequality of eye-movements is not uncommon it is not the rule. Subject J., 5,600 gr. birth weight, often had a hint of 'cross-eyes.' At 25 days he occasionally had an unusual condition—his eyes being entirely open he would drop the balls in such a way as to show much more of the sclerotic coat above one pupil than the other. In other subjects, 6, 7, 10, 11 and 15 days of age, the sclerotic was noted as visible above the iris. Subject H., whose entire system of reflexes was markedly different (the one baby examined whose hands would not clasp on a small round rod), had peculiarly symmetrical use of his eyes, but his eyes were never seen to fixate the light nor to follow a hand moved in front of him. Subject W.'s eyes worked independently: starting to the left in unison the right eye would move more rapidly than the left, and starting to the right the left eye would move more rapidly than the right. A large percentage of the babies fixated on the light at birth. Subject S. continued to do so when removed to the side of the room. When we turned her head away she turned it back at once, bringing the light into her line of vision. She would also fixate on the figure of a person who interrupted the light. At 8 days of age she would gaze steadily at the electric light in the nursery, rolling her eyes up, when a change of position necessitated it, keeping the light in her line of vision. An interesting 'by-product' was the persistent paddling of her hands at such times and a recurrent spasmodic smile. Subject M. at birth gazed without blinking until her face was covered. Subject L., 34 days old, held by mistake in bright sunlight, first shut her eyes, then opened them, fixating directly on the sun. Her pupils contracted to the size of pin heads. At 30 days of age she would, when carried down the corridor, gaze at a high light on the painted wall, arching her back and finally throwing back her head as she was carried away. She would also, while holding her head still, follow the figure of the nurse around the room. At 32 days of age her eyes caught a red velvet bag held in bright sunlight, and followed it, with her head held still,

around the nearly complete half circle of her vision. The eyes of many of the infants followed a slowly moving hand a few moments after birth. Subject S. would follow a hand moved slowly 8 or 9 inches from her face without any accompanying movement of the head.

Subjects S., A., M., F., and J. gazed at the light above the birth bed and also followed a moving hand. Subjects F. and K., neither of whom gazed at light or followed hand at birth, were seen to do both on the 8th day. Subject K. at 8 days, Subject R. at 10, and Subject L. at 26 days focused first on one and then another face. Subject S., 13 days, fixated a large paper bag which was being handled. A dim light moved slowly at half a meter was followed by subjects 8 hours, 18 hours, 36 hours, and 3, 4, 5, 6, 14, 15, 21, and 30 days of age. Subjects which did not follow were aged 9 hours, 3, 5, and 14 days; 17 in all were tested.

Preyer observed in two cases, 11 and 13 days, a turning of the eyes from one bright light to another. Miss Shinn's niece[1] fixated her eyes on a face first at 25 days. Preyer, Rochlman, and Wilkowski observed 'real fixation' on the tenth day. Dearborn[2] notes seeing the eyes of a child 13 days old follow a slow-moving desk light at 18 inches, and says had he the courage of his convictions he should say he saw the baby's eyes follow a hand slowly on the first day. Preyer's baby followed a desk light at one meter at 23 days, much to his surprise, as he says "other children do not follow a moving light until after many months." According to him, however, Lacy saw this the second week and Darwin the 45th day—but a majority of investigators say the 5th, 6th and 7th days.

9. *Eyes during Nursing.*—These babies, as a rule, had their eyes open at the beginning of nursing. As they began to get enough food the eyes would close. Some babies, however, were most active after nursing.

The following observations were made:

[1] Shinn, Millicent, 'Notes on the Development of a Child,' Book No. 1, p. 30.
[2] Dearborn, G. V. N. 'Moto-Sensory Development.'

Subject	O. K.,	5	days	old	................right eye open, left shut,
"	H.	3	"	"	.................eyes partially closed,
"	R.	7	"	"	.................near end of feeding, shut,
"	W.	7	"	"	.................near end of feeding, shut,
"	R.	14	"	"	.................both eyes open,

and subjects 4, 5, 5, 5, 9, 15, 20, 21, and 25 days old with eyes open. Miss Shinn[1] says: "I have watched and enquired about several other babies and found none that nursed with eyes open."

Facial

10. *Tears*.—On the testimony of Miss Liphart, Subject P. cried tears at birth. Subject C., whom I saw, had tears in her eyes at ten minutes after birth. As silver nitrate is administered as a routine measure some time during the first two hours, a reliable record of the activity of these glands was not obtainable. Subject B., birth weight 5,600 gr., at 4 days cried profusely with right eye. He was an overdue baby, breach delivery and with, consequently, no head moulding. Subject B., an exceedingly cross-eyed baby, cried tears on his 6th day. They were not observed in him again, however, in the remaining 4 days he was in the institution. Subject S. showed dampness in corner of eyes, uniformly, on the 13th day, Subject L. on the 15th, and crying with tears seemed well developed with her on the 34th day. The first average normal show of dampness was 13 to 16 days. The first regular flow of tears 28 to 34.

11. *Smiles*.—Spontaneous smiles at an early age are rare. Subject S., 4 days, smiled spontaneously while at the breast after feeding; Subject O'K., 7 days, at end of bottle feeding; Subject S., 13 days, while looking at bright light; Subject K., 8 days, smiled and immediately regurgitated; Subject C., 28 days, smiled repeatedly after feeding at the breast, but the first smile had followed a light touch of the mother's nipple on his cheek. Subjects 1, 2, 4, and 6 days old smiled slightly to a tickling touch under the chin when awake and comfortable.

12. *Facial Expression*.—According to Preyer, the corners of the mouth are not drawn down until the eighteenth week.

[1] Shinn, *op. cit.*, Book 1, p. 12.

He quotes Darwin as noting it in the 6th week to the 2d and 3d month. Subject M., 10 minutes, pulled the corners of her mouth down. Subject S., 30 minutes, rolled the lower lip so as to show the inside surface, pulled the corners of his mouth down, and after remaining in this typical pouting attitude for a minute started crying. Subject J. did this constantly as a preliminary to crying, sometimes accompanying it with a whimper-like grunt which made him seem remarkably mature. He had also the square box-shape mouth in crying, as had Subject L. on the 7th day and again on the 14th, 17th, and 23d days.

Subject G., 6 days, had a horizontal line directly across his nose from corner to corner of his eyes. In crying he would pull down the inner corners of his brows making wrinkles that radiated from the horizontal cross line. Subject C. at birth had the horizontal line between his eyes. Horizontal lines across the forehead are not exceptional when an infant looks up. It was seen in Subject M., 6 days, Subject P., 15, 19, 21, and Subject L., 7th day, and often thereafter.

Head

13. *Turning Head (when Lying on Face)*.—The ability to turn the face (when the nose is firmly planted) in such a manner as to get air is a disputed point. Preyer[1] says: "Newborn children cannot so much as free the face by turning the head when one lays them on a pillow with face downward." In making the experiment I used a pillow only once, but as I had no way of ascertaining how hard the pillow was which Preyer had used I made my tests on the lap of a nurse and tallied each test on a hard table. In the one case of the pillow a female child of 3 days freed her face at once and held her head upright for several seconds. The tests made on lap and table did not differ in results nor did those on the birth bed. Subject C., 30 minutes old, rotated her head in such a way as to leave her mouth and nose free. Subject G., 1½ hours, Subject B., 1½ hours, and Subject M., 9 hours, could do equally well. On one occasion 15 healthy babies

[1] 'The Mind of the Child,' p. 266.

were tested and all were positive but one—Subject G., 8 days old, who lay until turned. Subject L., 8 days old, not only turned her head but raised it in the air also, entirely unsupported, and held it so for 30 seconds by stop-watch, when she was interrupted. At 15 days the same baby held her head so for 5 minutes. This test was repeated time and time again. Even the sickest babies could do it when the air supply was cut off.

14. *Holding Up the Head (when Held in Upright Position).*—Preyer[1] says: "During the first ten weeks no trace could be discovered, in the case of my boy, of an attempt to hold the head in equilibrium." "In this important step is expressed an unquestionable, vigorous act of will." There has been some confusion among the different writers, due I think to the fact that the exact test conditions were not given. In my tests the subjects were held on an open lap, not against the body, were supported back and front for two inches above the umbilicus. Subjects 2, 6, 7, 10, 10, 11, 13, 13, and 15 days old could support their heads for times varying from 1 to 6 seconds. Subject L., 22 days, held her head quite erect for 3 minutes, turning it from right to left. Subject F. made numerous attempts with only the shortest periods of success. Subject Y., 22 days old, badly underfed and weak, made no apparent effort. Subject W., 22 days old, child of retarded parents who is also weak and undernourished, failed likewise. Any weakness in the neck muscles must be in those which draw the head forward, as numerous and successful attempts were made on the ability to lift the head when laid on the face (see *turning head*, when lying on face).

Arm and Hand

15. *Hand Movements at Birth.*—Subject E. at birth spread his fingers and closed his hand, repeating this 4 consecutive times. Subject S. stretched his fingers with first joint of first, second and third fingers bent. This was with both hands at the same time.

16. *Grasping Reflex.*—Reflex closing of the hand to a

[1] 'The Mind of the Child,' p. 264.

touch on the palm has been noted a very few moments after birth as in the case of Subject C. who grasped the artery clamps which held his umbilical cord. It was seen in Subject K.'s gripping of the doctor's finger to the degree of slowing up the entire operation of caring for his umbilical cord. Experimentally of course the first step was putting a small rod into the hands of the infant to excite the reflex and then to lift the rod so that the full weight would be supported. My apparatus was a small skirt hanger with a rod about a quarter of an inch in diameter bound with a very thin wrapping of gauze. The prevailing custom of oiling a baby at birth was counteracted by giving the hands of the baby a good cleansing with alcohol. This reflex has been described as one which is most pronounced at birth and which diminishes rapidly afterwards. I have not found this to be so. For whereas a baby inside of the first few hours of life will cling 1 or 2 seconds, Subject Sm. on her 23d day, weighing 4,260 grams, held on for 20 seconds and was then laid down still gripping the rod. This reaction is very hard to elicit from a quiet baby, very easy from a crying one, and the crying when set up has the sound of what is called in the nursery an 'angry cry.'[1] Subject A., 20 hours old, clung 2 seconds and 10 seconds, Subject L., at 52 days, supported herself with one hand on the rod for 42 seconds.

Abnormalities either mental or physical affect this reaction less than any other. Subject B. clung tenaciously at 10 days even when life was practically extinct. He would grip a rod put into his hand so strongly that it was necessary to undo his fingers. Subject Q., 5 weeks old, dying of malnutrition, lifted his entire weight and only relaxed on being laid again on a solid support. An interesting by-product was the climbing movement of the legs occasionally noted. But these alternation movements of the legs are also the movements of kicking.

17. *Grasping during Nursing.*—Major found in his child that the finger clasp was firmest when the child was nursing.

[1] Robinson says in "Darwinism and the Nursery" that at the beginning he found that babies had grip and muscle power to sustain their own weight on a horizontal bar, and he adds the comment that they even delight in the process.

My examination of 15 babies before, during the beginning of, and after nursing did not seem conclusive. Subject W. gave definitely harder pulls at the breast, according to the mother, as the pull of my finger in his hand made him grip firmer. One subject's grip was better before nursing; 2 were the same all 3 times; 2 were the same before and during nursing, and poorer after; 1 was negative before and after and only very poor during nursing; 1 pulled only slightly during and after but had about the same grip on both occasions; 1 was definitely better during nursing and was absent after nursing; 4 were best during nursing; and 3 were best after nursing. No one baby was poorer during nursing than before or after, but as will be noted in 3 cases it was no better during nursing than at one of the other trials; and in 4 cases it was definitely poorer than on one of the other trials.

Leg and Foot

18. *Creeping.*—In one case (aged 7 days) there was a creeping movement in which a backward movement of six inches was accomplished in twenty minutes' time. By marking the position of the head at the beginning and end of the period the amount of movement was ascertained. On each occasion the contracting and relaxing seemed mainly of the abdominal and back muscles. The great hindrance to successful movement in each trial seemed to be the insufficiency of the muscles of the arm used to draw the arm from a position on a line parallel with the body to one at right angles to it. The legs often at any time after birth assume the crawling position. The head (see *turning head*) can be held up out of the way, but the relaxed position and relative weakness of the shoulder muscles make forward progress impossible. This condition lasts beyond the 30th day and certainly well into the 3d month, probably longer. An interesting accompaniment of this creeping movement is the opening of the mouth and the bumping of the head, set up, it seems to me, by the well-organized face and mouth reflexes. The accidental touching of cheek and chin to the unyielding surface of the table and his inability to get any

part of it into his mouth, seemed elements in the arousal of his crying.

19. *Foot Movements and Reflexes.*—Subject S., at birth, held the foot rigid, raised all 5 toes uniformly and pulled them down uniformly 4 times. The Babinski reflex was found present in 10 cases tested, and apparently absent in one. Subject S., when tickled on the center of her foot, drew the two outside edges towards each other. The statement is frequently made that babies can cling with their feet. I have never seen this. Placing a fine wire under the toes, a reflex movement downward caught the wire between the pads of fat on the toes and the ball of the palm, but the slightest pull would remove it.

20. *Kicking* and to a less extent moving of the arms is almost continuous for from 15 to 30 minutes after delivery, which period is commonly followed by a sleep of from 6 to 8 hours. The kicking was greatest on the side to which there had been a rotation of the head, this rotation apparently being decided by the position of the child in utero for the last period before birth. For instance Subject M., whose head rotation was to the right contrary to the usual rule, was a remarkably active child, and kicked excessively with her right leg and waved her right arm. Her reflex to the tying of the cord and to alcohol was however with the left side of the body. After delivery it was noted that she had a preferential side, which in this case was the left side, and this preference for the left continued for the term of observation. Her right-side activity immediately after birth might be explained by the cramp of the right side in utero and the relatively free position of the left.

General Responses

21. *Turning Over.*—Subject M. at 10 minutes after birth given a slight, unintentional advantage by the slanting of the mattress on which she lay, turned from her face to her back. Subject T., at 7 days, turned repeatedly from face to back when not impeded by clothing. Placed face downward on an unyielding surface her arms outstretched in line with

her body, she would immediately start crying. In crying, relaxing and contracting of the legs, arms, abdomen, and back muscles are natural accompaniments. Pulling her knees under her and contracting her muscles generally, she would, when relaxed, have her arms nearer by a fraction of an inch. In 10 minutes, after 9 repetitions of this general maneuver, her arms would be sufficiently near her side so that with a final contraction she would roll over.

22. *Reflex to Stimulation of Umbilical Cord.*—There was apparently a reflex to the clamping, cutting and tying of the umbilical cord. There is no direct nervous connection between this and the body. There are of course nerves in the opening surrounding the cord at the end where it is attached to the skin of the abdomen. The findings were not uniform. I should say that the deliveries which I witnessed were divided between three surgeons. Subject L.–1 straightened leg at clamping of cord with artery clamp. Subject L.–2 (twin to L.–1) same; Subject S. straightened leg to tying, not to clamping. Subject C. kicked at tying of cord, no reflex to clamping; Subject B. kicked lustily at tying at 1½ hours (I did not see clamping); Subject R., no reaction to clamp, positive to tying; Subject R., positive to tying; Subject S., positive to clamp only. Three babies kicked vigorously when clamping and tying were done; Subject F. and Subject M. were negative to both. The only explanation was that in clamping and tying there was some pulling and the reaction was from nerves lying next to the umbilical opening. One of the surgeons kindly undertook to hold the cord much laxer in all the operation than even the technique called for. There was no reaction to clamping but a distinct doubling up of the legs to the tight tying of the tape.

23. *To Dropping.*—The best example of this reaction was mentioned in the section on grasping. Watson and Morgan[1] have described dropping as a stimulus to fear as follows: "To suddenly remove from them all means of support, as

[1] 'Emotional Reaction and Psychological Experimentation,' by John B. Watson and J. J. B. Morgan, *Amer. J. Psychol.*, 1917.

when one drops them from the hand and allows them to be caught by the assistant, the child being held over a bed on which has been placed a feather pillow." Additional evidence of reaction to dropping appeared in the course of this study. On 3 occasions when the box in which newborn babies lay was suddenly lowered there was a marked movement of the arms and in one case brief holding of the breath. This same thing occurs repeatedly when the infants are being weighed. After the removal of a big weight the scale pan in which the infant lies may drop suddenly; the result in many cases was the same upward movement of the arms. In this case the jar of the pan hitting the support is to be taken into consideration.

24. *Stretching.*—This movement varies from the mere full raising of the arms and a complete stretching of the legs and toes, to arching of the back and abdomen and pushing the arms until they trembled, accompanied by the bending of the end flanges of the fingers. There was in one case a movement of the neck and pulling forward of the shoulders. The inferior maxillary was drawn inward and downward, giving a most adult cast to the countenance. Infants stretch with the greatest freedom at the removal of the clothing and especially at the removal of the diapers now in use. Subject L., 25 days, crying for a late feeding and very wet, was hushed at once by being held to the fire with feet uncovered. Here she stretched her legs into muscular combination after combination and finally juxtaposed the palms of her feet and went to sleep. She accompanied every marked change of position and tension of the muscles with an opening of the mouth, and each relaxing with a closing of it. Subject S. stretched on being put with a bright light in her line of vision. This was repeated 6 times, each time giving the same result.

25. *Response to Sound Stimuli.*—The reaction to sounds immediately after birth is unusual. Subject M., lying in a high-walled tin box, on a metal table, resting on tiles set in cement, reacted vigorously when one of the doctors in passing hit a metal stool, a foot or so away, with the heavy door. The

baby was covered with a blanket. The stool did not hit the table, and the only vibrations reaching him were through the tiled floor or through the usual air vibrations. He threw his arms forward forcibly enough to dislodge the covering, at the same time moving his entire body. It seems reasonable to conclude that his reaction was to sound. At two days this baby jumped similarly when a metal tray was hit with a fountain pen at a distance of one meter. The tray was held stationary. Three other subjects the same number of days old gave no response. Subject H., 6 days old, batted his eyes when metal disk was struck with a hammer behind his head. She responded thus the first two times, and the third time she did not respond. Subject H., 6 days old, and Subject T., 5 days old, were tested with tuning forks. The forks were struck and the boxes held opposite the ear of each baby when they were crying. There seemed to be a slight diminution in the crying each time. Subject L., also, at 7 days, seemed to be quieted by the big forks. Subject L., 20 days, and Subject H., 16 days, did not react to the Galton whistle at any pitch. Subject P., 29 days old, turned her face 5 times in the direction in which I was whistling softly—about half a meter from her head. There were several elements to be considered, however, as I was sitting on the side of her next an open fire, and, also, my breath may have touched her face. Her general movements, her expression, and the immediateness with which she turned each time as soon as I began whistling, incline me to believe that there was localization as well as auditory response. She was also seen to turn three times in the direction of the basin into which water had suddenly and forcibly been turned. Subject S., 13 days, as mentioned under eye-movements, turned at once in the direction of a heavy paper bag which was being folded. Her face was away from the bag, turned towards a mild fire, and her movement in turning was decisive, her eyes catching the bag unhesitatingly and holding it steadily. It was a very interesting example of the coordination of audition and vision. Repeated experiments showed that babies showing no reaction to other sounds would react positively to these rattling paper bags. In 13 trials with subjects 4 to 14 days old, 12 were positive. Four were hushed in crying, 4 turned in the direction of the bag, and 2 in the opposite direction, but since the movement of body and eyes was markedly different to what it was when the experiment started, those turning away were considered as giving a positive reaction. Subject B., on the second day, would jump at a whistle or the sudden sound of a voice on the far side of the room. The sudden dropping of the scales, 6 feet away, would cause marked convulsive movements. This condition persisted until about the 4th day, when his response gradually became less marked, until on the 7th he would not respond at all.

Response to Noxious Stimuli

26. *To Deep Pricking of Big Toe.*—For laboratory purposes it was necessary to draw blood from 3 babies at 2 times. Two of the babies were pricked 2 times on the left big toe, one once on the left and once on the right. At each time the other foot went up at once with a pushing motion against the other ankle. As this motion is also one of the motions of kicking no conclusion could be drawn. There was no pronounced cry but one of them was crying when taken up, and all were given, between the two operations, a lifting test which always elicits crying.

27. *To Lancing of Infected Finger.*—Subject Le S., crying with hunger, had to undergo the lancing of a badly swollen and infected finger. It was necessary also to hold the arms firmly, which very uniformly has the effect of making the subjects cry. His cry during the lancing and cleaning of the finger continued to be of the same character, but much exaggerated.

28. *Circumcision.*—Unfortunately the time selected for circumcision was immediately before a feeding. The element of hunger had therefore to be taken into consideration as well as that of pain. A small gauze sponge dipped in whiskey and sugar or wine and sugar was given the baby by the Hebrew physician who performed the operation. The

crying seemed loudest while preparation was being made, stopped slightly at handling of the penis, began again at manipulation of the foreskin, and was slightly accelerated after the cutting. The babies were in every case quiet during the ceremony which followed, but this was insured by the amount of alcohol administered.

Response to Dermal Stimuli

29. *To Prick on Wrist.*—On one occasion I tested 21 sleeping infants by lightly pricking the wrist. I attempted to use the same pressure on the pin each time, making it just hard enough so that when tried on my own wrist it aroused a pain response. Thirteen gave a movement of the hand or forearm in response to the pin touch and 8 did not respond. Subjects giving positive results were 2, 2, 2, 3, 6, 7, 8, 8, 8, 16, 17, 17, 19 days old. Those giving negative were 2, 2, 3, 6, 6, 8, 9, and 26. As an interesting note on the experiment it is worth mentioning that of the 8 giving no response 6 were the children of either one or two defective parents.

30. *To Being Rubbed.*—The cleaning of the newborn with oil affords a most interesting opportunity for the observation of dermal and deep pressure reactions. The rubbing of the head, and especially the rubbing of the back, brings out the most active reactions which in all probability the baby will be called on to make in the first month of life. The screaming which invariably ensued was of such a volume as is hardly attained in the first 6 weeks under ordinary conditions. During the scrubbing of the head 15 of the babies which I saw delivered impeded the progress of the operator to a greater or less degree by a constant moving of the hands. Subject K. caught the hands of the operator through the whole of the cleaning process. Since the vernix caseosa is most heavily deposited on the back, rubbing the back brings out a response of the lustiest crying, and in 3 cases, when the infant was supported by the left hand of the surgeon in a crawling position with hands and knees touching the bed, crawling movements were elicited. In neither of these cases of course is the stimulus entirely dermal,

since a good deal of pressure is exerted on the muscles and deeper structure; and certainly the squeezing of the body as well as the pressure on the unattached skull bones undoubtedly introduced kinæsthetic factors. The cry elicited is identical with the 'angry cry' of the nursery and is in fact so called by a great many operators and nurses.

Subject F., 13 days, raised her right arm at each of seven strokes of her nose, but did not approach the hand to the nose. Subject S., at 30 minutes, moved both hands to her nose when it was held, but on the second trial moved them in the opposite direction.

31. *To Dampness.*—Immediately after the voiding of urine the napkin has a higher temperature than usual. After a certain length of time the wet diaper becomes colder than usual. When the element of temperature is eliminated there is little evidence to show that babies up to 6 weeks show any particular response to wetness or dryness of their diapers. In a nursery in which there were 16 wet babies, 10 were quiet and 6 crying. A few minutes after changing, 10 were crying and 6 were quiet. Substantially the same results were obtained in another group of 19 infants. In the course of these observations enough cases have been investigated to show that it is extremely difficult to predict whether a baby is wet or dry because of its cry. There are nursery saws to the effect that babies 'cry because they are wet' and are still 'because they are dry.' Rounds of the nursery will often show that neither conclusion is justified, as babies under 30 days void frequently, and out of a nursery of 25, 15 to 20 wet babies can be found at any time within 30 minutes of changing. It is not uncommon to have them void immediately on changing, stimulated perhaps by the coolness of the fresh diaper. Subjects 4, 6, 10, and 14 days old were on one occasion crying and wet, but the diapers were quite cold to the touch as well, and subjects 4, 5, 9, 11, and 21 were wet and quiet and the diapers seemed about body temperature.

32. *To Warmth and Cool.*—Preyer[1] says: "Sensibility to

[1] 'The Mind of the Child,' p. 183.

contact is, in the first hour of life, much inferior to what it is later; the sense of temperature does not exist." I have noted 6 occasions on which there was marked shivering within 15 minutes of birth, on two occasions it persisted until the infant had been brought near to a hot water bottle for some time. The skin of this baby was cold to the touch. The shivering was so pronounced as to make a marked movement of the toothless lower jaw which under other conditions we call 'chattering.' The reaction to warmth during the second day of life was marked. Uncovering the lower part of the infant's body to the heat of a mild fire caused curling of the toes both upward and downward. Subject L., 7 days, when she had been some time with the outside of her right leg next a rather hot fire, crossed it over her left and thus away from the fire. Subject S., at 9 days, when placed in a warm tub, moved her arms outward and downward, palms backward, elbows straightened, and on the forward movement, bent—much as in swimming. She opened her mouth wide, with her tongue over the surface of her lower lip; her breathing became noisy and accelerated, the air being forced out of her nose in what might be called a snort.

33. *Alcohol.*—An excellent test of the reaction to cold of the skin of the very young was offered by the application of alcohol dressings to the stump of the umbilical cord. The surgeons delivering obligingly dropped small amounts where I indicated. When dropped on the lower half of the abdomen kicking resulted uniformly; dropped on the lower left part of the abdomen the left leg would respond; on the lower right, right leg; lower center, both legs or either right or left. Above the umbilicus the reaction was not so certain but when obtained was a movement of the legs.

Response to Kinæsthetic Stimuli

34. *Kinæsthetic Sense.*—Dearborn[1] says: "It is one of the anomalies of psychology and of physiology, that kinæsthesia, most basal and important of the senses, has been so

[1] 'Moto-sensory Development,' p. 1.

relatively neglected. Without it no infant could become more humanly efficient than a plant." This sense is probably the earliest of all developed, coming as may reasonably be supposed before kicking does in the fourth or fifth months of life in utero. Miss Shinn[1] refers, under Rhythm, to the 'superior quieting influence of a monotonous jarring as compared with a smooth motion.' Experiments on placing infants to the breast to quiet them were made as early as the third day of life, and as the amount of milk gotten up to that time is negligible it is fair to assume that changes in the position or pressure was the cause of the resulting quiet. Walking with the babies quieted them as early as the first day. Ten subjects under 4 days responded by ceasing to cry. One of these was crying with every symptom of abdominal pain. It was noted that babies crying in the corridor were exceedingly rare and the approximate number of trips daily through the corridor with infants was 120. Of the many dozens of trips made to and from the laboratory only one child was known to cry the entire distance; one cried the first third of the way and was quieted. Subjects at 6 hours, 18 hours, 1, 3, 3, 4, 6, 6, 7, 7, 9, 11, 14, and 18 days who were crying in their cribs were quieted by lifting and gentle pressure. Subjects at 10 hours, 4, 6, and 9 days were quieted by pressing the infant's body between the hands. Subjects 1, 3, and 6 days were quieted by pressure on chest. Subjects at 6, 8, and 10 days were quieted by firm pressure around head.

Reflexes Connected with Feeding

35. *Cheek and Chin.*—This test was a light touch on either cheek in a straight line parallel with the mouth and about an inch removed from the corners, and a light touch on the chin exactly above the center. The subjects giving a positive reaction moved their mouth in the direction of the touch. Subject M., 5 hours, positive; Subjects Mon., 5½ hours, K., at 7 hours, H., at 8½, and D., at 9 hours, were all positive. Subject F. at 9 hours was positive to right cheek, negative

[1] Note book II., Miss Milicent Shinn.

(did not react to touch) on left cheek and chin. Subject B. (microcephalic in dying condition), 10 days, positive to right cheek and negative to left and chin. Two hours later Subject H. (positive in above list) gave a complete negative when in a very deep sleep. After feeding this reaction is hard to elicit. During hunger it is easy and the infant often moves with surprising quickness, catching the testing finger in its mouth. One infant giving a positive reaction at 5 hours showed a complete negative at 7. Inquiry showed that through a mistake in routine the baby had been put to the breast in the interim.

36. *Lip Reflex* (*Thompson's*).[1]—This is gotten by tapping lightly with the tip of the finger below or above the corner of the mouth of a sleeping baby. The result is a closing and pouting of the lips into a nursing position. Tests were made on 14 sleeping babies an hour before feeding. The most marked result was Subject R., 7 days, who pursed his lips, protruded the tip of his tongue between them and sucked it vigorously. Subjects 16 hours, 4, 5, 7, 9, 10, 11, 19, and 21 days old pursed the lips and protruded the tongue but did not suckle. Subject M., 9 days, pulled his head back quite markedly and rolled out his under lip. There was no suggestion of pursed lips or suckling. Two subjects at 10 days jerked the head slightly but gave no other response. Subject Y. at 19 days gave no reaction. This baby was markedly malformed and the child of retarded parents.

37. *Tongue Reflexes*, at birth and later. Movements similar to the movements and position of the tongue and lips in the lip reflex (Thompson) may be seen immediately after birth. These movements which appear to be merely unrelated and random are related to suckling. The tongue protrudes from the mouth and the edges are curled upward and over in such a way as to make the partial vacuum essential to sucking, much easier than if the back part of the tongue, only, was depended on. The use of the muscles in the back of the mouth apparently requires a higher type

[1] Thompson, John, 'On the Lip-reflex of Newborn Children,' *Rev. of Neurology and Psychiatry*, Vol. I., 1903, p. 145.

of coördination (see suckling and swallowing below). The forming of the tongue into this protruding tube can be seen in infants up to 30 days, at the time of feeding. If the food is removed before satiety is reached there is often an active sucking of the protruded tongue.

38. *Sucking Fingers.*—This seems to vary mainly with the manner in which the fingers are gotten into the mouth. Subject M. at 20 minutes put his thumb directly into his mouth and began sucking it. Subject S. during the first 2 hours of life put his right first finger into his mouth 6 times. His action was not fumbling. He touched his face either below or above his mouth, which set up the sucking reflex noted under lip reflexes (36) and the finger was pulled directly into the mouth. Once the sucking had been started there was rarely any fumbling. Three exceptions to this rule also showed weak swallowing reflexes and each of them had either one or two retarded parents. Subject S., child of imbecile mother and retarded father, attempted for 15 minutes to get her finger in her mouth, but did not succeed. These babies' mouth reflexes are given more fully under swallowing and suckling below. Subject G., a 'blue baby' two hours old, put his fingers directly into his mouth. Subject D., 2 days old, Cæsarian delivery, very feeble, was seen sucking two fingers so vigorously that it required a decided effort to remove them. She put them back in at once without trouble. She was also seen with all 4 fingers in her mouth. Subject B., a mal-formed baby mentioned elsewhere, 10 days old, in dying condition, put his finger in his mouth after 4 trials. Both his sucking and swallowing reflexes were moderately good. The former were better than the latter.

39. *Suckling and Swallowing.*—A demonstration of the presence of the ability to suckle immediately after birth was attended by many practical difficulties on account of the sterile operating field surrounding the subject. A sterile nipple was supplied at each delivery and the surgeons very kindly held them in the babies' mouths. As the process of obtaining sterile nipples became difficult on account of some

local conditions the expedient was devised by one of the surgeons of cleansing his gloved hand in sterile water and letting the baby suck his finger. Subject S., immediately after spontaneous cry, was given the nipple. The reflex faintly present was perceived as a slight movement of the lips as in sucking, and as a swallowing movement in the throat. Subject O'K., aged 23 minutes, suckled slightly. Subject Sc., very blue, and with umbilical cord around neck at birth, at 10 minutes sucked well, at 25 minutes sucked so violently that it was necessary to hold the unattached nipple with an artery clamp. Subject Le. S. sucked definitely but not vigorously at 15 minutes. Subject M., 6 minutes, sucked vigorously and at 30 minutes sucked very hard. Subject R. sucked vigorously at 10 minutes. Subject S. sucked definitely at 13 minutes and vigorously at 25. This list continues with little variation through the whole series. Occasionally it was not practicable to get the trial immediately after birth, but in no case did the baby refuse definitely to suckle during the first hour of life. Nothing but the most marked retardation or injury seems to affect this reflex. Swallowing, with sucking, tongue, lip and cheek reflexes go to make up the feeding reflex. The evidence is strongly in favor of the fact that weakness in the ability to swallow is at least suggestive of mental retardation whereas sucking is present at birth in most infants. Interesting in connection with this is the fact that certain forms of retardation which are accompanied by speech defects show a lack of coördination in the same muscles which seem defective in swallowing—namely, the control of the soft palate and back part of the tongue and throat. Going back over this list, Subject S. was the child of healthy and mentally well developed parents. Both her swallowing and sucking reflexes were good. She was dismissed at the end of 10 days and was considered excellent in every way.[1] Subject O'K., child of intelligent parents, dismissed in 14 days, rated excellent. Subject Sc. sucked violently at nipple but did not swallow well and was not

[1] A discharge was not given these patients until the best possible feeding habits had been established.

dismissed until 21 days old with a rating of moderately good. Her mother was a high-grade moron. Subject L., whose mother was graded as 12 years by the Binet-Simon intelligence test, was dismissed at 18 days. This infant's swallowing reflexes were very poor. Subject M., child of mother rated as 10 years mental age and who also showed a marked tendency to certain psychopathic conditions, fed moderately well, but at 2 months is not thriving as well as at 2 weeks. Subject R. (child of deaf and dumb primipara of 32, who, on account of her deafness, was not given a mental test, but whose practical ability for learning was poor and who showed a marked tendency to cruelty to her baby) left at the end of 3 weeks, her swallowing still faulty, with a grading of poor. Subject S. whose sucking at birth was vigorous enough left at the end of 3 weeks without having learned to swallow at all. The only food that this baby obtained was that which went down when it was carefully moved in such a way as to assist gravity. This baby sucked actively but ineffectually with his lips. At 2 months he was little more than a skeleton and could not swallow. The mother of this child was a low-grade imbecile who could not even put on her own clothing and who had to be fed with a dull-edged spoon. Subject P. (not on the list above) who possessed the sucking reflex, but in whom the habit of regurgitation was most marked, had an intelligent mother but the father was cruel, a drunkard and an habitual deserter, who had never been able to finish the lower grades at school. Twin babies K., whose mother's mental age was 10, had to be fed with 'Boston' feeders which push the milk into the mouth. It was exceedingly hard to get them to swallow. Subject K. remained in the hospital 2 months. He was the child of incestuous relations, the mother a low-grade imbecile and the father the same. In this baby even the sucking reflexes were absent and tube feeding was necessary. At 4 months he was just alive. Subjects W., Y., and K., whose swallowing reflexes were exceedingly weak, had each been in the hospital more than the usual time. Each regurgitated constantly. The mothers of these three did not exceed 10 years mental age.

Of value, in this connection, will be a study of the eating and speech habits of older children of retarded mentality.

Social Behavior

40. As the subjects were often found crying in groups, observation was made to determine social influence as an element. Note was made of each baby that cried whether in a group or singly through a number of hunger periods. There were 28 in groups of two or more, and 35 who cried singly. In observation I., two babies were kept together in a quiet room, and both slept. In observation II., Subject W., 10 days, slept. Subject Sm., 7 days, cried 20 out of the 60 minutes. Finding nursery conditions not under control, graphophone records were made of Subject L. when crying with hunger. These records were then played for 6 infants from 1 to 14 days of age. The results were negative. In observation IX., Subject L., 8 days old, and Subject W., 9 days old, were placed on a couch so that the conditions would resemble the nursery with regard to vibrations. Subject W. cried but Subject L. remained quiet, and later Subject L. cried and Subject W. remained quiet (and awake). Observation X., Subject M., 7 days old, quiet and awake (during graphophone record) made some suckling movements with mouth and tongue and his breathing became a trifle irregular—but afterwards, in an interval of quiet he made the same movements. Result negative. Observation XI., a final trial was made: smoked drum records of breathing were taken on several infants while the phonograph crying record was being run through. The resulting breathing curves differed in no way from those made of breathing in a quiet dark room. Conclusion: there seems to be no positive evidence of social influence of this character on babies under 15 days of age.

Summary of Results

In summing up the observations it will be seen that the reflex and instinctive equipment of the child at birth is more complex and advanced than has hitherto been thought. This discrepancy is due perhaps to the rarity of the opportunity for unrelated persons other than nurses or physicians to observe during this period.

During the first twenty minutes of life may be observed sneezing, yawning, tears, sucking at nipple, fixating on light, putting thumb in mouth, jumping to loud sounds, grasping, crying with box-shaped mouth, crying with the corners of the mouth pulled down, following a moving hand with the eyes, turning of the head in such a way as to get air when placed on the face, turning over when given a very slight advantage, complete erection of penis, and most indicative, perhaps, the cry of so-called anger immediately after birth, justifying perhaps Kant's oft denied statement that the cry of a child just born has not the tone of lamentation but of aroused wrath.

The responses of the child, under twenty-four hours, to pressure and the completeness and effectiveness of the food reflexes as well as the value of the response to kinæsthetic stimuli to this complex of food reflexes are most interesting, and the possibility of a direct relation between the intelligence of the parent and the swallowing ability of the child hinted at in topic 39 suggests a field for further research which would be of direct value to those interested in the development of speech.

INDEX OF REFLEXES (Topic Numbers)

DISCUSSION

A CRITIQUE OF THE YERKES–BRIDGES–HARDWICK COMPARISON OF THE BINET–SIMON AND POINT SCALES[1]

The readers of this journal are doubtless well enough acquainted with the Yerkes-Bridges Point Scale to make unnecessary an extended descriptive account. It consists of twenty tests, nineteen of which are selected or adapted from the Binet-Simon series, but arranged, like the 1905 Binet series, in general order of difficulty, without being grouped by ages. The score is given by points, so many points being allowed for success in each test or part of test. Much emphasis is laid on the fact that degrees of success in meeting the several tests are distinguished, and some credit given for lesser degrees of success, as distinguished from the so-called 'all or none' procedure of the Binet-Simon Scale. The highest score which can be obtained is 100. The score of any individual is to be interpreted by comparison with a norm which has been determined by the examination of a group of individuals of the same sex, race and social level. Further details will be commented on in connection with the comparison between this scale and the Binet-Simon Scale.

Since the justification of the Point Scale rests confessedly on a criticism of the Binet-Simon Scale, and since this criticism is severe, and if it is accepted, must cause the abandonment of the Binet-Simon Scale, it is worth while to consider this criticism in detail and follow out the comparison of the two scales.

The criticisms of the Binet-Simon Scale are by implication statements of advantages of the Point Scale, and the advantages of the Point Scale are by implication faults in the Binet-Simon Scale; hence we may bring under the same head the disadvantages of the one and the advantages of the other. They make the following long list:

1. *The Binet-Simon Scale falsely assumes that the mental development of all individuals proceeds by similar stages.*

[1] 'A Point Scale for Measuring Mental Ability,' by Robert M. Yerkes, James W. Bridges and Rose S. Hardwick. Baltimore, Warwick and York. 1915.

There has been a good deal of criticism of the Binet-Simon Scale on the score of the assumptions which the critics find to underlie it. The authors of the Binet-Simon Scale do not tell us that these assumptions are involved, and even if some such assumptions as are laid down do seem to be involved it is most natural to regard them as working assumptions and the scale as a working instrument rather than a highly precise and accurate engine to apply a precisely known law. It is perhaps no exaggeration to say that the Binet-Simon Scale and the results of its application has given us more information regarding the actual course of intellectual development than all the other testing of children put together.

But take the assumptions themselves. Must not any scale which uses age norms make some such assumption as is indicated in the first criticism? The very purpose of the scale is to measure the deviation of an individual from a norm, and if there is no norm the deviation has no significance. As a matter of fact, the Point Scale makes this assumption in exactly the same degree as does the Binet-Simon Scale.

2. *The Binet-Simon Scale falsely assumes that the correlation between different functions is the same for all individuals at a given stage.*

If no flexibility in scoring were allowed in the Binet-Simon Scale the second objection would have some force, for there would then be no opportunity to make up for a deficiency in one mental function by unusual performance in another; but the possibility of gaining advanced credits at least in part overcomes this objection. Besides, the probability is that the correlation in rapidity of development of the different functions is fairly close.

3. *The Binet-Simon Scale falsely assumes that each stage of mental development corresponds in turn to a certain physical age, and that there is a 'correlation between the different functions at different stages of development.'*

Since any norm must be based on physical age, unless the age principle is entirely excluded by finding functions which do not develop with age, it does not seem possible to avoid the third assumption—that is, the first part of it. The second part of the statement is not amplified and the writer is not able to tell what it means.

4. *The Binet-Simon Scale falsely assumes that the mental development of the Paris school children follows the above-mentioned course of development.*

The fourth assumption is not fundamental to the scale, but only to its first form. It can be obviated, and has been obviated by revision.

5. *As a consequence of these false assumptions the Binet-Simon Scale employs the erroneous principle of age grouping of tests.*

It follows from the above statements that the age grouping of tests is not a fundamental point of difference between the Binet-Simon Scale and the Point Scale, since both scales involve the age principle, but that it is only one of the possible devices by which the age principle may be applied. The Binet-Simon Scale uses the age principle in the classification of the tests, and the Point Scale uses it in the construction of the norm. The issue is purely one of convenience. If many norms are needed it is easier to have them independent of the classification of the tests. Otherwise not. This question we shall discuss in a moment.

6. *As a further consequence of the age grouping the Binet-Simon Scale employs the crude all-or-none method of scoring.*

Considerable emphasis is laid on the distinction between the all-or-none scoring of the Binet-Simon Scale and the partial scores of the Point Scale. Here again the issue is not fundamental, and the Binet-Simon not only can, but does use the device of partial scoring by including several grades of difficulty of the same test and putting them at different ages. This is done, for example, with memory, interpretation of pictures, and weight discrimination. The same type of organization could easily be extended without in the slightest degree altering the general structure of the scale. In passing it may be remarked that the Point Scale in its present form assumes that the weighting of the various tests which it adopts is correct without giving it an experimental basis.

7. *Because of the age grouping of tests the Binet-Simon Scale cannot make due allowance for different rates of development due to sex, race and social or educational advantages.*

The bearing of the seventh objection depends on the interpretation which is made of the degree or basis of the sex, racial or social differences. If these differences require different norms the point scale method furnishes an easier device than the age grouping method. But if sex, race and social level all are factors which necessitate separate norms, the task, as we shall see in a moment, is a very considerable one. Let us first ask concerning the necessity. It seems to the writer that, instead of a case being made out for the necessity of sex norms, the results of the application of the

Point Scale indicate that no distinction of any clear sort can be made out. The lines which represent the scores cross and recross, and the crossing points in the English-speaking group are different from those of the non-English-speaking group. The race comparison which is reported brought entirely negative results, so far as total scores are concerned. Even if marked race differences had been found, the question might at least be raised whether it does not suit the practical purpose of an intelligence scale better to measure different race groups, at least within the same community, by a single norm than by separate norms. For classification in the school it would seem certainly better; and if feeble-mindedness is conceived in functional terms it would seem better for the selection of feeble-minded individuals also. With reference to social groups the chief question is whether the differences are inherent or the product of environment. If inherent, no separate norm is needed; otherwise it is. The probability is that the differences are at least in part inherent. In so far as they are not, the application of different norms would be exceedingly difficult. How many groups would be distinguished, what would be the basis of the distinction, how would a particular child be placed in the proper group, etc.?

Let us assume that we do have norms for sex, race and social level, as the authors propose, and that we have scales for the four types of mental function. A short calculation will show that the number of norms that would be necessary is sufficient to daunt the hardiest investigator. If we provide for three races only, and for three social levels, no less than seventy-two norms would be necessary. We start with four scales. The sex distinction necessitates eight, the racial distinction twenty-four and the social distinction seventy-two. The derivation of these norms would require the examination of at least seventy-two thousand children.

8. *Since in the Binet-Simon Scale 'later and more difficult tests have no more weight in making up the score than do earlier and easier ones, the same mental age may correspond to records far from equivalent'* (p. 33).

The feature which is criticized in the eighth article is given as one of the chief advantages of the Point Scale. It certainly is true of that scale, and is the direct opposite of the characteristic which is criticized in the second article.

9. *In its present form the Binet-Simon Scale does not give uniform opportunity to the various mental functions at different levels.*

The ninth article is similar to the second, and does hold true of the original Binet-Simon Scale, but is not essential to it.

10. *The Point Scale, as contrasted with the Binet-Simon Scale, is capable of giving results of ever-increasing reliability and precision as data accumulate and norms are established.*

Theoretically, the tenth point is well taken. Practically, the establishment of a norm is not a very extensive process, after the choice of tests has been made, and for practical purposes it is desirable not to defer the establishment of a norm too long. It is probable, for example, that the present Point Scale norm will be used for some time, since ideal programs are frequently long deferred in realization.

11. *There is less influence of the personal equation of the examiner in using the Point Scale.*

If the examiner has a constant bias in the direction either of undue conservatism or liberality in scoring the child's responses it is clear that the error is greater the larger the units which are represented in each judgment, and the larger the number of judgments which are made. A judgment which determined a large share in the child's rating would, if in error, cause a greater displacement of his rank than one which had a small share in his rating. Thus the breaking up of the Binet tests into subsidiary tests in the Point Scale, by making each unit smaller, reduces the error which is due to constant bias in either direction on the part of the examiner. As was pointed out in discussing the all-or-none method of scoring, this difficulty can be overcome in the Binet scheme by breaking up the tests and placing the constituent parts at different appropriate ages. The size of each error being determined by the value of the unit upon which it is based, the size of the total error will be determined by the number of the individual errors. An error can occur only in the case of those tests in which the judgment of the examiner varies from the true judgment. In the Binet Scale the range of such tests is confined to those of a few ages immediately adjacent to the child's mental age. In the Point Scale, because of the partial credit system and the scoring by points, the range of tests from which the child's score is made up is much greater. Hence the number of judgments which are factors in his rating is relatively larger. For this reason there are more opportunities for error which will affect the score. It would appear from these considerations that theoretically—and thus far we have no other basis for judgment—there is one respect in which the Point Scale offers less opportunity for error on account of a constant bias on the part of the examiner, and one in which the Binet Scale, even as at present constituted, offers less opportunity for error. If this (admittedly hazardous) reasoning is correct the situation is about a 'stand-off.'

12. *The Point Scale 'works with a smaller amount of testing material, and thus makes possible a better choice of the same.'*

The twelfth point is not self-evident, particularly since the program of the Point Scale calls for the multiplication of the present number of tests four fold.

13. *The Binet-Simon Scale cannot—and by implication the Point Scale can—diagnose moral imbecility, dementia and intellectual degeneration, and occasional phenomena of degeneracy such as impulsions, obsessions and delirium.*

The present Point Scale cannot diagnose moral imbecility, etc., any better than can the Binet-Simon Scale, and no proposals are made which promise to make such diagnosis possible.

14. *The statistical results of the application of the Binet-Simon Scale are unsatisfactory, and a comparison of the results of the examination of the same 100 cases by the two scales are favorable to the Point Scale.*

The comparison of the statistical results of the application of the two scales to the same 100 children do not seem to the writer to be conclusive. In the first place, these 100 children were part of the group the examination of whom formed the basis of the Point Scale norms, and their Point Scale ratings were to some degree tested by a comparison with themselves. Furthermore, the mode of selection of the cases is not stated, and when the two ratings do not agree we have no means of knowing which is more nearly right. They are apparently not random selections, since only one individual is rated as normal by each scale.

15. *The Point Scale can be developed so as to give differential rating in the different kinds of mental processes, namely, receptivity, imagination, affectivity and thought.*

The development of differential scales is highly desirable, but the essential problem concerning it is the selection of tests, and when this is done they can be arranged either on the age principle or on the point scale principle, after the manner of the present scales.

This review does not attempt to pass judgment on the comparative merits of the two scales, but only to criticize the comparison which is made by the authors of the Point Scale. If this criticism is drastic it is not more so than the criticism of the Binet-Simon Scale on which it comments. These comments may be summarized in the statement that the criticisms of the Binet-Simon Scale, which are made by the authors of the Point Scale are for the most part not essential and may be obviated by revision, or are equally applicable to the Point Scale itself.

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