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THE

Psychological Review

EDITED BY

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JAMES R. ANGELL, University of Chicago (*Editor Psychological Monographs*).

WITH THE CO-OPERATION FOR THIS SECTION OF

Volume XVI., 1909.

THE REVIEW PUBLISHING CO.,
41 NORTH QUEEN ST., LANCASTER, PA.
AND BALTIMORE, MD.

Agents: G. E. STECHERT & CO., London (2 Star Yard, Carey St., W. C.); Leipzig (Hospital St., 10); Paris (76 rue de Rennes); Madrid, D. JORRO (Calle de la Paz, 23).

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PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

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N. S. Vol. XVI. No. 1. January, 1909.

THE PSYCHOLOGICAL REVIEW.

A STUDY OF GALVANOMETRIC DEFLECTIONS DUE TO PSYCHO-PHYSIOLOGICAL PROCESSES. II.

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The following are some of the results obtained:

Table I.

Experiments on Dr. B.

Galvanometer Readings.[1]	Stimulus.
22.10	
22.10	
10	
10	
⋮	← Sudden question: When did your father die? (Intense emotion.)
22.10	
20	
30	
40	
50	
60	
60	
60	
22.55	
50	
45	
40	
30	
20	
22.00	
22.00	
22.00	
22.00	

	Galvanometer Readings.	Stimulus.
No change.	20.20	
	20.20	
	20.20	
	20.20	← Difficult calculation.
	20.20	
	20.20	
	20.20	
	20.20	
	20.20	
	20.20	← Slight start by sudden sound.
	20.30	
	40	
	45	
	45	
	40	
	30	
	30	
	30	
	30	
	20.30	
	22.30	
	30	
	30	
	⋮	← Laughter.

[1] All readings in this and subsequent tables are in centimeters. Horizontal lines indicate the end of the experiment.

Galvanometer Readings.	Stimulus.
22.40	
50	
22.60	
70	
80	
70	
60	
50	
40	
22.35	
22.30	
22.30	

Galvanometer Readings.	Stimulus.
24.80	
80	
80	
80	
⋮	← Pinch.
90	
95	
25.00	
25.10	
10	
05	
25.00	
95	
90	
85	
80	
80	
24.80	

	Galvanometer Readings.	Stimulus.
	⋮	
No change.	15.35	
	35	
	⋮	← Looked at pictures.
	35	
	35	
	35	
	35	
	35	

Galvanometer Readings.	Stimulus.
24.75	
75	
75	
75	
⋮	← Pinch.

Galvanometer Readings.	Stimulus.
24.80	
85	
90	
95	
25.00	
00	
24.95	
90	
85	
80	
75	
70	
70	
24.70	
⋮	

Galvanometer Readings.	Stimulus.
15.40	
40	
40	
⋮	← Pin prick.
45	
50	
55	
60	
65	
70	
75	
80	
80	
70	
65	
60	
55	
50	
45	
15.45	

	Galvanometer Readings.	Stimulus.
No change.	22.30	
	22.30	
	⋮	← Thinking of being pricked.
	22.30	

	Galvanometer Readings.	Stimulus.
No change.	21.45	
	21.45	
	45	
	⋮	← Thinking of father's recent death.
	21.45	

	Galvanometer Readings.	Stimulus.
No change.	21.45	
	45	
	45	← Recalling dream of seeing dead father.
	:	
	21.45	
	21.45	
	45	
	:	← Imagines pleasant experience.
	21.45	

Galvanometer Readings.	Stimulus.
21.45	
45	
:	← Slight laughter.
50	
60	
70	
80	
90	
22.00	

Galvanometer Readings.	Stimulus.
22.00	
:	← Burn with lighted cigarette. (Very painful.)
10	
20	
30	
40	
50	
60	
70	
80	
90	
80	
70	
60	
50	
22.40	
:	

Galv. dropping.
Exp. discontinued.

TABLE II.

EXPERIMENTS ON DR. ST.

Galvanometer Readings.	Stimulus.
17.40	
17.40	
40	
:	← Burn.
45	
50	
55	
60	
65	
65	
60	
55	
50	
50	
:	

Galvanometer Readings.	Stimulus.
18.00	
00	
00	
:	← Dropping weight.
05	
10	
18.15	

Galvanometer Readings.	Stimulus.
18.20	
30	
35	
35	
30	
25	
20	
15	
10	
05	
05	
05	
05	
05	
18.05	

Galvanometer Readings.	Stimulus.
18.30	
30	
30	
:	← Pinch.
35	
40	
18.45	

	Galvanometer Readings.	Stimulus.
	18.50	
	55	
	50	
	45	
	40	
	35	
	35	
	35	
	35	
	35	
	⋮	

	Galvanometer Readings.	Stimulus.
No change.	20.30	
	30	
	⋮	← Thinks of disagreeable experience.
	30	
	30	
	30	
	20.30	← Slight artificial laughter.
	30	
	40	
	50	
	60	
	70	
	80	
	90	
	80	
	70	
	60	
	50	
	⋮	

	Galvanometer Readings.	Stimulus.
No change.	22.60	
	60	
	60	
	⋮	← Multiply 17 × 15.
	60	
	60	
	55	
	55	
	60	
	60	
	60	
	60	
	22.60	

	Galvanometer Readings.	Stimulus.
No change.	22.60	
	60	
	60	
	⋮	← Divide $\frac{1728}{12}$.
	60	
	60	
	60	
	60	
	60	
	60	
	⋮	

	Galvanometer Readings.	Stimulus.
	22.60	
	60	
	60	
	⋮	← Presented mirror to face unexpectedly
	65	
	70	
	75	
	80	
	85	
	90	
	95	
	23.00	
	00	
	22.95	
	90	
	85	
	80	
	75	
	70	
	65	
	60	
	⋮	
	22.60	
	60	

	Galvanometer Readings.	Stimulus.
	23.00	
	00	
	00	
	⋮	← Pinch.
	05	
	10	
	15	
	20	
	23.25	

Galvanometer Readings.	Stimulus.
23.30	
35	
40	
40	
35	
30	
25	
20	
15	
10	
05	
23.00	

Galvanometer Readings.	Stimulus.
22.90	
90	
90	
⋮	← Electric shock.
22.95	
95	
23.00	
05	
10	
15	
15	
10	
05	
23.00	
22.95	
90	
90	
⋮	

Galvanometer Readings.	Stimulus.
22.80	
22.80	
80	
80	
⋮	← Coughed.
85	
90	
95	
23.00	
05	
10	
15	
20	
25	
30	
30	
25	
20	
15	
10	
05	
23.00	
22.90	
80	
22.80	
⋮	

	Galvanometer Readings.	Stimulus.
No change.	22.80	
	80	
	⋮	← Capital of Portugal?
	22.80	
	⋮	← Capital of Ireland?
	22.80	

TABLE III.

EXPERIMENTS ON DR. S.

Galvanometer Readings.	Stimulus.
24.00	
00	
⋮	← Pinch.
05	
10	
15	
20	
25	
24.30	

Galvanometer Readings.	Stimulus.
24.35	
40	
40	
35	
30	
25	
20	
15	
24.10	

Galvanometer Readings.	Stimulus.
24.05	
24.00	
⋮	
13.60	
60	
60	
⋮	← Dropping weight.
65	
70	
75	
80	
85	
90	
90	
85	
80	
75	
70	
65	
13.65	
⋮	
23.90	
90	
90	
⋮	← Burn.
95	
24.00	
05	
24.10	
15	
20	
25	
30	
35	
40	
40	
35	
30	
25	
20	
10	
23.90	
17.20	
17.20	

Galvanometer Readings.	Stimulus.
17.20	
⋮	← Artificial laughter.
30	
40	
50	
60	
70	
80	
90	
18.00	
⋮	
23.50	
60	
70	
80	
90	
24.00	
00	
00	
00	
24.00	
24.00	
⋮	
13.70	
70	
70	
⋮	← Smelled CS_2.
80	
85	
90	
95	
14.00	
00	
13.95	
90	
85	
80	
75	
13.75	
⋮	

No change.

Galvanometer Readings.	Stimulus.
15.35	
35	
⋮	← Thinking of something disagreeable.
35	
35	

	Galvanometer Readings.	Stimulus.
No change.	15·35	
	35	
	⋮	← Solving difficult problem.
	35	
	35	

	Galvanometer Readings.	Stimulus.
No change.	15·35	
	35	
	⋮	← Shown pictures.
	35	
	35	

TABLE IV.

EXPERIMENT ON DR. C.

Galvanometer Readings.	Stimulus.
24·75	
75	
75	
⋮	← Burn.
80	
85	
90	
95	
25·00	
00	
24·95	
90	
85	
80	
75	
70	
24·70	
⋮	

Galvanometer Readings.	Stimulus.
24·55	
55	
55	
⋮	← Pinch.
60	
65	
70	
80	
90	
90	
80	
70	
24·60	

Galvanometer Readings.	Stimulus.
24·55	
50	
50	
24·50	
⋮	

Galvanometer Readings.	Stimulus.
23·25	
25	
25	
⋮	← Sudden noise.
30	
40	
50	
60	
70	
80	
90	
24·00	
00	
23·95	
90	
23·85	
⋮	
(Experiment stopped.)	

Galvanometer Readings.	Stimulus.
23·30	
30	
⋮	← Solving difficult problems mentally.
30	
23·30	
⋮	

Table V.

Experiments on Mr. R.

Galvanometer Readings.	Stimulus.	Galvanometer Readings.	Stimulus.
18.20		23.35	
20		30	
20		25	
:	← Drop weight.	20	
25		15	
30		10	
35		10	
40		23.10	
45		:	
50		12.00	
50		00	
45		:	← Suddeu loud shout.
40		10	
35		20	
30		30	
25		40	
18.25		50	
:		60	
23.15		50	
15		40	
15		30	
:	← Slight noise.	20	
20		10	
25		12.00	
23.30		:	

Plotting galvanometric deflections as ordinates and time as abscissæ a series of curves is obtained.

Out of a large number we have selected a few typical ones which show clearly the relative variations of galvanometric deflections under various conditions of stimulation. Where requisite we indicate in a short note the essential characteristic of each particular curve.

An examination of the tables and curves shows that pure ideational processes such as thinking, calculation, solving problems, representing pleasant or painful experiences and even æsthetic experiences such as looking at pictures have *no* effect, while *sudden violent emotions* and especially *intense sensory stimulations* of a painful or of a very disagreeable

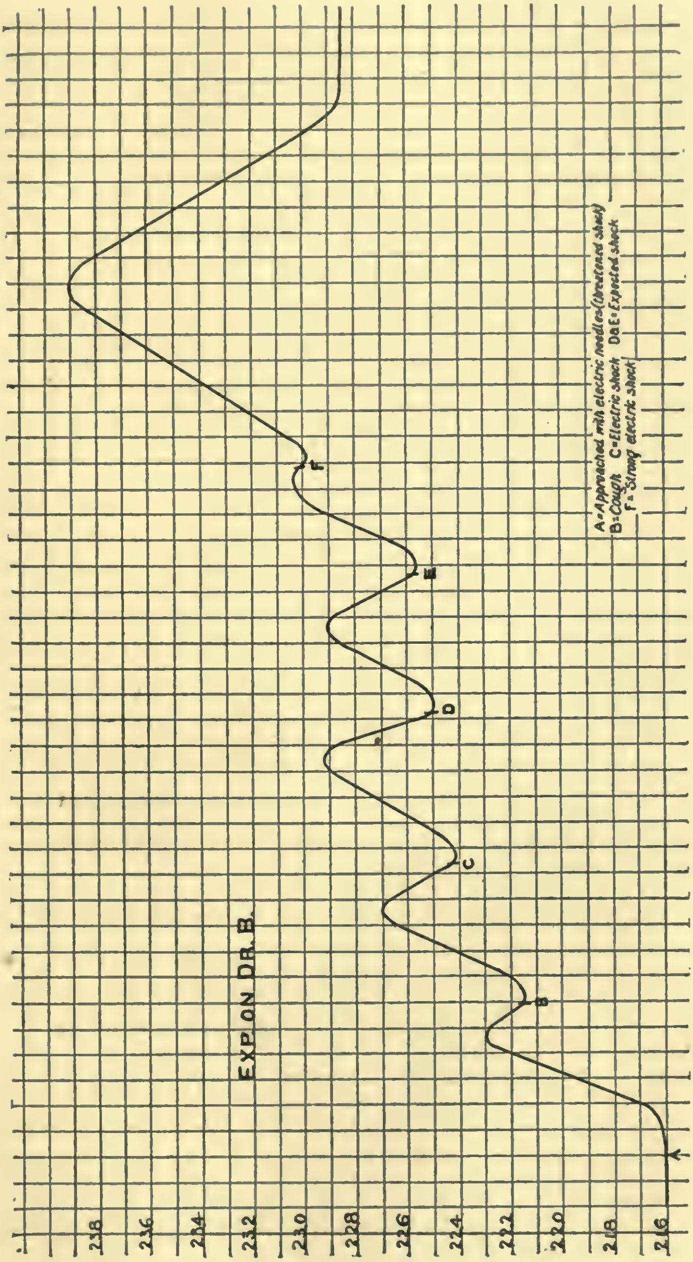


FIG. 2.—Dr. B. sensitive to electricity. Shows the influence under strained expectation of electric shock.

character, such as burns, pricks, pinches, electric shocks and unpleasant smells are followed by marked galvanometric deflections. The deflections diminish and finally disappear with the repetition of the *same* sensory stimulation.

It will be observed that there is a latent period between the time of stimulation and the beginning of the rise of the curve. This latent time is somewhat variable, but is of the order of

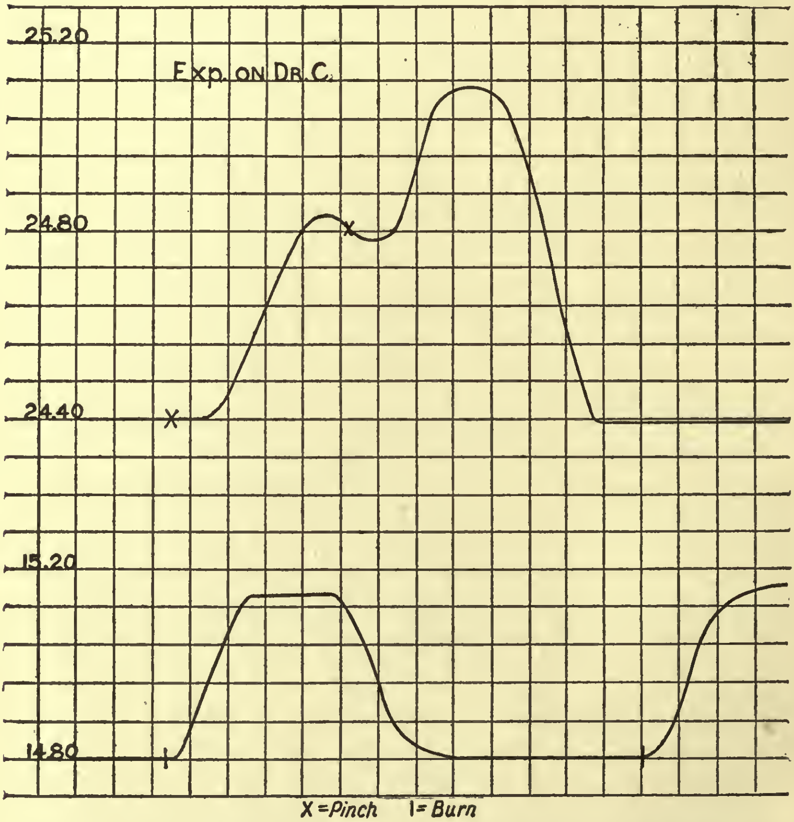


Fig. 3.—The curves show deviations due to *simple* sensory stimulation and superimposed sensory stimulation.

magnitude of a few seconds. No attempt was made to study accurately such latent periods; the curves represent the magnitude of the deflection in terms of arbitrary time units. Particular attention will be paid to this point in a subsequent study.

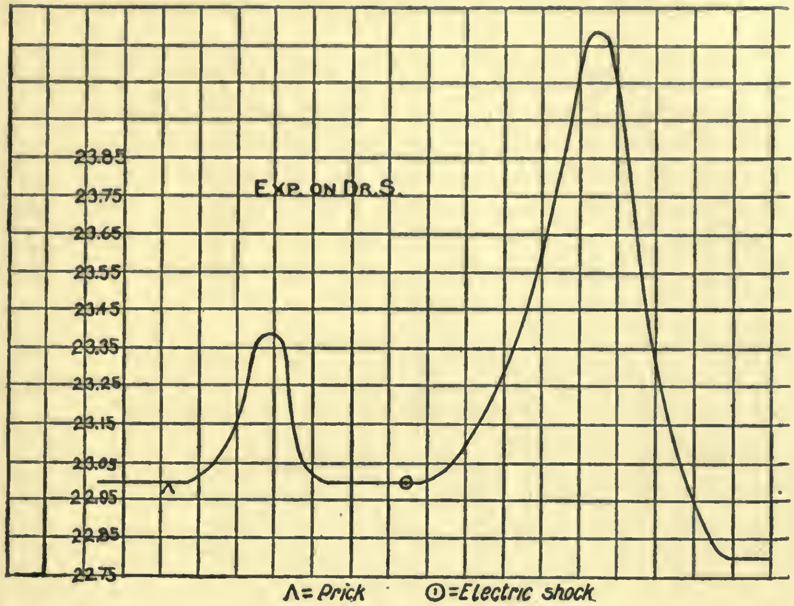


FIG. 4.

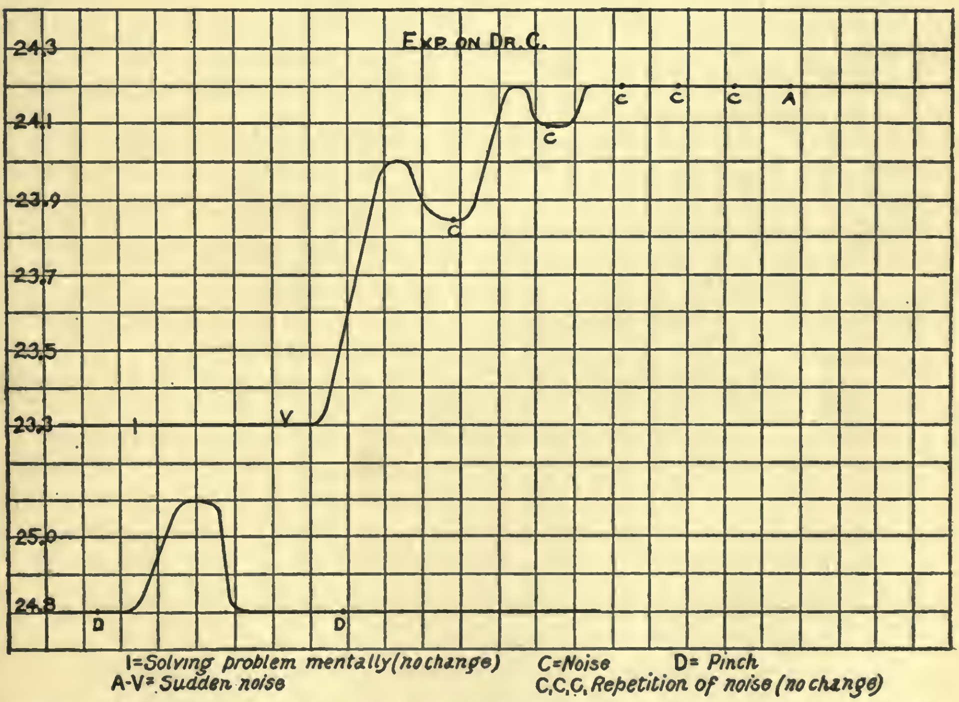


FIG. 5.—The galvanometric deviations *diminish* and finally *disappear* with superimposed stimulations of same kind.

It need hardly be pointed out that such definite variations cannot possibly be ascribed to changes in the circuit such as the introduction of thermo-electromotive forces, magnetic effects and the like; for these could scarcely time themselves to occur just at the instant of stimulation. For the sake of completeness of demonstration, however, a resistance box was introduced into the circuit across the electrodes *EE* in place of the human

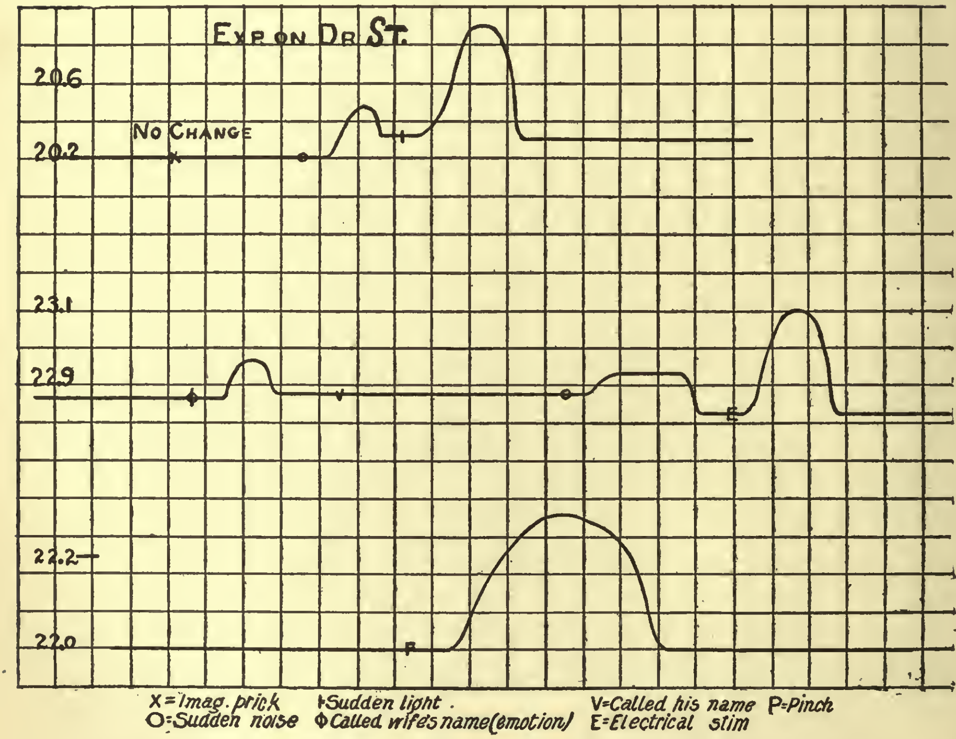


FIG. 6.—This curve brings out well the relative deviations due to pure representation, as compared with *emotional* and sensory processes.

body. The reading remained steady to within one half millimeter for an indefinite time in spite of the jarring and disturbances which were purposely made more violent than during the experiments on the subjects.

Hence we conclude that the observed galvanometric changes do not take their origin in the physical part of the circuit, but *are caused by physiological processes concomitant with the mental states aroused by the stimuli.*

We next pass to the study of the nature of these physiological processes.

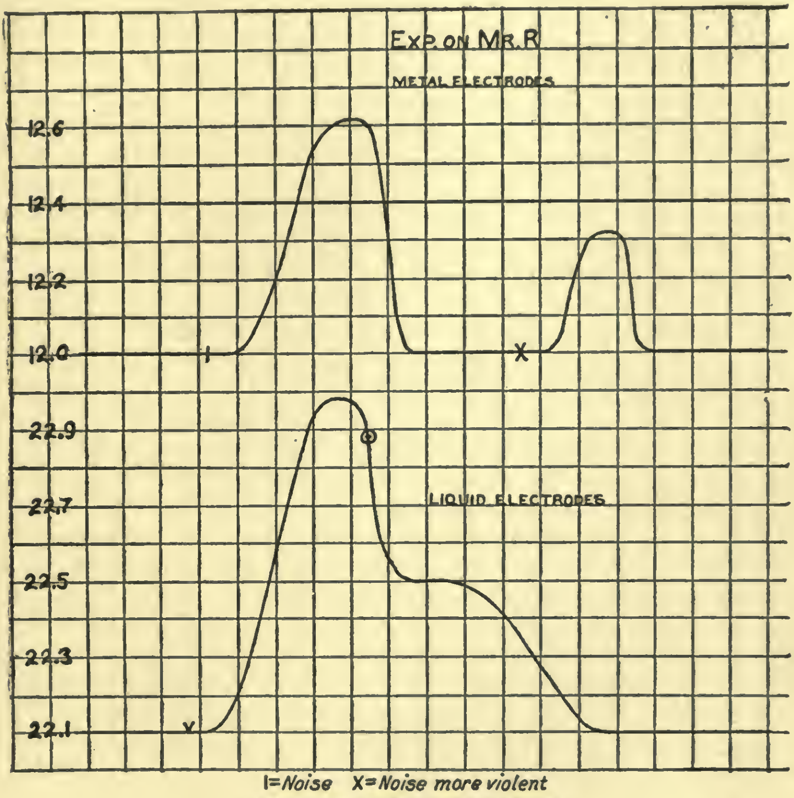


FIG. 7.—These two curves of galvanometric deviations with metal and liquid electrodes under the same conditions of sensory stimulations may be regarded as typical.

PART II.

I.

The galvanometric variations during emotional states may be taken to indicate that the physiological processes accompanying emotions change the resistance of the circuit by changing the resistance of the body. This resistance factor is the one to which these variations are commonly referred by previous investigators. Further experimentation, however, points in a different direction.

It seemed highly probable that not an inconsiderable fraction of the total resistance of the body measured by the immersion of the hands in our liquid electrodes was due to the skin layer. Variations in the skin area in contact with the electrodes were

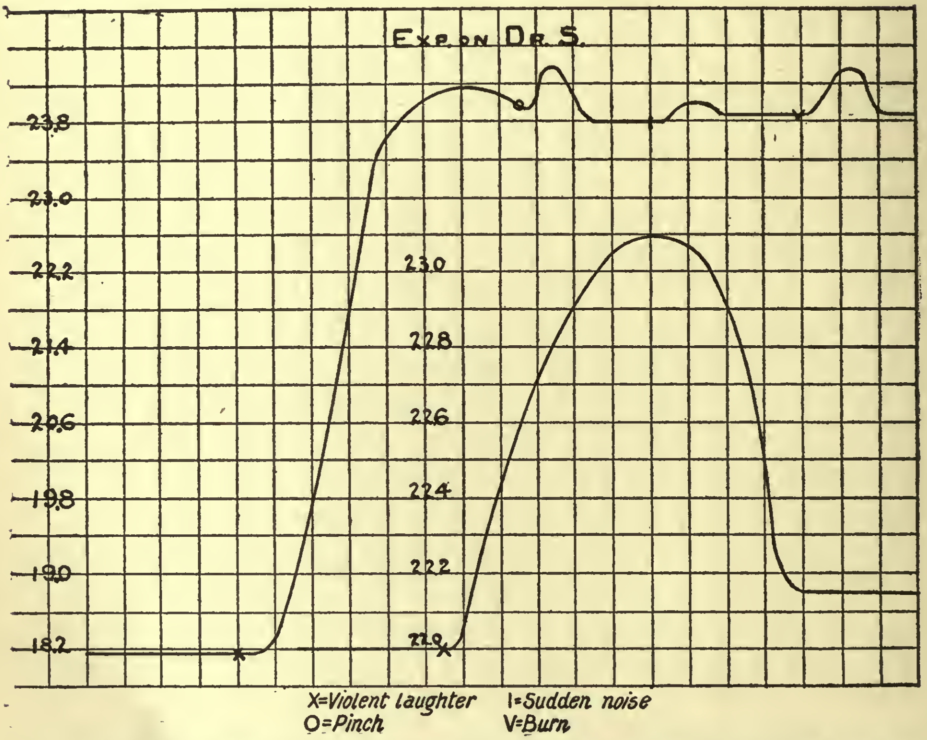


FIG. 8.—This curve is especially instructive, showing the galvanometric deflection produced by laughter as compared with variations caused by sensory stimulations.

eliminated by the use of liquid electrodes instead of metal electrodes used by other investigators. The shellac and paraffin with which we covered the subject's wrists as well as the splints put on the hands made the skin area washed by the liquid electrodes constant, so much so that violent stirring of the liquid with the hands did not change the reading of the galvanometer. The galvanometric variations observed under conditions of stimulation could not therefore be referred to variations in skin contact.

If resistance be the factor, then the galvanometric variations observed may either be due to changes of resistance of the con-

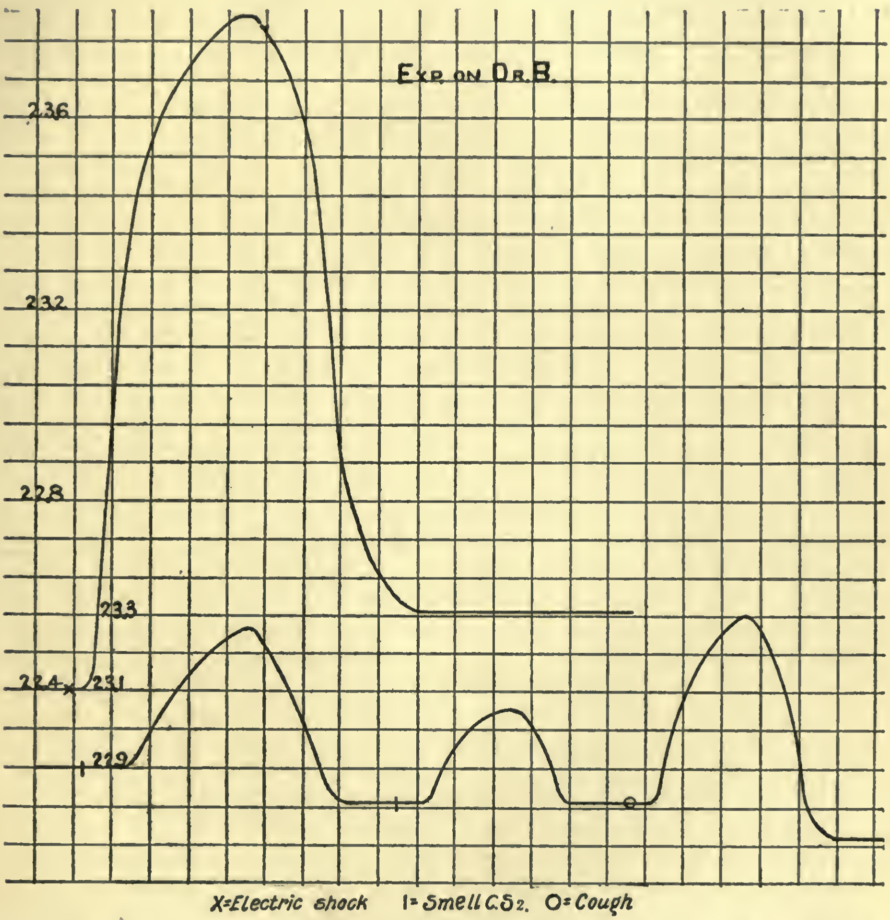


FIG. 9.—Dr. B. throughout the experiments gives a large deflection to electric stimulations even to expected ones as he is quite sensitive to electricity.

stant area of the skin or of the body through which the current passes. That the galvanometric deflections are due to variations in resistance of the skin is a view commonly held by many investigators.

Skin resistance can, however, be eliminated by the following procedure: Hypodermic needles were inserted well under the skin until blood flowed freely. The hands with the needles in position were placed within the liquid electrodes. The change in deflection was slight, about 2 millimeters in a total deflection of 20 centimeters, or about 1 per cent. After a few minutes the reading was the same as before the insertion of the needle

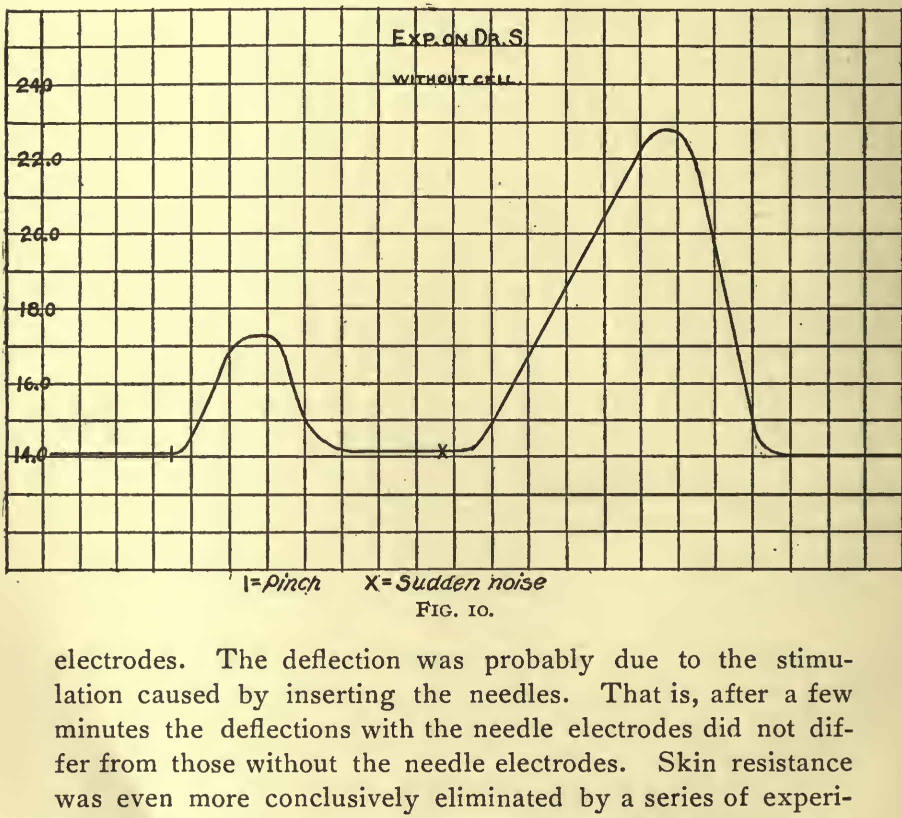


FIG. 10.

electrodes. The deflection was probably due to the stimulation caused by inserting the needles. That is, after a few minutes the deflections with the needle electrodes did not differ from those without the needle electrodes. Skin resistance was even more conclusively eliminated by a series of experiments which will be described in their appropriate place. With the needles inserted curves identical in form with the preceding ones were obtained. Following are two curves typical of a number obtained under these conditions.

In considering the resistance of the body we may possibly regard the temperature of the body as a factor concerned in the observed galvanometric deflections. Electrolytes have a positive temperature coefficient of about 2 per cent. per degree. It was thought that contractions of the muscles, voluntary and involuntary, as well as other catabolic processes that may go on in the body during an emotional state may possibly develop heat and thus account for the change in the deflections. Granted that sufficient heat is developed by muscular and chemical activities involved in the catabolic physiological processes

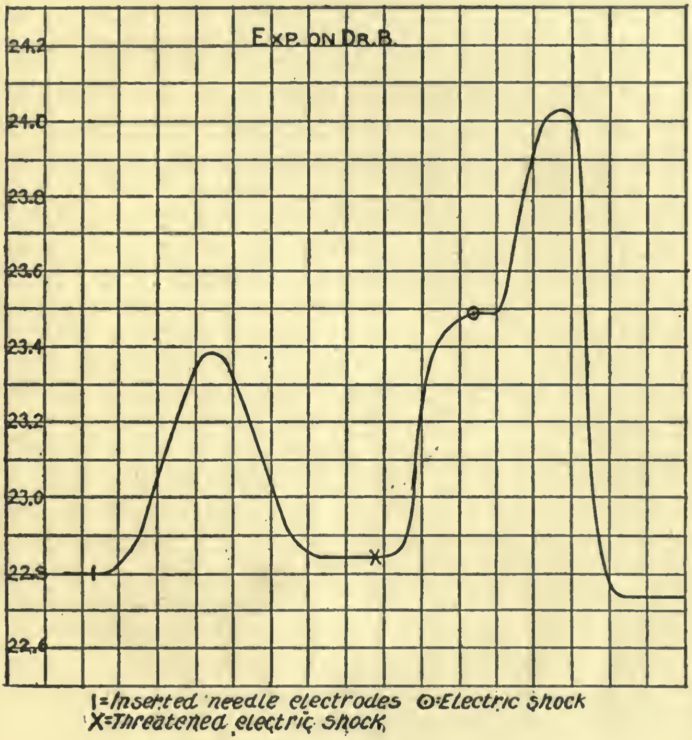


Fig. 11.

concomitant with emotional states we may well account for the galvanometric variations. The factor of temperature had all the more to be taken into consideration as the experiments performed seemed to point in that direction. That is, bending of the arm, strenuous bending of the head, rising, sitting, coughing, laughing, whether spontaneous or artificial, any violent muscular strain or exercise especially of the arms gave rise to appreciable variations. Thus in some cases violent laughter, though artificial in character, caused a galvanometric deflection of 6–8 centimeters. Under other more favorable conditions described further the deflection amounted to more than 50 centimeters.

We may also call attention to the experiments in which we artificially varied the temperature of the arms. Heating and cooling the arms put in an Esmarch bandage so as to exclude circulatory variations brought about galvanometric deflections.

The experiments with hot and cold applications gave but slight variations insufficient to account for the galvanometric

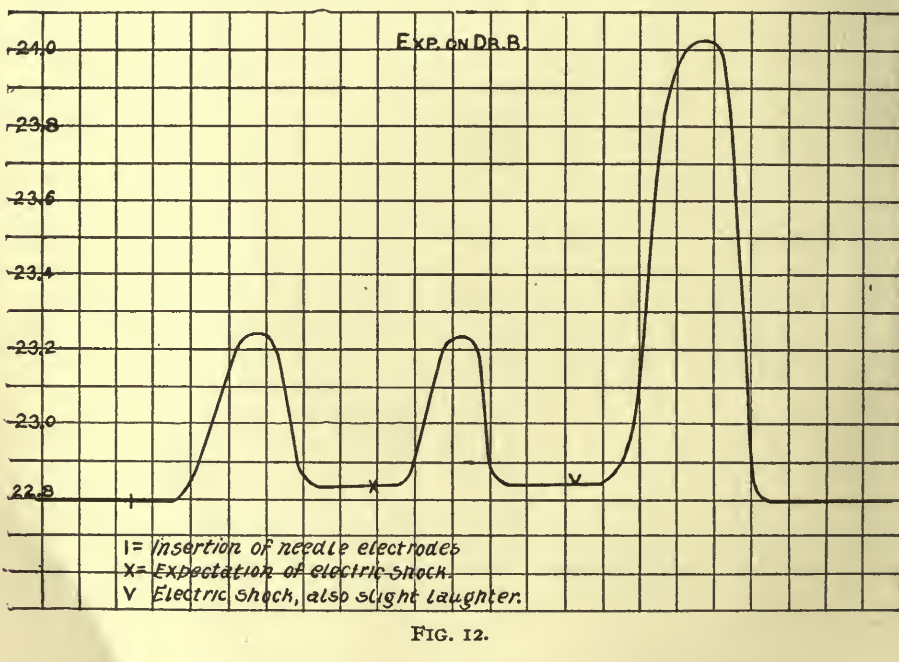


FIG. 12.

phenomena observed under the influence of emotional states. The variations due to raising the temperature did not differ from those due to lowering the temperature. Furthermore, after a minute or two of continuous cooling or heating the arms the reading was the same as that before the temperature change. The hot and cold applications acted therefore in the nature of mere temperature stimulations.

The galvanometric variations observed upon rising and sitting were just as marked when the subject was raised passively and lowered passively as when he raised himself actively so that there was no question of exercise. Rising and sitting, changes in the position of the arms, was responsible for variations. The curves, Figs. 13–19, show the effect clearly.

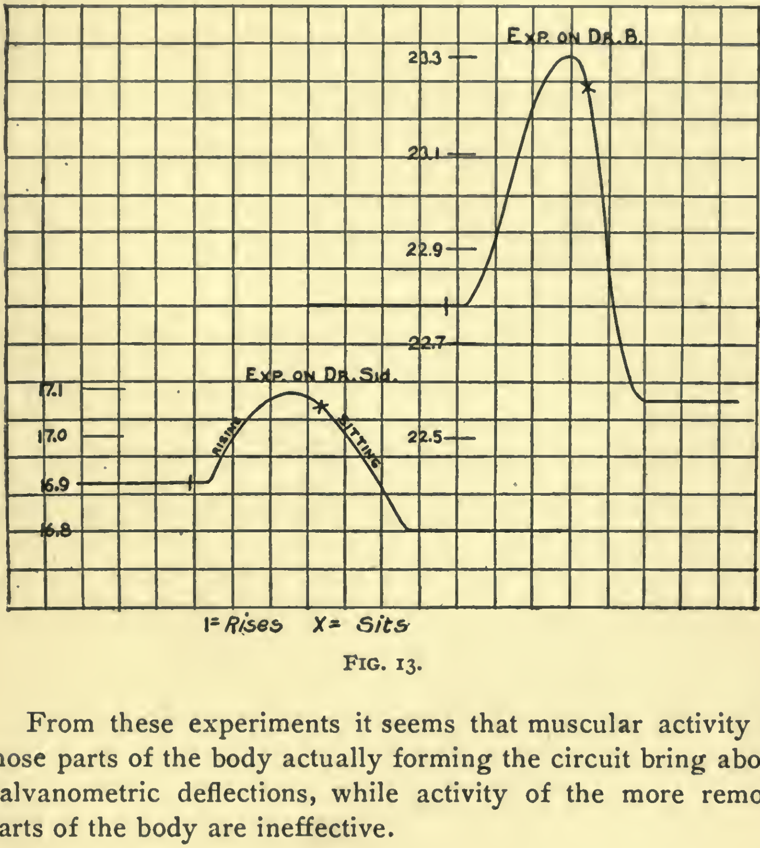


FIG. 13.

From these experiments it seems that muscular activity of those parts of the body actually forming the circuit bring about galvanometric deflections, while activity of the more remote parts of the body are ineffective.

II.

It was supposed that the galvanometric deflections might be due to variations of the circulation under the influence of emotions. The circulation was cut off by Esmarch bandages. A galvanometric deflection was observed on putting on the Esmarch bandages and also on taking them off, as it was in the case of any other intense stimulus. What, however, completely eliminates circulation as the determining factor is the significant fact that when the Esmarch bandages were on, galvanometric deflections were obtained under conditions of sensory stimulation and arousal of emotional states. The Esmarch bandages were kept on the subject as long as he could stand

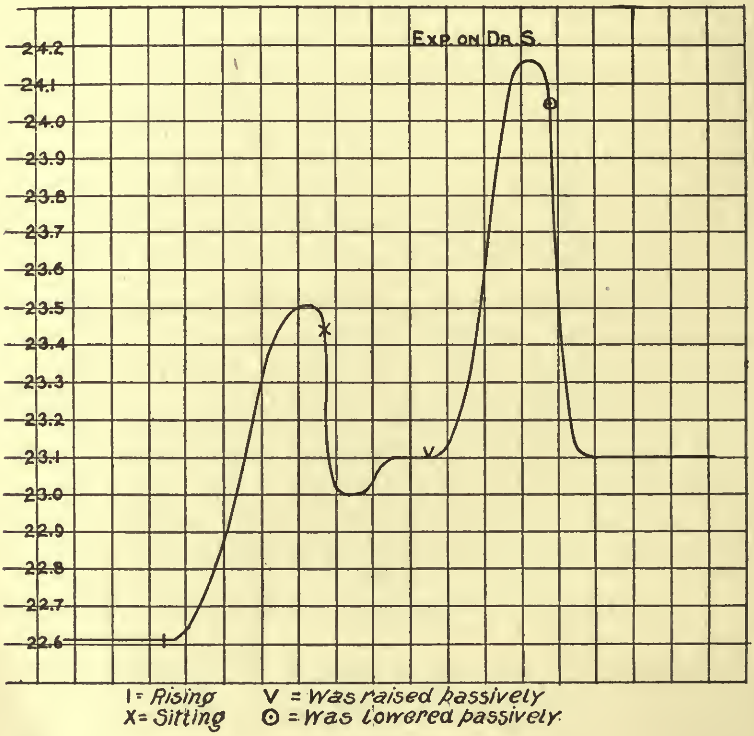
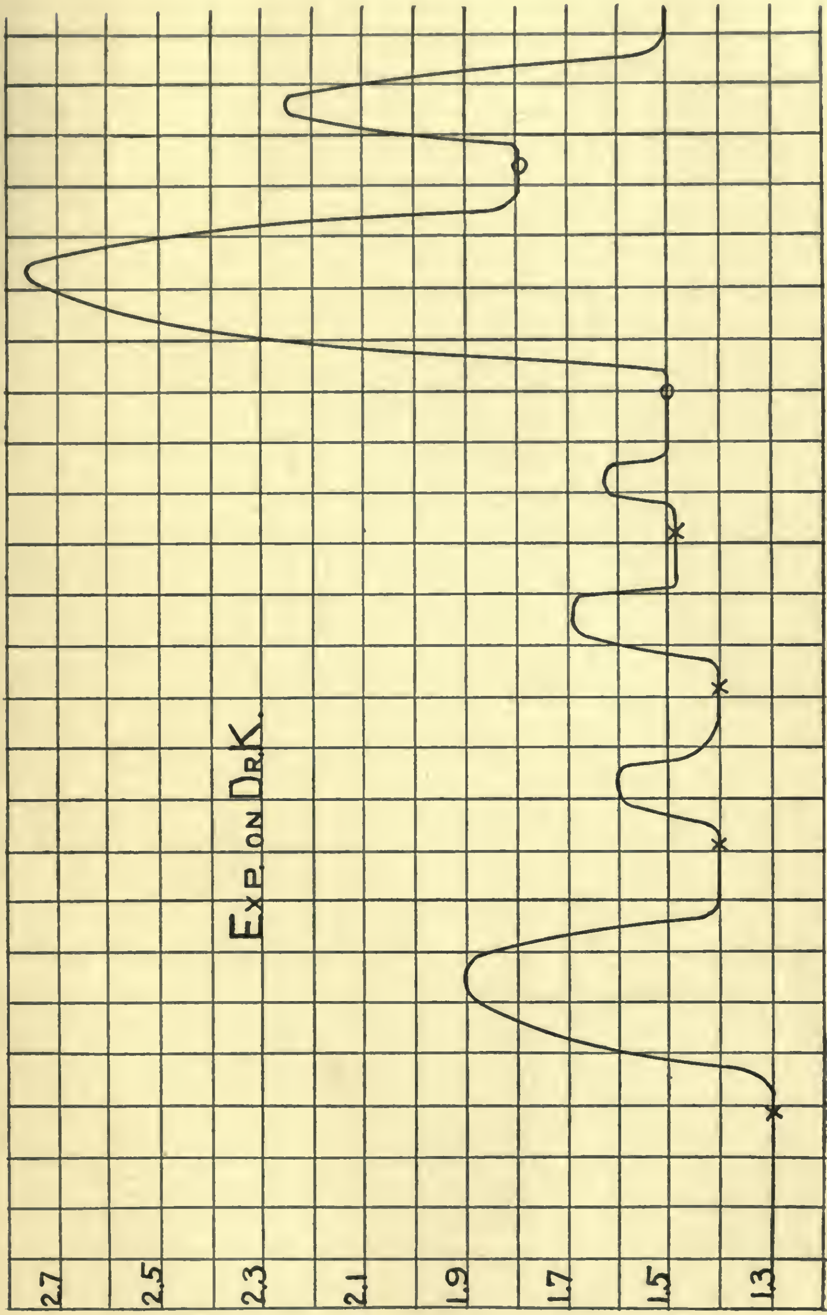


FIG. 14.

them. The circulation was effectually cut off, the pulse was gone and the hand assumed a cadaverous hue; still the same galvanometric deflections were easily obtained under the same mental and purely physiological conditions, such as emotions, sudden sensory stimulations, rising, sitting, coughing, laughing and muscular activity, especially of the arms.



X = pinch (arms stiff.) O = Sudden noise (arms stiff.)

FIG. 15.

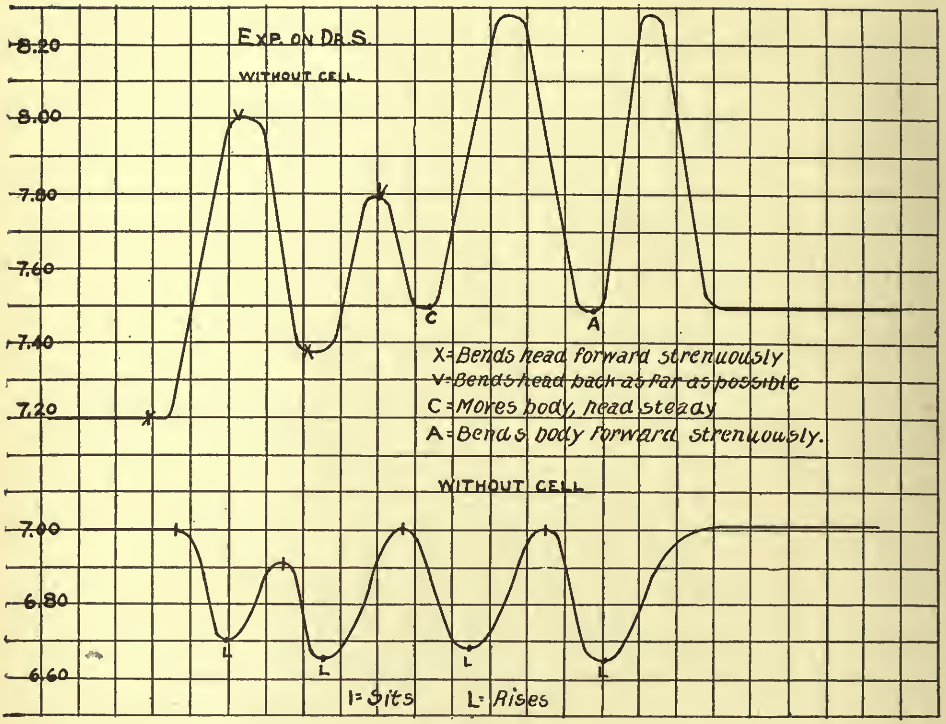

EXP. ON DR. S.
WITHOUT CELL.
8.20
8.00
7.80
7.60
7.40
7.20
7.00
6.80
6.60
X = Bends head forward strenuously
V = Bends head back as far as possible
C = Moves body, head steady
A = Bends body forward strenuously.
WITHOUT CELL
I = Sits
L = Rises

FIG. 16.

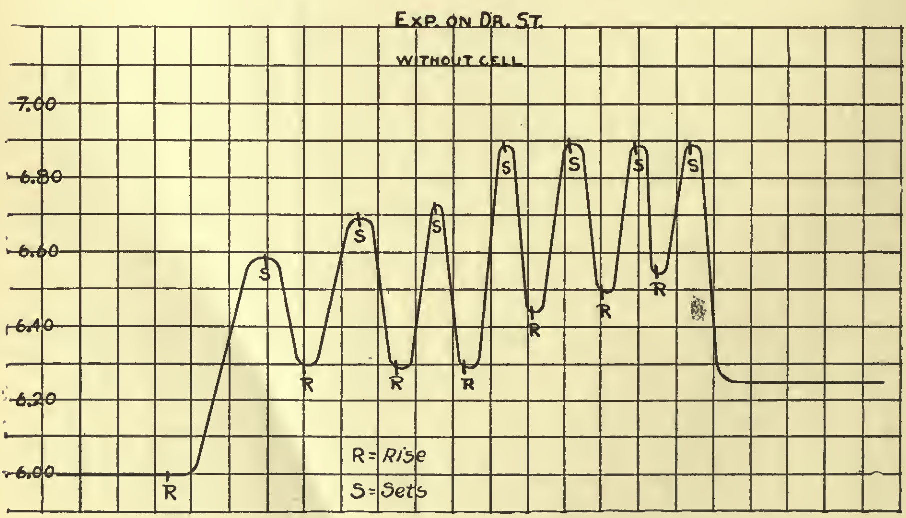

EXP. ON DR. ST.
WITHOUT CELL
7.00
6.80
6.60
6.40
6.20
6.00
R = Rise
S = Sets

FIG. 17.

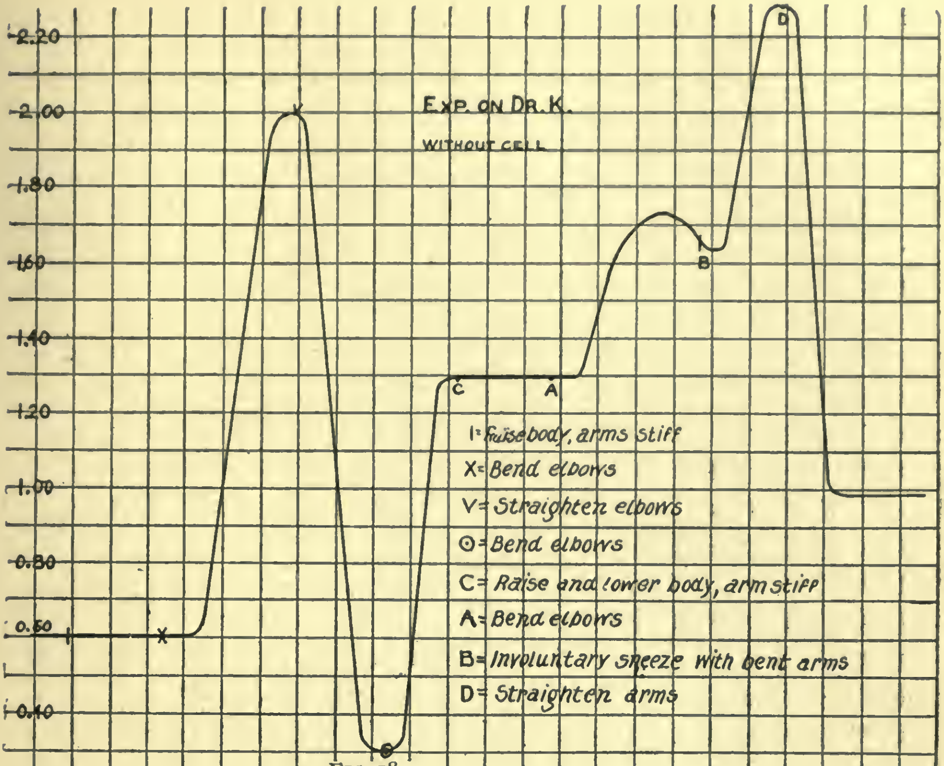


Fig. 18.

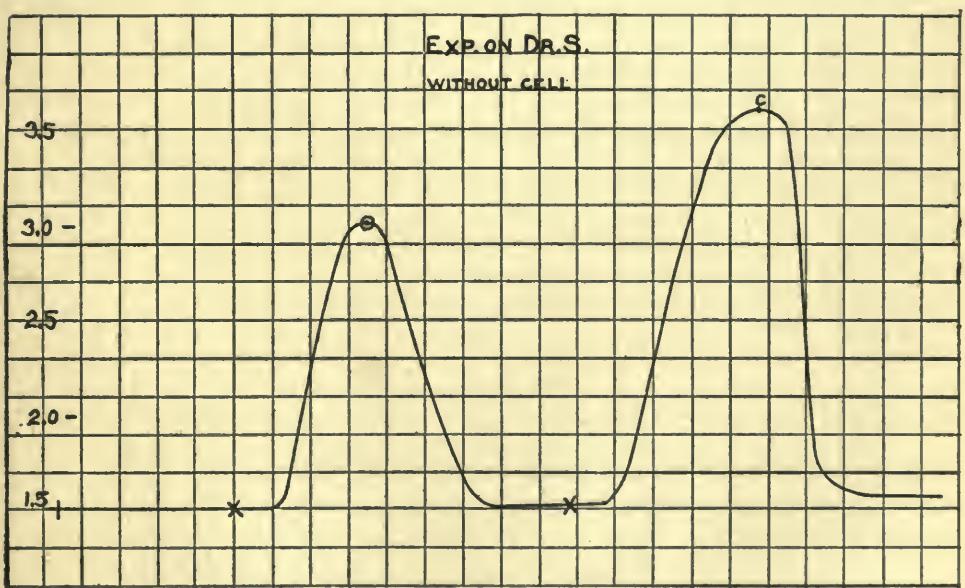


Fig. 19. I= Raising body, elbows stiff; O= Stiffening arms; X= Bending elbows; C= Stiffening elbows.

The following curves may be regarded as typical of the rest:

These experiments prove conclusively that the circulation has nothing to do with the galvanic phenomena under investigation.

Our experiments go to prove that the causation of the galvanometric phenomena cannot be referred to skin resistance, nor can it be referred to variations in temperature, nor to circulatory changes with possible changes in the concentration of the body-fluids. Since the electrical resistance of a given body depends on two factors — temperature and concentration — the elimination of both factors in the present case excludes body-

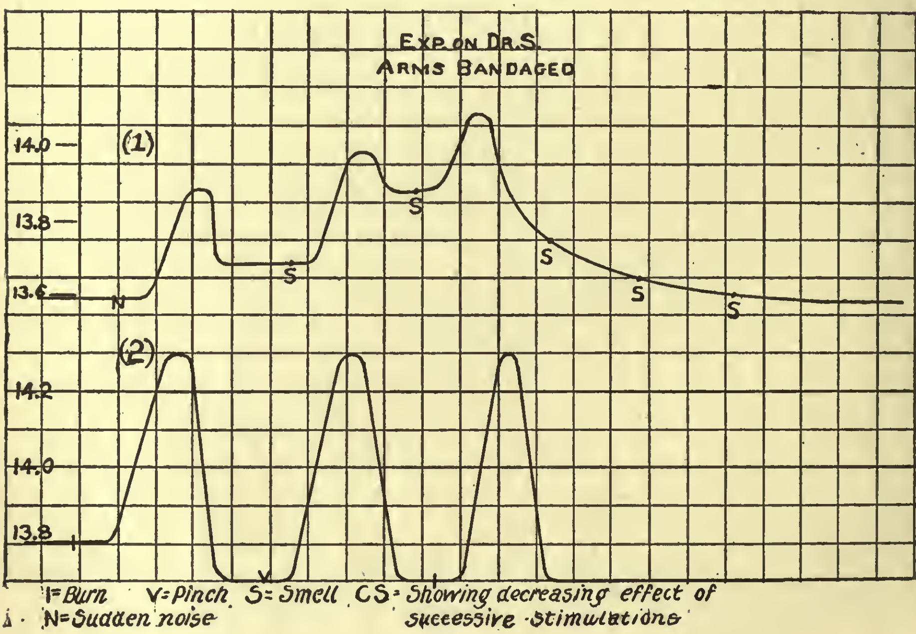


FIG. 20.

resistance as the cause of the deflections. Our experiments therefore prove unmistakably that the galvanic phenomena due to mental and physiological processes cannot be referred to variations in resistance, whether of skin or body. *Resistance being excluded the galvanometric deflections can only be due to variations in electromotive force of the body.*

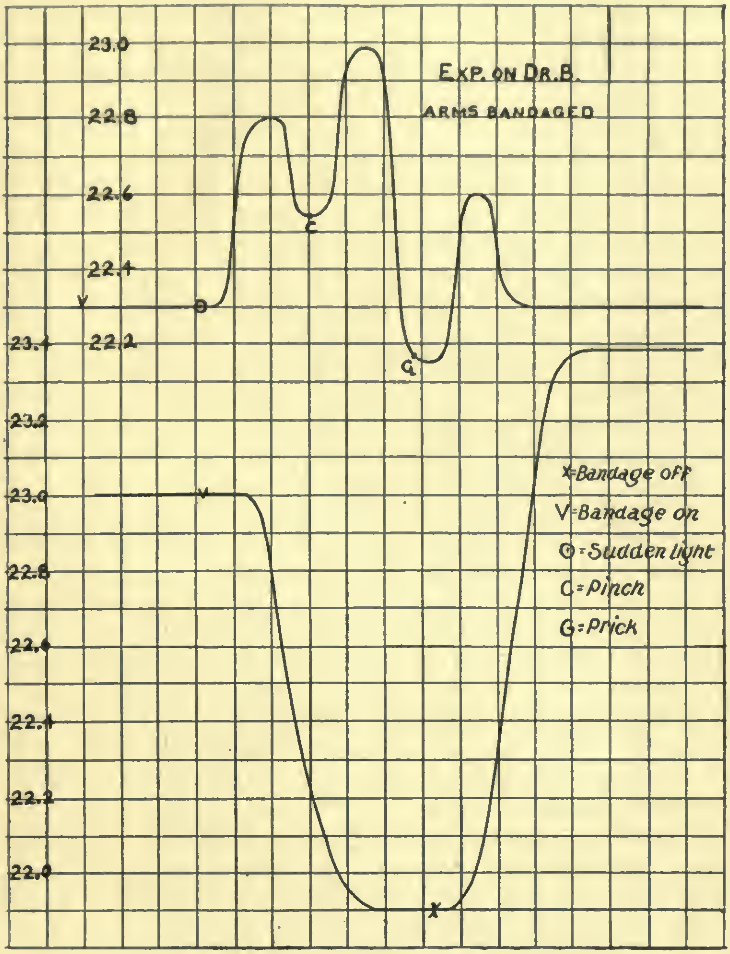


FIG. 21.

III.

In our experiments on the electromotive force we were partly guided by Dr. Waller's work on the electromotive changes connected with the beat of the mammalian heart.[1] The heart-beat should be taken into consideration as one of the possible causes of galvanometric deflections due to various psycho-physiological

[1] *Philosophical Transactions of the Royal Society of London*, Vol. 180, p. 169, 1889.

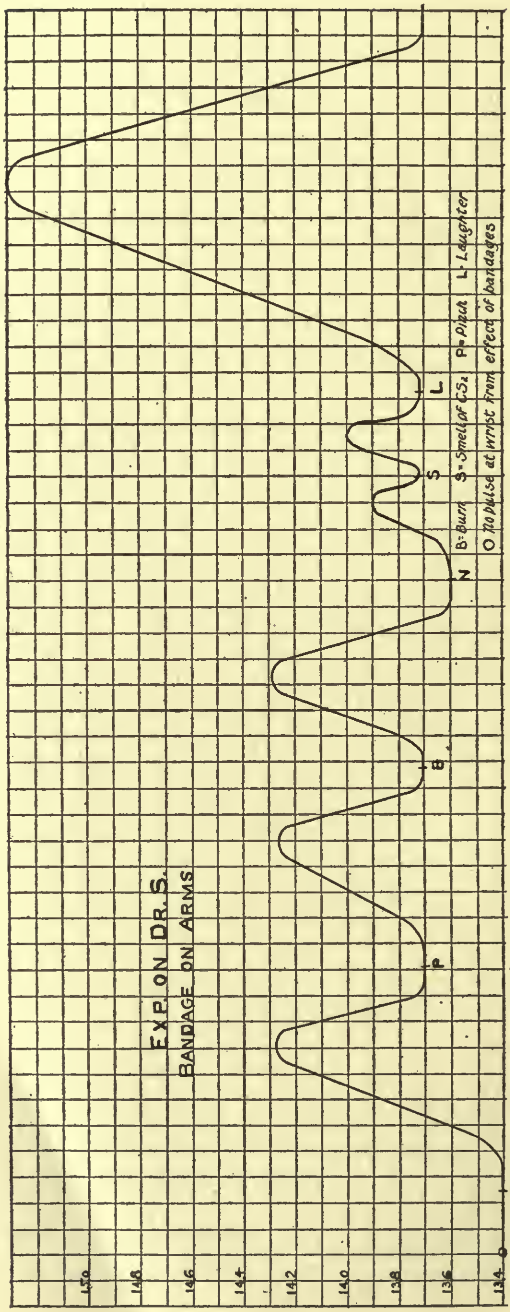

EXP. ON DR. S.
BANDAGE ON ARMS
B=Burn S=Smell of CS2 P=Pinch L=Laughter
O no pulse at wrist from effect of bandages
15.0
14.8
14.6
14.4
14.2
14.0
13.8
13.6
13.4
B
P
B
N
S
L

FIG. 22.

and purely psychological processes. Experimenting with the capillary electrometer Waller came to the conclusion that "a marked electrical variation is manifested at each pulsation of the heart." This electrical variation was manifested by 'leading off' from the surface of the body. Thus with each beat of the heart an electromotive force is set up causing a non-symmetrical distribution of potential over the body. To quote Waller: "The contraction of the ventricles is not simultaneous throughout the mass, but traverses it as a wave. Inequalities of potential at different parts of the mass are consequently established at the beginning and at the end of each systole. Or to reverse the order of statement, the inequalities in question are proof of the passage of a wave of excitation. The distribution of these inequalities of potential is represented diagrammatically in Fig. 24.

"These data being transferred to the entire body as in Fig. 24 we have the portion *a*, *a*, *a* . . . as the area in which the potential of *A* is distributed, and the portion *b*, *b*, *b* . . . as the area in which the potential of *B* is distributed.

"Electrical variations will be manifested when any two points *a* and *b* are led off; no electrical variations will occur when any two points *a* and *a*, or *b* and *b*, on the same equipotential line, are led off; small electrical variations will be obtained when two points *a* and *a*, or *b* and *b* on different equipotential lines are led off."

Working with the capillary electrometer Waller found that certain combinations were favorable, while others were unfavorable to manifestations of marked electrical variations due to cardiac beat. The favorable combinations were the following:

Left hand and right hand.
Right hand and right foot.
Right hand and left foot.
Mouth and left hand.
Mouth and right foot.
Mouth and left foot.

The unfavorable combinations were:

Left hand and left foot.
Left hand and right foot.

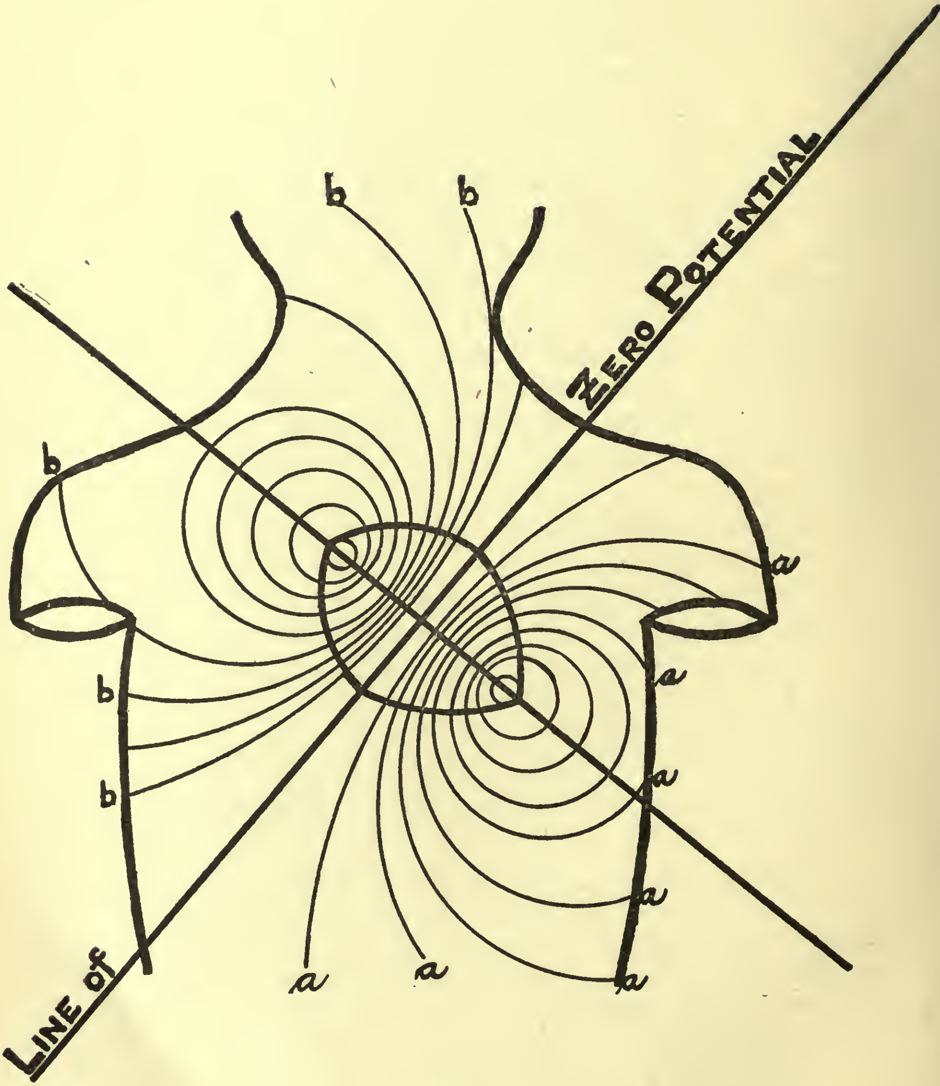


FIG. 24.

Right foot and left foot.

Mouth and right hand.

In short, according to Waller, electrical variations are observed when two dissimilar points are connected with the electrometer, while electrical variations are absent or faint when two similar points are connected, similar and dissimilar points being defined according to Fig. 24.

We repeated Waller's experiments, but we could not confirm his results. Our experiments do not confirm his favorable or unfavorable combinations. Further work is in progress in this connection. What we did find was that a reversal of position of the hands — putting the right in place of the left hand — made a difference in the magnitude of the deflection and occasionally in its direction.

To define positive and negative deflections for our experiments, a cell was introduced into the circuit and the direction of the deflection was called positive. The terminal of the galvanometer to which the positive pole of the cell was connected was consequently defined as positive. In the following record of data, therefore, l. h. (+) means that the left hand was connected to the positive terminal of the galvanometer thus defined. The following tables give the results of our experiments:

Table VI.

Experiments on Dr. S.

First Series.

zero reading	+ 25.0
l. h.[1] (+)	+ 8.5
l. h. (—)	+ 18.0
zero reading	+ 25.0
l. h. (+)	+ 9.5
l. h. (—)	+ 18.5
zero reading	+ 25.0
l. h. (+)	+ 9.5
l. h. (—)	+ 18.0
l. h. (+)	off scale +
l. h. (—)	off scale —

Repeated 5 times; same results.

Table VII.

Experiments on Dr. S.

Second Series.

l. h. (+)	— 8.0
l. h. (—)	+ 5.5
l. h. (+)	— 7.0
l. h. (—)	+ 5.8

[1] l. h. stands for left hand; r. h. stands for right hand.

l. h. (+)	−7.5
l. h. (−)	+5.2
l. h. (+)	−7.2
l. h. (−)	+5.0

Table VIII.

Experiments on Mr. R.

l. h. (+)	+10.0
"	+10.5
"	+11.0
l. h. (−)	0.0
"	0.0
"	0.0

	Defl. 1	Defl. 2
mouth (−) r. h. (+)	7.2	7.5
mouth (+) r. h. (−)	7.6	6.0
mouth (−) l. h. (+)	7.2	7.5
mouth (+) l. h. (−)	6.5	6.0

(2) is a repetition of experiment (1).

Table IX.

Experiments on Dr. B.

zero	−25.0
l. h. (+)	+ 3.0
l. h. (−)	− 7.0
zero	−25.0
l. h. (−)	+ 3.0
l. h. (−)	− 6.5
zero	−25.0
l. h. (+)	+ 3.0
l. h. (−)	− 6.5
l. h. (+)	+14
l. h. (−)	−13
mouth (+) r. h. (−)	+25.0
mouth (−) l. h. (+)	+25.0
mouth (−) r. h. (+)	+25.0
mouth (+) l. h. (−)	+25.0

Table X.

Experiments on Dr. K.

l. h. (+)	−12.5
l. h. (−)	+11.0
l. h. (+)	−13.0
l. h. (−)	+11.4
l. h. (+)	−12.0
l. h. (−)	+11.5

Table XI.

Experiments on Mr. Col.

First Series.

l. h. (+)	+ 6.0
l. h. (—)	+ 11.0
l. h. (+)	+ 5.0
l. h. (—)	+ 10.0
l. h. (+)	+ 4.5
l. h. (—)	+ 10.0
l. h. (+)	+ 4.5
l. h. (—)	+ 9.0

Table XII.

Experiments on Mr. Col.

Second Series.

l. h. (+)	—4.5
l. h. (—)	—7.0
l. h. (+)	—3.0
l. h. (—)	—7.0
l. h. (+)	—3.0
l. h. (—)	—7.2
l. h. (+)	—2.8
l. h. (—)	—7.3

Table XIII.

Experiments on Dr. St.

l. h. (+)	+2.7
l. h. (—)	+1.4
l. h. (+)	+2.6
l. h. (—)	+1.2
l. h. (+)	+2.2
l. h. (—)	+0.9

From these tables it is clear that the absolute magnitude of the deflection varies according to the varying conditions of the experiment. Different experiments performed with different concentration of electrode solutions gave different deflections, in fact the direction and magnitude was varied at will in this way. Also substituting lead electrodes for copper electrodes changed the deflections largely, *and different parts of the skin gave different original deflections.* However, superimposed upon this original steady deflection is a deflection due to the various stimulations given.

IV.

A further study offers direct evidence that the deflections due to stimuli are caused by electromotive forces. Following are the experimental results:

Table XIV.

Experiments on Dr. K.

l. h. (+) steady reading	— 11.50
Stimulus = pinch, reading rose to ..	— 14.0
l. h. (—) steady reading	+ 10.0
Stimulus = burn — rose to	+ 11.5

Table XV.

Experiments on Dr. S.

l. h. (—) steady reading	+ 9.0
Stimulus = pinch	+ 10.0
l. h. (+) steady reading	+ 11.0
Stimulus = pinch	+ 11.7

Table XVI.

Experiments on Dr. St.

l. h. (+) steady reading	— 3.5	
Stimulus = electrical shock	— 4.5	
l. h. (+) steady reading	— 3.0	
Stimulus = burn	— 4.2	
l. h. (+) steady reading	— 3.0	
Stimulus = electrical shock	— 3.7	
l. h. (—) steady reading	— 3.8	
Stimulus = electrical shock	— 3.0	
l. h. (—) steady reading	— 4.0	
Stimulus = electrical shock	— 3.1	
l. h. (—) steady reading	— 4.0	
Stimulus (1) = electrical shock	— 3.5	
Stimulus (2) = electrical shock	— 3.0	
l. h. (+) steady reading	— 3.2	No change.
Stimulus = multiply 3½ x 7⅓	— 3.2	
l. h. (+) steady reading	— 3.2	
Stimulus = (What is the capital of Japan?)	— 3.2	

Table XVII.

Experiments on Mrs. S.

l. h. (+) steady reading............	— 2.5
Stimulus = sudden noise	— 4.0
l. h. (—) steady reading............	+ 4.0
Stimulus = prick..................	+ 6.0

Table XVIII.

Experiments on Dr. St.

l. h. (+) steady reading............	— 1.3
Stimulus, ice application to arms....	— 0.9
application removed	— 1.3
l. h. (+) steady reading............	— 1.5
Stimulus (1) hot application.........	— 1.3
(2) changed to ice application..	— 0.9
l. h. (+) steady reading............	— 1.8
Stimulus, hot application	— 0.9
l. h. (+) steady reading............	— 2.3
Stimulus, hot application	— 2.1
l. h. (—) steady reading............	— 0.2
Stimulus, cold application..........	— 0.3
l. h. (—) steady reading............	— 0.5
Stimulus, cold application..........	— 1.0
l. h. (—) steady reading............	— 0.8
Stimulus, hot application	— 1.1

Stimulus repeated gave no further deflections.

An examination of Tables XVI. and XVIII. shows that the effect of the stimulus is sometimes to increase the current and sometimes to decrease the current. This cannot be due to change of resistance of the body, but must be due to an electromotive force.

Table XVIII. shows that both increasing and decreasing the temperature caused the absolute deflections to diminish when the l. h. was (+) and to increase when the l. h. was (—). That is, the change of temperature acted simply as a sensory stimulus causing in every instance an E.M.F. in the same direction relatively to the body, whether the change be an increase or a diminution of temperature. With the l. h. (+) the E.M.F. due to the stimulation was in the opposite direction to E.M.F. already existing in the circuit and consequently diminished the existing deflection, while with the hands reversed (*i. e.*, l. h. (—)) the

E.M.F. due to the stimulation was set up in the same direction as before within the body, which now, since the body has been reversed, is in the same direction as the E.M.F. existing in the circuit. Hence upon reversal the existing deflection was increased.

From all these data it is evident that a stimulus causes a definite deflection superimposed upon the original deflection which is not always in the same direction. Had these superimposed deflections been due to resistance-changes under stimulation then reversing the hands would not have changed the direction of this deflection. But the above data show that in many cases the *direction of the superimposed deflection is reversed with reversal of hands. This effect then having definite direction must be of the nature of an electromotive force.*

Is the electromotive force produced by stimuli, by emotional states and by various other physiological processes due to variations of secretion-currents in the skin?

That the skin has little or nothing to do with the phenomena under investigation can be proven by a series of experiments in which the skin is totally excluded. The skin was covered with shellac and paraffin leaving only the finger nails exposed. Under such conditions definite galvanometric deflections were obtained, deflections induced by emotional states and physiological activities. The following tables obtained with no cell in the circuit are characteristic:

Table XIX.

Experiments on Dr. St.

Metal Electrodes — Finger Nails Only.

l. h. (+) steady reading	0.0
Stimulus = rising	+ 6.0
l. h. (—) steady reading	0.0
Stimulus = rising	+ 4.0

Table XX.

Experiments on Dr. K.

Metal Electrodes — Finger Nails Only.

Steady reading	0.0
Stimulus = laughing	+ 6.0
Steady reading	0.0
Stimulus = coughing	+ 1.5

TABLE XXI.

EXPERIMENTS ON DR. S.

Metal Electrodes — Finger Nails Only.

Steady reading	+ 19.0
Stimulus = laughing	+ 50.0
Steady reading	0.0
Stimulus = laughing	+ 4.5
Steady reading	0.0
Stimulus	+ 4.5

Similar experiments were performed with tinfoil over finger nails to improve contact:

METAL ELECTRODES.

Steady reading	+ 13.0
Stimulus = laughing	+ 21.0

NACL ELECTRODES.

l. h. (+) steady reading	— 30.0
Stimulus = laughing	— 8.0

This experiment was repeated several times with same results.

l. h. (—) steady reading	+ 30.0
Stimulus = laughing	+ 35.0

In these experiments evidently all skin effects were excluded. Deflections under the influence of sensory stimulations were observed which, as in our earlier experiments, show definite directions and which are consequently referable to electromotive forces in the body.

Our experiments thus clearly point to the fact that active physiological, sensory and emotional processes, with the exception of pure ideational ones, initiated in a living organism bring about electromotive forces with consequent galvanometric deflections.

We take great pleasure in thanking Dr. W. Bernis, Dr. A. Stevenson and especially Dr. D. F. Comstock for the valuable assistance they have given us in performing these experiments.

The experimental part of this research was carried out at the Research Laboratory of Physical Chemistry of the Massachusetts Institute of Technology and at Dr. Sidis' psychopathological laboratory.

THE NERVOUS CORRELATE OF ATTENTION. II.

BY PROFESSOR M. MEYER,
University of Missouri.

IV. Automatic Action.

'Automatic action' is not used by all psychologists as meaning the same facts. Some use it as a synonym for instinctive action, indicating by the former term merely that the instinctive action to which they refer is accompanied by little or no consciousness. Others use the word automatic to designate action which is not instinctive, not innate, but the outcome of habit, indicating by the term automatic that it is a kind of habitual activity which is no longer accompanied by much, if any, consciousness. I shall accept the latter definition. The question then is: What is the nervous correlate of automatic action?

Neurological research has made it probable that automatic action resembles instinctive action in this respect that it is independent of the higher nerve centers, that it may continue even after the higher nerve centers have been destroyed. While this resemblance between instinctive and automatic action has hardly been established beyond doubt, let us accept it as a fact. There seems to be, then, a contradiction between this fact and our theory of habit formation. Habits can be formed out of instincts only by uniting the motor part of one reflex arch with the sensory part of another reflex arch through mediation of higher connecting neurons. How, then, can these higher connecting neurons be eliminated without breaking again the newly formed path? It is obviously necessary to develop a special hypothesis for the explanation of automatic action. Neurology tells us that there are always, even at an advanced age, millions of undeveloped nerve cells in our brain, consisting only of a plain cell body, without any fibers and branches. Perhaps we hit the truth in assuming that the purpose of these nerve cells is to make possible the establishment of automatic action.

Suppose a new habit has been formed by reducing, in the manner described, the resistance of the path leading from the sensory point S_p to the motor point M_q. S_p and M_q are supposed to belong to two reflex arches which are very remotely connected, by connecting neurons of a very high order. It follows from our theory that the *formation* of the habit must then be accompanied by much consciousness, since the nervous current from S_p, in order to pass out at M_q, has to take a very indirect, roundabout path, consisting of neurons over which processes from many different sensory points have previously passed, whose corresponding sensations are now reproduced as images. There is also much opportunity for pleasantness-unpleasantness. Fig.

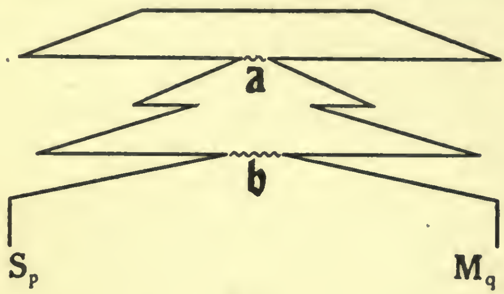


FIG. 2.

2 may be regarded as diagrammatically representing the path of the nervous process at the time when the habit is just established. We notice that at *a* two points of the path are by chance very near each other. Let us assume that in such a case something happens analogous to the electrical tension if the path were a metallic conductor carrying a high-potential electric current. In the latter case a spark would be likely to occur at *a*. Let us make the hypothesis that when a 'tension' of this kind occurs in the nervous system, this stimulates undeveloped nerve cells to send out branches in the direction of this tension. The consequence of this development of a new connecting neuron is a shortening of the path leading from S_p to M_q by putting out of function the part above *a*. This means that the response at M_q occurs with greater quickness and with greater definiteness, since, the shorter the path, the less interference of the current by other currents is likely to occur. It means

further that the nervous process S_pM_q is accompanied by much *less consciousness*, since all those images which depend on a current passing over the neurons above the point *a*, are now excluded.

The same shortening of the path by the development of a new connecting neuron out of an undeveloped nerve cell may occur later at *b*. The total path leading from S_p to M_q is then scarcely longer than a reflex arch. Accordingly the response at M_q to a stimulus at S_p must occur with the same quickness and definiteness as an instinctive response, with little or no accompanying consciousness, and independent of any accidental destruction of those higher nerve centers without which the establishment of this habit would have been impossible. That automatic action is brought about in exactly this manner, we need not assert. Our task was merely to show that the establishment of automatic action, including all the peculiarities mentioned, may be comprehended as a comparatively simple event occurring in the brain.

If the distance between the two points at *b* were less than the distance at *a*, the shortening of the path would immediately have occurred at *b*. This, too, is an interesting conclusion, since we actually notice that habits sometimes pass very quickly from the fully conscious stage into the completely automatic stage, whereas sometimes they become only gradually less and less conscious.

It is plain that the shortening of the path means also that pleasantness and unpleasantness are less likely to occur. Although the path may not be so short that no consciousness at all accompanies the nervous process, yet its shortness makes the process less liable to come into touch with other nervous processes, and without this, as we have seen, pleasantness or unpleasantness is impossible. We understand thus why habituation, familiarity, reduces the possibility of 'feeling.'

V. Vividness and Intensity.

In speaking of attention many psychologists use the term vividness. Let us accept this terminology and call mental states more or less vivid. What then is the nervous correlate of vividness? It is difficult to conceive of its nervous correlate as anything else than the intensity of the nervous process, the flux. But what is the nervous correlate of the attribute of intensity of a sensation? Must we not conceive of this, too, as the intensity of the nervous process? What distinction is then left between vividness and intensity of a mental state if their nervous correlates are identical?

That there is no fundamental difference between vividness and the attribute of intensity seems to follow from the fact that it is practically impossible to be inattentive to an impression resulting from a very intense stimulus. However, there must be some kind of difference. Our theory can help us to understand this. The intensity of the nervous current, the flux, according to our theory, depends on a number of conditions: (1) On the intensity of the stimulus which calls forth the motor response. (2) On the resistance offered by the path leading from the point of stimulation to the point of motor response. (3) On the presence of other simultaneous nervous processes which may be forced to join the process mentioned first. (4) On the presence of other simultaneous nervous processes which are capable of deflecting the process first mentioned from its course. Our *consciousness* is influenced by all these conditions, and in addition, (5) by the directness or indirectness of the path mentioned under (2), since processes in the very lowest connecting neurons seem to be unaccompanied by consciousness and processes in the highest neurons are accompanied not only by the consciousness corresponding to the point of stimulation but also by consciousness corresponding to the points of stimulation from which previous processes took their way over these same higher neurons, that is, by images. It seems to me that psychologists speak of the *intensity* of a sensation in so far only as the *degree of consciousness* is determined by the — objective — condition stated under (1), of *vividness* when the *degree of consciousness* is determined by any of the five — including the subjective — conditions.

With respect to the first condition it is clear that vivid consciousness must result from an intense stimulus in all senses in which (an exception is mentioned below) stronger physical stimulation causes a stronger nervous flux. This is true, however, only when in accordance with our fifth condition the nervous process takes a sufficiently indirect path. A process going on exclusively over a reflex arch is unconscious, not attended to, however strong the nervous flux may be. Yet even in case no indirect path has been specially prepared thus far by nervous function of the past, the process can take an indirect path by diffusion if the stimulation is so strong that the simple reflex arch cannot carry the full process. We may say, therefore, that as a rule a strong stimulus brings about vivid consciousness.

Secondly we mentioned the resistance. However strong the stimulation, there cannot be much flux if a great resistance must be overcome; and there may be a considerable flux even if the stimulus is weak, provided there is very little resistance. However, directness and indirectness of the path are as important here as in the former case. There can be no consciousness, however great the flux, if the flux is restricted to the lowest connecting neurons. We perform many reactions with great promptness and force, and yet without foreseeing them, that is, without attention.

Thirdly, the nervous process resulting from a fairly strong stimulus under favorable conditions of resistance is joined by other and weaker nervous processes because of the law of attraction of weaker processes by a stronger one. This can happen only in case the nervous process passes over higher nerve centers, since otherwise it could not meet and attract the weaker processes. Going on in the higher centers it is accompanied by consciousness. Being strong from the start and further strengthened by other nervous processes, it must be accompanied by very vivid consciousness. At the same time we see — according to the theory developed in my previous article — the conditions of pleasantness fulfilled. This explains to us the law, much emphasized by psychologists, of the parallelism of feeling and attention. The conditions favorable to the feeling of pleasantness are also favorable to vividness of the sensational consciousness. I should think, however, that *parallelism*, not causal relation, between feeling and attention is the proper term to be applied to this case. We have no right to say, either that the pleasantness is the cause of the attention, or that the attention is the cause of the pleasantness, since neither is a regular antecedent of the other.

Our fourth condition is that a nervous process, after becoming established, is deflected by another which is stronger. This means unpleasantness. Here, as in the last case, there is activity in the higher nerve centers, since otherwise there could be no deflection; and the total flux is great because the deflected process joins the deflecting. The accompanying consciousness must be vivid. We see, then, that our statement concerning the parallelism of feeling and attention holds good for unpleasantness as well as for pleasantness.

Thus far we have not found any distinction between the mental correlates of the intensity of nervous flux which would necessitate the use of two terms, intensity and vividness. If there is any justification for distinguishing intensity and vividness of mental states, it can be found only in *accessory experiences*. As above stated, the term 'intensity of the mental state' seems to indicate merely that the subject pronouncing the judgment *knows* that the degree of consciousness (otherwise called vividness) is determined in this case *exclusively* by the objective condition of physical intensity of stimulation. Let us illustrate this by four applications.

(1) In all psychological (especially psychophysical) work concerning the attribute of intensity it is a rule (generally regarded as self-evident) that the subjective conditions must be made, not only constant during the experiment, but also most favorable to the stimulus used; that there must be 'a maximum of voluntary attention.' If we do not succeed in complying with this condition, the experiment is thrown out. This shows that we are concerned here with the degree of consciousness as a function (in a mathematical sense) of an objective condition.

(2) Let us apply our view to the question as to the intensity and vividness of imagery. Imagine a weak sound, and then imagine a strong sound with equal attention, that is, with equal vividness. Has the latter auditory image a greater intensity than the former? Most psychologists will answer: no — there is no difference of intensity; there is no intensity at all. Is this a

strange fact that an imagined sound should have no intensity? I think not. It follows from our theory that the concept of 'intensity of sensation' is not applicable to a case where the intensity of the nervous process, the flux, depends exclusively on subjective conditions, as in our case, where there is no auditory stimulation at all, and where the subjective conditions are identical except for the visual percept of the word strong having been substituted for the visual percept of the word weak. The vividness does not differ, for the percept of the word strong can scarcely have a nervous correlate of greater flux than the percept of the word weak; and of intensity there is none.

(3) Another interesting application is this. How is it possible that the sensation resulting from a very weak stimulus can increase in intensity when attention is given to it? Of course, there are those who deny that this is true. Others, however, assert the fact. In such a case it is well to look for a theory which does justice to both parties, as, I think, our theory does. The nervous process is increased by subjective factors. The mental state shows a corresponding change in the degree of consciousness. There is nothing wrong in saying that the sensation is stronger, as long as we have not adopted any definite language for the description of this experience. On the other hand, no change in the mental state has occurred *in consequence of any change at the sensory point* stimulated. Those who wish to emphasize this fact are quite justified in saying that it was merely a change in vividness. In our *direct* experience, according to our theory, there can be no difference between intensity and vividness. The difference is merely one of *circumstances which may or may not be expressed in our judgment.*

(4) That there are some sensations (*e. g.*, visual) to which the term *intensity* as above defined cannot be applied, is widely recognized by psychologists. But according to our theory such a sensation can possess vividness. Black means the absence of light, but not the absence of stimulation and of a definite nervous process. The nervous process which is the correlate of the sensation black has no less power of deflecting the nervous process which is the correlate of white than the latter has the power of deflecting the former. The nervous correlate of black can-

not be mere nothing, for in that case it could not deflect an actual nervous process. If the nervous correlate of black were not an actual process of variable flux, black could have no vividness — it would be impossible to give attention to blackness. Our refusal to speak of the intensity of a visual sensation expresses merely the fact that the intensity of the physical stimulus in this case does not influence the intensity of the nervous process in the same *direct* manner as the intensity of most other kinds of stimuli does, but through complex physiological agencies in accordance with complicated laws. Nevertheless, the visual nervous process has an intensity of flux, and its mental correlate has vividness.

I can see no objection, then, to regarding vividness and intensity as essentially the same kind of experience, as degree of consciousness. The distinction between vividness and intensity does not mean a distinction between two kinds of mental correlates of nervous flux, but only refers to accessory experiences, to knowledge of the conditions by which the nervous flux is determined in the particular case.

In spite of regarding vividness and intensity as experiences having the same nervous correlate, I doubt if it is advisable to call vividness an *attribute of sensation*, as proposed by Titchener. He calls it the attribute of 'clearness' and regards it as an attribute which, like duration, is common to all sensations except to pleasantness and unpleasantness. These, the 'affective' states, he regards as primitive sensations which, in consequence of arrested development in racial evolution, have not been able to acquire the attribute of clearness. I intend to discuss this matter in another article. It is of fundamental importance in this respect how 'attribute' is defined. My definition[1] differs greatly from that of Titchener[2] — indeed, is quite irreconcilable with his.

VI. Attention as a Faculty.

What does attention, thought of as a power, a faculty of the mind, bring about? It clears up our mental states, Titchener says. It increases the vividness, others say. But all agree

[1] 'On the Attributes of the Sensations,' Psychological Review, II, 1904.
[2] Titchener, *The Psychology of Feeling and Attention*, p. 8, 1908.

that it does more. It unifies mental states. It forms out of the atom-like sensory material unitary groups made up of smaller groups which possess themselves a secondary unity. What is the nervous correlate of this function?

The fundamental law of nervous function correlating with this function of attention, is according to our theory, the law that *a stronger nervous current attracts a weaker nervous current* if the nervous connections and their resistances make this possible. According to this law two independent conscious processes are impossible. Two independent instinctive or automatic processes may occur, for the nervous processes in these cases pass over lower nerve centers only and thus may be unable to act upon each other. But when we have two conscious processes, the nervous processes take their ways over higher nerve centers and thus must inevitably affect each other. Fig. 3 (identical with Fig. 4 of my previous article) is a diagram

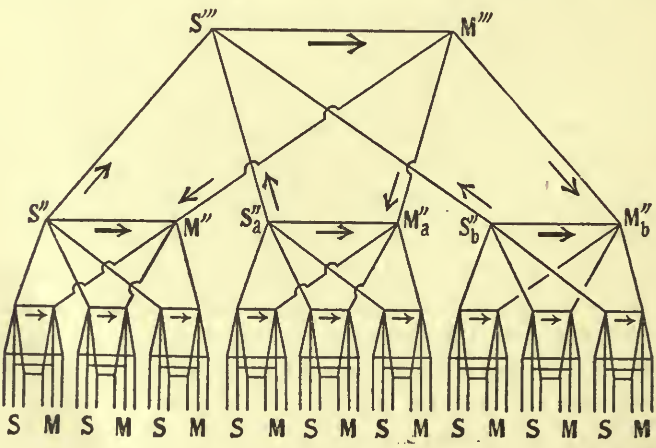


FIG. 3.

illustrating the nervous connections of twenty-seven reflex arches. Suppose the reflex arch belonging to the sensory point farthest to the left and the one belonging to the sensory point farthest to the right are carrying currents. Those currents may go on without in any way interfering with each other. But if the two nervous processes do not take the shortest paths over the reflex arches to reach the motor points, but take the paths up to S''' and from M''' down to their respective motor points, they must inevitably interfere with each other, for the only condition of non-interference, exact equality of the flux for the whole time of their existence, cannot be fulfilled in an organism except by a miracle. If the one process is — even slightly — stronger than the other, it deflects a part of the other's flux. Thus it becomes capable of forcing further processes started later, to join it rather than the other process. Being strong, it soon reduces the resistance of its path, thus becomes still stronger and deflects yet more of the flux of the rival process. The outcome is that we have only one motor reaction instead of two or more; and only one train of thought made up of related ideas instead of two or more unrelated trains of thought. The *unity of consciousness* is simply the result of the above described fundamental law of nervous activity.

At any moment of time the nervous process may be compared with the current of a river. Just as the river is not, as a rule, the result of the direct union of innumerable small creeks, but of taking in here and there large tributaries, so the unitary nervous process receives its large tributaries. Each of these tributary processes is the nervous correlate of one of several chief subdivisions of the unitary consciousness. Thus we have no difficulty in understanding the actual make-up of our 'field' of consciousness, the 'focalness' of a part, corresponding to the main stream, the lower level of vividness of other mental states corresponding to the tributaries, larger and smaller, down to the lowest degrees of consciousness in the 'fringe,' represented by little streams which have but little flux themselves, although without their existence the great flux of the river would be impossible. The question whether attention has but two or a greater number of levels must be answered from the point of view of our theory by saying that *sometimes it has many levels, sometimes only two.* A river may have practically no large tributaries, but be formed by innumerable small streams emptying into a lake. The mental analogon of this is the consciousness on two levels only. The analogon of a river receiving many large tributaries, which in their turn receive many smaller streams, and so on, is the consciousness on many levels. I do not believe that the question as to *the exact number of the levels of consciousness* has any scientific significance.

It seems to me that this will also solve a problem which has been discussed by Sidis in recent issues of this Review. Sidis lays much stress on the insufficiency of the theory which distinguishes only two classes of mental states aside from feeling — sensations (perceptions) and images. He points out that there is a great difference between the kinesthetic consciousness of heaviness when suggested, say, by reading the word heavy, and the kinesthetic consciousness of heaviness when I 'see' that a vase standing on the mantelpiece is heavy. Only in the former case will he speak of a kinesthetic image; in the latter he speaks of a secondary sensation. However, we can also charge this theory of Sidis with insufficiency, for there is a great difference between the kinesthetic consciousness of heaviness when I, sitting on a chair, 'see' that the vase on the mantelpiece is heavy, and the kinesthetic consciousness of heaviness when I, having risen from the chair, 'make up my mind' to take this heavy vase in my hands in order to place it on the table. Shall we, then, distinguish primary, secondary, and tertiary sensations? I do not see the value of these distinctions. There are obviously infinitely many degrees of flux of the nervous processes in the connecting neurons of our nervous system and equally many degrees of vividness. Nevertheless, the theory which distinguishes *sensations* and *images* agrees with our observations in ordinary life, where *as a rule* we find either a very high or a very low degree of vividness, stability, and distinctness (detailedness) of our mental states.

The last problem which I shall take up here is this: Why is it impossible to give attention to feeling? Or — as some psychologists would prefer to state it: Why do we destroy, or at least interfere with, our feeling by giving attention to it? This fact appears indeed very strange to one who regards attention as a faculty, a tool, which we apply to our states of consciousness. Why should we be able to apply this tool to all other kinds of consciousness? But if we attempt to apply it to our feelings, they disappear. This kind of thing seems to fit only a fairy tale. According to our theory, however, the fact is not at all strange. Attention means vividness of a mental state, and the nervous correlate of vividness is the intensity of the nervous flux. But pleasantness and unpleasantness are not the mental correlates of nervous flux, but of changes in the nervous flux if these changes take their origin at points other than sensory points of the body. These changes may be great or small and accordingly we may speak of intense or weak pleasantness and unpleasantness, of intensity of feeling, using the word intensity in its broadest sense, as it is used in ordinary life. But the term 'vividness,' in its special sense, in which we use it in connection with 'attention,' cannot be used here, since it refers to the *quantity of flux*, whereas we are concerned here with the *quantity of change of flux*. That is, we cannot 'give attention' to feeling. This is obviously implied also in the second description quoted above, according to which feeling disappears, when we attempt to give attention to it; but what is meant by attempting — by *willing* — to give attention to feeling?

I accept the theory that the will to give attention is the foreseeing of attention. If we will to give attention to feeling, this may mean that we are vividly conscious of the word idea 'vividness' or a synonym thereof together with the word idea 'feeling,' or 'pleasantness,' or 'unpleasantness.' A vivid consciousness of these *word ideas* has a strong nervous correlate. Now, according to our theory, this nervous correlate must deflect, interfere with, the other nervous process which is the nervous correlate of our consciousness of *the situation*. If the process itself is destroyed, the changes of flux occurring within it are of course destroyed too. That is, we cease to be conscious, either entirely or at least vividly, of the situation; and we also cease to be conscious of its pleasantness or unpleasantness. We see, then, that the fact that we cannot give attention to feeling, that our attempt to do this destroys the feeling, is a simple logical consequence of our theory of the nervous correlates of feeling and attention.

THE WANING OF CONSCIOUSNESS UNDER CHLOROFORM.[1]

BY ELMER E. JONES, PH.D.,
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The phenomenon of the waning of consciousness under an anesthetic is familiar to every physician, yet it is entirely probable that no introspective records have been taken from patients who have submitted themselves to the operating table. The reason for this lack of psychological investigation is to be found in the fact that usually the patient to be operated upon is in no state of mind to give accurate introspective evidence because of suffering, anxiety as to the result of the operation, or intense emotion as a result of some previous experience which renders the operation necessary. Likewise, chloroform to most individuals is so nauseating, and the ordeal of having the drug administered so very unpleasant, that few individuals could be induced to give their attention to the phenomenon of the disappearance of consciousness, and make introspective statements which could be accurately relied upon. Again, physicians themselves do not regard the psychological phases of anesthesia as having any great significance upon the success of the operation, or the recovery of the patient, and consequently no definite experimentation has been carried on.

The field, however, seems rather fruitful. It is probable that it may throw some light upon the stability and deep-seatedness of the various sense impressions, and ideas, and their tenacity under the deteriorating effects of this powerful drug. I am of the opinion also that a thorough experimental study of the waning of consciousness under anesthetics will throw much light on the psychology of death, which obviously enough has never been reported. While not of very much pragmatic value, it nevertheless is of interest, because it is the experience through

[1] Abstract of a paper read at the Washington meeting of the Southern Society for Philosophy, etc., December, 1907.

which all must pass, and probably is the phenomenon which is more universally feared than any other strictly human experience.

The following introspective report is based upon three ministrations of chloroform to the writer, once for the purpose of performing a slight operation, and twice a few months later for introspective purposes alone. In the first event the physicians and attendants knew nothing of the psychological study which their patient was making on himself during the trance. In the latter instances, however, a physician and attendants were employed to administer the drug in the usual way, and to assist in carrying out the tests as previously arranged by the subject. The chloroform was administered rather slowly, in order to give ample opportunity for introspection, and for carrying out the tests previously determined upon. All clothing was removed so that there would be no interference with movements and tactile impressions on various parts of the body. The eyes were uncovered, and every effort made to allow all the sense organs to have full play. The subject being placed on the operating table, the drug was administered in the usual manner as for an operation. It was prearranged to make the following introspections during the waning of consciousness.

First, the disappearance of sense perception, both as to the character of the decrease in acuteness of sense, and the order in which the various senses disappear. In order to study the visual sense the colors of the spectrum were placed on the ceiling directly above the subject, and various objects, such as a book, a few printed words, some digits arranged in the form of a problem in addition, and a few geometrical designs. For the auditory sense it was prearranged that an assistant should read from a book slowly and distinctly, during the entire period of anesthesia, so that the subject might watch carefully the character of the auditory impressions so long as they were felt in consciousness. With reference to touch it was also prearranged that an assistant should touch the body continually in various places with a pointed instrument using as nearly as possible the same pressure. In addition to this test the subject himself, by moving his hands, arms and feet as long as possible, could introspect both touch and the kinesthetic sense.

Second, it was also planned to make introspections upon some of the deeper processes, such as imagery, memory, and reasoning.

At the first inhalation of chloroform there are marked sensations in the vicinity of the heart. The musculature of that organ seems thoroughly stimulated and the contractions become violent and accelerated. The palpitations are as strong as would be experienced at the close of some violent bodily exertion, such as a hundred yard dash, or chinning the horizontal bar very rapidly a few times. The cardiac movements are so accentuated that they are easily felt as pressure sensations on the intercostal muscles, and stretching sensations on the skin immediately over the heart. This violent cardiac reaction is a good example of the efficiency of the sympathetic system in counteracting disintegrating influences in one part of the organism by extraordinary activity in another. Were it not for this stimulating effect of chloroform upon the cardio-musculature the drug could not be used for anesthesia.

Immediately accompanying these cardiac sensations, a peculiar stupefying feeling proceeds throughout the whole body. The blood conveying the drug, as it surges through the body, is felt very clearly throughout its whole course. With each ventricular contraction, as the drugged fluid is forced out into the aorta, it can be clearly sensed in its passage through the various curves and windings of the blood vessels, clear out into the smaller subdivisions, and even into the capillaries. This experience is a decidedly pleasant one, just a little stupefying, and producing in consciousness an effect closely akin to drowsiness, though clearly artificial. One is a little startled at the rapidity of the blood flow, for in a very few seconds every part of the body has been permeated by the chloroform and the anesthetic effects are beginning to be felt.

With reference to the order in which the various types of consciousness disappear under an anesthetic, it should be said at once that there are two sharply defined stages; first, the complete damping down of all the sense organs, so that there is no communication with the outside world whatever; second, the disappearance of memory, all types of imagery, associational processes, reason, and isolated ideas.

In the very earliest stages of anesthesia, probably for the first ten seconds, the visual sense is slightly stimulated. The colors in the spectrum appear a little brighter, letters and figures somewhat clearer, and the light in the operating room, which is always very bright, seems a little more intense. Hearing, for the first few minutes, was almost normal, save a slight roaring, which for a considerable time did not appear to interfere seriously with perfect audition. At this early stage of the experiment various movements were made to test the kinesthetic sensations. For the most part these sensations appeared normal, though the ability to innervate seemed difficult, and to initiate a movement seemed slightly fatiguing. There also appeared in the movements themselves two illusions which were watched with a great deal of interest. First, all movements made appeared to be much longer than they actually were. A slight movement of the tongue appeared to be magnified at least ten times. Clinching the fingers and opening them again produced the feeling of their moving through a space of several feet. Winking gave the peculiar feeling of a great curtain slowly shutting out the light and as slowly rolling back again. Second, all movements seemed much slower than they actually were. Almost from the first this illusion was noticeable, yet the attendants did not detect, in the reactions to the movement stimuli given, any tendency to make the movement slower than under normal conditions. The tactile sense in the early stages of the experiment seemed slightly dulled to the touch of the pointed instrument, yet it could be very distinctly sensed, and accurately localized. At the close of the first two minutes it may be said that there existed a general bodily stupor, accompanied by decidedly pleasant feelings throughout. Senses were slightly damped down and consciousness was agreeably lethargic.

In the three tests made by the writer, the first sense to break down under the influence of chloroform is hearing. While vision is still perfectly clear, and the tactile sense only slightly blunted, audition has begun rapidly to wane. The roaring in the ears, previously mentioned, increases and is accompanied by occasional loud buzzes and thumps. The voice of the assistant who continued reading throughout the test appeared to lose its articulatory value, and short words could not be heard at all, and all words became considerably fused and blurred. Eventually only an occasional very long word could be identified, and this with great difficulty. Direction of sound was lost very early — about the time when the smaller words were first heard indistinctly. After this the words heard appeared to come from nowhere, and the familiar intonations of the assistant's voice could no longer be recognized. After eight minutes the auditory sense is completely damped down and silence reigns.

It should be said just here that all the deeper conscious states are perfectly normal at this time. Memory is not impaired, the imagination is very active, and a problem in addition was added with as much ease as under normal conditions.

The tactile sense is the second to disappear under the influence of chloroform. As in the muscular sense, so here we find some interesting illusions. At one stage of the experiment when the foot was touched with the pointed instrument, it seemed so far away that the subject wondered if it were possible that his whole body were in a single room. With the disappearance of the tactile sense and hearing the body has completely lost its orientation. It appears to be nowhere, simply floating in space. It is a most ecstatic feeling. Consciousness is now almost pure ideas; it is free from any disturbing stimuli from the sense organs, and is probably just about what is meant traditionally by a free spirit, though it is quite evident that is has decided limitations. My feelings corresponded very closely with Cardinal Newman's description of death in Gerontius's Dream when he says,

> "Down, down, forever I was falling,
> Through the solid framework of created things."

Closely following the disappearance of the tactile sense all muscular control is lost. Muscles are contracted with great difficulty, and innervation is greatly weakened. But it is interesting to note that after all movements have ceased, it is still possible to send the impulse to the proper muscles from the motor centers in the brain. At least there is a distinct feeling of the impulse so moving. This experiment, thus, throws some light upon the much discussed question of sensations of innervation.[1] If the impulse is clearly felt to pass from the motor centers during partial anesthesia, when it is impossible for the slightest movement to be made in response to it, it seems quite clear that there must be such sensations; for all other factors save the inauguration of the impulse and its passing have been eliminated.

The last movements to disappear are the most highly specialized ones. The figures could be moved for a considerable time after the biceps and triceps refused to contract. The organs of speech could be innervated to movement a considerable time after most other muscles refused to act, but of course speech was defective, and the attendants stated that after seven minutes, words could not be understood because the tongue was unable to make the finely coördinated movements necessary for articulation. However, tongue movements and movements of the eyes were the very last to disappear.

At this stage of the anesthesia the sense element in consciousness has practically been eliminated, but it remains for us to say a few words with reference to vision. This sense yields to the influence of chloroform more slowly than any other. So long as the eyes could be held open voluntarily, vision seemed quite normal, save that the colors of the spectrum faded out into a gray band, and the details of objects could not be seen very well. It was quite clear that the peripheral regions of the retina are the first to be affected, and that as the anesthesia advances the visual field becomes smaller. After all muscular control was lost the eyelids of the subject were opened by an attendant and vision was still quite distinct. All colors of the spectrum were faded out, but light and shadow, and the distinct outline of objects could be discerned. Figures and letters could not well be seen, but larger objects as a book, a watch, a pencil, and a hat were easily recognized. However, at this stage of the anesthesia the whole visual field seemed smoky and gradually faded out into shadows and darkness.

After all sensations were damped down completely there

[1] McDougall, *Physiological Psychology*, p. 87. Woodworth, *Le Mouvement*, p. 45.

still remained an inner consciousness which for the most part was perfectly normal. Memory seemed pretty accurate, and the reasoning powers only slightly deficient. At this stage the subject successfully imaged the faces of several friends, at least two rural scenes, and a piece of music; but failed to image certain familiar movements, such as throwing a ball, lifting a weight, and mounting a horse. An easy theorem in geometry was demonstrated, and each step in the whole process was as clearly seen as if the subject had had a figure before him and could have used his eyes and vocal organs in following out the various processes before him. The memory was tested by repeating a short poem, which was perfectly easy, and by thinking of the names of the presidents in order beginning at Washington. This latter task seemed more difficult, and James Monroe was the last one that could be recalled. At this point there appeared a pretty general disintegration of ideas, and all associations seemed considerably broken. Ideas actually appeared in spatial relations to each other and many miles apart. They were so infinitesimal that they disappeared very readily, leaving an entire blank. These lingering ideas were some of the very first ones gained in life. Memories of boyhood's home, parents, brothers, sisters, playmates almost forgotten, conceptions of a religious nature long since discarded, and a few æsthetic feelings of early childhood. These last ideas, it is true, were so vague and indistinct that they could scarce be recognized, yet the fact that they remained so long as the residual of weakened cerebral activity shows how deep-seated they are in the mental constitution.

TRUTH AND AGREEMENT.[1]

BY PROFESSOR J. E. BOODIN,

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Both realists and idealists have joined in maintaining that truth is *agreement* with reality. But they have failed to state the nature of this agreement. Is truth a duplicate of reality or is it merely symbolic of reality? If the latter, what is the rationale of inventing this symbolism? Dogmatic realism and dogmatic idealism alike fail to break up reality and so fail to show the different meaning of agrement, according as truth is a copying process or is an artificial device. I hope to make these problems a little clearer in this paper.

The problem of correspondence was a simple affair for naïve realism, because naïve realism only dealt with one kind of stuff, one grade of reality. Whether it is a case of like perceiving like, as with Empedocles, or opposites perceiving opposites: cold perceiving hot; the light, the dark; etc., as with Anaxagoras, we still remain within the one nexus of changes; we still have one kind of stuff. This is equally true of the effluences of Empedocles, the *εἴδωλα* of Democritus, and the forms of Aristotle and the Schoolmen, with the passive imprint which these forms are supposed to make upon the the wax tablet of the mind. With a sharp distinction between mind and body, which took definite form with Augustine and was revived by Descartes, the difficulties as to how one set of processes can make a difference to another set of processes, thickened. So we have the terminism of Occam and the phenomenalism of Hume and Kant. There can, on this view, be no correspondence between

[1] Since sending this paper to the publisher, I have read Professor Baldwin's splendid chapter on 'Truth and Falsity,' *Thought and Things*, Vol. II., Ch. XIII. This takes up the same problem from the genetic point of view, and with important agreements in some instances. As I cannot adequately recognize Professor Baldwin's novel treatment without writing a new paper, I offer this as a supplement to the same discussion, dealing with the problem from the more traditional approach.

knowledge and reality, for knowledge moves within a world of its own. It is at most a sign language. We can know nothing about the real world. We know it only as it terminates and is elaborated in our experience. There can, however, be phenomenal verification or anticipation within experience. The world of shadows, also, to use Platonic language, has its uniformities, which make prediction possible. If we are doomed to the world of shadows, we can at least get ready for future shadows.

Idealism, in insisting again upon one kind of stuff, *i. e.*, mind stuff, tries to return to the original simplicity of like acting upon like. So long as the question of the ego is not raised, the problem is easily stated as merely purposive realization or logical connection within one context or unity of thought. When the question is raised, however, as to whose experience or unity, the problem grows more difficult. The idealist must either raise himself into a solipsistic absolute or, in modestly recognizing his own finitude, face the dualism of an internal and external meaning, and struggle over the seeming fragmentariness and darkness of our world.

A new theory of knowledge has been developed in recent times by William James and others, which tries to avoid the idealistic difficulty and presumption by treating knowledge as merely an instrument having no relevancy to the object to be known, but being valid in case it can be exchanged, in the course of the process, for immediate experience, as wares are exchanged for gold. While such a theory, with abundant illustrations from natural science, accounts for how knowledge can control the world of processes, it leaves us in the dark as to the real question — the relevancy of knowledge to its object.

Before we can have purposive selection and correspondence, our selection is determined by our instinctive tendencies. The infant does not have any definite program ; it is not as yet a self and so is not concerned about self-realization. It is so constituted, however, as to respond in characteristic ways to certain stimuli, such as moving things, bright things, loud things, things to eat, to grasp, to be afraid of, etc. There is no question of intention here and therefore no question of truth. The infant, as the result of the evolutionary process, is such a slot as can be set off by just such pennies. What adaptation, fitness or correspondence to its environment there is, *means* fitness or correspondence only to a more developed stage of experience. Its movements do indeed show a certain degree of adaptation, its sense-responses may be said to correspond to stimuli of so many vibrations per second. But they do not mean correspondence to the infant.

Agreement *means* agreement only when we intentionally select in the realization of a certain purpose. Only then does truth or error exist. If I point to Peter when I mean Paul, to white when I mean black, I have failed to carry out my intent and so have erred. To correspond or agree means to realize my purpose or at any rate to be able to act as if my hypothesis were true. Correspondence, however, has a two-fold significance, the instrumental relation of the knowing attitude to its object and that of sharing, to use a Platonic term.

In so far as reflective thought sets its own conditions, irrespective of the inner meaning of the processes, to which it refers, aiming simply at prediction or control of the object as a means to its own purposes — in so far thought is instrumental. Whether the object has any meaning itself or not, such meaning or claim is ignored. And thought must always be instrumental when it deals with that which is immediate and which, therefore, is transformed and done violence to in being dealt with reflectively. This is equally true of brute immediacy and of immediacy on the higher æsthetic level, which presupposes thought life. If reality, therefore, in its ultimate meaning must be conceived as mystical appreciation, which passes knowledge, as the mystics from Plotinus to Bradley have insisted, then knowledge would always have to be instrumental. Again, in bringing our categories — the result of our instinctive equipment and social, historic setting — to bear upon the sense material which furnishes us with our data of nature, with its coexistences and sequences, we can only hope to have instrumental or phenomenal knowledge. We cannot agree that because nature can be made to realize purposes, it is itself purposive, any more than because a knife cuts meat it must itself be meat. It must indeed be something, *i. e.*, it must be capable of making predictable differences to us. But we cannot treat it as purposive. If there is purpose governing nature, it must be extra-natural, determining survival. The old idea of correspondence, which Kant subjected to such searching criticism, deals with this relation of the concept to the non-reflective or physical world. Here it is easy to show that there can be no real correspondence or copying as we cannot get at, much less reproduce, the inwardness of the processes which we investigate. *We* make the system of nature — unify it, in obedience to our tendencies, on the one hand, and the data of immediate experience on the other — so as to meet the requirements of the environment and, so far as possible, control it for our needs. We are here limited to phenomena.

Sometimes even knowledge of ideal objects is legitimately of this instrumental kind. Treating the circle as made up of infinitesimal straight lines, though convenient, does not correspond even with our ideal reality. The census tables do not correspond to any real order. They are sorted facts for an artificial purpose. Sometimes we ignore the claims of the reflective consciousness, because we regard it as criminal or pernicious to our standards of truth and right. But sometimes we ignore the claims of other meanings because of our moral blindness. The cardinal crime, the crime of crimes, as Kant has shown, is to neglect the inner significance of our fellowman and to treat him merely as a thing. What we respect as having a claim on its own account must differ widely, too, in different stages of development. For the savage, what is outside of the tribe has no meaning which needs to be respected. On the other hand, nature phenomena, ghosts, etc., are treated with more than human respect. In general we find that it is easy to recognize a meaning if it agrees with our own, but difficult the greater the divergence.

Knowledge may be instrumental, then, for two reasons. It may be instrumental because it lies in a different dimension from the object it strives to know. It may be a systematic arrangement, in the service of our purposes, of facts which themselves *know* no system. This must hold wherever science deals with non-reflective facts, as with the physical sciences. It holds of the psychological sciences, too, when they are not dealing with processes of the reflective or meaningful grade, or when they are decomposing the reflective attitude for purposes of naturalistic description. In so far as our analysis and reconstruction must always fall short of the real object, all our knowledge becomes infected more or less with the instrumental character. We can never, in our description, give the complete equivalents of the real gold or the real Socrates. This can be only when our purpose creates its own object.

But some objects of knowledge at any rate have a meaning of their own, a rational purpose and value, which we must acknowledge. Even here, knowledge, to be sure, must be in some degree instrumental, as we have seen ; but this is only incidental, a stage in the process of sharing or sympathizing with the object. The problem here is no longer one of mere manipulation. The correspondence here cannot be exhausted in the one-sided relation of hypothesis to immediacy within the process of individual experience. The judging attitude here is a different one from that of means and end. The fulfilment of our purpose here is conditioned upon partaking of an extra-individual realm of meanings, respecting and sympathizing with them. We do not want to make over or control Shakespeare's Hamlet or the Sistine Madonna or the friend that we love. We want to understand and appreciate them. Our knowledge, when it is concerned with social or ideal structures, is primarily of this sharing character. It is not the business of the historian to make over the past; but to understand it or share its meaning. Even when our aim is that of the practical reformer or when we must revise the scientific hypothesis, it is first incumbent upon us to understand or share the ideals which we would revise or reinterpret. To fail to recognize in the universe any purpose but our own, is to be a bore or a criminal. Some individuals must be respected as having a meaning of their own and cannot be treated merely as things, if we would live fairly and, in the end, accomplish our purposes. To be sure, our limitations as finite beings and as part of the time-process makes such sharing difficult; but it remains, nevertheless, a real aim. Plato has a word for us, as well as the modern instrumentalist.

In instrumental knowledge, as we have seen, the question is merely how the facts *seem* to us; how they can be controlled by us; whether our concepts terminate in perceptions. Not so in the knowledge of the sharing type. Here the truth attitude is not merely an artificial tool, like an astronomical ellipse or a census table; but of a piece with the real object which we strive to know and accommodate ourselves to. The knowing attitude and the object are of the *same kind* or belong to the same grade of reality. In so far as the knowing attitude here can be completely realized, it is no longer *of* reality; but it *is* reality. To know the meaning of Hamlet is to have the reality of Hamlet. Leibnitz's monads are a splendid illustration of a universe which might exist in many copies.

To be sure, here, too, the concept or hypothesis must terminate in immediate experiences, present or future, within our individual history. But these become signs, real energies though they are in their own grade, of another reality which we strive to reach. We do not stop with the spoken or printed words. These become symbolic merely of the meaning. The difference in the two attitudes may be said to be a metaphysical difference, *i. e.*, a difference as regards the ultimate intent of the knowing process, rather than methodological. The finite test of the correspondence in either case, the test available from moment to moment in individual life — whether in knowledge of the instrumental or sharing type, is an internal test or the corresponding of our purpose or hypothesis with the ongoing of experience. It means an attitude of fulfilment or forced acknowledgment in this ongoing.

The knowing process, however, is *really* valid, only when it reproduces or copies the object, *is* the nature of the object. The only valid hypothesis about a reflective object is the attitude that acknowledges the meaning of the object and succeeds in sharing it — aims beyond sense-experience at its metaphysical reality. Whether this aim or intent is true or not must be tested, as in the instrumental case, with reference to further experience. But this attitude, if true, *terminates in sharing* and not in mere perceptions and their uniformities.

Another center of experience is acknowledged, which has put its prior stamp upon our self-stamped facts. The attitudes in the cases of sharable and non-sharable realities are built out in different ways; the former has over-beliefs that the latter does not have, and so requires a different verification — a verification including the over-beliefs. When such sharing is impossible, we must be satisfied with such artificial or phenomenal correspondence as the uniformity of our perceptions makes possible.

By copying I do not mean a mere photographic copy, as is sometimes meant. I cannot see what meaning such copying has in the process of knowledge. To suppose, for example, that our sensations are copies of independent characters of the object, assumes a duplication of our sensations to which I cannot subscribe. The sensations are not copies; they are definite energetic relations of our psycho-physical organism to the objective world. Neither are our images as such copies. They are relatively persistent processes of experience, modified by intervening rearrangement. They become representative when, at least functionally, they are the same in more than one context, and therefore when excited in the context of present experience can suggest another context with its dynamic coefficient and time value. When it comes to meanings, the question of copying, even as regards our perceptual meanings, can only arise when we have in mind the sharing of such meanings by several subjects. What the copy theory of sensations implicitly assumes is a social consciousness, finite or absolute, in which the sensory qualities exist as such, and therefore the individual must regard them as prior to his experience. But that simply amounts to that they are not arbitrary, but arise under definite conditions.

I agree with the realistic insistence upon the transsubjective reference of the momentary meaning. But the paradox, often pointed out by the realists, that the object must be both in and out of experience, must remain an absolute mystery so long as we deal with meanings as subjective pictures, enclosed within the magic circle of an epiphenomenal consciousness. This paradox is ignored, not solved, by having recourse to mystical or esthetic theories as regards the continuity of the meaning with reality. If we, however, regard the universe under the conception of plural energetic centers, with various ways of connecting up, and some at least capable of inner content and meaning; and if we regard purposes as themselves energies, evolving in complexity in conjunction with, and having survival value through their control of, other energies such as the physiological, then the paradox is resolved, even though the practical limitations remain. We have at least found a motive for our ideas seeking agreement with their intended reality, for successful adjustment depends upon such agreement.

The object at any rate is more than the intent. If the drama of reality consists only in a series of subjective doubts, readjustments and satisfactions, then Protagoras is indeed right, if we may trust Plato's quotation, that "to whom a thing seems that which seems is." But in that case, what need could there be of readjustment within the stream of meanings? Why does not the meaning at any one time exhaust 'the situation'? Why should there be failure or the necessity for accommodation to a larger world? Evidently the meaning does not exhaust the reality of the object.

This inadequacy of the internal meaning to constitute its own object can be shown equally well on the level of sharing as on that of instrumental knowledge. Is Ibsen's meaning made or created in each stage of the process of the reader's interpretation? Is not the object here something preëxisting and external — not made by the critic? And must not the critic's meaning conform to this in order to be valid of Ibsen's meaning? By ideal construction we try to reproduce for ourselves the meaning of Ibsen's play. We gather data accordingly; but the truth we have first when our meaning imitates the other meaning, when it gives an adequate copy of the other meaning. In such a case the idealists are quite right that the agreement must be with truth, an objective constitution of truth, and not merely with immediate experience. I cannot, however, see what agreement with truth can mean unless you assume that the object itself is a truth process. If the universe as a whole is truth, a system of experience, then of course all truth ought to be a copy process. But I do not think this has been proven. Stringing nature on our reflective unity does not make nature a reflective unity. There is, in so far as we know, no truth or system in nature. Nature only furnishes certain changes, interactions and constancies which we can seize upon and systematize to suit our needs.

In the case of the knowledge of other egos, we easily recognize that there must be not only internal fluency, there must be also an intent, a creative imagination, taking us beyond the stream of subjective processes. Other egos must be *ejects*, not mere percepts. Hence no theory of mere fluency or coterminousness is sufficient. There must be this but more. And if the other egos respond *as if* our intent were true, then we share their meaning. In regard to nature, too, what we *intend* is not merely immediate experience, whether sensory or affective. Sensations are not the object of sensations. Satisfaction does not give satisfaction. By the uniformity of nature we do not mean mere sequence within experience, but a regularity in nature which accounts for the uniformity of our perceptions and to which we must accommodate ourselves. While in the case of nature, the inwardness must remain problematic, here too, as well as in the case of our fellow-men, the ego means more than the stream of individual experience. It means to meet and adjust itself to a world beyond that experience, even though capable of being energetically continuous with it. This objective reality, in however phenomenal a way, must ratify our intent.

The immediatists themselves have fretted a great deal lately at their misinterpretation by others. But why should they fret? Their critics, realists and idealists alike, seem to be satisfied with their interpretation; and that is all the immediatists ought to ask. If they say that the critics ought not to be satisfied, they have evidently insisted upon a reality beyond immediacy and something beside subjective satisfaction as the test of truth — upon correspondence with an objective reality.

We never shall have a true theory of knowledge until we recognize the complexity of reality in its various stages. We have seen that those who have made the knowing attitude exclusively instrumental have borrowed their illustrations altogether from the physical part of reality. They talk about knives and chairs and chemical formulæ. They are apt to ignore another part of the environment, which to a human being is at least equally important with the physical, viz., the institutional. Could the object be treated altogether without any reference to any purpose or meaning of its own, then the instrumental theory would indeed cover the field. Were reality through and through reflective or conceptual, on the other hand; must we acknowledge it as one system of meanings, then Plato and all his disciples would be right, that all knowledge in the end must be expressed in terms of sharing or imitation — a copy of the inner meaning of the processes at which it aims. In so far as it should succeed in this, the distinction between truth and reality would disappear; the idea would *thicken* into being. As it is, it is both sanity and fair play to treat reality as its nature demands, instrumentally, where no purpose need be acknowledged; sympathetically where the conditions so demand.

Whether a man shall be an idealist or a materialist is not a matter of consistency, but of claims which we must meet. Where we must recognize ideals, as in dealing with the institutional life of the race, we must be idealists. Where our ideals have no inner relevancy to the processes with which we deal and the aim is merely control, we must be materialists. Here a one-sided *a priori* consistency is as mischievous as in other departments of life. To institutionalize nature by giving it reflective life and ideals of its own is to leave evidence for fairy tales. To ignore purposes and meanings, where we ought to understand and meet them, is to show one's lack of imagination and unfitness for social life. Thus the truth of Plato, as well as of Kant and James, is recognized. The one-sidedness of the instrumental theory consists in ignoring that part of the evironment which is institutional; is itself meanings or ideals. The one-sidedness of Plato and his followers is that they attempt to institutionalize nature as well as man.

But the instrumental theory does not satisfy the claims of the successive moments of individual life any more than it does the social claims. It is not fair to regard each moment of appreciation or reflection as a mere instrument to another moment. If each moment has no significance or worth of its own, is a mere instrument for meeting a future moment, then life as a succession of moments can have no significance. Instrumentalism, bare and simple, must lead to brankruptcy. Each moment must be respected as end, as well as means. Every genuine moment is a thing of beauty and of joy forever, as well as the parent of a new moment. And again, every false and perverse moment is a tragedy never remedied, as well as a call for reconstruction, if there is such a call, or an obstruction to further living. The universe, in other words, is not merely fluid. If it were, it would be nothing. Each moment and each stage of life is an individual reality with its own warm and living meaning, which to lose is to lose all.

The knowledge of purpose by purpose I have called real knowledge. It is so to a degree at least, *i. e.*, just so far as the purpose, whether institutional or individual, is grasped. Real knowledge is knowledge of the thing-in-itself; and human purposive wills are among such things. Knowledge of the merely instrumental kind we may, out of regard for Kant, call phenomenal knowledge. If we say that these attitudes, *i. e.*, the instrumental and sharing attitudes, are different hypotheses in regard to our world of objects, we must not forget that these hypotheses, owing to a long survival process, are instinctive or intuitive so far as the individual is concerned, long before they become conscious hypotheses or postulates.

The confusion in recent discussions has come in part at least from the failure to distinguish between truth and reality.[1] Truth is our version of reality. The geological ages existed as characters or processes of reality long before we discovered them, but the truth about them did not exist before we discovered them. It is nonsense to speak of an hypothesis, which is our meaning or attitude, as true previous to verification; but previous to verification there exist certain conditions, which make some hypotheses come true. These conditions, in most cases, are not altered by our hypothesis. The chemical properties of gold are not altered by our faith; the condition of our nerves may be. The 'laws' of nature are contributed by the man who discovers them; and science very properly, therefore, deals with the laws biographically, as Newton's law, Carnot's law, etc., though once discovered they become social and eternal.

[1] It is very evident that we need to use terms in a technical sense in order to prevent the discussion of truth from being more than a play on words. I believe, however, that we would only increase the confusion by adopting the distinction between truth and truthfulness suggested by James in the *Journal of Philosophy*, etc., for March 26, 1908. The term truth has a definite meaning, and it is hardly possible or desirable to change it.

Nature furnishes existences, uniformities of various sorts, but no laws, no truth. These laws or expectancies become true when nature behaves in the predicted way. This is all that correspondence in regard to nature means. It is not a one to one correspondence, as we only hit at best a few places of reality; and only a few are significant for us. Truth, looked at from the individual point of view, becomes agreement with truth, when we imitate or make our own truths already existing, hypotheses already verified, social truths. Here we do copy truth, within the limitations of human nature. Truth need not mean, and cannot except to a small extent mean, individual verification. An hypothesis or law is true, if some one has really verified it. Going over it again in such a case does not make it true. It simply relieves our nervousness and confirms our belief. But our belief or doubt neither verifies nor undoes the verification of an hypothesis, though it may furnish a motive for testing it.

As I see it, both the anti-pragmatists and the pragmatists have contributed to the confusion — the anti-pragmatists by tacitly, often unintentionally, assuming an absolute system of truth with which we must agree; the pragmatists by their intense individualism in practically insisting that truth is not truth, unless it has passed through their particular cranium. Of course a truth is not *my* truth unless I make it my own by going over its grounds, tracing it to its termination in the intended facts. But going over an hypothesis already verified does not make it true or valid. This is a social fact. Whether I make it my own or not is tremendously significant for me, but not, unless I improve upon the hypothesis, a contribution to truth. Whoever the legatee or individual producer of truth may be, it is quite sufficient that truth exist in one individual consciousness, as his systematic meaning, whatever the other individuals may mean. If everybody should sleep the sleep of Endymion, there would be no truth. If, on the other hand, there is an omniscient, ever wakeful God, his possession of the truth would give it all the *validity*, that its possession by billions could possibly give it. The question in any case would be, Does it terminate in facts? Does it, as judged by either past or present or future experience, or all of them, meet the reality we intend or which is forced upon us?

N. S. Vol. XVI. No. 2. March, 1909.

The Psychological Review.

TOWARD THE CORRECTION OF SOME RIVAL METHODS IN PSYCHOLOGY.[1]

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I.

The ampler purpose which marks as a class the psychological studies of our time is made evident by the variety of ways in which we can view the one aim of all our work. We should all agree, I doubt not, that this one aim is to understand the mental life, to bring it int othe light. Yet this purpose, which in some careless moment may seem so single and alike for all, can break up in an instant into a thing of many parts.

To understand a mental fact means, for some of our number, to dissect it, to show its internal construction; for another the center of understanding is reached when we view the thing in living action, noting its behavior. Still another believes that no fact is understood until its causes can be told; or perhaps he finds chief satisfaction when the fact is put with those that are like it, when it is classed, defined and named. Another's idea of understanding is to view the object in its proper system, not according to mere likeness, but according to vital continuity: it is part of a person, and cannot be understood except in its full relation to the self. Another insists on the story of the growth of the mind in which the fact occurs; while still another is less eager for causes than for results, and declares that the effectiveness of anything living is the key to its explanation, that we must see its office, its function in the larger economy of life, if we are to understand it.

[1] Presidential address, before the American Psychological Association, at its Baltimore meeting, December, 1908.

This variety of interest is a sign of health in the intellect. We are supplements, fulfillments, of one another. We are beginning to be aware how intricate is intellectual interest, and that whatever really interests us about a fact has a place in its understanding. Pedants may say that to understand is to deal with the fact after some special mode — that it is analysis, or narration, or the disclosure of its genesis or of its effects — but the tide of intellect washes away their petty boundaries. The mind will play freely around and through its object. What interests us in the mental life we wish to see inside and out, and all that it is connected with, all that it can change into, or be like, or accomplish. No one can set bounds to curiosity, and science is but curiosity drilled and organized.

This rounded view — let us not call it 'explanation,' for this is apt to imply an interest merely in causal antecedents; nor let us call it 'description' or 'narration,' for these too readily suggest an utter want of such an interest; perhaps some less trammeled word, like 'explication' or 'elucidation' will serve us best — this free play of the mind over and through its object is the purpose of our science. It is a wide purpose wherein many talents coöperate. Our work, we may be thankful, is rich and vital enough to permit and encourage sects. With perfect justice we can have and avow our personal preference and aversion: your gorge rises at the demand to define and classify, or perhaps to analyze; but others find in these their meat and drink, and are unnourished by your explanations and genetic accounts. Let sects increase, but with them a spirit of toleration, even a hearty appreciation of diverse gifts. For all our personal bias, the science itself is catholic, it feels justified of its children. Whatever rivalry of method there is, then, lies in us and not at the heart of psychology itself.

Indeed, since in all likelihood the mental world is not a whit less spacious than the physical, the full explication of the mind will call for as many sciences as the physical world requires. What we call psychology is really a writhing brood of young sciences, and he can have no feeling for the future who would try to stifle any of them. The apparent rivalry of our aims and methods is in part but the sign of the coming time when for the mind there will be something analogous to physics and chemistry and geology and geography and astronomy, organized to survey in distinct ways the same great system of phenomena.

Thus I make no plea for any special type of explication, believing as I do in the policy of the 'open door.' The catholic temper welcomes all these types, since all are needed for the work. But the policy of the open door, as we know, does not mean the policy of *laissez-faire*, does not imply that we are to keep hands off of these partial methods of understanding mental facts, leaving them undisturbed in their present form. If the science needs them, it needs that each should be at its best, that each should show an inward vigor and symmetry, so that the purpose for which this particular mode of elucidation stands shall be accomplished with honor and adequacy.

A believer in them all may thus criticize without jealousy, working for their improvement. And this is what I shall venture to do, with your indulgence, in the present hour. I shall speak of the weakness or want of balance that appears in the exercise of the three methods that have to do with the Significance or Office of a mental fact, with Causal Explanation, and with Analysis. Of these three, the third alone, that concerned with analysis, will be dealt with at any length. I beg of you to expect no novelty in what is offered; it will be at best but a revisiting of old scenes. Yet our science rightly teaches that old thoughts reappearing can never seem quite the same.

II.

Those of our number who are attentive to the office and significance of mental processes hardly propose, I imagine, to discover the ultimate end of any mental fact, a work more appropriate to philosophy and religion. But in ends that come within the circle of our observation, psychology has a deep and proper interest. The rôle which a single mental item, or which mind as a whole, plays in the system of observed events has long attracted men, and they have conceived of it in various ways. Moralists have dwelt upon the ministry of all outward things to man and especially to his inner nature. Yet among moralists there has also been the opposite thought, that contact with the physical world, the enlargement of sense-experience, takes from the mind's power of true perception. The mind is a prisoner in the body (we remember from our *Phædo*), and reason comes into its own when we withdraw from this world and all its confusing images. Empiricists, in a somewhat different vein, have insisted that the very form and structure of the mind comes from external events; it is as wax taking the impression of outer things. But in these diverse accounts the one theme is the influence of the outer world upon the mind.

In contrast to this entire mode of thinking stands parallelism, the denial that the physical has any real effect upon the mental life, or contributes to it in any way. But though many of our number still call themselves parallelists, this does not keep them from the still more modern thought, different from all those yet mentioned, that the mind is merely a handmaid of the body. Instead of asking, why the mind has a body, we now ask why the body has a mind. Memory and intelligence have 'survival-value,' we are told, and this explains their presence in the organism. They perform a function not unlike that of kidney and liver: they ward off destruction, aid in adaptation and control, help the group to multiply. The excuse for mind is that it can get into our muscles. The cognitive operation, says one of the ablest of our functionalists, has its 'whole significance' in this, that it is a device 'to further the efficiency of the motor response'; memory, imagination and reasoning are 'simply half-way houses between stimuli and reactions.' It is not, you note, that the mental operation has *some* of its significance in the fact that it helps the motor apparatus; it has its *whole* significance in this. We might likewise say that the feet have their whole significance in the fact that they transport our shoes.

For empirically there is just as much reason to say that the body contributes to the mind as that the mind helps the body. The facts, if we divest ourselves of preconceptions, point both ways. The relation of the mind to the body is most intimate, and in many respects each appears master and each appears slave. Now men who are chiefly interested in physiology and in the development of the body may be excused for viewing all things as furtherers and modifiers of the bodily life. But I cannot but regard it as a weak yielding to external attraction when we psychologists follow a neighboring science in this particular bent. A more balanced scientific judgment would be shown if we refused to regard the mind merely as a servant of the body or merely as its lord, but stated calmly and without undue assumption the exact nature of that intercourse between mind and body that appears under careful scrutiny. In the whole range of our modern discoveries there is nothing whatever to indicate that it is truer that intelligence has survival-value than that the body performs an educative function for the mind.

And this same balance of judgment would also be in harmony with a right consideration of the reflex-arc, that has influenced our thought so deeply, making it appear that consciousness is but a link in a chain whose beginning and end is in the physical world. All consciousness is motor, all things mental are for the sake of muscular reactions, we have long been taught and long been teaching. But the whole process, so far as the facts are concerned, can quite as well be viewed from the opposite side. Every reaction of ours alters the world of our experience; every muscular movement brings a change in the field of our sensations. The reflex-arc process is in reality circular; the motor act, in its turn, stimulates our sensory nerves. And by looking at the process, no man can tell which is beginning and which is end.

Now though we be heady, and assume to know outright what is the entire office and significance of mind, yet our functional method itself commits us in no such a way. The method simply implies that the mind has some office, has some significance that can be discovered empirically. Our true course then is to advance without prejudice, telling what we find to be the value of the mental for the physical, but quite as truly the value of the physical for the mental, and of the mental for itself. Such a preservation of balance in our functionalism does not mean the introduction of some special philosophical system into our science. We shall not be called upon to take sides as to whether the mental or the physical is the more fundamental of realities. The rounding of the method will simply make for poise.

III.

With this brief word as to the proper method of studying the office and significance of mental facts, let us pass to explanation, to the interest in causes. Here, as before, I shall urge the avoidance of unnecessary assumptions, shall urge empirical reserve.

Now those of us who are interested in causes are right in assuming that mental events are caused; so much is proper and essential to the method. But some of our number do not stop with this; they assume that the causes of mental occurrence lie exclusively on the physical side. One of our most honored members, as we all know, leans that way; usually he can be relied on to declare that the cause of a particular fact is neural, and to offer a brain-diagram. The cause of such and such mental phenomena, he tells us, is the law of habit in the nervous system. It is not until some special schematism which he proposes is 'incorporated in the brain that such a schematism can represent anything *causal*.' And another whom we honor states, in his unfaltering way, that the causal relation cannot apply to things mental but only to things physical. With persons of this view, it is interesting to mark the sense of accomplishment they often show, upon translating into neural terms some definitely observed mental event. Here at last the facts are actually explained!

No one need object to this easy translation of mental events into brain events, except that it gives an illusion of discovery. But it does seem a false step in method when we assert that only in the region of brain-action is there anything causal. On the whole it tends to discourage the search for psychological antecedents; it closes the door upon these, and confines the problem over to a region where as psychologists we are not equipped to follow it. As a device of research it therefore seems ill-judged.

The assumption that all causes are physical is due to a number of motives, of unequal weight. The field of consciousness, with its deep transformations in sleep and stupor and in those strange amnesias and resurgences in the hysterical, seems to display less stability and continuity than does the brain, and consequently to be a less hopeful region in which to find steady causal connections. Yet our increasing sense of the richness of the unobserved mental life — the rich regions that are beyond our introspection — will perhaps more and more weaken this appearance of mental interruption. While we usually assume that a mental event is connected with some neural process, yet we can actually observe its frequent connection with certain mental events, and conceivably this connection is universal. To brush aside all this direct experience of the mental setting, all the observed connection with other mental facts, and to give explanatory value only to the physical connections seems to me a kind of affront to the established canons of induction. Does it not arise in a large measure, not so much from the persuasion of evidence and of logical need, as from a certain instinctive emphasis on physical objects — from an extra-scientific prejudice to which we are subject and which illicitly affects our scientific methods?

Moreover if we admit a distinction between things physical and things mental (and without such a distinction there is no excuse for psychology's existence), and if we then declare that the causal relation holds only between physical events, this naturally implies that the mental event is really uncaused and not open to 'explanation.' Yet but few are willing to admit the utter impossibility of a cause for things psychic; they usually assume, rather, that mental events are caused, and caused *physically*. In this case it is believed that the causal tie not only binds physical items together, but also can bind a physical cause to a psychic result. But if we can defend the assertion that the causal relation can, at least at one end, attach to things mental, I do not see why theoretically we could not consistently have the other end also at times attached in the psychic realm. Our difficulty in conceiving mental objects to be *efficient* is largely due, I believe, to physics, with its specialized and rigid idea of causation. Rather than break with this authoritative idea, most of us would rather affirm that psychology can never hope to be an explanatory science.

Yet we must remember that the exact marks which two events must display before we can regard them as cause and effect are themselves, for the most part, determined empirically. Our tests of the causal relation differ from time to time and from science to science. The idea of causality should normally be in a fluid state; only those of scholastic temper would have it crystallized. Psychology, like any other science, is free to modify the idea of causation to suit its own system of facts. Indeed we have in something very close to Hume's account a conception to our needs. When we are ready to regard as causal any group of antecedents that observation invariably discloses, then we are freed from the *a-priori* assumption that causes must always be physical. In our psychology we then become empiricists instead of *a-priorists*, and our method of explanation no longer carries an unnecessary load. We are ready to accept as a cause whatever on sufficient observation seems to be a cause. If, after careful testing, the causes all turn out to be physical, well and good! But observation itself does not as yet point strongly that way. And in assuming what test and observation do not indicate, are we not making a somewhat foolish concession to that impulse to settle things off-hand and to regard as ineffectual whatever cannot be weighed and handled?

With this we may pass from the method of explanation. I trust you will not think the meaning here to be that psychic causality should supplant the physical in our work. On the contrary my intention is that we should impartially accept any causes that can show credentials, whether they come from the physical or the mental realm. The unencumbered truth seems to be, that the cause of anything mental is always a strange mixture of elements from both regions. If you and I prefer to lay stress on one of these sides to the neglect of the other, this should be recognized as a personal trait, a matter of taste or convenience, and not as the outcome and utterance of the scientific method itself.

IV.

In the two important types of elucidation that we have considered — the functional and the explanatory — the eye plays over the surroundings of the phenomenon, taking in its outer connections or setting. We shall now turn to a different type of investigation in which the attention is held upon the phenomenon itself, noting its inherent character. The frequent defects in this mode of research seem to me so to color the supposed 'results' of our work that mere carnal considerations of fatigue must not prevent a somewhat extended discussion of it.

An account confined to the phenomenon itself may take a form either narrative or descriptive; but in either case, at the heart of the method lies analysis. And with regard to analysis and the test of its success, most persons would agree with G. H. Lewes, that it is perfect 'when the pieces that are obtained can be put together again, and form the original whole.' Psychological analysis accordingly is understood to mean the discovery of the constituent elements of the mental fact before us — its sensations, perhaps also its affective features, or whatever else is regarded as belonging to its simple ingredients. In thinking of this analytic work the picture that almost inevitably comes to mind is from the chemical laboratory, when, for instance, water is broken up into hydrogen and oxygen, in the proportion of two to one; or sulphuric acid into H, S, and O, in the proportion of two to one to four. On receiving such a description of the fact we seem admitted to its secret constitution.

Yet in attempting to carry out a similar analysis in psychology a difficulty at once confronts us. Is the nature of the mental compound accurately seized, after all, when we have told off its constituents, even in their right proportion? To many students no such scruple occurs; for them, to recount the simple parts is to describe in the one perfect way the complex fact itself. And yet nothing, it seems to me, could well be farther from the truth. For the original mental fact which we would describe has, in most instances, what we might call architectural features, and its nature and quality consists not only in the character of its materials but in the manner of their union or arrangement.

If chemical analysis has misled us here, it can also set us right. For chemists are now familiar with the fact that the same elements, combined in exactly the same proportion, may give now one compound and now another, each with its own peculiar properties. And therefore in fully describing such compounds the chemist is forced to tell, not simply the elements that enter into them, but their manner of arrangement: this compound has a right-hand arrangement of its atoms, this other compound shows a reverse order, a left-hand arrangement. Lactic acid, tartaric acid, and a number of other substances, reveal differences of this kind.

Any analysis that names merely the ingredients may therefore miss the full truth; it may note no difference in compounds that actually are different. The safe and reliable description of the more complex mental facts accordingly requires that our idea of analysis be revised to include an attention to the architectural features of such phenomena, including of course their manner of change. Or if we prefer to let analysis mean what it ordinarily has meant, then only when analysis is supplemented by an account of the form of the process or object is there any guarantee that the description will be faithful to all the fulness of the reality.

Let us think of mental fusion — *e. g.*, of two tones — and its well-known differences of grade or completeness; or let us recall the different degrees of associative connection amongst ideas — as when 'health' is more loosely associated with 'wealth' than with 'sunshine.' Would it seem more reasonable to describe these differences as due to the presence (or absence) of special elements in each case? Or would it not seem rationally more inviting to suppose that the same elements in exactly the same proportions can change their relations, change their degree of intimacy or cohesiveness? — somewhat as a mixture of oxygen and hydrogen, upon application of a match that causes it to explode, undergoes a profound change of relation without any difference in the number or quality or proportion of the original elements. Two ideas, one in your mind and one in mine; two ideas in the same mind, one of them present yesterday and the other present to-day; two ideas in the same mind at the same time but in dissociated systems of thought; two ideas associated by 'contiguity'; two ideas in intimate judgmental union; — each of these pairs stands out against the others; you cannot fail to notice a mental jar as you pass from one to the next. And yet I find myself quite unable to describe them with any hope of success except in terms of relation. By merely listing different 'elements' as present or absent, I feel utterly amiss as to the true properties of these astonishingly contrasting examples of reality. The difference is most naturally and successfully described as a difference of *position*, a difference of mental locality, rather than as an influx or dropping-out of special elements.

V.

With regard to the analytic method, I have so far hardly more that expressed my conviction that we cannot describe with accuracy our more elaborate mental processes so long as we take account merely of their constituents. It would now perhaps be well to indicate, by an illustration or two, the working of the method in this revised form, even though one cannot hope to show the rich and definite detail that it would lead to if skilled hands were to use it with the perseverance that has marked the more traditional analytic search.

And first of all, when it is said that the nature of a mental fact lies in its architecture as well as in its materials, we should not expect this metaphor to go on all fours. The formal character of anything psychic is rarely *fixed*, like the plan and elevation of a building; oftenest it is like that of a flame or a dust-eddy, perpetually changing though with a definite character maintained. And a true account must set forth this instability of the process, this shift and leap of arrangement. In emotion, for example, one can hardly fail to note the inconstancy. The constituents of anger, could they all rest together as a stable compound, would lack the peculiar qualities of anger. And so of fear. The shift of attention, the swing and rebound of impulse, the storm of organic sensation — all these are characteristic marks, especially of the more restless emotions.

But quite as characteristic as this flutter of attention, of impulse, and of sensation — this form of the process when we view it in its temporal progress — is the peculiar arrangement at the acme of the emotional course. The interest, the attention, while agitated, makes its swift excursions within rather narrow bounds. The mental field is often of limited range; great systems of ideas and impulses are in abeyance, dissociated from the group in control. And in the controlling system the somatic sensations have a place, but no central place; the interest is at a distance from them; they serve as a background against which the object of the emotional stir appears. Or, perhaps better, they are the murky atmosphere through which the object is descried. If, for a moment, you become interested in your organic turmoil, you have transposed on the instant the normal order of things; even though all the old constituents can still be found, yet the emotion itself has momentarily been destroyed. I have at certain times of emotional stress basely directed my attention to the sweep of organic impressions. Turning upon them they still continue with considerable life; but the state as a whole has now become one of curious and controlled observation, contrasting strangely with the passion that went before.

Thus the essence of emotion lies in many things; but some portion of that essence certainly is in the way the parts are put together at the moment and in their succession. There is a scale and order of importance which the items must observe — a scale which we may afterwards in memory review, but which in the active present is unrecognized. The emotion does not consist in the impulses, nor in the attention, nor in the somatic sensations, nor in all of these together. It is in the *manner of behavior* of them all — in their hurry and rush and conflict. It is in their interplay, in their system and order, in their manner of grouping — with certain of them at the center and certain of them to the rear and around.

And something like this seems true also of our acts of will. There is often here a mass of sensations coming from the striped muscles and less predominantly from the unstriped. But these of themselves are but the raw material of the volitional fabric, and but part of the raw material at that. Indeed they can almost, if not wholly, disappear, as in the case of intentional thinking, and there still remain the characteristic look of will.

The anticipation of the outcome of our act, the presence of an idea of our reaction to the stimulus before the reaction itself has taken place, is an essential constituent of a voluntary act. Yet though essential, it is not, as some have held, sufficient. For in reflexes that have occurred frequently — like winking or the patellar reflex — as the stimulation approaches, I can foresee the muscular response; yet for all this anticipation, the action does not seem voluntary. So, too, there are with all of us certain trains of association so familiar that we can, on the approach of the initial idea, foresee in dim conception its associational train coming on with the fatality of knee-jerk. Imagine one who never carves a tough roast but that he must burst out into the inevitable mis-quotation,

> 'O, pardon me, thou bleeding piece of flesh,
> That I am meek and gentle with these butchers!'

He — but first his friends — come finally to see the shadow of its approach; yet here there is no mark of true volition. The outcome has not been sought and summoned.

And this too, must be said when *attention* is made almost the equivalent of will. Of itself attention lacks the full form and structure of the volitional act. I can have my attention on my knee-jerk or on some associational train, and yet not will this nor will its opposite. And yet no act of will is complete *without* attention; I must have in the forefront of consciousness what I intend. And since attention itself may be voluntary, the complete act of will may seem at times almost coterminous with the attentive act.

Attention, anticipation of the outcome, and often a mass of sensations from muscles and tendons and joints, are among the constituents of will; but not until they are all rightly ranged is the will there. The event must not only be foreseen, but approved, sought, adopted. Only then do we have the true flavor of intention. The unwilled, but foreseen, act is, in a sense, mine, but it does not have the right relation to the dominant center of my mental system. It seems to belong to some subordinate and outlying part, loosely bound to the whole. The willed occurrence, so far as we can see, would not attain reality except for its intimate relation to this energetic central system; and in its coming it is greeted and adopted as part of this system. When voluntarily solving a problem, the answer to the problem, while still unknown, is consented to, sought and adopted; then the actual solution, when it comes, takes the place of this empty volitional wraith; and the core of us, all waiting, embraces the definite and concrete fulfillment. Sensational and affective elements are in the process at every point, as steel and steam are in an engine. But we may name the elements to perfection, without a perfect description of the active whole. The sensations from muscles and joints, the associational image of the outcome, the pleasurable and painful elements — these are but stuff and filling of a reality whose more interesting features appear in the changing form and movement of these and in their arrangement around a center already complexly organized. The characteristic nature of will is not found wholly in what is simple and unanalyzable — although there is plenty of such within it — but also in the drill and officering of all that here has a place. The same units differently organized could be something quite different, just as the men of a military company might, when properly combined, be an athletic club or a prayer meeting or a fire brigade.

VI.

A number of objections must have occurred to you in listening to accounts like these. In the first place, they are not clear cut, not light and intellectually portable, like the results of the more familiar analyses. But much of the blame for this may perhaps be cast upon the facts, rather than upon the method. Simplicity of account is not the end and aim of our work. If the *facts* are complicated, as I believe them to be, the *description* of them will also have to be complicated. In scientific work we are always tempted to ascribe to concrete reality a more elemental character than it actually possesses; perhaps description would be impossible unless we yielded in some measure to this temptation. But we should make a stout fight and yield no more ground than we have to. I admit that were I required to describe the great monument here in Baltimore, it would be easier and in a sense more intelligible and far less liable to error to say merely that it is a combination of marble and cement with perhaps a dash of bronze. The more ambitious attempt that went into the exact form of the statue and of the column and base could not compare with it in compactness and scholastic cut; but nevertheless with all its shortcomings the more complicated description would be moving in a right direction. So in the present case; whatever we may agree as to the inaccuracy of the descriptions I have offered in illustration, let not this conceal from us their general trend, nor prejudice the question whether their method may not, after all, assure us of a more complete, even though more confusing, picture of the facts.

Farther than this, some of you may have wondered whether, in the method I am commending, there is virtually anything more than an emphasis on those 'relational elements,' 'feelings of relation,' 'transitive states,' familiar to us all. I am perhaps mistaken in thinking that the two modes of viewing the case, while having much in common, are not identical. Relational elements, feelings of relation and the like are often in effect conceived as but one more material or ingredient added to the rest, added to sensations and to pleasure and unpleasantness. And the account then pursues the evil course of describing a mental fact by attention to its stuff and materials only. In other cases, even when the relations are not regarded as stuff and material, yet they are conceived as different from those which it seems to me necessary to invoke. For often by the *form* of a mental fact writers mean simply the intellectual bond that holds the parts together — the relations *felt* between them. But in reality the arrangements of mental data are by no means confined to the relations felt at the moment; indeed they need not be felt at any time by the person in whose direct experience they occur. When an idea that arose yesterday in my mind is followed by an idea to-day, the succession is real, even though there be no feeling in me of the succession, no awareness of the relation. An illusory spatial distortion may have now one direction and now another, without the relation between the two distortions — that is, their difference of direction — being cognized or felt in any way. All such relations are of course *capable* of being felt and known, but their *esse* does not consist in being felt and known. They therefore seem to me quite apart from anything properly to be called a relational feeling or an intellectual relation.

All this seems still more clearly true when we bear in mind the many other varieties of relation which accurate description must use — subordination and prominence, position within and without the focus of interest, changes of rate and direction, especially those deep reversals so characteristic of the emotional state. Here the incessant change which is part of the very substance of the process occurs not only in the attention, but in the sensations, in the hedonic tone, in the impulses. It therefore is not a modification in some intellectual process exclusively, nor does it have to appear before the intellect in order to be real. Nor can we regard as an intellectual relation or as a feeling of relation the interplay of certain mental groups, the indifference or dissociation of other groups, noticeable in psychic health and disease.

With regard to many of these relations we therefore seem forced into a kind of realism. Some relations are represented *in the psychic state itself*, as connectives *directly experienced* at the moment. When I pass from sunshine into shadow *the difference may be felt;* when I look over this room now and look over it again a moment later, *the likeness may be felt.* Here there is what a certain one of our association would call a 'relational element' in the state. But in other instances the relation has no representative in the state at all — for example, when I pass from sunshine into shadow and do *not* notice the difference, or when I see the same object twice and lack all feeling whatever that it is the same. The relation of likeness or difference is now no less real, it does not the less characterize the experience, that the relation has no conscious place in the experience itself. A host of real relations thus *apply* to mental data, without necessarily having any conscious presence or representatives among these data. And we must make use of these relations in our psychological description even when we cannot find them there as 'elements' in the fact we would describe — just as a chemist makes use of the relations of space and time, without scruple, even though they do not appear in his list of elements along with iron, oxygen, and sulphur.

VII.

Thus we are at the end of all that I can in any conscience ask your attention to, in my review of some of our methods of understanding the mind. What has here been said in regard to explication, whether of causes, or of the office and significance, or of the inner constitution of a mental fact, moves (as you may have noticed) about a certain common center. My sympathy throughout is with a scientific psychology, a psychology exact in its methods, intellectually clear as to its purpose, that appreciates its bond and debt to other sciences and yet has a fund of self-respect. Psychology will one day, in all probability, have a dominant place among the sciences, instead of its present somewhat humble rank. During the whole 2,300 or more years of its existence psychology seems almost always to have been passing through an impressionable period. I need not recount how at one time all her thoughts go out to metaphysics or, again, to logic, while later it is mathematics that controls. The strong influence of physics in our day, the strong influence of physiology and the sciences of organic development — all this indicates a nature admirably docile, but with a touch of immaturity. A great need in the past has been to learn the use of the tools and methods of the physical sciences. With the present and future the increasing need will be of critical courage to adapt these tools and methods to our own stubborn object. We have our own peculiar field and problems. If physics develops an idea of causation appropriate to its work, it does not follow that it will be entirely appropriate to ours. Nor does it follow that because it is inappropriate to our work, that we should thereupon declare that no causal connection exists among psychic facts. A loosening and limbering of our fixed ideas about causality has therefore seemed to me desirable; our explanations would be better with less assurance beforehand as to the exact place where causes are to be found, and as to the exact color and marking that is to make them acceptable.

In a like spirit it has seemed to me well to keep our methods clear of any declaration beforehand for either physical ends or psychic ends exclusively. The facts can be looked at in each way and in both ways, and our method should here leave us full scientific freedom.

And finally, to carry farther this recognition of the full rights of what is mental, I have urged that our account of the inner constitution of psychic facts be not narrowly limited by the older idea of chemical analysis. We should recognize, as the more modern chemists have been forced to do, that description is more than a statement of elements and their proportions; that other relations and modes of interconnection are important. We should, moreover, not be surprised to see modes of interconnection in the psychic field that are not recognized in the physical sciences; and if we actually do see them, our method should place no obstacle in the way of their recognition. My own feeling is, that with our methods thus purified the work will have more the character of a frank and open investigation, and less that of a system moving by imitation and prejudgment; it will therefore be more truly scientific in its spirit.

THE PROPER AFFILIATION OF PSYCHOLOGY — WITH PHILOSOPHY OR WITH THE NATURAL SCIENCES?[1]

BY PROFESSOR J. MACBRIDE STERRETT,
George Washington University.

You will thank me, I am sure, for an act of mercy. Instead of reading the whole of a very long address prepared for the occasion, I will make enough omissions from it, to leave half an hour for your discussion of the topic[2] — of the topic, I say rather than of the address, because custom has sanctioned the President's address as taboo.

I am indebted to a happy accident for the topic. I need not tell you how many seductive themes offered themselves. . . . Then I reflected that ours was a society for psychology as well as for philosophy. So I went to the Library of Congress to look through some of the recent works on psychology. Somewhat to my surprise I found the works on experimental psychology classified under the heading of *Physiology*.

Thus under physiology are grouped experimental psychology, physiological chemistry and experimental pharmacology. That was the accident that suggested the topic which I wish to open for your discussion — *i. e.*, the title of our society: *The Southern Society for Philosophy and Psychology*, or more specially the conjunction *and* between the two socii — philosophy *and* psychology. For the geographical part of our title is of minor import. In the North there are two separate societies — one for philosophy and one for psychology. Our provincial title, *Southern*, however, goes with a more generous and organic view. But it might be said that we meet together as one society only because we are too few and too scattered to be able to maintain two societies. I take the higher and more genial view —

[1] President's address before the Southern Society for Philosophy and Psychology, Baltimore, December, 1908.

[2] The topic was discussed at some length by Professor J. G. Hume and Professor Ladd. (See PSYCHOLOGICAL BULLETIN, Feb., 1909).

the old-fashioned view of the organic kinship between philosophy and psychology. Yes, we are sociæ — mother and daughter, philosophy *and* psychology.

In what I have to say to-day I must not be taken as questioning for a moment, the desirableness of our union. Nothing could be further from my thought than to suggest a divorce in this domestic circle. It is quite possible however that the umbilical-cord-interpretation of the conjunction *and* will not be accepted by all the members of our own society. There is room for debate on the general subject. I need not remind you how philosophy suffered by the self-ex-matriation of many of her childrem. . . . Is psychology, we may ask, one of these ingrates?

Is it to be a case of the cuckoo in the sparrow's nest? Or is it not, perchance, a case of the ugly duckling? — the arising of a new science — a beautiful daughter of the more beautiful mother? The new psychology has been coquetting with natural science, and philosophy has been looking askant at her wayward daughter. . . . In the near past the copula *and* has been stretched almost to the breaking point. . . . But now the *rancor* of the strife is past, and we are left with a purely *academical* question as to the proper affiliation of psychology — a question of the classification of the sciences. It may be a mere question of age.

> "Crabbed age and youth
> Cannot live together."

Or there may be a more vital and organic cause. The very spirit and method of psychology may be much more akin to those of the natural sciences than to those of philosophy. Thus either party may raise the merely *logical* question as to the propriety of the conjunctive *and*. Are the two, philosophy and psychology, well paired? Is the vinculum of filiation valid, or has it become so unnatural that it should be broken? Does the conjunction *and* look well parading with a saint Cecilia on one arm and a madame Blavatsky on the other? Will a lion and a lamb consent to such a side-by-side conjunction? Will not the conjunction *and* be changed into the preposition *within* — the lamb *within* the lion? Then comes a question, which is the lion?

President Stanley Hall answers that *psychology* is the *lion.* In his St. Louis address he says: "Our science is still like Milton's tawny lion." Till recently it has been philosophy that made the lion — claim of inclusiveness. For her part, I may say, she is now willing to give up the claim. Bishop Wilmer told a story of a colored groom coming back shortly after his wedding and asking to be *un*married. "Why," said the Bishop, "did you not take Dinah for better or for worse?" "Yah, yah," said Sambo, "but then she is a *heap wusser* than I took her for."

If not worse, psychology is certainly so very greatly changed as to make philosophy rather willing to have her go her own way. Meanwhile *psychology* has been putting forth *her* claims of *inclusiveness*. . . . Here I need only to refer to the vivacious remarks of Professor Münsterberg on the expansionist policy of the new psychology. "Certainly," he says, "the good appetite of psychology has sometimes become voracity in our days, and she has begun to devour all the mental sciences — history and social life, ethics and logic and finally alas! metaphysics." But this, he claims, is pathological and terms the disease *psychologism*. . . . There seems to be a glamor about the term psychology that to-day is ousting the term philosophy and evolution from their place. The press is pouring forth books and pamphlets and articles on the psychology of this, that and the other thing—the psychology of ants, antics and antiques; of cant, canticles and chanticleers and so on, from A to izzard. I would like to read you the titles of a hundred articles that I have picked out of the psychological journals. I will mention a few of them: The psychology of profanity, pain, pity; of laughing, landscapes and lies, and finally the psychology of literature, of science, of art, climaxing in the title of an article on the *psychology* of *philosophy*, or in a more recent one, on the psychology of psychology. Well! really, we older folk must gasp at this claim of psychology to be the *scientia scientiarum.* Frankly, we may admit that the new psychology has made good her claim to be a new science, and recognize to the full the enormous amount of good new work done by it. We may take it at its own pretensions and yet we may debate the question as to its place in the classification of the sciences, more specifically whether she ought to be divorced from philosophy. . . . It is a question of *quid juris* rather than of *quid facti*. Should psychology any longer affiliate with philosophy and the philosophical sciences, or should she be bidden a god-speed to conjunction with the long-wooed natural sciences — *similia similibus?*

At the joint meeting of the American Philosophical and the American Psychological Associations in 1905, held in the new Emerson Hall at Harvard University, this question of the proper affiliation of psychology was mooted. With delightful *savoir faire*, rather than with logical self-consistency, Professor Münsterberg, one of the strongest champions of psychology as a natural science, held that the housing of psychology in the new Emerson Hall of Philosophy settled the question, *i. e.*, for Harvard University. Psychology had accepted the invitation and entered the hall as a co-habiter with philosophy. But that local and accidental arrangement did not touch the question of the *quid juris* of the affiliation. President Stanley Hall more logically held that psychology, as understood by both Professor Münsterberg and himself, should be recognized as one of the natural sciences and so be divorced from philosophy.

He said: "Psychology is a branch of natural science and can be fruitfully studied *only* in connection with the phenomena of the material world. Its business is to examine the *physical and physiological conditions* of mental states, and this it can do only by employing the methods of the natural sciences. As an empirical science it has nothing to do with metaphysics." Otherwise the tone of the discussion there was genial rather than logical. It was a house-warming party in the new hall of philosophy. The local *quid facti* was not to be seriously questioned, and so the prevailing *sentiment* was for the validity of the hereditary affiliation of psychology with philosophy.

But here is a later and a more significant incident. It was supposed to be a settled custom for these two large associations — the American Philosophical and the American Psychological Associations — to hold their annual meetings at the same time and place, and to arrange for joint meetings. Last year, however, the American Psychological Association chose to hold its annual meeting in affiliation with the American Society of Naturalists in Chicago, rather than with the American Philosophical Association at Cornell. The long and rather vain coquetting of psychology with natural science here culminated in a throwing of herself as suitor into the arms of a *very lukewarm* beloved.

Our present meeting occurs in the very pandemonium of the sciences. Here our own society for philosophy and psychology has been partially absorbed into the American Psychological Association. Here too the great American Association for the Advancement of Science appears as a vast scientific trust — the Lion's den to which all other societies are making tracks. In fact we find that its *section H* is given to anthropology and psychology. As far as psychology is a science, that I believe is its proper home. Philosophy could not get in *even if it wished to do so.* For, philosophy, whatever she may be, is not, and from her very nature, never can be, one of the sciences — in the modern narrow sense of the very catholic term *science*. . . .

A full discussion of this topic would involve the consideration of the nature, aim and methods and, of both philosophy and science and of the *differentia* which determine whether one branch of study is one of the philosophical disciplines, or one of the natural sciences.

Some of these questions are too well agreed upon to demand discussion before this audience. We all know the nature and the claims, or at least, the pretentions of philosophy, in its intensive sense of epistemology and ontology as held by catholic philosophy.

So too there is a general agreement as to the marks which assign any science to a place with the philosophical disciplines. The chief mark, I should say, is whether or not a science is *normative.*

We can also limit the discussion by our unanimous consent to affiliate *rational* psychology as represented chiefly in this country by Dr. Wm. T. Harris with philosophy. It is philosophical or nothing. The same is true in regard to the modern experimental form of the older introspective psychology as set forth by Ladd, Stout, Ward, Höffding, Baldwin, Calkins and others.

Without discussion we may accept philosophy at its well recognized position, not as one of the sciences; not as merely the unification of all the sciences, but as the science of the principles of all knowing and of the *absolute reality* back of all that with which the natural sciences deal. In her own eyes she walks a *queen* and we do not question her right, "We do it wrong, being so majestical." But as to psychology in its many new empirical forms we can say:

> "Thou comest in so *questionable* shape
> That I will speak to thee."

Here I confess myself to be a mere inquirer, and I shall not be surprised to learn that every one of my opinions on the subject is false. I am perfectly sure, that from the standpoint of the new psychology, I am a *back number*, an old fossil of the defunct arm-chair psychology — one of those whom Dr. Stanley Hall has characterized as '*paranoiac* minds' 'surcharged with paleo-atavistic traces,' 'sitting in prison' 'under the greatest delusion of the ideality of space.' I shall assume the standpoint of the philosopher, who is not technically acquainted with the new psychology, and base my remarks upon quotations from those who are specialists in the new science. . . . I certainly am not competent to give any but a very second-hand account of the present state of psychological literature. In the flux and flow of its development I cannot tell just what is an überwundener Stand-punkt, or what is just now the dominant view. Let me confess to a state of mental bewilderment, of what they call a 'functional or organismic feeling of non-orientation'; an attitude of 'unrelationalized psychic quality-content' when facing the literature and the laboratory work of the new psychology. . . . It is all so unlike the old psychology!

Its literature bristles with the technical terms of physics, physiology and biology. I forbear giving you a list of its working terms that seem so unfamiliar to one of the old school. It speaks largely in an unfamiliar tongue and works at largely unfamiliar problems.

Ribot says: "The new psychology differs from the old in its spirit — it is not metaphysical; in its end — it studies only phenomena; in its procedure (or method) it borrows, as much as possible, from the physical sciences." Professor Angell says: "The tendencies which have contributed to render psychology so largely independent of philosophy are for the most part identical with those which have brought it under the guiding influence of biology." . . .

We note too the large space occupied in its literature by a sort of an internecine warfare, a mutually destructive criticism of its different forms, so that one might be tempted to follow Hume's method of dealing with the various forms of religion, setting them, like so many Kilkenny cats, at an internecine struggle till nothing is left of them but tails. . . . One quotation from Professor Titchener must suffice: "I have," he says, "little sympathy or patience with these experimentalists who would build up an experimental psychology out of psychophysics and logic; who throw stimuli into the organism and take reactions out, and then, from change in the nature of the reactions, *infer* the fact of a change in consciousness. Why in the world should one argue and infer, when consciousness itself is there, always there, waiting to be interrogated. This is but a penny-in-the-slot sort of science. Compared with introspective psychology, it is quick, it is easy, it is often showy." . . .

Münsterberg says, what no *philosopher* would dare to say: "It seems to me that the new discoveries in modern psychology have often an existence of only four months." We might say then that it seems to be as near to being a science as pragmatism is to being a philosophy. It may weary you — if a quotation from Professor James could ever weary any one — to have repeated the closing words of his *Psychology:* "It is indeed strange to hear people talk triumphantly of the *new* psychology and write histories of psychology, when into the real elements and forces which the word covers, not the first glimpse of clear insight exists. A string of raw facts: a little gossip and wrangle about opinions; a little classification and generalization, but not a single law in the sense in which physics shows us laws. This is *no* science; only the *hope* of a science. At present psychology is in the condition of physics before Galileo and the laws of motion, of chemistry, before Lavoisier and the notion that mass is preserved in all reactions." . . .

Dr. Stanley Hall, who seems to be a sort of a Thersites in the camp, in both his Harvard and St. Louis addresses is garrulously querulous, almost senescently adolescent in his declamation against metaphysics in the new psychology. "The present glowing twilight of the new psychology," he says, "is that of the dawn, not of the evening." But even in its present early form of adolescence it is ever lapsing into senescent reminiscence of the metaphysical problems of the old psychology. . . . And so we find this constant accusation made against every form of psychology. All accuse each other of being metaphysicians. *L'ennemi c'est la metaphysique.*

. . . I omit, as I have said, *rational* psychology and all forms of what I choose to call *psyche-psychology*. All forms where at least one's empirical self or soul or active consciousness is the basal fact for study; all forms where the concept of personality stands as the ideal and the problem and the inspiration of the work — whatever method may be used such forms of *psyche-psychology* rightly, by common consent, should be affiliated with the philosophical disciplines.

Affiliating all forms of *psyche*-psychology with philosophy, we may turn to forms which, with Lange, "calmly assume a psychology without a soul"; that at least reduce psyche to a non-active epiphenomenon of physical phenomena; where the interest is chiefly with the *non-psychical* as the material from which a non-psychical, psychical automaton may be constructed — that is, to all forms that may be included under the term *scientific* psychology, in the rigorous and narrow sense of the term scientific.

But here let it be said that there are *some* forms of *structural* psychology which belong to psyche-psychology and there are others which belong to this latter group.

The same may be said as to some forms of *functional* psychology. That is, the ideal and problem may be such as to classify some forms of functional psychology with *psyche*-psychology, while there are others where it is little more than a branch of biology, and the others where it is a form of psychophysics with the accent upon physics. Thus with Angell, the problem of the functionalist is one of determining just how *mind participates* in accomodatory reactions, thus putting the accent upon psyche.

The term the new psychology is too broad being inclusive, as it is, of modern forms of the psyche-psychology. If we can submit to the rigorous but defective modern definition of science, we may classify them as (1) the old or *unscientific* psyche-psychology, (2) the new or *scientific* hypo-psyche-psychology. But both of these may be experimental. For the experimental method, which is claimed as the distinctive mark of scientific psychology, is also used by the other school. The only question is, whether these experimental methods are *analogous* to, or *identical* with those of physics and physiology and whether or not they are applied to the same subject-matter.

We find both structural and functional psychologists to be chary and wary of *psyche*. They fear its recrudescence, just as biologists fear any recrudescence of vitalism. They fear the introduction of any *teleological* explanation, or any recurrence of any form of the old faculty-psychology, where faculties, as organic manifestations of a substantial mind were made explanatory of mental processes and results. They hold that, historically and scientifically psyche has been a vanishing factor.

Dr. Stanley Hall regrets the lingering hold that the questions raised by the old psychology has even yet upon scientific psychologists.

His attitude toward all questions raised by the other older psychology is quite like that of Callicles in Plato's *Gorgias* (485) one of pity and contempt, except as a training stage for the young. Thus, he says: "For many, if not for most, a touch of it, but not too much of it, is perhaps, a part of the complex initiation of youth into its world; but the severer types of this discipline seems more suited to senéscent than to adolescent men and races." To be *scientific* then, all forms of psychology must banish an active self-manifesting and self-realizing psyche. Hamlet must be left out of the play.

As to *structural* analytic or *idea*-psychology. This form of psychology is strictly analytical of psychoses, states of consciousness, ideas, as mere phenomena, abstracted from any *active psyche*. It is after the elements in any psychosis and their quantity, so as to construct a psychic-automaton. It is held to be scientifically irrelevant to ask *whose* psychosis one is analyzing. Ideas are atoms, instead of being experienced functionings of a self. Explanation is sought in non-psychic terms by reference to physiological distinctions. It treats its analyzed elements as real parts, whose mechanical composition *is* the mental life. Whether there is any *personal mind* back of the stream of ideas, says Titchener, is a question that cannot be raised in psychology.

Functional psychology is a bit less abstract. It has at least processes instead of cross-sections of consciousness, or rather of the psycho-physical organism in its reaction to external environment. At first it looks like a bit of teleological self-activity being introduced into the psychological automaton. But this is a mistake. The reactions, the controls are not within the organism, but from the environment. Description, it is true, is in terms of value. Function is identified with *use*.

But when we ask, useful *for whom*, we find only the impersonal psycho-physical organism. *That* is its basal fact, not a self-active or conscious self. Hence its concern is with the sensori-motor processes of this organism, body-reactions and attitudes, whose processes and functions are biological rather than psychological phenomena. Activity in relation to environment becomes mechanical physics. Reactions are considered as essentially motor processes. They are mechanical functions, not activities of a functioner. In this psycho-physical organism purely physiological functions are recognized but not any purely psychical functions. There is a *body*, an organism but there is no psyche to function. Hence functional psychology is logically a branch of biology. Professor Kirkpatrick suggests that functional psychology be broadened to include the fuctioning of all organisms, whether conscious or unconscious, and suggests the term *organosis*.

Genetic psychology seems to take a further step toward a concrete form of psychology. Lloyd Morgan says that it takes its place between biology and such normative sciences as ethics and æsthetics, with their doctrine of worth for the ideal life of man. "The starting point is in close touch with purely biological reactions and the *goal* is our systems of knowledge and our ethical conceptions." It is functional, teleological, evolutionary and synthetic. It puts the control system, a sort of effective consciousness, within the purely organic activities in their reaction to environment. That is, it uses the conception of 'purely psychologically-guided or intelligent behaviour, as distinguished from reactions which are purely automatic.' Thus we may have a genetic psychology in place of the genetic biology of functional psychology, and thus a form of psychology which should be affiliated with philosophy, whether in the form of individual, social or race psychology. As critical of structural psychology, of its barren, abstract, cross-section-piece-of-consciousness mythology, and as carrying forward the more concrete view of functional psychology, we may believe with Professor Baldwin that genetic psychology is fast coming to its rights, and that it has a great future before it. I say we *may* have such a form of genetic psychology. But I am compelled to say *we do have* forms of it that are to be classed with the purely evolutionary physical sciences. For disguise, despise, abjure metaphysics, as we may, we do not and cannot find any form of psychology which does not presuppose and rest upon both an epistemology and a metaphysic.

Professor Baldwin says that the two principles which distinguish the new psychology are its adherence to the principles of naturalism and positivism, both as to spirit and method.

But Baldwin pronouncedly divorces both these terms — naturalism and positivism — from the metaphysics that often go with them. He says, that the platform on which he describes the development of modern psychology is "that of cognitive and reflective *self-consciousness* of such a sort as that which the individual has attained when he thinks of his inner life as a more or less consistent unity, passing through a continuous and developing experience: a *self* different from things and also different from other selves: yet finding its experience and exercising its functions in closest touch with both." Again, Baldwin says: "What is it that feels and knows? It can only be a *unitary subjectivity*, *additional* to the unity of sensory content, *i. e.*, the synthetic activity which reduces the many to the one in each and all the stages of mental growth." It is, indeed, only on the theory of a self-active subject that any truly genetic psychology can be had.

With *such* a conception of a self, a synthesizer, a functioner, a self-realizing activity, we may use all the methods of naturalism and positivism; all the methods of modern experimental psychology — structural, functional, genetic; all laboratory, all psycho-physical and psycho-metrical methods. What I have termed *psyche*-psychology welcomes and uses all the results of all the *methods* of both naturalism and positivism; and only objects where methodology is bottomed on an ontology, naturalistic and positivistic. Through all these results there is an enrichment of our conception of the psyche that is fundamental in any logical form of psychology. As long as psychic phenomena are not analyzed into non-psychical factors, so long every analysis of the constant activities of the complex psycho-physical organism with the emphasis upon the *physical*, will be useful material for the psyche-psychologist. All grain that comes to his mill are his grists.

But where we have 'structure' or 'idea' without a 'whose,' or function without a functioner; or genesis without a generator; or a measure without a measurer — in a word, to be both epistemological and ontological, where we have mere phenomena or epiphenomena in a numerical and quantitative order and causal relation — there we have a form of psychology that should be affiliated with the natural sciences. I have a profound admiration and respect for the large amount of fine original work done by all the workers in scientific psychology. But I confess that a very large part of their work as well as their method seems to me to be unpsychological. They are studying *something*, but it is not consciousness or psyche, and psychology is the science descriptive of *consciousness*. *That* should be the *root* of the matter, whereas, with them, it is at most a convenient general abstract term to hold together a lot of abstractions, from the activity of the concrete psyche. I have no obnoxious religious or philosophical criticism to make on the scientific study of this subject-less, ego-less, psyche-less, phenomenal matter of the new psychology. But the question that I raise is as to the logical affiliation of this *scientific* work with the philosophical disciplines. Leaving all forms of psyche-psychology whether new or old, whether empirical, inductive experimental, descriptive, analytic or not — leaving all these forms affiliated with philosophy, we ask where is *scientific psychology* to be placed? Under this come some *forms* of structural and functional and genetic psychology. But more distinctively *scientific* are psychological psychology and psycho-physics.

We find many of the exponents of all these forms of psychology strenuously denying affiliation not only with philosophy, but also with the philosophical disciplines. In spirit, aim, method and, largely, in subject-matter they are not merely analogous to, but identical with those of the natural sciences. We hold that these forms of psychology should be affiliated with the natural sciences for the following reasons: It is the wish of their exponents. This wish comes from sympathy and congeniality with the spirit, aims and methods of the natural sciences. . . . These methods are applicable not to qualitative, intensive states, of the *internal sense*, but only to sensuous spatial phenomena. Kant made a mistake in his first edition which he corrected in his second edition which the neo-Kantians and all positivists have resolutely enforced. That is, the application of the categories of physical science to phenomena of 'the internal sense' is denied in the second edition and confined to only external, *spatial* phenomena. Hence Kant holds, as logically do his neo-disciples, that psychology can never become "a natural science of the soul or even an experimental doctrine in regard to it."

It is notable too that in his *first* edition he treats empirical psychology as a stranger within the philosophical fold, though then holding that the categories were applicable to phenomena of the inner sense. That is, granting that there could be a science of the data of the inner sense, it would be merely empirical and have no philosophical interest. In the *second* edition he denied the possibility of an empirical science of the data of the inner sense and hence of any science of the psyche. The categories or the analogies of experience, as the principles of science, are not applicable to the phenomena of the inner sense, but only to those of the outer sense, *i. e.*, spatial phenomena. We have, Kant then declares, "from things without us, the whole material of our knowledge even of our inner sense" (Pref., XL.). And in his incomprehensible *Refutation of Idealism* he says that "our inner experience is itself only possible *mediately* and through external experience."

In a word, he pointed to physiology and anthropology as the only forms in which we can have a *scientific* psychology — the way that Lange and the neo-Kantians and positivists resolutely enforce. It is this regnant phenomenalism in psychology that accounts for the consistent refusal to take as a factor, a psyche, self, or consciousness, because that is not a sensuous phenomenon. The term *psyche* has been so greatly implicated with religious ideas that there has come a preference for the term *consciousness* or conscious subject. But this should not obscure its ancient and perduring usage as a philosophical term. We need not quarrel about the term. It is the fact of a permanent, substantial, self-realizing ego that is denied in scientific psychology. It abides by Hume's contention — "Show me the impresson from which this idea of self arises." . . . Identity and continuity are here, like the causal nexus, but a fiction of the mind, which itself is a fiction. Certainly positivism and atomism dominate the work in structural psychology. Münsterberg says: "From the standpoint of psychology consciousness is only an abstraction from the totality of conscious facts. Consciousness does not *do* anything, it is only the *empty place* for the manifoldness of psychic facts." Thus denuded of all concreteness and activity, psyche is but the *verbal ghost* of the *Gheist* regnant in the older psychology. It is a general term, a mere *flatus vocis*, enjoying perpetual *otium cum dignitate* in the work of the phenomalistic positivists. But the fact is that *conscious states* are abstractions. Consciousness itself in an abstraction. The concrete given reality is *self-consciousness*, *with* states, etc. States of consciousness are really states of a conscious being, self, *psyche*. . . . Professor Bush speaks of consciousness as 'a diaphanous medium through which, on occasions, objects are united,' or as 'the faint rumor left behind by the disappearing soul upon the air of philosophy.'

Professor James discussing *Does Consciousness Exist?*[1] says (J. P., 147): "For twenty years I have *mistrusted* consciousness as an entity; for seven or eight years past I have suggested its non-existence to my students. It seems to me that the hour is ripe for it to be openly and universally discarded."

As to the '*Ich denke*' that 'muss alle meine Vorstellungen begleiten Können,' the merely *logical permanent self* of Kant. Professor James, who always dares to say very daring things in a very brilliant way, finds it to be but a careless name for the stream of *breathing*. He says:[2] "The 'I think' which Kant said must be able to accompany all my objects is the 'I breathe' which actually does accompany them. There are other internal facts besides my breathing (intercephalgic muscular adjustments, etc.) and these increase the assets of consciousness but breath, which was ever the original of 'spirit,' breath, moving outwards, between the glottis and the nortrils, is, I am persuaded, the essence out of which philosophers have constructed the entity known to them as consciousness. *That entity is fictitious* while *thoughts in the concrete are fully real. But thoughts in the concrete are made of the same stuff as things are.*" . . .

The *older* psychology is both descriptive and normative. As normative, it affiliates with philosophy as do the other normative sciences. But *scientific* psychology is not a normative science. The very terms norm, ideal, teleology are deep-dyed red rags of metaphysical popery. Mechanism, mechanical causality is the regnant concept of scientific psychology. It does not seek to describe concrete, active consciousness, but to find the causal tie between the parallel physiological processes and the abstract mental atoms so as to construct a psychical automaton. It denies any causal tie between these psychical atoms and also between them and their physiological and

[1] *Jour. Phil., Psy. and Scientific Methods*, I., p. 477.
[2] *Jour. Phil., Psy. and Scientific Methods*, I., p. 491.

physical conditions. Here I need to do no more than refer to Professor Münsterberg's popular exposition of this in his *Psychology and Life*, where he allows that the psychical automaton thus constructed by *scientific psychology* is far from being a description of *the real living* self. From his statement it appears to be but a caricature rather than a character-sketch of the real concrete psyche — a merely kinematographic *simulacrum* of psyche.

As to the protesting real psyche that he hands over to history, ethics and religion, though he calls it 'will,' he speaks of it in such negative terms, as 'non-psychic,' 'non-personal' as to make it seem a nondescript nonentity — *sans culotte* — *sans tous les choses*. It is unnecessary for me to repeat what he feels called upon to say *ad populum* about the limitations of the psycho-construct of scientific psychology — at the expense of affording pleasure to its enemies — "It is not at all an expression of *reality*, but a complicated transformation of it, . . . an abstract psychical antomaton." He adds, "Every fiber in us revolts and every value in our real life rejects such a construction." . . .

Scientific psychology aims to predict or, to vary the phrase, to explain. And explanation, to be scientific, must always be causal, and so, in non-psychic terms and of non-psychic phenomena.

It is a case of *mechanism versus* the *teleology* regnant in all the normative philosophical disciplines. Scientific psychology explains by giving the invariable set of antecedent physical facts. The causal nexus is found between the physical facts, but they do not, and, on their epistemological theory, they cannot allow any causal *nexus* between the parallel psychic phenomena — nor indeed between the two parallel set of phenomena. It is impossible to see how they can get any *psychical* automaton constructed. On the other hand we may note that since the days of Plato and Aristotle, ultimate explanation looks not backward for explanation, but forward to the function or final cause as the real explanation. *Respice finem.*

The only *psyche* in the work is that of the psychologists, and it is reduced to a mere spectator of objective phenomena with hypothetical parallel concomitant psychoses — the inert, causeless and uncaused accompaniments of certain transformations of matter and energy. Any psychologist may well fear for his reputation for modernity if he upholds a psyche. That must be religiously disavowed. In psycho-physics the psyche is only permitted because of its union with physics. Psyche alone — well, that is snubbed and bowed out, or only taken in at the back door and told to keep quiet, as quiet it must keep, as it can make no acoustic utterance. This utterance is, after all, the real thing and so psychology is a form of physics. It is a science, but not a science of the soul. It would not care to be styled a science of no-soul, though even functional psychology comes near this in its treatment of a body-soul, psycho-physical organism. Here, as Dewey says, "the distinction between the physical and the spiritual is one of interpretation of function rather *than of kind*." That is, the distinction is merely mental, functional distinctions within one organism. The body may as well be said to have a soul as the soul a body. It is all one organism in time and space conditions. There is no known soul. We have a *soul-idea*, which is obnoxiously intrusive in all scientific study. But it corresponds to no reality. We can trace its psychological genesis out of such stuff as dreams are made of. It is granted that this concept will survive as long as men are religious, or as long as they ask the inane questions that Rational Psychology asks.

It may occur to one to ask by whom or through whom was the soul-idea conceived, if not by a psychical conceiver and his confreres. Or is it a construct of impersonal atoms in their causal nexus through all ages, especially the early ages? But ages of whom or for *whom?* There are no ages for a *what*. Through the ages for psyche, psyche has constructed the psyche-idea, as implicative, as revelations of her real self. A construct needs a constructor. Is it not an absolute *μετά-βασις ἐις ἄλλος γενος* to pass to a *psychical* construct from a *physical order?* At least if the psyche-idea is a construct of physical antecedents, why is it not just as valid as the concept of causality, which must have been generated by the same sort of physical antecedents? One here very naturally raises the question as to the propriety of keeping the *name psychology*. Karl Lange, who gives the epistemological basis of this non-ontological science, himself raised the question. In arguing against the hypothesis of a soul he says: "But does not psychology, then, mean the doctrine of the soul? How then is a science conceivable which leaves it doubtful whether it has any object at all? Well, here we have a charming example of the confusion of a name and a thing. We have a traditional name for a considerable, but by no means accurately defined group of phenomena. This name has come down from a time when the present requirements of strict science were unknown. Shall we reject the name because the object of the science has been changed? That were unpractical pedantry. Just calmly assume a *psychology* without a soul and the name will still be found useful."

My friend, Rev. Dr. Frank Sewall, of Washington, suggests the term *hypo-psychics* as a better name for the new science.

Later on we find Lange demanding that all psychological definitions be replaced by physiological ones, thus leaving psychical phenomena to stand only as provisional indices of physiological ones. But all such efforts to reduce psychology to physiology, in order to have a *scientific* psychology is to virtually *abolish psychology* in order to make it into a science. They proceed on the theory that science obtains only where the relation of cause and effect can be reduced to a relation of identity or continuity, so that quantitative equations become possible. This can only be in the material sphere. Only where we have quantitative states can there be science. Therefore parallel quantitative states, physiological manifestations, are to be substituted for the psychical ones to get quantitative determinations carrying out a strict causal connection, in order to have a scientific psychology. Lange set the example of decrying the inane method of introspection in favor of external observations of a physiological sort. Thus contempt of introspection has been a flaunting note of the new school until we find a noteworthy exception in Titchener who, deprecates "That neglect of introspective control in psychology, which has been the besetting sin of many whose direct interest lies in psycho-physics," insisting upon introspective analysis as *the* method of psychology.

"The course of experimental psychology he says in recent years has been away from simple psycho-physical determinations and towards introspective analysis." A philosopher would not dare to characterize some of the laboratory work in psychophysics as Professor Titchener does — as 'a penny-in-the-slot sort of science.' "Tables and curves of reaction etc.," he says, "are useful, and the psychological laboratory is the place for them. But there is no reason why one should gain *psychological* credit for them — still less for erecting a speculative psychology on their foundation. This mode of psychologizing is inherently as vicious as any of the constructive modes of the older psychology."

But most of the work in the new science is carried on without introspection, the only door into the specific subject-matter of psychology. Thus Cattell says: "Most of the work done by myself and others in my laboratory is nearly as independent of introspection as work in physics or zoology."

But all this is not psychology and indeed no part of psychology, unless it be held as merely supplemental to that which treats of the processes of the self-activity revealed only by introspection — itself a form of self-activity. If the old psychology be termed metaphysics, the *new* may well be termed *hypopsychics*. . . .

We have noted the reason Münsterberg gives for this abstract construction and how strenuously he affirms that it by no means resembles the *real* self, the actual psyche of himself or any of his fellows. . . . We confess to being unable to appreciate the end to be served by such a psychic-construct. In later chapters he disclaims any use for it as applied to pedagogy, art, history, ethics or religion.

It is just here — granting a possible limited use for their psychological automaton — here where it logically calls halt — that we would plead for a further function of psychology in describing the teleological processes of real life, in ethics, art, history and religion. That is, we plead for the place of *another form of psychology* — that of the *old* psychology in modern form — for *psyche*-psychology.

This pleads for the subject-matter revealed only by introspection — the self-activity of a knowing, willing, feeling self, that no eye nor microscope can ever see, and that no mechanical causality can order into a mechanical science. Kant's paralogisms were only aimed against such a natural science of the self, without whose synthetic self-activity there would be neither subject-matter or form for natural science. The self for whom these are objects cannot itself be an object like them. The whole of his *Critique*, which gives the principles of science, is itself a refutation of his paralogisms so far as they are directed against the self-consciousness that is the source of all the categories. The source of the categories cannot be subjected to them. The center cannot be put as a point in its own self-generated circumference, because the source — the generating center — is more real than its own phenomenal creations.

The old psychology only pleads for the recognition of this knowing self, that knows itself, not as it knows its objects, but by an act of pure apperception or self-consciousness — that knows itself in knowing objects and only knows these in knowing itself; that knows its own knowing in its work of gripping all its objects into ologies, from geology to theology. Such a psychology may be ridiculed as being an *art* rather than a science. But that only reveals the limitations of science. . . .

One illustration may suffice. We go back to one of the oldest psychologists of the psyche-psychology, Socrates. Münsterberg himself refers to how scientific psychology would explain the causes of Socrates turning a deaf ear to the plan of his friends for him to escape from prison and death. "Socrates remained in prison because his knee muscles were contracted in a *sitting* position and not working to effect his escape, and these muscle-processes took place because certain psychophysical ideas, emotions and volitions, all composed of elementary sensations, occurred in his brain, and that they, again, were the effects of all the causes which sense stimulation and dispositions, associations, produced in that organism." But the fact is that Socrates remained in prison because his inner *monitor*, *deamon*, conscience, sense of duty, forbade him to be disloyal to his mother, the state. Socrates is himself criticizing this very old hypothesis that has been recrudesced in the new scientific psychology. The whole passage in the Phaedo is worth many repetitions. I give it in part. Socrates was holding the principle that *psyche* is *self-active* and the cause of action. The natural science teacher tells me, he says, "that I sit here because my body is made up of bones and muscles, which are moved by contraction and relaxation, and this is why I am sitting in a curved posture." But the true cause Socrates replies is "that *I have thought* it better to remain here and undergo my sentence; for I am inclined to think that the muscles and bones of mine would have gone off to Megara or Beotia by the dog of Egypt — they would, if they had been guided only by their own ideas of what was best, and if I had not chosen as the better and nobler part, instead of playing truant, and running away, to undergo any punishment which the state inflicts." With this illustration we rest our contention for *another* form — for the form of the old *psyche*-psychology, which seeks to understand the self-activity; the self-revealing and self-realizing character of psyche in and through the personality that works out the attainments of the truly-human in art, history, ethics and religion and philosophy — in both the objective institutions and in the creations of the ideals that lead ever upward and onward. With the acknowledgment of this as the *primary* function of psychology there need be no undervaluing of the work of scientific psychology in its very limited function of giving us formula of the relatively mechanical aspects of our mental life, by means of which to calculate the future actions of our fellow men. Psyche-psychology only asks that the new science may not pursue its work with a sort of persuasion that there is no inner self-active being at its heart. It asks that a non-spatial, non-sensuous psyche be not regarded as an obnoxious intruder, but as something more real and more substantial than all its physiological and physical conditions.

Doing this it may then go on with its own special lines of study of these conditioning environmental stimuli — these enthralling means of self-liberation — conditions of body, age, sex, race and clime. It would then be a most valuable *part* of *psychology* — the science which describes and explains the evolution — the self-realizing and ripening of the soul, self, personality, or spirit that forms the central subject-matter of psychology. It would thus supplement and enrich the work of the *old unscientific* or hyper-scientific psychology.

I believe that if a vote of the workers in scientific psychology could be taken, the majority would be in favor of the affiliation of the new science with the natural sciences. But philosophy would here hold with Mathew Arnold that "numbers," the majority, is bad. Philosophy like Plato's God is not envious. Like the New Jerusalem which is above, she is the mother of us all, and rejoices at the adolescence of each one of her offspring. For psychology she has a special interest and a mother's yearning to keep it at home, when she realizes that the time has come for it to go out as an independent science. In its new form she needs it, indeed, but only as she needs all other sciences.

Philosophy is burdened with the consciousness of the chaos of experience which she was born to turn into a cosmos. She needs the material of all the sciences. She needs specific questions raised. And there is no science which raises so many profound questions as psychology raises. Indeed it may be shown that all philosophical problems emerge by a psychological necessity from the study of psychology.

In this way it should continue to be the special propædeutic to philosophy. No one ignorant of psychology can enter philosophy — however much Plato may say about mathematics. It is psychology that especially gives philosophy the Antean touch with mother Earth that she may rise *with* earth to heaven. Psychology, instead of being the cuckoo in the sparrow's nest may, even in her scientific form, be the ugly duckling that at maturity leaves the puddle to join her sister philosophical disciplines in the empyrean blue. Psychology will have to be a "heap wusser" before philosophy will wish to be unmarried from her.

ANALYSIS OF SIMPLE APPREHENSION.[1]

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We do not attempt an exhaustive analysis of simple apprehension, but consider the matter on its cognitive side alone. Our aim is to learn something of the psychology of that mental function which culminates in judgment. As to the meaning of 'simple apprehension' there is not quite uniform usage. First let it be understood that 'simple' is a relative term, and cannot, at least before investigation, be taken to preclude all inner complexity. In regard to the meaning of the whole phrase, we somewhat arbitrarily choose the following interpretation. It will refer to those rather simple states or processes called 'presentations' when they have the single additional cognitive attribute of objective reference; or, more briefly, simple apprehension = consciousness of a definite object. It is a state just above the 'anoëtic' line; the amount of definiteness of the object may however vary considerably. Here is an example: as I was thinking about the subject of this paper, I heard, in the margin of consciousness, a noise from the adjacent street. That noise was to me then a fairly definite object of thought. Yet I did not, when hearing it, think *about* it; I did not ask myself, or know, whence it came, what made it, what it was like. Introspection can discover no ideas connected with it, no reasoning, no abstraction, nothing in fact but consciousness of a single object, the heard noise.

The simple apprehension which we are to study is thus a very bare thing. From a psychical standpoint it excludes all the 'higher' intellectual processes, such as ideas, abstraction, judgment proper, reasoning, etc. To state at this juncture just how it differs from these processes would be to give a complete psychology of cognition, including some of the

[1] Read at Baltimore meeting of the American Philosophical Association, December, 1908.

special results of this analysis; in dealing with our limited problem we must be content to say, that since all the 'higher' states involve ideas — no matter what is one's theory of the nature of ideas — they all contain the dualism of *symbol* and *fact*. Or if they do not contain this dualism psychically, they at least presuppose that it has already been learned by the thinker. So, for example, if an object is apprehended *as being* physical, or psychical, or conceptual, or as a table, a virtue, as bad, or humorous, or as anyhow qualified by a previously learned meaning, we have the dualism of fact and idea-applied-to-fact. This accordingly is not simple apprehension, for it is just this dualism that simple apprehension lacks. We may simply apprehend an object as of such and such a quality, only in so far as that quality is immediately felt as in and of the object, *not* in so far as it implies an already present idea, understood to be numerically distinct from the fact, and referred to it. And for this reason memory and expectation too are ruled out, since they involve the ideal meanings 'past' and 'future.' Simple apprehension, as we use the term, is confined to the noticing of the presence of some content or object, taken as nearly as possible for itself alone. If, as we shall perhaps learn, this is not *quite* possible, there is so close an approximation to it that the distinction is for working purposes sound.[1]

Poor though it may be in connotation, it is rich in denotation. We may simply apprehend not only sense-qualities, but any kind of content whatsoever. In the margin of my consciousness I detect the presence of what later reflection would call logical meanings, verbal images, muscular tensions, emotional moods, conscious resolves, etc. If introspection happens then to be my conscious aim, I notice still more subtle shadings and connections of psychical material. As I wrote just now, I was aware of a verbal image, 'number.' It was not central in attention, and I did no more than incidentally notice its presence: that was enough to make it a simple apprehension. One can, after too much work on a mathematical problem, have certain

[1] In this paragraph, as will be seen, I am indebted to the analysis of Professor Baldwin. Cf. especially *Thought and Things*, Vol. I., Ch. 3, and Vol. II., Ch. 2, pp. 14–29.

conceptual meanings, *e. g.*, a^n, $\sqrt{-1}$, $\int e^{-x^2}$, hover unceasingly on the edge of his conscious field. He notices their presence, but thinks not at all *about* them, nor does he apprehend them *as being concepts;* their presence simply is noticed. So too with any sort of material that may flit through the mind. Examples could be multiplied without end; the kind of stuff we apprehend is quite indifferent.

It is this sort of fact we are to analyze. Yet before proceeding to the analysis we wish to insist a little on two characters which render our problem more important than is perhaps generally thought. These are, the integrity and the frequency of simple apprehensions. They are not mere abstractions or aspects of a fully developed judgment-process, but are found in as much independence and self-existence as judgments themselves have; and they are very numerous indeed, constituting perhaps a majority of our cognitive psychoses. As to the first: one may admit in a general way that cognition, feeling, conation, coexist as a rule, yet should remember that the absolute universality of this rule, in the nature of the case, can hardly be proved. No doubt, however, interest, desire and movement play a large part in determining and constituting many of the objects of simple apprehension. But it is of their cognitive integrity we treat. Again and again the 'higher' thought-processes are, in the cases we study, verifiably absent. Consider those marginal sensations of which we are conscious (not the overlooked ones) in moments of moderate preoccupation. Gazing absently about a room, we are often distinctly aware of colors (of wall-paper, books, rugs, etc.) or perhaps sounds (from the house or neighborhood) or even cold or warmth, etc. These definite objects of consciousness are most obviously not identified at the time, or named, or classified, nor do we refer ideas to them and thereby make judgments. The mind is mainly occupied with other things; we have just enough attention to give these objects, to make them into simple apprehensions, but no more. It is difficult to see how one could deny these introspective facts, or what evidence could be produced for the opposite view. What is in the *focus*, indeed, we do often, perhaps almost always, pass judgment upon, for our intellectual energy is centered upon it; but what is in the margin, we are too busy, and too uninterested, to reflect upon. To be sure, one might suspect that the reason for the absence of 'higher' processes lay in the habitual character of the experience, whereby those processes dropped from consciousness. We find however plenty of cases of novel objects, which are no more reflected upon or judged than the old ones. Almost every, perhaps every, marginal content noticed shows this; for, do any of them fail to be in some way new? Certainly few, at most. Our view then must be that simple apprehension is cognitively quite an integral state.

Second, as to the frequency: we have seen that the margin of consciousness forms a natural home of simple apprehensions. This is enough to show us how great is their number. How many do we have at any one moment? Of course it is hard, if not impossible, to draw the sharp line between focus and margin, or between margin and what is below the threshold (for later reflection); yet at a given moment one could find a large number of contents which he can be sure he was just noticing in passing, which were not focal or thought about at all. Let any one try the experiment for himself. The extent of the focus, on the whole, seems much more limited, so that it does not seem too much to say that, inasmuch as there is a rough correspondence between margin and simple apprehension on the one hand and focus and 'higher' states on the other hand, the former constitute a majority of intellectual states. And when we remember that simple apprehension is not only of sense-qualities, but of any kind of content under the sun, this claim of frequency is strengthened. Moreover, we do not deny that simple apprehension can be focal. When we wake from a doze with a start, due to some sudden noise, we are conscious of the noise as a certain definite quality, yet are too torpid to think *about* it, to judge its source or character. We have then a focal content, yet our whole mental state is so infantile at the moment that it does not become a judgment or lead to reflection. These cases, however, are no doubt exceptional.

Notice that we are here treating of adult mental life. Simple apprehension, primitive though it is logically, and predominant as it must be in the babe's life, is still flourishing vigorously in the mature intellect. There is here a nice analogy with biological phenomena. How many hundred acorns are produced, for one that grows into an oak? How many simple apprehensions arise at any one moment, as compared to the few that, happening to be in the circle of present interest, blossom into judgments? As the acorns die without further result, so do the simple apprehensions. We overlook them because of their insignificance and marginal residence. There is to-day a strong tendency to insist that psychic life is always a unity. We must not let this tendency, correct though it is in the main, blind us to certain obvious facts. The 'stream of thought' has a central current, but it carries along numberless disconnected, broken-off pieces of things, and contains side-currents and eddies. Such are the disjoined simple apprehensions. They contribute little perhaps to the useful cargo of the stream, for they are the intellectually useless remnants; yet their name is legion.

We may now take up the analysis of these facts. What is their essential make-up, regarded as cognition? The examination should be genetic as well as analytic, but this paper takes only the latter method (as the title indicates) inasmuch as genetically simple apprehension is so low in the order of development that it is difficult to deal with it. At the same time we must admit that our results are open to, and need, supplementation from the genetic side: though it is believed that no supplementation can falsify them.

In all these cases, two facts are easily made out. There is some actually present content, and it is discriminated. As to the content, its nature, as said above, is quite unrestricted. It has psychologically (and may even have psychically) intensity, duration, complexity and other attributes of psychoses. The meaning of the word 'content' is to be interpreted parsimoniously. It is "mere stuff or matter presented to consciousness, considered as stripped of the special meanings and modifications peculiar to the psychical process then going on."[1] For later reflection, the content is usually distinguished from the object of the apprehension; how far this distinction reaches, and whether it masks

[1] Baldwin, *Thought and Things*, I., p. 40.

an underlying sameness or not, are well-known and delicate issues into which we cannot here go. This inadequacy, however, will not affect our inquiry, inasmuch as the object, in so far as it does differ numerically from the present content, is somehow beyond the momentary consciousness, *i. e.*, is not psychical; there is psychically no dualism of object and content in their stage. And our inquiry is just now concerned with the psychical make-up alone. Accordingly in what follows I shall pay no regard to this distinction of object and content.

As to the discrimination, it seems equally essential with the content. "All processes of thought are *eo ipso* processes of discrimination,"[1] says Dr. Stout. So too Professor Baldwin: "the essential thing about a mental object is, that it is in some way grasped as a distinguishable unit of presentation or meaning."[2] As we use the term, discrimination means, that the content is apprehended in distinction from something else;[3] this something being in, or part of, or the whole of, the (psychical) spatial or temporal or other environment. Thus, a tone is often heard in discrimination from the just-past silence, or other just-past tones. An object in space is commonly distinguished in shading or color from the surrounding objects. A thought, logical meaning or mere fancy which lurks on the margin of consciousness may be noticed in passing, in that its content is so different from the other thoughts of the moment. Indeed, what we are now claiming is but a tautology. A 'definite' object is, and means, an object to some degree singled out, distinguished from other objects. We emphasize this tautology because it is the nature of the discrimination in particular, that we wish to examine.

The discrimination consists in our apprehending the object as distinct from something else. This must of course not be interpreted (at least as yet) in the sense of predication. We do not qualify the object by the adjective 'distinct' or the relation 'other than something else,' for such an employment of adjective or relation could not take place until a consciousness of sep-

[1] *Analytic Psychology*, Vol. I., p. 48.
[2] *Op. cit.*, Vol. I., p. 41.
[3] Subject to a qualification which will appear later.

arable contents were present. And not only have we no right to suppose this: it is demonstrably not so in many cases. What we apprehend is, one present complex: object-distinct-from-environment. Many questions might be here considered: *e. g.*, is there apperception here, or association? What part does interest play in furthering the discrimination? What part does the objective stimulus? Are we mentally active or passive here? These I simply neglect: adding only the remark that while the discrimination shows the selective character of all attention-processes, leading us to single out certain objects, that is not a case of abstraction. Abstraction is *derived from* selective attention, but does not itself occur in simple apprehension.

Note, first, the nature of the content and the psychical environment from which it is felt as distinct. These two are psychically to each other much as focus and margin. This way of describing their connection I believe to be accurate, even though the whole subject-matter of apprehension be in the margin of consciousness. For there are very often, at any one moment, two or even more foci and margins; and whereas the content simply apprehended is itself not the main object of attention at the time, but is in the margin, it too has a margin or fringe which is peculiarly its own, namely, the psychical environment which is immediately concerned in the discrimination of it. This statement may seem, at first sight, an over-subtle fancy, quite beyond verification; yet let us see. As I write this I hear a sound which I later interpret as that of a blacksmith's hammer. That sound stood out distinctly from the general background of dull noise, of my pen rustling over paper, of distant wagons, etc. I merely noticed the sound in its distinctness for a moment, without thinking about it at all; it was to me a definite object, a matter of simple apprehension only. But notice: it was in the margin of my consciousness then, for the focus was occupied with the argument of this paper. And introspection plainly shows me that it was not in any way felt as directly connected with the focal topic (until later I used it to illustrate). It *did* contrast with *other tone-sensations* and thereby stood out in distinctness before my attention. Those latter thus formed a background belonging peculiarly to the sound of the hammer:

yet that background, while undoubtedly in consciousness, was not in as great a degree the object of attention as was the sound of the hammer. So I call the background of tone-sensation the margin of the latter sound; together they form a secondary pair, focus and margin, over against the primary focus and margin, the argument and the rest of what was in consciousness at the time. Thus the secondary pair (or pairs perhaps) forms part of the margin of the primary pair; it is not so central in the field of attention, yet does itself form a subordinate node or center of emphasis. Are we here attributing to conscious states more complexity than they actually possess? Is there anything beyond the reach of introspection in this? The matter seems to me eminently verifiable.

The object of describing the matter in terms of focus and margin, is to bring out a rather difficult point: namely, that while the content apprehended is always discriminated or distinguished from its own felt margin, sometimes (perhaps often) a special case arises in which that margin is, psychically, as good as lacking. That it is entirely absent, would be too hard to prove: that it is for all practical purposes absent, may I think be verified. Let us consider some cases. When I am wakened from sleep by something I know not what, I may be aware of the stimulus that wakens me as a certain vague quality, and distinct, too, a definite object; yet with what is it contrasted at the moment? From what is it distinguished? Psychically it is impossible to say. We feel that there *must* be a margin, against which as a background the waking stimulus should appear, but on looking back, we cannot find that we were aware of any margin. A very good reason appears for this too: the margin was so indistinct and commanded so little attention as to fall below the threshold, to disappear. Here then we have the curious situation, that a content A which we should expect to be discriminated from its environment B is present alone, and that too with distinctness, while yet that from which it is distinct is in no sense present. Later reflection, looking back, finds what it must interpret as a relation (of difference or otherness) with only one term. The content A is distinct, yet from no particular other content. But, we may be asked, how can you say that A is distinguished, when there is felt nothing from which A is distinguished? Why not say we here apprehend an isolated content, the waking stimulus, without discrimination? Then there would be, for later reflection, nothing that could be described as a relation with but one term. We reply, *it is not an adequate account of the psychical facts, to stop with the one apprehended content A;* there was felt, at the time, A's definiteness as well (not of course as a *general* or *abstract* quality). Perhaps the best way of describing it would be to say that we should normally tend to view A in connection with some other then present content, but that the tendency was unfulfilled owing to the fact that the margin speedily disappeared, or was from the first quite anoëtic. We do not however insist on the presence of this tendency. The important thing is that there is more than just consciousness of one content, yet not a consciousness of two.

Another case is this: if we see in darkness a light shine suddenly, then disappear, while the mind is attending to some intellectual topic, we are then and there aware of the light as a definite object, and it is quite distinct and discriminated, yet at the time we probably had no thought of the darkness from which it should be discriminated. The whole matter being in the primary margin, the secondary margin, or darkness from which the light is discriminated, was not just then strong enough to be above the threshold. It was merely anoëtic, mere sentience. Yet the light is felt as distinct, standing out, discriminated. This strange absence of a second content, while yet we may in reflection speak of a relation attaching to the one content apprehended, may seem more probable when stated in physiological terms. Let simple apprehension, as involving discrimination, be relegated to a 'higher' center. To this center the physiological counterparts of the two contents (the object apprehended and its own margin) are brought from two 'lower' centers. Now the physiological counterpart of this margin will often be weak, so weak as completely to vanish before getting to the 'higher' center, *i. e.*, to vanish in the sense of, to be too weak a stimulus to make itself felt. Yet it would be physiologically present and, combined with the other content (or its counterpart), would serve as a disposition toward a discrimination or consciousness of distinction, while yet, being physiologically so weak, it would not be able actually to bring about a consciousness of a second discriminated content.

We have spoken of a relation with but one term, as the interpretation which later reflection could make. Speaking psychically, however, we cannot call the apprehension of a content in distinction from another, a consciousness of relation. In regard to the discreteness or otherness of the parts which make up the cognitive whole, Professor Baldwin says: "The psychic awareness of this is so far the beginning or rudiment of a meaning which we may call *relatedness*"[1] and "To consciousness the meaning is not yet relation, it is *mere togetherness or joint participation in a cognitive whole or object*."[2] It is thus only the germ of relation of which we treat. The interesting thing is that even in such relation-germs we have that same lop-sidedness which appears in some relations as such. That it does appear in the fully developed consciousness of relations, has been shown by James, Woodworth and others. From the ideal point of view of logic these relations or relation-germs with but one term (or in other cases than our present ones, even no terms) seem to be impossible. And as the ideals of logic have so long dominated our observation in the psychology of knowledge it may seem strange that such irrational entities can be the object of even the simplest cognition. Professor Woodworth says in this connection: "The logical axiom that a relation is nothing without its terms should not be psychologically misinterpreted to mean that a feeling of relation is nothing without the feelings of its terms. The feeling of a relation may exist without the feeling of any pair of terms."[3] These words are, I believe, very important for those who investigate the psychology of cognition.

There remains the problem of defining relation psychically; a very difficult one, that has hardly received its just due, and must here be ignominiously shelved. How does consciousness

[1] *Op. cit.*, I., 178.

[2] *Ibid.*

[3] 'The Consciousness of Relations,' in *Essays Philosophical and Psychological in Honor of W. James*, p. 493.

of relation differ from that of complexity? If the latter may be a term, why not the former? Are all psychical relations without terms 'transitive' states or 'substantive' states, or both? These are questions whose answers are requisite to a complete exposition of our view, but they must be neglected. We must content ourselves with saying that we call the additional factor which gives to the content its distinctness, a relation-germ, because it seems to be of the same sort as the factor which later reflection finds accompanying the apprehension of two contents and which it calls a relation. That must now be our only defense for speaking of relations, or relation-germs, with but one term.

So far we find the structure of the psychical subject-matter of simple apprehension to be, at a minimum, a content and a relation-germ: let us denote this by Cr. Of course there may be another content (the environment or margin), or several contents distinguished in one awareness. Then we should have the symbol $CrC'C'' \cdots$. But the minimum essential is Cr.

Besides this structure of the subject-matter, we find a trace or disposition left in the mind, which would normally lead to belief in opposition to suggested doubt or denial. This is at the time no more than a mere feeling of presence, or 'reality-feeling.' It is, I think, doubtful how far this is a psychical matter: usually we are certainly not conscious of the presence of A in any way that is different from being conscious of A itself. Psychologically however the trace of A's presence exists; the proof of which is that introspection leads us, in such a case, to say 'I really did see that, hear that,' etc. Of course it is true that many objects are apprehended without belief ensuing; and that there is a wide distinction between being conscious of an object and believing in it. Dr. Stout has shown this clearly,[1] and we follow his position in the main; the case of 'make-believe' and of imagination's play or esthetic contemplation, if consciously such, are by that very fact precluded from belief. Otherwise they are indeed believed, in the absence of inhibition from sensation, etc. To test this one has only to become wholly absorbed in some imaginary situation; he will find himself behaving in every way as if it were real. But the tendency to

[1] *Analytic Psychology*, I., pp. 101–107.

believe (meaning, to insist upon the presence of, against doubt or denial) is simply a part of the fact of memory. For its full development, many genetic stages are needed, of course: but the trace left by the content apprehended sets the development going. The very pertinent question of the nature of belief we must leave entirely aside.

Besides the disposition to believe is a property, not psychical, but true only for later reflection. A simple apprehension may, to a certain extent, be true or false. The views of Hobhouse [1] and Cornelius,[2] among others, decidedly conflict with this assertion. Cornelius in particular claims that error is possible only where we refer to something beyond the present content, as in a memory-image. He (and Hobhouse) seem to me to overlook the fact that simple apprehension may, even at its minimum, be so tied up with a margin just beyond, or even in the edge of, present consciousness, that the least bit of error is possible. In fact, Cornelius himself has urged [3] that apprehension involves at least a discrimination of the present content from the just past. Our account has also emphasized the relation-germ of distinction, though not insisting on distinction from the just past as the one essential. But on either view, there is room for some error in the apprehension.

Two cases arise: when the whole content apprehended is immediately present in the conscious field, and when the marginal part, from which the object is discriminated, is as good as absent. In the first case, there is indeed practically no error, yet there is theoretically just the least possibility of it. Thus: if a tone is apprehended in distinction from a felt margin of other sounds, or an object in distinction from its spatial surroundings, there is, as we near the outer edge of the felt-margin, room for some uncertainty as to just what we really do hear or see. This is usually very slight, but might be appreciable. It would not be, of course, an error of interpretation: it is that we cannot always be sure whether we had this or that sensation or not. One might think he felt a quality that psychically he

[1] *Theory of Knowledge*, pp. 32-37.

[2] *Existentialurteile*, p. 29.

[3] *Op. cit.*, pp. 17 and 22-23.

did not feel, in the margin. And when the object apprehended is dependent, for its nature as apprehended, partly on the margin from which it is distinguished, then the object itself may to some slight extent be wrongly apprehended. If the margin feels to me like silence, then the tone whose heard nature depends on contrast with that silence, will be heard louder, than if the margin does not seem silent (other things being equal). And if I am mistaken in thinking I hear no sound (as on the very margin it would be difficult to tell) then I mistakenly apprehend the tone itself. So too of vision: if the margin of a seen object looks darker than it really is felt to be (to careful introspection) then that object itself will be apprehended, by contrast as brighter than it is really felt to be.

If this is true of the case where both object and margin are present, it is more evidently true in cases where the margin is absent. If the darkness in which a bright light shines out is not thought of at all when we notice the light, its value for apprehension is of course not estimated at all, and may very well be such as would change the feeling of discrimination which accompanies the light as seen. If we do not know the other term of a relation, there is even more obviously an occasion for error than if we know it rather uncertainly. It might seem here that there could be no error, as there is only the object and its distinctness, both of which are in the focus (primary or secondary), so that we cannot find any region of uncertainty out on the edge of consciousness. Its distinctness, however, may be such as would normally imply or suggest a certain kind of a content, and that alone, as the margin — while if we had paid attention we should have found a quite different margin. The darkness we did not notice might be brighter than the distinctness of the object would normally carry with it; and if we looked again, the felt darkness would a little alter the distinctness with which the object was apprehended. The liability of these latter, as also of the former, cases to error lies, it will be noticed, in the fact that they are, or are directly connected with, what is just the least bit beyond the center of clear consciousness, or even farther beyond. Thus, as we hinted at the outset, it is not *quite* possible for simple apprehension to be confined to the immediate present. This property endows it with a very important logical consequence, as we shall now see.

That consequence is, that we may fairly call simple apprehension a logically primitive kind of judgment. This is due to its liability to error; for the two most generally accepted criteria of judgments, perhaps, are *belief* and *liability to error*. And these two are predicable of simple apprehension.

To many this result may seem an unimportant commonplace, and to others an unwarrantable confusion. We are of course aware of several differences between judgment proper and simple apprehension, and yet it does not in the least obliterate those differences that we insist on an underlying identity. As simple apprehension seems to be the lowest grade of knowledge logically, may we not go so far as to say that we have shown a criterion of all cognition? Whether or not this is a sufficient one, it does appear to reveal a bond of union that has not to my knowledge been pointed out in recent psychology, between the lowest and highest stages of knowledge, and furnishes another argument to those who insist on continuity of mental function.

We may now draw some conclusions as to the general theory of judgment (using the term to include simple apprehensions). In this field are found two extreme positions; the theory of Brentano, and the 'predication-theory.'[1] Brentano held that a single content may be believed, or accepted. We find that the minimum which is believed is a content in a certain relation (or 'relation-germ' as we called it). The simplest kinds of judgment then are 'two-membered.' This is also the view of Cornelius.[2] But on the other hand, the predication-theory would go too far. It lays claim to two contents in relation, whereas we have found that but one content is necessary; the other member of the cognition being a relation (or relation-germ). And further, in the structure Cr above found, there is nothing of the subject-predicate relation, not even an analogy to it. The r is certainly not felt as a predicate of the C; we should go so far as to say that even for later reflection the subject-predicate description was not a just account of the matter.

[1] Cf. Professor Baldwin's treatment of these two theories, in relation to fully developed judgment, *Thought and Things*, Vol. II., pp. 14–36.

[2] *Op. cit.*, p. 31.

The statement of Wundt[1] applies most fittingly here: "of the total mass, certain ones appear as the bearers of the whole idea, while others have lost their self-existence." This dependence which Wundt mentions is about as near to predication as we can get, and it is certainly far enough from it. Of course we recognize that the two extreme theories we here treat, were not meant as theories of simple apprehension. We bring them in, as they show so clearly what simple apprehension is not.

The analysis is not yet finished, however. We have to ask if the connection between belief and the subject-matter *Cr* is quite accidental. Could we on later reflection accord our belief to a single content *C* just as well as to one in a relation, *Cr*? In other words, is there any such connection between the structure *Cr* and the belief that goes with it, as to show that each is really essential to the other, and both together form a unitary process? Is there any special fitness or adaptation between the structure *Cr* and belief?

Let us note first, that disbelief or doubt of a given content is due always to the relations in which it is felt to stand. Thus, we disbelieve in mermaids, not because the idea of mermaid has any peculiar unreality-sign about it, but because it conflicts with, or is inhibited by, other contents which are already believed. If the mermaid-content were before attention in complete isolation, without any relations to other contents, it could not be disbelieved or doubted. This is the familiar criticism of Brentano's doctrine, made by Sigwart,[2] Jerusalem[3] and others. We cannot reject an idea just by itself while attending to it; disbelief and doubt are directed toward an idea (it would be better to say content) as in this or that relation.[4]

A doubted or disbelieved content then must be viewed in relation to others, in order to be doubted or disbelieved. But these other contents must be already believed; we have got to start with contents believed for their own sake, in themselves, before inhibitions can work.

[1] *Logik*, Vol. I., p. 14.

[2] *Logic* (English translation), p. 72, footnote.

[3] *Urteilsfunktion*, p. 66.

[4] This is close to Meinong's *Objectiv*, and suggests one more important use for the *Annahmen*. Cf. *Ueber Annahmen*, Ch. III.

And there seems no reason for claiming that what is originally accepted with reality-feeling need be viewed in relation to other experience, in order to be accepted. Apparently an isolated content could be so accepted, merely by itself. Here is the strength of the intuitive or realistic definition of existence, as an irreducible *quale*, not relational. We must admit that as regards mere reality-feeling, there is something irreducible; and a content C would seem not to need to be viewed in relation to other contents, in order to be thus accepted. But reality-feeling as we have it in cases of simple apprehension, tends to become more, to develop into belief in the sense of resolved doubt, firm against suggested disbelief. Viewed psychologically, it is a disposition to believe against doubt. Now belief against doubt, on the intellectual side at least, consists in recalling the original reality-feeling and also the relations or context in which the content in question was felt to stand. This is proved by introspection. Thus, if you ask me, did I just now really have the idea of a mermaid? I say, I did have that idea, and the ground of my belief against doubt is that I recall having just felt the presence of that idea, and also, *as added evidence against doubt*, that it had its place in my consciousness over against certain other feelings I then had, which formed its context. I believe, against doubt, in the presence of that content, in so far as I can repeat the reality-feeling and add to it the psychically present relations that content bore. So always: we confirm our beliefs not only by recalling our natural reality-feeling but by viewing the believed content in the relations which, as believed, it bears.

To see the universality of this rule, we need only ask, could the content be treated thus, unless it had been felt in certain relations (or relation-germs) at the very outset, when we had only reality-feeling, only the disposition to believe against doubt? No, we reply, for it could not have been recalled as in a relation or context or setting unless it had at sometime or other been immediately felt in that setting. *What* the setting is, is apparently indifferent; it may be any relations or context you please, and apparently need not be confined to the relation of *distinction* or *difference*. But some there must have been immediately felt, if it is to be recalled: and the minimum is, of course, *Cr*. Recent genetic psychology has confirmed this analytic result. The conscious organism has advanced from reality-feeling through doubt to certainty, by viewing each content in its setting, its consequences practical and theoretical. Accordingly we seem justified in affirming that there is an adaptation between such a structure as *Cr* and the tendency to believe against doubt.

If the argument is correct, it has shown that (1) simple apprehension as here defined is independently actual and unexpectedly frequent in mental life, (2) it has the essential structure of content in relation, or germ of relation (two contents not being needed, and one alone being insufficient), (3) it is closely akin with fully developed judgment, in possessing that intrinsic property of thought, liability to error, (4) there is perfect adaptation between the structure of its object-matter and the function it performs as a mental process of belief. Of the four, the second is perhaps new as applied to cases of belief, and the last, it is believed, is quite new. However that may be, these two seem to the writer the most important of his results.

ÆSTHETIC IMAGERY.

BY H. HEATH BAWDEN.

Every notable theory of the æsthetic consciousness has had to reckon with the question of the mutual relations of the sensuous and the ideal elements in beauty. Extreme sensationalist theories have reduced it all to terms of sense. Extreme intellectualist theories have reduced it all to the ideational or thought element. It is now generally recognized that both are essential, the problem at the present time being rather the respective parts played by each of these indispensable factors.

The Sense Element in Art.

The word 'taste' originally, of course, referred to sensations of the palate. But it came to be used for æsthetic taste, and æsthetics is often called, as by Kant, the theory of taste. But in spite of Kant's protest against the use of the word 'æsthetics' it has come into more general use than his own phrase 'judgment of taste.'

'Æsthetics' by its etymology emphasizes the sense element. Kant distinguished the agreeable and the beautiful, *i. e.*, the pleasures of sense and æsthetic pleasures. Mere sense, he held, could not be made the basis of an æsthetic judgment — there must be an intellectual element. Hence the lower senses and even color and form and tone and rhythm, in the case of the higher senses, yield only the subjective appreciation 'I like it'; they do not yield the objectively valid æsthetic judgment 'It is beautiful.'

Kant's main contention must be admitted, that there must be an intellectual ordering of the sense materials in order to yield an æsthetic experience. Indeed, we have already seen, in terms of Dr. Marshall's discussion, how it is the ideal element, the imagery, which gives permanency to the pleasure-field of æsthetic emotion. But, on the contrary, it must equally be admitted that there can be no emotional, and therefore no æsthetic, experience without a fundamental basis of sense elements. Emotion is described in current psychological theory as a complex of so-called lower sensations. But we need not appeal to this still uncertain psychological theory of emotion, for it is as true of ideational as it is of emotional life: there is a basis of sense underlying the most abstract intellectual processes.

The problem, therefore, concerns the respective parts played in the æsthetic experience by these two elements or factors — the sensuous and the ideal. Spinoza said that experiences of beauty are 'confused acts of thought,' and Baumgarten, following his lead, tried to establish a science of vaguely *felt* perfection (æsthetics), a science of obscure knowledge, alongside of the science of clearly *thought* perfection (logic), the science of precise knowledge. Kant agreed with them in holding that there are some things which can be felt which cannot be thought or willed, but he regarded the æsthetic experience as a union of sense and reason in accordance with certain laws of the understanding itself. In this respect Kant laid the basis of the subsequent development of æsthetic theory at the hands of the idealists. In Schelling beauty is 'the infinite represented in finite form' or the finite is racked and stretched to become an expression of the infinite. For Hegel beauty is the sensuous embodiment of the ideal, the revelation of meaning by matter, spirit shining through sense, the infinite and eternal manifested in the finite and temporal. Through all these modifications of its function in determining the nature of beauty, the sense element abides as an indispensable factor.

The eye and the ear are called the æsthetic senses primarily because they are the higher or more intellectual senses: the sense material is more mediated by thought. But this supremacy of the eye and the ear has interfered with the true understanding of the æsthetic experience — since beauty on the emotional side is grounded in the so-called lower senses. The distinction between the higher and lower senses is of ethical origin rather than intrinsic to æsthetic inquiry. The fact, for example, that the lower senses are more personal and interested, is not sufficient ground for ruling them out of the æsthetic sphere, for, as Professor Santayana says, even 'disinterested' and 'unselfish' interests 'have to be somebody's interests' (*Sense of Beauty*, p. 39) : it is not the fact that touch and temperature and smell and taste are personal, that they are not ordinarily regarded as æsthetic, but because they are relatively unmediated.

It is asserted that the higher æsthetic senses are less violent and extensive than the lower senses. But rhythm is a striking exception to this rule. The universality of æsthetic pleasures is contrasted with the personal isolative character of the lower sense pleasures. But "nothing has less to do with the real merit of a work of imagination than the capacity of all men to appreciate it; the true test is the degree and kind of satisfaction it can give to him who appreciates it most" (*Sense of Beauty*, p. 43). The truth is, that the æsthetic character of an experience turns, not on the particular character of the sense elements present, but upon the use made of them when present. Odors, tastes, contacts, resistances may serve as the sensuous elements in art as truly as color, line, tone and rhythm.

Under what conditions, then, does a lower sense quality become æsthetic? This question may be answered in various ways. When more than one sense is stimulated at a time, the sensations involved in such consentient stimulation present the conditions for æsthetic treatment, since here is provided the opportunity for associative imagery to set in motion its machinery of irradiation of the feeling-tone and interpretation of one sense value in terms of another. In other words, here is provided a permanent pleasure-field with its focus and context, to use Dr. Marshall's metaphor. An unconstrued sense experience — a succession of ripples or bird-notes — is not æsthetic. To become æsthestic stimuli must be, not merely perceived, but apperceived. Beauty, as Professor Santayana says (*Sense of Beauty*, pp. 49–52), is pleasure objectified, pleasure regarded as the quality of a thing; æsthetic satisfaction is a mediated satisfaction; only the intellectually pleasurable can be æsthetic. Eating and drinking are æsthetic just in the degree that they differ, by being humanly mediated, from the feeding of brutes.

But this intellectualizing or mediating process must not be understood in too narrow a sense. Civilized man is ear-minded and eye-minded and the beautiful is the perfect for eye and ear;

but because this is true for most men it does not follow that it is true for all, nor that in time the other sense experiences may not become æstheticized. The reason why art in the past has been chiefly in terms of vision, hearing and tactile-kinæsthetic combinations with these, is because here we have found the conditions of the maximum combination of meaning and emotion, of stimulation and repose. The reason the lower senses (with the exception of rhythm and a few plastic effects) have not been the avenues of æsthetic appreciation to a greater extent is that they have been relatively poor in social, practical, scientific, ethical and religious content — not because there has not been a wonderfully rich emotional content of a personal sort in these sense experiences, but because of a warped and unfrank self-consciousness in things personal, these aspects of experience have remained unmediated and impulsive. Beauty is 'the characteristic in as far as expressed for sense-perception or for imagination' (Bosanquet, *History of Æsthetics*, p. 6), *i. e.*, for the intellectual imagery most closely connected with the habits and emotions. As Schiller said, "In the eye and ear aggressive matter is already hurled back from the sense, and the object is set at a distance for us, while in the animal sense we are directly in contact with it" (*ibid.*, p. 294). The highest type of beauty is found in the living object, because here we have the maximum of meaning with sensuous embodiment. Nothing dead or conceived of as dead seems as beautiful as the living. A flower growing in the woods is much more beautiful than cut flowers in a vase. Professor Knight suggestively brings out this in asking us to suppose the opal to be alive: how that would enhance its beauty!

The practical, logical, ethical, religious meanings, in other words, must be put into sensuous form before they can become æsthetic. This means that they must be put in concrete rather than abstract form before they can arouse the matrix of organic and tactile-æsthetic sensations and images which constitute the core of that pleasurable emotion which is essential to the æsthetic experience. In the words of Sully-Prudhomme, "It is only by first caressing our senses that art arouses our feelings and awakens our thoughts" (quoted by Hirn, *Origins of Art*,

p. 99). *To the degree that the so-called non-æsthetic intellectual contents can be organized into the art product and still arouse this emotional background of sensuous elements, the higher and greater the art.*

The Thought Element in Art.

Sensation is the material which is ordered and controlled by that thought and reason which transform mere agreeable feeling into æsthetic emotion. This factor of control is the ideal element in art. Pleasure becomes æsthetic only when it becomes significant, when it serves to usher in an idea which is expres sive. Hirn says: "When a savage had attained so high a state of development as to be able to control the impulse to dance and yell for joy, the first dithyramb had been composed" (*Origins of Art*, p. 49).

Thought is man's method of managing his experience. The image, idea or ideal is an instrument of control. There is no faculty of imagination or idealization. Imagery is a fact, not a faculty: it is a mere name, like attention or will, for the fact that experience goes on in a certain way and in accordance with certain laws, these laws being mere descriptive shorthand for this observed uniformity. The image or idea must not be regarded as an entity existing outside of consciousness and having an existence whether the individual is thinking or not—the fallacy of the associational psychology. Nor is it the mere copy of a reality lying outside of our experience—the fallacy of the representative theory of knowledge.

An image when it is not performing its function as an image, is a physiological habit—a part of the neural structure of the organism. There is no such thing accordingly, as the storing up of images as such: they are stored up only in the sense of producing modifications of structure in the nerve elements. The image originates in the irradiation and retention of the effects of sense-impressions after the immediate excitation has ceased. Every feeling or sensation produces a disturbance of the entire organism so that "a process set up anywhere in the centers reverberates everywhere, and in some way or other affects the organism throughout" (James, *Psychology*, B. C., p.

371). That is, physiological traces from every sensational experience are left in the nerve centers. These physiological traces are what in the race we call instincts and in the individual habits. These traces get organized into systems, and, under suitable conditions of difficulty or tension in adjustment, are brought to consciousness as apperceptive systems. Memory and imagination are just conscious habits.

But why and when do these habit-systems come to consciousness as such systems of images? This is the important question. The answer is: When, due to the relatively novel conditions of a situation requiring new types of adjustment, these habits are brought into consciousness for the sake of revision and modification. Let a habit fail to work in the new situation and it is thrown into the region of consciousness as an image where it remains until the adjustment is rendered adequate. The image is a middle term or intermediary between an old and a new experience; it is the bridge by which we pass over from one state of relatively immediate experience to another — it is the machinery of mediation. Imagination is simply *image*-ination, the turning over of habit-systems into chains of ideas (association) or systems of ideas (apperception). An idea is a habit turned outside in.

The sense element in art represents the materials of beauty in so far as they as yet are inadequate in calling forth the æsthetic response — in psychological terms, the sensation represents the relatively unstimulating and inadequately stimulating stimulus. The ideal element in art, the æsthetic imagery, represents inadequacy on the side of the habits of the artist or appreciator; they do not enable him to control the conditions, hence they must be brought to consciousness for reconstruction in the form of imagery: in psychological language, the æsthetic image results from the obstructed or inhibited or inadequately responding response.

The tactile-kinæsthetic imagery is the fundamental imagery of meaning in art, as elsewhere, because it is the imagery of action. Helen Kellar can have a highly developed intellectual life and rich experience of values because she has this primary imagery. It is inconceivable that she should be able to have this, or even to survive, if it were lacking. It is not the most efficient instrument in relation to the ends of science. The visual and auditory imagery excel for purposes of verbal analysis and definition. But any image may mediate the æsthetic experience, if it fulfills the conditions of the law of stimulation and repose, because all images are more or less motor, *i. e.*, have a tactile-kinæsthetic basis. This is implied in the part they play as instruments of control in the reorganization of experience. The character of an image is determined primarily by its function in relation to the revision of habit systems, and this is a matter of sensori-motor coördinations — a matter of action. The value of an image lies therefore in its function as a motor cue, not in its being a good visual picture or auditory echo. The reality of an object must ultimately be defined in terms of our overt or incipient reactions to it. "Any object — a tree or chair, for instance — is a cluster of all the possible modes of touching and manipulating it that we do not carry out. . . . It stands for a number of suppressed contact reactions. . . . The image or object, therefore, as built up in human experience, represents an intricate system of translations, substitutions, inhibitions," and since the image is merely one experience used to get another, standing for it and controlling it, it follows that the final image 'coalesces with the object, *is* the object" (Adams, *The Æsthetic Experience*, p. 16). In other words, the image lasts only as long as the experience is problematic, and falling short of what it aims to become.

But the image does in a sense and to a degree accomplish what it sets out to accomplish, and in so far as this takes place there develops a new phase which may be described as the distinctively æsthetic aspect. Miss Adams distinguishes between what she calls the 'working image' and the 'æsthetic image' (*The Æsthetic Experience*, pp. 17–18). The working image is the purely intellectual or conceptual aspect prominent in all serial or successive types of association. It is worn down to a mere cue or signal, having lost most of its fulness of sensory detail and emotional warmth. Drudgery exhibits the working image in its extreme form. The æsthetic image is one which has incorporated a more or less wide range of rich collateral materials of a sensory and emotional character into a relatively simultaneous synthesis. If imagery in general represents controlled impulsive and habitual responses, the æsthetic image represents the maximum of such mediation or control compatible with the experience as a whole remaining pleasurable. And since all successful control is normally pleasurable, it follows that all practical and intellectual reconstruction of experience tends to culminate in an æsthetic moment. There must be stimulation, diversity, cognitive differentiation, conflict of habits and antagonism of impulses, in order to lift the experience from the plane of mere animal sense-impression. The æsthetic consciousness 'stands for the fullest possible simultaneous excitation of these old tendencies to response' (*ibid.*, p. 76) compatible with its remaining a predominantly pleasurable experience. This is doubtless Ruskin's meaning when he says: "That art is greatest, which conveys to the mind of the spectator, by any means whatsoever, the greatest number of the greatest ideas, and I call an idea great in proportion as it is received by a higher faculty of the mind, and as it more fully occupies, and, in occupying, exercises and exalts, the faculty by which it is received."

The Relative Freedom of the Æsthetic Image.

The chief characteristics of the æsthetic image are its relative freedom or disinterestedness in form, and the intrinsic character of its content.

Kant says that "beauty is the form of purposiveness of an object so far as this is perceived in it without any representation of a purpose." That is, 'we contemplate beautiful objects as if they were purposive, but they may not be so in reality.' The æsthetic image must be a free image, not tied down to any non-æsthetic utility. The æsthetic judgment is an optional judgment, not instrumental to some transgredient end. "Everyone must admit," says Kant, "that an æsthetic judgment in which interest plays ever so small a part is partial and illegitimate. To be a judge in matters of taste, the existence of the thing to be judged must be indifferent to us." Nature is beautiful, he says, only when it exhibits the purposiveness of art; but art is beautiful only when it exhibits the freedom of nature.

The æsthetic judgment may not depend on any utility, since that would interfere with its disinterestedness. It may not be determined even by a standard of perfection, for according to Kant, the idea of perfection implies a criterion outside and beyond, and the æsthetic judgment must find its justification wholly from within. Kant distinguishes between free beauty and dependent beauty and holds that a perfectly free disinterested beauty cannot express an ideal, since an ideal suggests dependence on something beyond itself. Flowers in the state of nature, humming-birds, sea-shells, ornamental borders on wall-paper, he says represent free or self-subsistent or true æsthetic beauty, but flowers as they appear to the botanist, beauty of the human form, a church building, have only dependent beauty because they imply a certain purpose or use. Many writers since Kant have followed him in this doctrine of the aloofness and uselessness of art — it is one of the fallacious meanings of that ambiguous phrase 'art for art's sake.'

But if our preceding analysis of the relation of the sensuous and ideal elements in beauty is correct, it is not the presence or meaning of purpose but the irrelevance of the purpose, which interferes with an object being beautiful. Meaning is no barrier to beauty if the meaning be intrinsic, *i. e.*, relatively adequate as an embodiment of the relations which it suggests. We are free in the æsthetic experience, but we are not free from all ends or meanings; we are free only from necessary relation to an extrinsic end. Indeed, the freedom is gained just by the controlled or relatively adequate organization of what would otherwise be transgredient ends, into terms of an harmonious but internally diversified system.

Kant says: "We could add much to a building which would incidentally please the eye, if only it were not to be a church. We could adorn a figure with all kinds . . . of lines, if only it were not the figure of a human being. And again this could have much finer features and a more pleasing and gentle cast of countenance provided it were not intended to represent a man, much less a warrior" (Bernard's translation of *The Critique of Judgment*, p. 82). But the true principle here should be to so embody the generic meanings of the religious institution in the church building or of humanity in the warrior, that these individual creations would enhance these meanings, not seek to purify the æsthetic judgment by emptying it of all relevant meanings whatsoever. Not the presence of purpose but the particularity and arbitrariness of the purpose militate against beauty. There must be relevancy as well as elusiveness in order to call out those deep-lying intellectual habits whose exploitation along not too unfamiliar lines is pleasurable. The meaning, in other words, must be organic with its sensuous expression or embodiment: there must be a free interaction of its parts with each other. This is the truth in the doctrine of the freedom of the æsthetic image and the disinterestedness of art.

Its Meaning Relatively Intrinsic.

There is nothing that in itself has æsthetic value. Beauty comes from the making intrinsic of the other values. Anything is artistic or beautiful in the degree that it involves the consciousness of an end in terms of an organic and functional synthesis of the means. "A mere work of art is a baseless artifice," says Professor Santayana (*Reason in Art*, p. 208). Art for art's sake is art become self-conscious in the bad sense, art become professional: the artist should have no consciousness beyond that of adequately organizing the meanings which come to him from other spheres of life. Beauty is its own excuse for being because it is just the other values finding adequate expression.

The æsthetic object, the thing of beauty which is a joy forever, consists of a functionally complete synthesis of the relevant elements in the situation. *The fundamental principle of artistic production and the key to æsthetic appreciation is this: such a disposition of the factors which enter into the object as will give to each its maximum meaning in the context.* "The purest beauty can only be said to exist where there is no portion of a contemplated total which is not considered part of an organic whole" (Spiller, *Mind of Man*, p. 485). The form must be an adequate embodiment of the content; the content must be an adequate individualization of the form. "Style is good," says Professor Buck, "only when it is precisely correspondent with thought, when it expresses faithfully just the idea involved. Style is bad when it is insufficient to convey the enfolded thought; bad when it obscures that thought with unilluminating words. . . . In style a word that finds its own life shall lose it; but the word that loses its own assertive identity for the thought's sake, the same shall find it." "Ornament construction, never construct ornament," said Richardson, the great architect. "In art," says Goethe, "there appears first a simple impression, then a stage of analysis, which is followed by a return and synthesis of the significant feeling of the whole, which is the æsthetic."

Fine art, from this point of view, is any human production whose form is a relatively adequate embodiment of its content. Adequacy here means *utility*. Art is the idealization of the useful. Anything that is well-adapted to its purpose is in the way to become beautiful. Adequacy means *relevancy*. Beauty is the truth of art and art is the splendor of truth. "Things are not really grasped in their truth unless they are seen in that harmonious relation to the whole which yields complete æsthetic satisfaction" (Mackenzie, *Elements of Metaphysics*, p. 126). Adequacy means cosmic *morality*, not necessarily morality in the conventional sense, but soundness ethically in relation to the abiding destiny of man. Adequacy means social *solidarity*. "We know of no world," writes Goethe, "save one that is related to man, and we may have no art except as an expression of that relation. . . . Each art demands the whole man. The highest attainment of art — the significant — demands all humanity."

Concrete Versus Formal Beauty.

The problem we have been discussing, on one side, is the problem of formal versus concrete elements in beauty. The formal elements, what may be called the principle of order, are found on the side of those sensuous qualities which give a pleasurable emotional consciousness. Such formal elements are color, light and shade, line, symmetry, proportion, tone, timbre, harmony, rhythm, arrangement or composition — any phase of the beautiful object which depends upon the exploiting of sensori-motor habits within pleasurable limits.

The concrete elements in beauty, the principle of the content of the beautiful, or the beauty of ideas, are found on the ideational side, in the meaning or significance of the beautiful object, its associations, its practical, scientific, social, ethical, religious values. The content of the beautiful, in other words, is dependent upon mediation by ideas. It is a question of the logic of the situation or what might be called the dialectic of beauty. The formal elements in art were emphasized by the Greeks almost to the exclusion of the concrete elements. The extreme emphasis on the concrete elements is found in the modern art-theory of the Romanticists.

Mere thought is not concrete enough to arouse the habits and emotions essential to æsthetic pleasure — it must be found in or put into sensuous form. Yet, on the other hand, a mere succession of sense impressions is not in itself beautiful — it must mean something. The mere singing of a bird is not art — certainly not to the bird, and not to man until he becomes civilized. This is the limitation on Darwin's theory of the origin of art in the phenomena of sexual selection. Not until conditions arise which give mating and courting songs ideal significance may the artistic, and in this case the romantic, element be said to have arisen.

This ideal element or 'significance' in extreme instances gives value even to objects lacking in immediate sensuous appeal, but it is only because the sensuous element is vicariously present in the penumbra of the æsthetic image. The sight of some once frequented garden, as Professor Santayana reminds us, may call up an æsthetic emotion, even though the present fact may be indifferent or positively repellent (*Sense of Beauty*, p. 193). The mementos of a lost friend may not in themselves be beautiful. A trifle is often valued for its associations. The beauty of a large proportion of the ornaments in many a drawing-room is associative — as witness the historic interest or symbolic meaning of heirlooms, books, pictures, curios, antiques, etc.

Man is not beautiful, says Lipps, because of his form.

The human form is beautiful because it is to us the carrier of human life. The orange is the most beautiful of fruits, says Fechner, because of the romantic associations with the South which it calls up. "When we behold a beautiful form," says Brown, "all the images suggested by it, live in like manner in it." And long ago Alison wrote: "Wherever the appearances of the material world are expressive to us of qualities we love or admire; wherever, from our education, our connections, our habits, or our pursuits, its qualities are associated in our minds with affecting or interesting emotion, there the pleasures of beauty or of sublimity are felt, or at least are capable of being felt. Our minds, instead of being governed by the character of external objects, are enabled to bestow upon them a character which does not belong to them; and even with the rudest, or the commonest appearances of nature, to connect feelings of a nobler or a more interesting kind, than any that the mere influences of matter can ever convey."

But in the highest art the sensuous is controlled by the ideal element. The relation of impulse to ideal is the same here as in ethics. Sensuous emotion is impulsive, uncontrolled emotion. Ideal emotion is controlled, defined and articulated by significance or meaning, by the ideal element. It is possible to get satisfaction in either way, but the satisfaction that comes from ideal emotion is more permanent, generic, universal: it alone is æsthetic. The highest art is typical, representative, as well as sensuous: it does not stamp out the sensuous element but utilizes it to enrich an ideal social, ethical, religious, industrial, scientific, philosophic content or meaning. The intellectualists are right in insisting that the sense element alone can never be the basis of the æsthetic consciousness, because of the lack of permanency and ideal significance in the lower sense pleasures. But the sensationalists are right in insisting that the most abstract thought experience is ultimately grounded on a sensational basis. The truth is that the æsthetic quality lies not in certain experiences rather than others, but in such a ratio or proportion of these sensuous and ideal elements as gives the maximum of ideal mediation combined with the maximum of sensuous pleasurable emotion.

The great work of art is always an idealization. But a mere ideal is a contradiction in terms. The ideal is the projected actual. 'An idea is a tentative view of the fact,' says Professor Dewey. The ideal arises when there is inadequacy of the real, when there is a problem. Hence the principle which should govern is to see that the ideal is a natural outgrowth of the *real* while yet in a sense transcending it, just because it itself represents the reconstruction of the real. The only difference between the function of ideas in science and philosophy and ideals in art is that in the latter case we put the limitation upon them that they must be pleasurable. We insist that the function of art is to inspire, not to instruct; but this, in the last analysis, means simply that its instruction shall be given in pleasing forms.

Great artists have always insisted that the aim of art is "to produce a representation of nature in which the essential characters enjoy an absolute sovereignty" (Taine). "Conception, fundamental brain-work — that is what makes the difference in all art" (Thomas Davidson, *History of Education*, p. 44). This is as true of art as it is of science. Art cannot get along without a content of great meanings if there is to be great art. Its insistence on a sensuous embodiment is not grossness nor sensuality: it means rather formativeness, inspirational character in relation to human personality, which does not understand much truth until it appeals to the 'whole man.' It represents the logical, scientific and other non-æsthetic meanings in the most adequate form compatible with their giving successful and therefore pleasurable control of experience. The artistic insight is the most adequate embodiment of the intellectual, the practical and the moral in so far as these stand for control. It is when they are imposed as abstractions upon the art product that they are felt as irrelevant. This is the true mysticism of art: not that it glimpses meanings which are beyond science and philosophy, but that the meanings it does glimpse are such as may be brought home to man's affective-volitional as well as to his intellectual nature.

Where this control by the highest intellectual or moral ideal is absent, beauty itself suffers, just because the æsthetic moment in such cases is not mediated to the furthest point compatible with inward reinforcement and repose. This is the platonic teaching — "To excite passions idly is to enervate the soul" (Santayana, *Reason in Art*, p. 176). "When moralists deprecate passion and contrast it with reason, they do so, if they are themselves rational, only because passion is so often 'guilty,' because it works havoc so often in the surrounding world and leaves, among other ruins, 'a heart high-sorrowful and cloyed.' Were there no danger of such after-effects within and without the sufferer, no passion would be reprehensible. Nature is innocent, and so are all her impulses and moods when taken in isolation; it is only on meeting that they blush" (p. 168). "And so when by yielding to a blind passion for beauty we derange theory and practice, we cut ourselves off from those beauties which alone could have satisfied our passion" (p. 186).

There is a Meaning: But that Meaning is Elusive.

There must be a meaning, as Professor Fite maintains, but that meaning must be elusive (Psy. Rev., March, 1901, p. 140). To say that there must be a meaning, signifies that some habit or habit-system is brought to consciousness in the form of imagery or apperception-systems which, on the whole, are familiar and agreeable to contemplate. But the mere presence of a familiar content which because of its familiarity tends to be agreeable, does not in itself constitute it an æsthetic experience. Too great familiarity without diversity or stimulation, results in monotony and automatism. There must be a meaning, but that meaning must be elusive or stimulating enough to function the habits as images within pleasurable limits. Lessing calls "for an incompletion of detail in the artist's work, that the imagination may have room in which to work its expansive effects" (cf. Marshall, *Æsthetic Principles*, p. 115). "Those things in nature and humanity are most beautiful which most of all suggest what transcends themselves" (Knight, *Philosophy of the Beautiful*, II., p. 15). This is well illustrated in poetry: "In all its types — whether lyric, epic, dramatic, comic, elegaic, satire, or descriptive — poetry begins with a representation either of what once was, or of what now is; but, being a new embodiment of reality, it invariably tends towards what is as yet unembodied, while it pursues the ideal through the maze, the imperfection, and the discords of the actual" (p. 110).

The adequacy of the form to the content, the insistence on a meaning, represents the factor of habit, pleasure, repose, in the æsthetic experience. The milk-maid's stool is beautiful not because of its adequacy as a stool, its meaning in itself, but because of its associations. A throne may be beautiful in addition because of the fine carving on it. A bench in the class-room or a common chair with no sentimental associations lacks the æsthetic quality just because of the too complete adequacy of the meaning, the lack of the element of elusiveness. In other words, the formal element in art, the sensuous and emotional element, is wholly dependent upon its relation to the concrete contentual element in producing the æsthetic moment.

The elusiveness represents the factor of relative tension, excitement, stimulation, diversity, variety, the relativity of the adequacy. The æsthetic quality of the milk-maid's stool is dependent upon the suggested associations. The elusiveness, the stimulating factor, in the case of the throne, is found in the rich decorations. The lack of elusiveness, the perfect obviousness of the meaning in the case of the common chair, accounts for its not entering the æsthetic sphere. The office of the imagination, as Alexander says, is to liberate the spirit from habitual and communal thinking" (*Poetry and the Individual*, p. 113). The ordinary photograph lacks this quality of elusiveness except to perhaps the few persons who know the person represented well enough to supply it vicariously; the Mona Liza and Whistler's portrait of his mother are a perennial delight.

The Elusiveness of Modern Art.

Elusiveness is everywhere essential to art. It is found in ancient as well as in modern æsthetic products. But there is elusiveness in modern art in a sense and of a kind unknown to earlier times. Along with the closer synthesis of man with nature which modern science has made possible has come the liberation of the imagination and the emancipation of the individual which have given us romanticism.

Greek drama depicted the completed act. Modern drama attempts to depict the activity in process, a line of action, the movement of the plot, the solution of the problem actually taking place before the eye — in terms of its psychological motivation. In a general sense, it may be said that the Greek artists were not artists but artisans, whose ideal was to reproduce certain fixed ideas of Hellenic civilization — Zeus, Minerva, Pallas Athene, etc. Modern art, on the contrary, is striving toward the production in sensuous form of the transcendent ideas of change, life, growth, development, evolution. We are inventing all sorts of devices for representing movement, activity, function. We think kinetoscopically. The aim of the Greek artist was the reproduction of ideas familiar to his audience. The modern artist boasts that he is not understood, that he represents an idea which transcends his audience. The content of the former is universal, typical, generic; but fixed, static, and, logically speaking, dead. The content of the latter is individual, moving, dynamic and functional. The Greeks represented their Gods as arrested in a state of immortal youth. The God of the modern is a sumptuous Interrogation-Point.

That is, in the case of the ancient artist, the solution is given *with* the problem; in the case of the modern artist the problem is given without the solution or *in process* of solution. The one is concerned with perfecting an already accepted form; the other is interested chiefly in the reformulation. Greek art was a closed circle; modern art is a spiral curve. In modern art we have problem after problem presented with, if any, only tentative solutions. This is the leading characteristic of most of the powerful modern novels. They suggest future vistas of possible solutions, working hypotheses only, rather than any ultimate interpretation or final evaluation. This is the inevitable result of the influence of the modern emphasis upon the psychical and personal and individual element in experience. Idealism and romanticism represent the influence of psychology upon art. Modern industry, science and philosophy, as well as its art, are becoming psychologized in this sense. Art is becoming more self-conscious in its method, with the result that it gives you a drift or an intent instead of a finished product. You catch the artist's soul still struggling in the toils of his great passion rather than the post-reflective contemplation of it from the vantage of its triumph or failure.

On the other hand, just this elusiveness in an extreme form is the defect of modern art, where it is not controlled by a great insight. Most of our impressionistic art does not go beyond the statement of the problems with which our modern industry and science are engaged — a statement falling often into the realistic fallacy of seeking to simply transcribe the facts. It has not caught the spirit of the technique of modern science with its elaborate system of controlled hypothesizing and experimental gambling with concepts. Mystery, as someone has said, is proportionate, not to ignorance, but to knowledge. Fear may spring from ignorance, but growing knowledge deepens reverence and adoration. Science is transforming nature into a work of art, and in the method of science must be found, not only the meaning which is at the heart of all beauty, but also that elusiveness which gives it a propitious form. Who will write the epic of evolution, the lyric of the hyper-space, the drama of the subliminal uprush, the comedy of the Absolute, a sonnet to radioactivity, an elegy on sex? In what monumental work of art will we embody our ideals of democracy and the superman and the new woman?[1]

[1] The MS. of this article was received October 20, 1908. — Ed.

ANNOUNCEMENTS.

The May issue of the Review will be a 'Darwin' number, devoted to a symposium on the Influence of Charles Darwin on the Mental and Moral Sciences and Philosophy.

The Review announces the beginning of the publication of a series of bound volumes to be known as the 'Library of Genetic Science and Philosophy.' It will include researches and treatises on topics relating to the genetic sciences generally, from organic evolution to genetic logic and philosophy. The first volume, now in press, is a study of 'Genetic Ethics' by Professor A. E. Davies, of the Ohio State University. (Review Publishing Co., Baltimore.)

N. S. Vol. XVI. No. 3. May, 1909.

The Psychological Review.

THE INFLUENCE OF CHARLES DARWIN UPON HISTORICAL AND POLITICAL THOUGHT.

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The preliminary work which needed to be done before people could apply Darwinian methods in history was not so great as the corresponding work which had to be done in biology. When Darwin presented the doctrine of evolution by natural selection to the zoölogists and botanists, he had to deal with men who for the most part did not believe in evolution of any kind. They had been brought up to regard different species as having an independent existence. The idea of development of types by slow processes of change was something new and foreign to their minds. In history or in politics the case was different. These sciences are based on a fundamental assumption of an evolutionary doctrine. If different historical events were independent of one another there would be no sense in writing history at all. All serious investigators in this field, from Thucydides and Aristotle down to the present time, have sought either to develop the details of this orderly and gradual evolution or to lay down the principles of its operation. The man who to-day reads the Politics of Aristotle for the first time will be struck by the prevalence of methods of thought which many biologists suppose Darwin to have invented. And the same idea of evolution thus used by Aristotle has been applied in varying forms by all who sought to develop a philosophy of history — by Hegel and his followers in Germany or by men of the type of Henry Thomas Buckle in England.

Not only was the idea of evolution thus familiar to the historians; the idea of natural selection was also prominent in the minds of many of them. The whole doctrine of John Stuart Mill concerning liberty was founded upon reliance on a process of natural selection. Look for your hero in all possible directions, he said, and you get the best chance of finding him. The issue between Mill and Carlyle reminds one of the controversies between Darwinian and anti-Darwinian in the field of biology. Carlyle believed in the special creation of a number of individual heroes; Mill, together with nearly all scientifically trained historians, believed in the evolution of heroes by natural selection.

The conception of economic or political conflict as a means of determining the survival of the fittest was seen perhaps even more conspicuously in Malthus's theory of population — a theory which Darwin himself regarded as having in some respects foreshadowed his own work. Malthus made it a fundamental basis of his doctrine that population tended to outrun subsistence; that the struggle for existence was a constant process of elimination of the weak; and that any attempt to interfere with this process resulted rather in the deterioration than in the improvement of the peoples that it was designed to benefit.

If then the idea of evolution had been a fundamental one in historical and political science for more than two thousand years, and if the idea of elimination by natural selection was by no means unfamiliar to political thinkers, what was there left for Darwin to do in this field?

He found at least two things to do. In the first place, he showed how natural selection was a means of developing, not only individuals of superior ability or intelligence, but types of superior adaptation to their surroundings; and he taught us further to regard this adaptation of the type to its surroundings as the thing which gave it its right to exist.

The first of these points is well illustrated by the history of the Malthusian theory before and after Darwin. Malthus and almost all the Malthusians before the time of Darwin talked of an actual struggle for food between different individuals. They thought that there was not enough food to go round, and that this fact was a direct means of keeping workers up to a certain standard of efficiency and prudence by the direct elimination of the weak. To-day we see that the result is far more indirect than this. There is, in civilized communities at least, no habitual scarcity of food. This has been avoided by the development of certain institutions like the family and private property and certain motives which go with those institutions which prevent the scarcity that would otherwise exist. A generation ago the critics of Malthus thought that the non-existence of the scarcity disproved the Malthusian theory. To-day we see that it confirms it. It shows that the type has adapted itself to its environment.

It is the institution even more than the man that has been marked out for survival by the process of natural selection. We have known for generations how elimination affected the development of individuals. It was Darwin who taught us to account in this way for the growth of species — in history as well as in biology. And in thus accounting for the origin and growth of institutions, he furnished for the first time an objective justification of the ethical standards and motives by which those institutions were upheld. Every prominent political thinker before Darwin, with the one notable exception of Edmund Burke, referred historical events to some preconceived ethical standard of his own, and judged them to be good or bad according as they conformed to his preconceived ideas. This is true even of a man like John Stuart Mill. He had great natural love of liberty, and was essentially tolerant in his disposition. Yet one can feel in all his work the underlying assumption that the chief reason for approving of liberty is its effect in developing the type of character represented by the liberal and tolerant Englishman of the nineteenth century.

This attitude of mind was a great help to Mill in arranging a coherent system of political economy; and as long as he addressed an audience whose general views and general standards were like his own, it enabled him to appeal to them with great force. But the instant he was brought face to face with a protectionist like Carey or a socialist like Lassalle, what had previously been an element of strength became an element of weakness. There was no common ground from which to reason, and no means of finding any. It was Darwin who furnished the common ground. It was Darwin who gave the judgments of historians and of political thinkers the possibility of reaching objective results which were previously unattainable. You like one kind of man and one kind of institution; I like another kind of man or another kind of institution. Very well; let us set to work to discover which, in the long run, is going to prevail over the other. That which will prevail in the long run must be right. This is for the historian the center and gist of Darwinism. We all assumed that orderly evolution existed; we most of us understood a good deal about a process of natural selection which was going on. But none of us until Darwin came had learned to take the results of natural selection as a standard; to make the fact of permanence the test of the right to remain; to assume the view of the philosophical pragmatist in dealing with the problems that came before us.

Of course this is a doctrine that needs to be applied with great care. The frank acceptance of survival as a test of right is attended with the great danger that we may take too short periods of history under our observation, and may think that an idea or an institution has won the race when it is riding most hurriedly toward its downfall. But in spite of all these dangers, the necessity of applying the survival test compels the man who is naturally dogmatic to be somewhat less so, and helps the man who is naturally objective to be somewhat more so. It is a restraint upon the man who does not want to have to prove his points; it is an assistance to the man who does.

This change in modes of thought and criteria of ethics did not come suddenly. It was far easier for popular writers to seize upon certain results of Darwin's thinking and try to apply them to history in the form of rhetorical analogies than it was to get at the Darwinian habit of mind in dealing with historical problems in general. Herbert Spencer's writings furnish a very marked instance of this error. Spencer's style was so felicitous and his works were so widely read that he did a good deal to retard the application of the really important results of Darwin's work to political thinking. Spencer and his followers made much of the conception of society as an organism; but they overlooked the fact that historians had been treating society as an organism for more than two thousand years. In the belief that they had occupied a new field, they permitted themselves to employ a number of loose analogies, in total ignorance of the fact that competent observers had already gone over much of the ground by scientific methods. Historians had been proving which forms of social life *did* survive; and this proof, defective or uncertain as it was in many instances, was yet better than the guesses of the Spencerian, on the basis of remote analogy, as to which forms of social life *were going to* survive. When Spencer pronounced evolution good or bad according as it did or did not 'proceed from an incoherent indefinite homogeneity to a coherent definite heterogeneity,' he was writing down in large letters the fact that he was born a good while before *The Origin of Species* had appeared. He had put on a few of the external attributes of the modern biologist; that was all. The hands were the hands of Esau, but the voice was the voice of Jacob. Or, to take an instance from a different field, when W. K. Clifford, in his now almost forgotten *Lectures and Essays*, proclaimed the right and duty of the unlimited exercise of private judgment, and called down anathemas on the head of every man who wished to exercise his own private judgment to the extent of differing from Mr. W. K. Clifford in this particular, he simply showed that he lived too early to have felt the full effect of *The Origin of Species* in leading people to substitute objective criteria for subjective ones.

But it would perhaps be toward the purpose to give instances of writers who were influenced by Darwin, instead of those who were not.

Among English economists, the man who was quickest to feel the force of the new movement was Walter Bagehot. Bagehot's Darwinian ideas are popularly known from his *Physics and Politics* — an interesting and often exceedingly brilliant set of conjectures regarding the operation of survival in prehistoric periods. But Bagehot's main work and main interest were always in the nearer parts of history, and particularly economic history, rather than the remoter parts. He it was who, in an age when England still followed John Stuart Mill blindly, first questioned the general admissibility of Mill's assumptions. In these twentieth century days, when competition is regarded, not as an axiom or postulate of political economy, but simply as an important incident in its development, it is difficult for us to understand the courage that was involved forty years ago in publishing two critical essays in which competition was regarded, not as a standard to which all things must conform, but as one among several alternative phases or modes of social service, whose relative claims were to be investigated and relative merits judged by their applicability to given conditions. In this mental attitude the English writer who has followed Bagehot most closely is W. J. Ashley, whose *English Economic History* may be taken as furnishing a clear exemplification of Darwin's influence upon the methods of modern economic thought.

Meantime a German investigator in economics, Adolf Wagner, of Berlin, had been taking up Darwinian methods on a larger scale and applying them with conspicuous success. Wagner may be said to have developed his Darwinism at the opposite end from Bagehot. Bagehot had been brought up in the methods of the deductive school of economics, and was impressed with their inapplicability; Wagner had been accustomed to the methods of the historical school of economics, and was impressed with their inconclusiveness. While Bagehot wanted to make his analysis broad enough to fit different kinds of facts, Wagner was concerned to make his synthesis coherent enough to bring him to some positive proofs and conclusions. Wagner's treatment of the theory of property right is a good example of his philosophical method. He rejects both the crude juristic theory that property right is based upon occupancy and the equally crude philosophic theory that it ought to be based on labor. Society has established property right because it has shown itself the best motive — in fact, apparently the necessary motive — in order to get industry well and efficiently managed. It is only by the application of this last theory that you can make a connection between what is and what ought to be; between your history and economics on the one hand and your law and ethics on the other. If the philosopher says that property ought to be based upon labor, the jurist can laugh at him. If the jurist says that property is based upon occupancy or upon the constitution of society, the philosopher can say that the occupants are bad men and that the sooner society changes its constitution the better. But if property is an institution which has survived while other forms of social organization have failed, because property preserves nations and socialism destroys them, then socialism is disproved by the logic of events — the logic that Darwin has taught us to apply to problems of this kind.

It is, however, not so much in its special applications that the Darwinian theory has affected modern political science as in the general habit of mind which it has fostered and cultivated. It has not led to many great discoveries which can be set apart from the general run of facts previously known; but it has led to changes in the methods of judgment which enable us to understand and use all historical facts in a more objective way.

A few years ago, when Dr. Jowett was master of Balliol, there was a discussion concerning two men who had attained high position at an early age. One of them had become a bishop, the other a judge; and the conversation turned on the respective merits of the two careers. One of the dons said, "I prefer the bishop. The judge can only say, 'You be hanged'; the bishop can say, 'You be damned.'" "Yes," said Dr. Jowett, sententiously, "but when the judge says 'You be hanged' *you are hanged.*" The influence of Charles Darwin on historical and political thought may be summed up by saying that he has made our historians cease to aspire to be bishops and content themselves with the more modest but also more effective position of judges. For broad principles of judgment which they could not apply effectively they have substituted narrower but clearer ones whose application can be made evident to their fellow men.

I have spoken of this attitude of mind as having been foreshadowed in the works of Edmund Burke. To him, as to the modern thinker, human history was the record of a process of elimination and survival. To him political institutions and political ideas had grown up as a means of preserving the race that held them. And to him also it was unwarrantable to attempt to tear down on *a priori* grounds beliefs and methods that had preserved the race that held them, unless you were able to substitute something practically better in their place. A thing did not seem to him correct which was logically good and practically bad. He suspected a defect in the logic. Was he right or wrong? In the first half of the nineteenth century the majority of men would have said that he was from a theoretical standpoint wrong. They admired his insight into the political conditions of his day, but they would have none of his theories. To-day the world feels a little less sure about some of his individual judgments than it did at the time when they were uttered; but as a matter of theory it has accepted his method as a sound one. It is in general prepared to make survival a test of right.

This is Darwin's contribution to political science; and the completeness with which this contribution is accepted is shown by the sudden cessation of public interest in books which do not apply or accept that test. Students of politics no longer read either Hegel or Comte. Buckle's *History of Civilization*, which in the years immediately following its appearance had a greater success than Darwin's *Origin of Species*, is now known only to a few specialists in literary history. Mill's *Principles of Political Economy* is valued for its contributions to the theory of banking; but as a work of political philosophy it has lost the place which its author, modest man though he was, confidently claimed for it.

We can get a curious idea of the kind of change which has taken place by comparing two works which are closely akin, by two men who were closely associated — Mill on *Liberty* and Morley on *Compromise*. The two writers deal with nearly the same topic. They approach it with nearly the same prepossessions. They arrive at almost exactly the same practical conclusions. Yet Morley is read to-day, and Mill, speaking broadly, is not. Why? Because Mill is constantly referring things to a subjective standard, and Morley to an objective one. Mill's whole argument is essentially an *argumentum ad hominem*, even when it takes the form of an appeal to experience; Morley's an appeal to experience, even when it takes the form of an *argumentum ad hominem*.

We may not be any more correct in our political reasoning than our fathers. I dare say that when the world contrasts the political philosophy of to-day with that of a generation or two ago it will reprove us for our crude judgments and for the irreverence with which we have cast aside work that was better than our own because it did not reach its results by our methods. But we are at least trying as no previous generation has tried to get *objective* standards on which different men and different ages can agree; and for this effort, and for whatever measure of success it has attained, we may thank Charles Darwin.

THE INFLUENCE OF DARWIN ON PSYCHOLOGY.

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I.

Darwinism has never been a really vital issue in psychology. Occasionally a theologian or a naturalist has inveighed against the Darwinian theory of mental evolution, but the psychologists as such have rarely uttered a protest. In view of the storm of vituperative scientific criticism precipitated by the publication of the *Origin of Species*, this fact is distinctly significant. Indeed, so much a matter of course have the essential Darwinian conceptions become, that one is in danger of assuming fallaciously that Darwinism has no important bearing on psychology. How Darwin's radical theories succeeded in gaining such easy access to the psychological sanctuary is a matter of distinct interest upon which a few speculative comments may be made.

It must be borne in mind, then, that Darwin's most revolutionary ideas on mental evolution did not appear until the publication of the *Descent of Man* in 1871. This was nearly thirty years after Weber's epoch-making experiments on sensations, almost a score of years after the appearance of Lotze's medical psychology, sixteen years after the issuance of Spencer's evolutionary psychology and Bain's work on the *Senses and Intellect*, with its excellent presentation of the facts of nervous organization, eleven years after Fechner's publication of the *Psychophysik*, nine years after the first edition of Helmholtz's *Sensations of Tone* and seven years after his *Physiological Optics*. It was only three years in advance of the first edition of Wundt's *Physiological Psychology*. There had thus been rapidly growing during the preceding thirty years a disposition to view mental life as intimately connected with physiological processes, as capable of investigation along experimental and physiological lines, and finally as susceptible of explanation in an evolutionary manner. Moreover, by the time the *Descent* *of Man* was published the weight of scientific authority, so heavily against Darwin at the time of the publication of the *Origin of Species* in 1859, had swung unmistakably to his support.

Another circumstance of probably more than negligible moment is found in the fact that the major interest of many psychologists has always been in the more narrowly analytical problems of mind. On these problems Darwinism has had little immediate bearing and has exercised only the smallest fructifying influence. Its contentions have seemed, therefore, to demand no very vigorous partisanship either one way or the other.

The effect of certain philosophic tendencies ought, no doubt, to be added to this brief survey of contributory influences, but the considerations already offered are probably sufficient to indicate in part, at least, why the publication of the doctrines of mental evolution expounded in the *Descent of Man* occasioned so little psychological flutter and in many quarters awakened so warm and enthusiastic a welcome. They also serve to explain why it is so difficult to assign with confidence the precise contribution of Darwin's thought to current conditions in psychology. Many convergent forces have been at work and the independent effects of each are hardly to be discriminated. Nevertheless, it is clear that Darwinism exercises a very potent influence in psychology, not alone as regards general standpoint and method, but also as regards certain specific doctrines.

In the matter of general method we may certainly attribute to Darwinism the larger part of the responsibility for the change which has brought into prominence functional and genetic psychology (including animal psychology), in distinction from the older and more conventional analytic psychology. Here again many influences have contributed to the final outcome, but it is fatuous to suppose that the genetic movement in psychology could have attained its present imposing dimensions had it not been for the inspiration of Darwin's achievements. The analytical methods will no doubt always retain a certain field of usefulness, and an indispensable one at that, but our larger and more significant generalizations, our more practically important forms of control over mental life are going to issue from the pursuit of methods in which growth, development and the influence of environment, both social and physical, will be the cardinal factors, methods which will in other words apply Darwinian principles with, let us hope, Darwin's tireless patience.

Darwin's more specific contributions to psychology may be grouped under three main headings: (1) his doctrine of the evolution of instinct and the part played by intelligence in the process; (2) the evolution of mind from the lowest animal to the highest man; and (3) the expressions of emotion. This is the chronological order in which these topics were given publicity by Darwin and we may properly adopt it in discussing the problems involved.

II.

The solution of the first issue, *i. e.*, the genesis of instinct and the part played by intelligence in such genesis, bears primarily perhaps on the field of animal psychology, but it certainly has a very definite interest for human psychology as well. At first blush it might seem that instinct is altogether a matter of muscular activities and neural mechanisms and that mentality has little or nothing to do with it. But a closer inspection of the actual manifestations of instinct serves to disabuse one's mind of that impression. Not only are human instincts honeycombed with psychic influences, but even animal instincts show themselves variable and adaptive to specific situations in ways which hardly permit any other interpretation than that of conscious adjustment. Take the imperious mating instinct as an instance. Among birds of many species there is every evidence that despite the impelling force of impulse, the female exercises a very definite choice in which to all appearances psychical impressions are potent. But the question still remains whether intelligence is a true cause in the *production* of instinctive acts, or whether it merely comes in occasionally to modify them. Herbert Spencer is cited with questionable justice as representing one extreme opinion in this matter.[1] It is alleged that he holds that instinct is simply compound reflex action and that

[1] Cf. Romanes, *Mental Evolution in Animals*, p. 256. I find it difficult to be certain from a reading of Spencer's own statement just what position he really holds on this matter.

it is always the precursor of intelligence. This is clearly the view of many modern physiologists and naturalists, of whom Bethe and Loeb are illustrations. From this standpoint consciousness is not essential to the formation of instinct. Among English and American writers G. H. Lewes and Cope represent the other extreme, maintaining that all instincts are originally intelligent conscious acts, from which conscious control has largely or wholly disappeared. Some authorities like Romanes have held that consciousness is at all times operative in instinct and that it is precisely the presence of consciousness which distinguishes instincts from mere reflexes. This general view held with sundry modifications by numerous writers, among others Wundt, is known as the 'lapsed intelligence' theory.

Darwin[1] himself seems to have been less interested in the question as to whether mind is always present in instinctive reactions than in the question of its relation to the origin of instinct. His view seems to have been that instincts are in part due to the inheritance of useful habits consciously acquired, and in part due to the effects of natural selection operating on chance variations in conduct. Of the two he regards natural selection as the more important, because many instincts cannot have been inherited habits (*e. g.*, those of neuter insects), and because the selection of slight variations in action through many generations seems to him plausible by reason of the conclusive evidence of a similar process in the evolution of structures.

Against the natural selection argument, as it pertains to the supposed preservation of incremental variations of a useful sort, it has been urged that in not a few instincts this is an impossible assumption, because the whole value of the instinct depends on the appropriate execution of each step in a long series of acts, each one of which alone, and any group of which apart from the others, is useless. Natural selection could only furnish an adequate explanation provided the whole series of complex acts sprang into existence simultaneously. To suppose that this occurs is to assume the miraculous. Stated abstractedly this

[1] Cf. Darwin, *Origin of Species*, Ch. VIII.; Romanes, *Mental Evolution in Animals*, Appendix.

criticism appears forceful, but in view of our profound ignorance of the stages through which complex instincts have actually passed, it seems wise to be conservative in estimating the significance of the criticism.

It will be noted also that Darwin speaks quite explicitly of his belief that acquired habits are transmitted. The doubt which attaches to this doctrine in the minds of competent contemporary zoölogists is well known. Darwin quotes as illustrating his point the alleged acquirement of fear of man by birds in certain of the oceanic islands remote from the mainland subsequent to the coming of men and the pursuit of hunting. Certain cases of alleged transmission of characteristics as a result of mental training among dogs appear also to have weighed heavily in his mind.

If such acquirements *are* transmitted by heredity, then it must be admitted that this factor, together with the natural selection of such instinctive variations as arise naturally and after the manner of structural variations, would no doubt largely account for the phenomena with which we are familiar. But as we have just pointed out, difficulties beset both parts of this program.

A compromise view which is put forward with the joint authority of Morgan, Osborn and Baldwin,[1] under the title 'organic selection,' maintains that consciously acquired habits are probably not directly transmitted, but that consciousness plays an indispensable part in the drama by enabling successive generations of creatures to accommodate themselves to the vicissitudes of life while the slow changes are taking place which finally issue in the completed instinct. Not only is consciousness operative in this way, but in all the higher forms of animal life it is held that conscious imitative activities also play a part, and with man a dominant part, in setting the racial pattern. Natural selection serves to lop off the feeble and incompetent, both among individuals and groups, while all this process is going forward, but the successful issue is fundamentally dependent on conscious reactions during the critical formative stages.

In the midst of uncertainty and speculative ingenuity such

[1] Cf. Baldwin, *Development and Evolution*, especially Appendices A and B.

as this, many minds will look with hope and a certain relief on the efforts of a group of zoölogists and physiologists — illustrated by Jennings and Loeb — who have made persistent and in no small measure successful attempts to modify instinctive behavior by experimental methods, thus securing at once some rudimentary insight into the mechanics of the instincts, instead of waiting for nature to reveal her secrets at her pleasure. In the lower organisms where such experimental control is most feasible, already the dependence of certain forms of instinctive behavior on conditions of temperature, light and oxygenation has been demonstrated and it hardly seems unduly optimistic to hope that through such means we shall ere long be able to substitute for speculative theories on the *modus operandi* of instinctive behavior something more nearly resembling knowledge. At present we can only say that we know with reasonable certainty that many instinctive acts are accompanied by consciousness, that practically all of them are variable within limits, that some of them appear to be modified by conscious forces, that possibly consciousness has played a part in the formation of some of them as it seemingly plays a part in their actual workings, that natural selection would certainly account for many instincts and perhaps for all.

We come now to consider Darwin's view of mental evolution.

III.

Darwin[1] held that the mind of civilized man is a direct outgrowth of the animal mind. He maintained that from the lowest animal upward we find evidence of mental processes which increase in range and power, but do not change in kind, until we meet their most complete expressions in man. In man himself he finds again no evidence of aught but continuity of development from the lowest savage to the highest genius.

Darwin not only teaches the continuity of mental evolution from the lowest to the highest forms of animal life, he also urges the value of mental factors in the operation of both natural and sexual selection. Men and animals alike that were alert and intelligent in their adaptive acts would enjoy a larger chance of

[1] Cf. Darwin, *The Descent of Man*, Chapters III. to V.

life and leave behind them a more numerous posterity. In those orders of animals where the female exercises selective control in the choice of a mate, he urges, as has already been indicated, that psychical factors enter in an important degree to determine the feminine preference.

His survey of mental characteristics on which these doctrines are based is somewhat naïve. The psychic qualities which he cites as a foundation for his statements are as follows: sensations, pleasure, pain, passions, emotions (terror, suspicion, fear, anger, courage, timidity, love, jealousy, emulation, sense of humor, wonder, curiosity), imitation, attention, memory, imagination (whose presence in animals he regards as proved by behavior indicating dreams), and reason, which in animals, he says, is closely allied with instinct. These categories are all taken quite simply and with no special effort to indicate precisely what may be meant by them. He contents himself by citing illustrations of animal behavior, which seem to him to indicate the presence of these several mental attributes.

He undertakes to fortify his general position by a refutation of the several stock arguments commonly advanced to support belief in the radical distinction between animals and man. Of these we may pause to mention only a few.

He meets the assertion that animals make no use of tools by citing the case of the chimpanzee who is said to use stones to open nuts, and by the case of the elephant who uses branches to protect himself from the assaults of flies. He might have cited many other similar cases, but it is to be observed that he makes no very satisfactory attempt to meet the further points that animals do not fashion utensils and that they do not use fire. For the present generation, however, this type of consideration has somewhat lost interest. He believes the opinion that animals do not form concepts and that they are incapable of making abstractions is not well founded. He cites as an instance of the appreciation by animals of something akin to an abstract idea, the attitude which a dog will assume in response to the exciting question, "Where is it?" The simple-mindedness of this conclusion must inevitably furnish amusement to the sophisticated animal psychologists of the present day. On the matter of language he occupies a position distinctly favorable to the possession of rudimentary language forms by animals. He cites the fact that many animals have calls expressive of emotion, and these calls he regards as essentially linguistic. He also mentions the use by parrots of significant words as a case demonstrating his contentions. Again, the sense of beauty has been held to be a purely human attribute. But this view Darwin feels is definitely controverted by the fondness which certain animals display, especially birds, for colors and plumage. The possession of conscience and the belief in God have frequently been urged as the sole possessions of humanity. To this assertion Darwin replies that the belief in God is not universal among human beings and hence not generically human, and the actions of many animals, notably dogs, indicate something closely akin to the feelings of conscience. To the contemporary psychologist all this sounds highly archaic and scientifically anachronistic and so no doubt it is. But in view of Darwin's extensive innocence of psychology, it represents, as he marshals his facts, an amazing range of original observation and a most intrepid mind.

In the last analysis, despite the statements of the preceding paragraph, Darwin regards the development of conscience, or the moral sense, as by far the most important practical distinction of man from the animals. He says, however, that any animal endowed with well-marked social instincts, such as the parental or filial affections, would develop man's conscience as soon as he developed man's intellectual capacity, or even approximated it. The social and gregarious habits of many animals obviously furnish an excellent point of departure for such a development. Moreover, sympathy, which plays an important part in all moral evolution, seems to be manifested by certain animals. There is therefore no evidence anywhere for radical differences between man and the animals.

It may be of interest to remark certain typical divergences from this general position in which, however, Darwin has found not a few loyal followers. Indeed, at the present time it is undoubtedly the case that most psychologists share Darwin's main convictions as to the continuity of mental evolution from animal to man, less perhaps as a result of careful scrutiny of the facts than as a consequence of a powerful drift from every direction toward the belief in a common origin for human and animal characteristics. We feel more comfortable nowadays in a world where simple and uniform rules obtain.

Probably the most persistent and most substantial point of dissent from Darwin is represented by writers who like Mivart[1] hold that although men and animals have certain forms of conscious life in common, for instance, sentience and memory, man alone can frame true concepts, and man alone can use true signs, can create and use language. Only man has ideas. Whereas we find essential continuity from the lowest to the highest of *bodily* forms, in *mental* processes we meet a real break, separating the human and spiritual, from the merely sentient and brute.

This type of view has always commended itself to a certain stripe of religious belief, because of its seeming provision for a somewhat super-naturalistic element in man, and its protest against regarding him, or at least his ancestry, as substantially on a level with the beasts of the field.

Moreover, it can summon to its support not a little apparently valid evidence wherein alleged instances of the animal use of language and signs are shown capable of another and more rudimentary interpretation. We are, of course, unable to intrude upon the inner workings of the animal consciousness, and it must be confessed that in so far as we judge by external conduct, few, if any, of the instances adduced to prove the formation by animals of concepts or of language really furnish unequivocal evidence of the thing to be proved. Meantime, it should be clearly recognized that this position, as advanced by Mivart at least, does not rest for its severance of man from the animals simply on the classical contention that he has a soul while they possess only minds. It is a distinction in the field of mind itself, which is here emphasized, an ascription to man, as his unique possession, of capacities which constitute the higher stages of cognitive activities.

Another divergent line is represented by the celebrated

[1] Cf. Mivart, *The Origin of Human Reason.*

naturalist Wallace,[1] who shares with Darwin a part of the credit for that revolution of opinion in the scientific world which generally is characterized with Darwin's name. Wallace is apparently willing to grant as a mere hypothesis that man's mind has developed *pari passu* with man's body, but he absolutely refuses to admit that natural selection could have brought this result to pass. He calls attention to three great familiar instances of alleged discontinuity in nature as suggesting that we should be scientifically hospitable to the idea of discontinuity. First, there is the breach between the organic and the inorganic, a breach which seems daily to shrink, but which has not yet been over-spanned. Then there is the equally marvellous break between the organic and the sentient, the conscious. And finally there is the break between mere sentience and rational intelligence — the distinction upon which Mivart dwelt so insistently.

Wallace cannot seriously call in question the possibility that natural selection should affect such mental qualities as quickness of eye and ear, accuracy of memory of former dangers and the like. It is the higher more definitely human qualities which apparently afford him foundation for his position. For example, what he calls the 'mathematical faculty' and the 'faculty for music' seem to him too remote from the life-subserving functions to have had any survival value, and unless they have such value, his position must be granted as having force against natural selection. On such grounds, in any case, he rests his contention that there is in man a spiritual essence not inherited from his animal forbears to whom he owes his bodily structure. By virtue of this essence human progress is possible and a spiritual life beyond the grave assured, for spirit cannot perish.

In reading Wallace one feels the presence of a vein of mysticism and the impelling influence of religious pre-possessions . . . influences which may properly be given a hearing, but which must not be treated as standing on the same logical level with ordinary empirical evidence. Whether natural selection can reasonably explain mental development in its higher ranges, is however, a perfectly fair question and one which deserves, and from ethical writers at least has often received, serious consideration.

[1] Cf. Wallace, *Darwinism*, Ch. XV.

It seems perfectly clear that certain familiar intellectual and emotional endowments would have had a very positive survival value both among animals and men. Those individuals who were mentally quick and inventive, who were courageous, cunning and pushing, would certainly be at an advantage over those who failed in these characteristics. Other things equal, the latter would live shorter lives and leave fewer progeny. When one takes into account the conditions of life under gregarious or social circumstances, one sees clearly how in a group the social virtues of sympathy, bravery, self-sacrifice, etc., may condition the dominance of the group over competing groups and consequently how a survival value may attach to these mental and moral characteristics. All this is familiar and trite and probably true. But what is to be said of Wallace's case as it concerns mathematical, philosophical and musical capacities, to the possessors of which men have customarily paid large respect? Wherein do such characteristics display a survival value, and if they have none such, how can natural selection account for their preservation and cultivation?

The reply, I believe, is quite in keeping with the reply as to the survival value of sympathy and pity and self-sacrificing bravery. In course of mental evolution, no doubt many characteristics are developed which are either harmful or useless. The congenitally insane illustrate the appearance of harmful forms. Other forms appear which may be useless or even harmful to the occasional individual, but to the group as a whole they are highly valuable and by virtue of this fact they secure perpetuity, either by social imitation, or by direct heredity. Now we have only to assume the appearance of a mental strain which has such social value, to expect with certainty that it will be encouraged in most of those who possess it markedly. Music and mathematics and philosophy do not represent such highly occasional mental sports as Mr. Wallace implies. A respectable amount of each of these capacities is latent in all normal individuals. Propitious surroundings are not always at hand and other more seductive interests often secure the field in advance, so that these capacities remain latent and undeveloped. But nothing is more certain than this, that if society did not at least *consider* itself benefited by the cultivation of these tastes, they would speedily disappear along with the taste for collecting scalps and wampum.

In other words, Mr. Wallace and others of his way of thinking take their natural selection too narrrowly when they come to the higher ranges of mental life. They forget the social pressure which is there exercised, not to create but to develop certain capacities.

Still another view which not only accepts but magnifies discontinuity in natural phenomena is conceived not in the interests of any idealistic metaphysical or religious tenets, but rather in frank hostility to such. This is the view typified by Loeb,[1] who believes that many of the lower organisms have no consciousness at all. This is a view which in more sweeping form Descartes long ago made famous, though on grounds quite different from those of Loeb. For Loeb, man's mind is a natural product of the evolution of animal mind, but animal mind itself begins not necessarily in the protozoa, but presumably at a relatively advanced point among the metazoa, at a point, namely, where we find creatures able to profit by experience, able to learn.

Accepting the analogy of many chemical phenomena in which a critical stage is represented, before and after which the resulting phenomena are apparently entirely discontinuous (*e. g.*, the formation of liquid from gas under given conditions of temperature and pressure) he urges that until precisely the correct molecular conditions are represented in the protoplasm of the nervous system, no consciousness will appear. But the moment these conditions are given, mind will also be present. It is not necessary to assume mind, or associative memory, as he prefers to call it, wherever we find a nervous system, much less wherever we find protoplasm in a living state. We have a right to allege the presence of mind only when the actions of an organism indicate its presence, and our only criterion for this presence is, as was above stated, the capacity to learn by experience, to improve the reactions made to stimuli.

The difficulty with this criterion is practical, not theoretical.

[1] Cf. Loeb, *Physiology of the Brain*, particularly pages 213 ff.

If one could always say with assurance that animals can or can not learn, the task would be easy. Unhappily such is not the case. Some animals learn to better a reaction after a few attempts, others require dozens of trials. Even the frog, whose intellectual capacities were once regarded as nil, has now been proved capable, under the advantages of higher education, of making some progress, but it is a progress which taxes both pupil and teacher, for it may require hundreds of experiences to improve even a very simple reaction. The criterion proposed, while theoretically admirable, leaves us as a matter of fact in much the position we occupied before, *i. e.*, inability confidently to allege that any living creature is wholly lacking in mind. Even the lowly amœba manifests certain peculiarities of action which may betoken consciousness of a low order.

An examination of these variants on the Darwinian view of continuity in mental development leads one to feel that the balance of probability distinctly favors the original formulation. Not only does modern psychology disclaim in man at least any such sharp lines between conceptual thought and the lower levels of sentient mental life, as Mivart and Wallace postulate, it has on the contrary expended no little effort in analyzing and defending the presence of just these conceptual processes in the sensory and perceptual activities of mind. Binet's[1] essay on the psychology of reasoning is a typical example of this tendency, exhibiting as it does the implicit reasoning process involved in every definite perception. To perceive that this object before me is a desk, involves identifying this present visual experience with antecedent visual experiences in a way which closely resembles certain phases of the process in syllogistic inference. Nor has this tendency in psychology been in any way influenced by partisan Darwinian prepossessions, so far as I know. It has been the inevitable outcome of penetrating analysis. The use of conscious meanings does not suddenly burst forth full-blown in a mind which before had given no indication of such an achievement. The simplest mental acts which as human beings we can detect in ourselves have some increment, however small, of this consciousness of meaning, this embryonic form of con-

[1] Binet, *La Psychologie du Raisonnement.*

ceptual thought. Nevertheless, it must not be forgotten that animals have certainly not been as yet proved to reason in human ways. On this score Mivart and his cohorts must be given their dues.

Nor is the dividing line which Loeb has proposed likely to result in any radical alterations in the general Darwinian position. For not only do we find it difficult to use the criterion Loeb offers, *i. e.*, educability, but in point of fact we have considerable evidence at hand to show that even the lowest animal forms modify their behavior somewhat to meet changed conditions, and that these modifications are of a kind which in higher animals would be regarded as indicative of the presence of consciousness.

IV.

This brings us to the work on emotion. In his treatise on *The Expression of the Emotions* Darwin has brought together with characteristic patience and industry the most extended array of observations bearing on the subject, an array which has been of notable value to the defenders of the James-Lange theory of emotion. As finally put forth the work is a defense of three familiar theses concerning emotional expressions. The first holds that serviceable bodily reactions become habitual and become associated with the state of mind in connection with which they arose. When the mental state recurs, the bodily reactions recur also, although they may long since have lost any immediate and obvious utility. The clenching of the fist and the showing of the teeth in anger illustrate this conception. The second thesis, that of antithetic action, maintains that a state of mind opposed to one calling out a definite bodily attitude may evoke an opposite bodily attitude. As an illustration may be cited the fact that an angry cat naturally lashes its tail from side to side. On the other hand a cat which is pleased carries its tail erect and stiff. The third thesis, that of nervous overflow, holds that apart from the two previous principles of explanation, conditions of emotional excitement are prone to release more cortical energy than can be effectively disposed of in the usual ways, and the superfluity pours out in muscular contractions of the most various kinds.

So far as concerns the adequacy of these explanatory hypotheses, it may be said that in the light of our present knowledge the first affords a highly probable account of certain emotional reactions, while it is quite inadequate satisfactorily to explain others. The second hypothesis has always been viewed askance, as something of a scientific *tour de force*, while the third, which Darwin himself treats rather as a catch-all to take care of cases found bothersome to handle by his first two hypotheses, is probably of much more fundamental import than he imagined. In any event later writers have been unable to improve materially upon Darwin"s catalogue of the causative influences provocative of our emotional attitudes.

V.

In conclusion we may venture a brief comment upon the methods now current in the study of evolving mind and more particularly upon the methods and points of view now dominant in animal psychology. A few words may also be added upon a group of problems suggested by Darwin's work.

The most marked and unmistakable change which we notice in method is the somewhat aggressive skepticism now everywhere entertained for the anecdotal foundation on which many of the early zoölogical doctrines about animals were based. Darwin himself quotes numerous tales to substantiate his positions and his disciples have far outdone the master. This condition of things has led not unnaturally to a reaction in favor of laboratory experiments and observations under conditions of control. To this procedure there is never lacking acrimonious protest on the part of those who hold that only under the conditions of nature can the intimate facts of animal life be seen and understood. No doubt there is a large measure of justice in this protest. But fortunately it is now possible in many of our laboratories and zoölogical stations to simulate with large success the conditions of life which are natural to many animal forms. The result has been a wealth of new material which promises quite to revolutionize many phases of animal lore. It seems not unreasonable to anticipate that the effect of such work will not only be felt in the direct increase of our reliable information gained through these channels, but also that the observation of animals in a state of nature will be rendered far more intelligent and precise by virtue of the suggestions which will be gained from work of this type. Certainly such work has already brought us new and more exacting standards of accuracy and taught us an invaluable caution and conservatism both in inference and in generalization.

Conspicuous among the many interesting psychological problems suggested by Darwin's work is that of the determination of mental types, species and genera, following rudely the analogy of species and genera in zoölogy. The practical difficulty in defining a species need occasion us no concern, because the *idea* of species has had great value, despite the perplexities attached to the satisfactory differentiations of particular classes. If the type of intelligence manifested by an animal be contingent upon the structure of its nervous system, as is apparently the case, it would seem to follow as a reasonable inference, that we might expect to find groups of animals evincing in their behavior psychic characteristics of a similar pattern, just as we find forms of nervous system highly similar to one another. It is of course conceivable that in different animals different nervous structures should function to produce similar psychic behavior. But even recognizing this possibility, it still ought to be feasible to group creatures together as belonging to various great psychical type-forms.

At present the common divisions follow other lines. Animals which belong to the same family, *e. g.*, the dogs, are thought of as resembling one another in general mental pattern and as differing from other animals partly in their instincts, but partly also in their capacity to learn non-instinctive reactions. This practical view of the matter leaves us with as many main patterns as there are genera and with no explicit and tangible description of any one. The other line of demarcation consists in cross-sectioning such a division as the preceding by distinguishing between such psychical characters as sentience, memory and reason, ascribing all these attributes to the higher creatures and denying one or another to the lower creatures. Amœba may be thought to have sentience, but not reason and only dubiously memory. The pigeon has sentience and memory, but probably not reason, whereas men and possibly some of the higher animals have all three capacities.

Obviously neither of these modes of classification affords us any real insight into psychic types. If Darwin's fertile investigations are to bear fruit in this direction in psychology, we must be able to portray the entire range of mental processes belonging to the great divisions of animal life, to show where and how these dividing lines part company with those which now bind animal forms together on structural lines. For ordinary zoölogical purposes the dog and the elephant have little in common except their mammalian hall-mark. But in their psychic types they may be very similar.

Such types may clearly be grouped around various central factors. Animals in which the so-called 'distance receptors' (auditory, visual, olfactory) are well-developed, may present a pattern with the psychic life all grouped about these processes. In other animals the 'contact and proprio-ceptive' organs may be the centers of psychic life and in consequence give rise to quite another mental pattern. In one or in both, the psychic operations may be of the most rudimentary and immediate sort, or they may, on the other hand, involve processes comparable with the simpler forms of human inference. The patterns may vary again in dependence upon the relatively large or relatively small amount of purely instinctive and reflex activity. They may vary with the phylogenetic antiquity of the form, newer types being more plastic than older ones. Many other principles of grouping will readily suggest themselves.

At the present moment we have the beginnings, but only the beginnings, of the necessary data for the solution of this general problem. We have learned, for example, that the mere presence of a sense organ does not argue such a use of it as casual inspection would suggest, much less such as is suggested by the analogy of human sense perception. We have accordingly learned caution in assuming that the sensory activities of animals involve the sort of consciousness which we know in ourselves. Indeed our whole tendency now-a-days is to recognize and frankly admit, that inasmuch as we must infer the psychic operations of animals wholly in terms of their behavior, we are under peculiar obligation to interpret their activities in the most conservative possible way. We know that the 'try-try-again, method' is the one commonly used by animals in solving laboratory problems. But we are for the most part profoundly ignorant as to just what occurs when progress is actually made, what sensory avenues are most important for giving information and how far the counterparts of human inference may at times be present. To secure these and dozens of other items of information needful for the execution of the program proposed will require long years of patient labor. Nevertheless, until this work is done, we shall remain powerless to describe the great stages of developing mind. The task is eminently worth while and is certain to be accomplished. Only when it is accomplished will it really be possible to entertain an intelligent judgment concerning the fundamental contentions of Darwinism concerning the evolution of mind.

DARWIN AND LOGIC.

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In attempting to estimate the influence of Darwin's thought in different fields of inquiry, it is advantageous to distinguish between the direct and the indirect results of the conceptions which he introduced. By direct results, I mean primarily the effect of the conception of natural selection, as an explanation of the formation of species, upon the problems and methods of the biological sciences. And, as all the characters and functions of living beings, mental as well as physical, are subject-matter of biology, the explanation of the mental endowments and characteristics of man and the lower animals through the principle of natural selection may be included under the same heading. This can be done the more readily because of Darwin's own employment of the principle to explain, not only the instincts and emotions of living organisms, but also to some extent the intellectual and moral endowments of the most highly evolved animal. Indirect consequences are always difficult to trace in detail. From the standpoint of science, the most obvious and important indirect result of Darwin's discovery is the confidence which it furnished in the fruitfulness of the method of tracing origins in all fields of inquiry. In the process of becoming, Darwin's procedure showed, things progressively define and specify themselves through their positive and negative relations to other things. In the impetus thus given to the evolutionary method, there was strengthened and extended the influence of an instrument of analysis whose full power and significance has scarcely yet been realized.

It is only a commonplace to say that the publication of the *Origin of Species* revolutionized biology. For this work transformed the evolutionary hypothesis of the gradual formation of biological species from an *a priori* speculation, which was scarcely if at all influencing workers in this field, into an established and fruitful principle of explanation. Thus in a marvelously short time the stone which the builders had rejected became the head of the corner. Important and far-reaching as this result is in itself, it is the wider application of the evolutionary conception, which Darwin may thus be said to have called into existence as a working principle of natural science, that gives to his discovery its main interest and significance. "If we may estimate the importance of an idea by the change of thought which it effects," says Romanes, "this idea of natural selection is unquestionably the most important idea that has ever been conceived by the mind of man." [1] In his enthusiastic estimate of natural selection, Romanes, of course, assumes that it was this principle alone which made possible an intelligible and workable theory of evolution. After showing how little scientific thought had really been influenced by the earlier evolutionary hypotheses, he continues: "It was the theory of natural selection that changed all this, and created a revolution in the thought of our time, the magnitude of which in many of its far-reaching consequences we are not yet in a position to appreciate, but the action of which has already wrought a transformation in general philosophy, as well as in the more special science of biology, that is without a parallel in the history of mankind." [2]

In general philosophy and in the historical and social sciences, the notion of development and the evolutionary method of investigation had made their influence felt long before Darwin's discovery of natural selection had rendered their application fruitful in biology. From the time of Leibniz the notion of a continuous development had been familiar to philosophers. It exerted little influence, however, until the second half of the eighteenth century, when it appears both as an hypothesis in biology and as an interpretation of the spiritual history of the human race. In biology, as we have seen, it remained without practical effect because its factors or definite mode of procedure had not yet been discovered. But in its application to history, the method made its way through the influence of Lessing, Herder, Schlegel and Kant, and finally became, one may say, the main motive of

[1] *Darwin, and after Darwin* (1901), Vol. I., p. 256.

[2] *Ibid.*, p. 259.

post-Kantian idealism. In Hegel, the notion of *Entwicklung* is, even more explicitly than in Fichte and Schelling, the guiding method and explanatory principle. In his *Logic*, an attempt was made to exhibit the laws of the evolutionary process in their complete universality — to give in general terms that are applicable within the whole range of experience something analogous to what Darwin afterwards furnished in the special field of biology, a demonstration of the stages and working principles of the movement. Hegel's philosophy influenced historical study very greatly; especially, it gave an extraordinary interest to investigations into the thought systems, language, customs, and institutions of human society. The same fundamental motive, though limited in various ways by special interests and arbitrary assumptions, shows itself in the work of Comte. In England, as is well known, Herbert Spencer had recognized the significance of the evolutionary principle and begun to work out its ethical and social consequences before the appearance of Darwin's great work. Even J. S. Mill — as I think is evident both from his logical and ethical writings — was influenced by organic conceptions, which he probably learned mainly from Coleridge and Comte, and was thus led to attach a much greater importance to the historical and social sciences than had his immediate predecessors.

In philosophy, then, and in the field of the humanistic sciences, it is evident that the application of the doctrine of evolution had not to wait for the discovery of the principle of natural selection. It might therefore be inferred that in these departments of knowledge the principle has no application, or is at least of subordinate importance. Whatever conclusions we may later reach regarding the direct applicability of the conception of natural selection to the humanistic fields of inquiry, it is necessary to recognize that, indirectly at least, these subjects were stirred into new life by the influence of Darwin's thought. For though, as we have seen, the concept of evolution was already being employed by workers in these fields, its influence was extending very slowly. It lacked definite and concrete formulation, and hence had never fully come to its own. Few even of those who were applying the principle at that time firmly grasped its significance, or realized clearly its transforming power. Hegel's unbounded confidence in his method, which has often been regarded as presumptuous, is really confidence in the validity and efficacy of his conception of development; and the *Logic* is his attempt to fully define and exhibit in detail, in the most universal terms of experience, the nature of that principle. But Hegel's detailed working out of the method of evolution was not generally understood, and exerted little influence on the succeeding generation. This was due partly to the artificial form which he gave to his exposition, and partly to his inability, through lack of material, to base his results upon the facts of the physical sciences and of psychology. His conclusions were indeed derived from a wide survey of facts, but these facts belonged to the inner life of man and society; and thus, as not directly given to sense perception, they were too remote from ordinary experience to appear concrete and impressive.

Darwin's formulation of the evolutionary doctrine, on the other hand, rested on observations of the commonest facts of daily life. It drew its support from the experience of the breeder of domesticated plants and animals. Moreover, it provided a definite working mechanism for the evolutionary process that rendered its operation conceivable in scientific terms. But these facts do not in themselves explain the extraordinary influence which Darwin's conceptions quickly came to exert outside of the field of biology. There can be no doubt that this was primarily due to the fact that Darwin himself showed that his theory definitely linked man to the lower animals; and this consequence was further emphasized and enforced by able disciples like Haeckel and Huxley. Not only did the proof of the 'descent of man' rouse popular interest and give rise to theological controversy, but it tended to break down the wall of partition between the humanistic sciences and biology. The success of the evolutionary method in biology brought fresh courage and renewed confidence in the fruitfulness of their method to the humanistic sciences that were already employing evolutionary conceptions. But this is not the only effect that Darwinism has produced in these fields. The place which the doctrine assigns to man as a member of the biological series seems to demand that the biological evolutionary conceptions shall be used to interpret all the phases and manifestations of human life, mental as well as physical.

The immediate problem of this paper has to do with the influence of Darwin's discovery on Logic. What I have said of the indirect influences of Darwinism has, of course, its application to logic, as will appear from time to time in our discussion. We may ask, however, at the outset, how far has the principle of natural selection 'furnished guidance' in the attempts to explain the development of thought and the structure of knowledge? As we have already seen, Hegel's treatment of logic is distinctly evolutionary, as is also that of other idealistic writers, the so-called neo-Hegelians who work with the same general conceptions which he employed. We will, accordingly, ask in what ways this older conception of logical evolution has been modified by Darwinian conceptions, and attempt in a summary fashion to furnish an estimate of the value of these modifications from the standpoint of logical theory.

Darwin's great service to biology consisted in his statement of the working factors of evolution. He was the first to give a 'sufficient reason' for the transformation of species by pointing to the natural causes which are continuously in operation. The *modus operandi* of biological evolution is given in the conceptions of variation, natural selection (including sexual selection), and heredity. It is an observable fact, says Romanes, "that in every generation of every species a great many more individuals are born than can possibly survive; so that there is in consequence a perpetual battle for life going on among all of the constituent individuals of any given generation. Now, in this struggle for existence, which individuals will be victorious and live? Assuredly those which are best fitted to live, in whatever respect, or respects, their superiority of fitness may consist. Hence it follows that Nature, so to speak, *selects* the best individuals out of each generation to live. And not only so; but as these favored individuals transmit their favorable qualities to their offspring according to the fixed laws of heredity, it further follows that the individuals composing each successive generation have a general tendency to be better suited to their surroundings than were their forefathers."[1] Darwinian evolution thus results in a continuous adaptation of the species to its environment through the elimination of the unfit and the accumulation of favorable characteristics through heredity. Hence natural selection, taken in combination with variation and heredity, is able to explain, not only specific and individual forms regarded as wholes, but also the special constituent characters and functions of the species that have survived. And, as the living organism is psychical as well as physical, these principles apply directly to the mental life of all animals, including, of course, that of man.

It is natural, then, that biologists, viewing man as a member of the biological kingdom, should extend the principles of their science so as to include within their range all the forms and functions of experience. Darwin's treatment of the instincts and the emotions opened the way to important results in psychology; and the functional view of psychology, which regards the mind as an organic function whose origin and modifications are to be explained in biological terms, is only following in the path which he marked out. But Darwin goes further, and, like some of his successors, seems to suppose that these principles of functional psychology or biology are adequate to explain all forms of experience. "Although perhaps nowhere distinctly formulated," says Alfred Russell Wallace, "his whole argument tends to the conclusion that man's entire nature and all his faculties, whether intellectual, moral, or spiritual have been derived from their rudiments in the lower animals, in the same manner and by the action of the same general laws as his physical structure has been derived."[2]

Now, it was not in accordance with Darwin's purpose to work out the detailed application of his principles to the mental life in the form of psychology, or ethics, or logic; and he recognized

[1] Romanes, *op. cit.*, Vol. I., pp. 259–260.

[2] *Darwinism*, 2d ed. (1889), p. 461. The author continues: "As this conclusion appears to me to be supported by inadequate evidence, and to be directly opposed to many well-ascertained facts, I propose to devote a brief space to its discussion."

that he had no special equipment for such investigations. He contents himself, therefore, with indicating the standpoint and material of such inquiries, giving details only when his own observations and reflections enabled him to call attention to new facts. His treatment of logical functions and judgments is much less extensive than his discussion of moral experience, though the suggestion which he makes in Chapter V. of the *Descent of Man* regarding the function of imitation has led to important results in logic, as well as in other fields. And further than Darwin, so far as my knowledge extends, no biologist has gone in explaining logical forms of experience. But the biological point of view necessarily explains the forms and categories of thought, the very nature of reason itself, as functions of the living being that are to be explained by the general laws of biological evolution. The carrying out of this program, however, Darwin rightly leaves to the psychologist and the logician.

Now, I cannot see why any objection should be raised to the biological method of explaining experience, so long as this is not taken for philosophy. Logical thinking and moral action, whatever they may be in addition, are from one point of view, modes of living, and as such undoubtedly prove of advantage to the organisms which are characterized by them. If the objection be raised that this standpoint fails to exhibit what is essential in these experiences, the reply is, I think, that philosophy cannot afford to ignore any genuine aspect of experience, and that what we may choose to call merely 'external relations' cannot be devoid of philosophical significance. This much at least is true: that the unitary view of the psycho-physical organism and its activities, which biology has emphasized, is a good antidote to the abstracting tendencies of both physical and mental science.

The objection to the biological interpretation of logic, and of experience generally, holds only when it is put forward as philosophy. The limitation of these accounts of experience does not consist in their lack of details — the details may be worked out in time — but is a limitation of principle. They simply do not raise the logical problem, or give any account of the values that are operative within experience as experience.

They look upon experience from the standpoint of an external observer, and hence can deal only with objects and the external relations of objects. But, though mentality is a life function, as *experience* it is internal or for itself. And this is equivalent to saying that it is now constituted by new functions implying new ends in the light of which it must be understood. To understand experience as experience, which is the special business of philosophy as distinguished from natural science, is to interpret its various developing stages in the light of the system of ends which is being realized. For logic, then, thinking is not rightly construed as adjustment to the environment, whether physical or social. External terms like 'adjustment' and 'environment' are misleading metaphors as descriptions of logical results and relations. Of course, the thinking of the individual grows out of life. But, as in the case of the state, which Aristotle remarks *originates* in life but *is* for the good life, we may say that cognition has a natural origin but *is* for the sake of truth and consistency.

Moreover, in logical experience the opposition between organism and environment, which is essential to biological evolution, has become transformed into the distinction between subject and object. This distinction falls within experience and is not a relation between experience and something external to it. Thinking, therefore, cannot be externally determined; it is a self-determining process whose 'developmental factors' are organic to the process itself. The moving principle of the whole is just the nature of thought itself regarded as a demand for completeness and consistency of experience. It is this immanent principle of reason or intelligence which, as the presupposition of all *experience*, is thereby presupposed *in all science*. Of course, the thinking experience from this point of view is no longer a function of an organism, a mode of experiencing over against the experiencing of other psychic individuals. As logical thinking, it is objective and social — the medium in which we are shut together with persons as well as with things.

This will be recognized as in general outline identical with Hegel's conception of the logical standpoint. It is in this sense that he speaks of 'absolute thinking' and 'absolute experience' — a mode of expression which has proved to many a stone of stumbling and a rock of offense.

Dissatisfaction with the standpoint and procedure of this idealist logic is, however, expressed in different quarters by writers whose main work lies within the field of psychology and philosophy, and in some of these writers the influence of biological conceptions is more or less directly evident. What is regarded as lacking in the logic of Hegel and his followers is: first, an account of the development of knowledge from the point of view of individual experience; and secondly, a detailed working out in concrete terms of the psychological motives and processes through which logical results are obtained. To overcome these defects and base logic upon psychology seems to be the program of the majority of recent writers on *Erkenntnistheorie*, in Germany, though in that country a controversy is still going on between the advocates of the 'pure' and the 'psychological' logic. The influence of biological conceptions is perhaps most clearly evident in Avenarius and Mach. Indeed, the latter might be perhaps fairly classified as 'Darwinian' in his general view of the origin and function of thinking, though his account of experience is given in terms of Hume's analysis.[1]

It is obvious that a complete account which should attempt to trace both the direct and indirect effect on logic of Darwin's contribution would extend far beyond the limits of this paper. I should like, however, to refer to the influence which this 'scientific' view of evolution appears to have exerted on the treatment of logical problems by certain contemporary writers in this country. This influence is manifest, I think, in many of the papers contained in the Chicago *Studies in Logical Theory*, and in various contributions to periodical literature by the same writers. It has perhaps also furnished the main inspiration for Professor Baldwin's work on logic. Though there are some important differences between Professor Baldwin's views and

[1] Simmel's name should also be mentioned in this connection. The application of Darwin's principles to logical questions is evident in his articles, 'Pensée théorique et intérêt pratique,' *Revue de Métaph.*, IV., pp. 160–178, and 'Ueber eine Beziehung der Selectionslehre zur Erkenntnistheorie,' *Archiv f. syst. Philos.*, I., pp. 34–46.

those of the pragmatic evolutionists, they belong together in general standpoint and aim. Not only do they both approach the problems of logic from the psychological point of view, but both alike derive their working conceptions from the biological formulation of evolution rather than from post-Kantian idealism. The 'newer' evolutionary influence is shown by the Chicago group of writers especially in their interpretation of thought as instrumental and practical, both in its origin and ultimate significance. Hence it follows that the logical problem is to describe and explain thinking in its dealings with a concrete situation. Thinking is always a process of adjustment, a means of securing adaptation, and, as such, does not give rise to any general problem regarding the nature of knowledge as such, and does not admit of interpretation in the light of any absolute end. Professor Baldwin, on the other hand, though holding to an instrumental view of the origin of the logical function and the tests of truth, refuses to adopt the pragmatic interpretation of the meaning and significance of knowledge. He seems to hold that, when the stage of logical experience is reached in the progression of cognition, new functions and meanings have emerged which cannot be adequately described in instrumental or pragmatic terms. In his case the Darwinian influence, however, seems to account for the dualism that persists throughout between the inner and outer 'controls,' which appears to be the survival under another name of the opposition between the organism and its environment. It is true that Professor Baldwin tells us that this dualism is to disappear in a higher form of experience of the type of æsthetic contemplation; but in the progress of logical development no genuine organic unity between thinking and its object is attained.

From the point of view of idealism, therefore, pragmatism is strong where Professor Baldwin's theory is weak, and weak where he is strong. The former position stoutly repudiates dualism, while he as explicitly refuses to construe logical experience in instrumental terms. While recognizing the force of the arguments that each of these parties directs against the other, the idealist is ready on occasion to demonstrate that the dualism and pragmatism, which each finds unsatisfactory in the other, have a common root, and are both the logical outcome of the 'newer' evolutionary approach to the problems of logic. This general conclusion has already been urged from various sides against pragmatism. Moreover, as pragmatism has been for a considerable time the storm-center in logical discussions, and as I have more than once expressed my views in relation to it, I shall turn to Professor Baldwin's work as illustrating Darwin's influence on the method and procedure of logic.

What seems to me especially significant in Mr. Baldwin's work is the account, in the first volume of his *Thought and Things*, of the stages and means through which the individual mind develops a fully conscious logical experience. It is in part the same undertaking which Hegel left so incomplete in his *Philosophie des Geistes*, and which he combines so strangely with other topics in the *Phänomenologie* as to be almost unintelligible. The progress of biology and psychology have made it possible for Professor Baldwin to present a concrete and detailed working out of this problem which is an immense advance on anything that previously existed. And yet I cannot help thinking that he has been hindered rather than helped by his working conceptions. As I have already indicated, his standpoint is dualistic: the development takes place through the interplay of an inner and outer 'control,' which seem to be a translation into other terms of the organism and environment. The primary responses of the psycho-physical individual consists of motor adjustments. These, as they come to consciousness, furnish the contents of mind. "What we think is a function of what we have done." The unity of thought itself is 'the conscious side of the unity or synergy of material actions.' In short, Professor Baldwin's account professes to show, not the means through which the mind becomes conscious of its own logical nature, but how that logical nature is engendered in it through the motor adjustments of the organism to material conditions. It appears to him essential to derive the logical from the biological; to begin with logic or reason as implicit is to shirk explanation and take refuge in mysticism. But, after all, is it not true that sensations of processes of motor accommodation are no more able to account for the organization of experience than sensations of any other kind. It is an old story, but nevertheless one that cannot be ignored, that a description of experience must take account of the mind as the central principle of that process. Leibniz's addition to the sensationalist formula — *nisi intellectus ipse* — has not been rendered superfluous by the progress of science.

Of course, in recognizing the function of interest or attention [1], even in the earliest forms of experience, Professor Baldwin may be said to admit the presence from the beginning of the interpreting activity of the mind. This, he might say, is 'the one continuous function' whose development and progression he is recording throughout his book. But although this 'universal function' is recognized in words, it is phenomenalized, equated with motor process, in the supposed need of 'scientific' explanation. One may, indeed, analyze attention into motor terms from the standpoint of structural psychology; but, as the function of meanings and the organizing principle of experience, attention is not a phenomenon to be explained at all, but is itself the presupposition of all explanation. This does not mean that the development must not be traced in detail. Professor Baldwin is quite right in insisting on the necessity of exhibiting the 'What' and the 'How' and 'Why' of the process. But it must never be forgotten that logical progression moves in the realm of meanings and functions, and that, consequently, the process is self-determining, the relation of its parts being the inner organic relation of means and end. That is, the account of the development of experience must be expressed in teleological terms, not in terms of cause and effect.

It is a common mistake to suppose that to employ teleology is to abandon analysis and resign oneself to a merely formal explanation. To appeal to this principle is supposed to be equivalent to an appeal from knowledge to faith. But philosophy has surely advanced far enough beyond Kant to recognize the necessity of teleology not only as a 'regulative,' but also as a 'constitutive' principle. Whether we are to hold that 'science' may be teleological, depends upon what we include in our notion

[1] *Thought and Things*, Vol. I., pp. 40 ff.

of science. At any rate, no one can deny that experience presents us with variously organized systems of value which require to be analyzed and described in order to be understood.

Now, Mr. Baldwin has in various writings insisted that in a genetic science the mechanical form of explanation no longer applies. He does not, however, abandon the causal category, but merely denies that in a developing series there is any longer an identity between the antecedent and consequent. It is the differentia of a genetic series that in the later terms something new appears which was not contained in the earlier. This appears to be equivalent to giving up all explanation; the 'something new' simply comes into the series as a miracle. But, although the conception is contradictory in principle, it enables Mr. Baldwin to escape the difficulties which a causo-mechanical theory would have to face, while at the same time assimilating his procedure to that of causal science.[1] It is contradictory in principle, for it exhibits no identity throughout the different stages of the process; it renders impossible the conception of experience as the development of one continuous function. But it is this latter principle, with the teleology that it involves, that has enabled Mr. Baldwin to reinterpret facts derived from psychology and sociology in a way that is significant for logic. The following out of this principle, however, is strangely crossed by and intermingled with an external 'scientific' explanation of experience in terms of the interplay of the organism with its physical and social environment.

That what I have called Mr. Baldwin's external mode of explaining logical experience is derived from Darwinism is still more evident from his presidential address entitled "Selective Thinking."[1] This paper is at once a program and an epitome of the work that he has since published in this field. Here the terms and conceptions are avowedly taken from biology, as is evident from the following statement of the problem: "Look-

[1] In a paper entitled "The Notion of the Implicit in Logic," which was read before the Philosophical Association at the Baltimore meeting, I have treated this point more in detail. This paper will appear during the present year in *The Philosophical Review*.

[2] Published in *The Psychological Review*, January, 1898, and reprinted in the volume *Development and Evolution*, pp. 238 ff.

ing at the question from a point of view analogous to that of the biologists when they consider the problem of 'determination' in organic evolution, we are led to the following rough but serviceable division of the topics involved — a division which my discussion will follow: (1) The material of selective thinking (the supply of variations); (2) the function of selection (how certain variations are selected out for survival); (3) the criteria of selection (what variations are singled out for survival); (4) certain resulting interpretations."[1] This formulation of the problem and the comparatively brief compass of the paper bring out clearly both the nature of the explanatory principles that he proposes to employ and also, I think, the ambiguity in the actual procedure to which reference has already been made. On the one hand, we are told that "it is just the nature of knowledge to be an organization, a structure, a system."[2] Variations are not fruitful "that do not fit into the coördinations of knowledge which are ours, nor bring about readjustments in the arrangement of them. The items, to appeal to me, must never quite break with the past of my knowledge: each must have its hand linked with that of the thought which begot it."[3] "The attention is a function of organization, a function which grows with the growth of knowledge, holds in its own integrity the system of data already organized in experience."[4] Moreover, Professor Baldwin points out that "the environment of thought can only be thoughts; only processes of thought can influence thoughts and be influenced by them. . . . Even in knowledge of the external world of signs, expressions, etc., we have to say that movement must be reduced to some form of thought in order to be organized in our knowledge."[5] In these and many other statements that might be quoted from the paper, the idealist recognizes familiar doctrine, and also that here fresh facts and illustrations are brought to its support. But the Darwinian conceptions, which play the main rôle, lead the author to 'genetic' results of the organization of knowledge which are

[1] *Development and Evolution*, pp. 238–239.

[2] *Ibid.*, p. 245.

[3] *Ibid.*, pp. 246–247.

[4] *Ibid.*, p. 252.

[5] *Ibid.*, pp. 260–261.

quite out of harmony with that indicated in the passages quoted. These are summed up in statements like the following: "Selective thinking is the result of motor accommodation to the physical and social environment; this accommodation taking place in each case, as all motor accommodation does, from a platform of earlier 'systematic determination' or habit."[1] "Thus organized knowledge in all its development may be looked upon as due to the *synergies* of motor process selected as accommodations to the world of things and persons."[2] This really amounts to a derivation, not merely of the contents of the mind, but of its organizing principles and categories from the control of the environment. Although we are told that 'the burden of mental progress seems to lie on the side of the organizing function,' that organizing function is itself derivative. "The individual's judgment, his sense of reality and truth . . . when genetically considered is both the outcome and the evidence of the control which the environment has all along exercised. Even though we assume certain innate norms of selection which the individual directly applies, still those norms must not only lead to workable systems of knowledge in the world of active experience, but they must also in their origin have been themselves selected from variations, unless, indeed, we go back to a theory of preëstablished harmony."[3]

It appears to me that it is necessary only to place such statements side by side in order to exhibit the difficulties of the position. Of course, Mr. Baldwin's view is that logical organization arises out of the earlier organization or platform of motor habits. But what is the principle of unity that holds together motor adjustments into an organization? What is meant by the 'synergy' or union of adaptive movements which is said to give unity and organization to the mental life? If we say that this is just the *consciousness* of the movements as related, do we not thereby imply that the unity and organization are involved in the very nature of consciousness? To form a system or platform,

[1] *Op. cit.*, p. 264.

[2] *Ibid.*, p. 265.

[3] *Op. cit.*, p. 266. It is interesting to note that here, as elsewhere, the alternative for Professor Baldwin is between deriving logical principles mechanically and *finding them existing a priori*.

the motor sensations must be interpreted, evaluated, or translated into terms of knowledge. Similarly, new motor accommodations cannot produce changes in this system. It is only thought which produces changes in the organization of knowledge. Attention is, indeed, in a sense 'action'; but can its function as the organizing principle of experience be adequately described in terms of what is 'motor, afferent, kinæsthetic'? And the Darwinian principles show their inadequacy in other respects. For thinking is not mere selection or elimination. Not only do the variations arise as differentiations of the achieved organization of experience at any stage, but they are linked to each other in such a way that they mutually define and determine each other. The variation finally chosen has itself undergone modification and determination through the process of elimination. Moreover, it is not simply added to the platform from which it arose, but enters into it as an organic member. In short, what we have is a living, organic process of internal transformation and growth to which no account in mechanical terms can do justice.[1]

The general result that we seem to have reached is that Darwin's conceptions can be fruitful for logic only when transformed in the light of an idealistic philosophy. When carried over directly into logic they furnish no really genetic or teleological principle of explanation, but throw us back on the mechanical and external categories which have already been tried and found wanting. Nevertheless, Darwin's work and method — infusing as they did new life into the psychological and historical sciences and opening up new problems and new fields of investigation — fortunately have not left logic untouched. Fortunately, for if logic is to fulfil its task of interpreting and exhibiting the principles of experience, it must rest upon the work of the physical and mental sciences, that 'first vintage' of truth, as

[1] In this attempt to trace out the influence of biological concepts on Mr. Baldwin's logical writings and to estimate their value, my criticisms have necessarily been stated somewhat summarily. The points involved are so fundamental that it is, of course, impossible to treat of them exhaustively or adequately in this incidental way. Readers of this REVIEW have, however, already had the main issues between idealistic and Darwinian logic ably presented on both sides in a notable discussion between Mr. Bosanquet and Mr. Baldwin, which was carried on in various numbers of this journal during 1902 and 1903.

Bacon might say. The vast accumulation of facts in various fields, and the new form of the results, offer fresh problems to logic and demand a new statement and interpretation from it. The new facts of biology, psychology, sociology, and history press upon logic for reinterpretation and revaluation in terms of experience. Not only will Hegel's work 'all have to be done over again,' as Green remarked; but logic, if it is to keep alive and fulfil its function, will have to be done over constantly and continuously by each generation in order to meet the new problems raised by the advance of the special sciences. There can be no doubt that the weakness of the Hegelian logic consists in the fact that its connection with psychological experience is not clearly and fully worked out. When we call to mind how comparatively little had been accomplished in the way of scientific analysis a century ago, either in physical science or psychology, we cannot but marvel at Hegel's achievement. That, working with such scanty materials, he was able to furnish an interpretation of experience whose essential features the advance of science has confirmed, is a striking evidence of his own profound insight, and of his ability to profit by the labors of his predecessors.

This work, however, must be done over again in the light of the new facts and laws that are furnished by science, and more particularly by the evolutionary sciences to which Darwin's discovery gave a new impetus and direction. It should be recognized that the movement known as Neo-Hegelianism constitutes an important step in this direction. That movement has succeeded in ridding itself of the formalism and abstractness that characterized Hegel's results, mainly by recognizing and making use of the new material that scientific analysis has brought to light, particularly in the field of psychology. The further reconstruction of logic that is urged by the pragmatists and Mr. Baldwin is undoubtedly made necessary by the discovery of facts of a new order, and most of all by the new conceptions under which the sciences are to-day presenting various aspects of experience. These writers have done good service, both by insisting on the need for a restatement of logic that shall take up into itself and serve as an interpretation of the psychological sciences, and by their own positive contributions toward such a restatement. The criticism that I have tried to justify in the case of Mr. Baldwin is that he has sought to bring about this reconstruction by adopting to some extent the standpoint and working conceptions of biology and psychology. For it must never be forgotten that logic has not to take over either facts or conceptions from the special sciences. It is rather its function to reduce these facts to its own terms, to estimate their value and assign to them their meaning in accordance with its own standards. Darwin's evolutionary principles, being formulated as a mechanical explanation of the adaptations to be met with in organic nature, can have no direct application as an explanation of experience.

After all, is not the fundamental issue between Idealistic and Darwinian logic, simply the old question as to whether reason and purpose can be explained in terms of relations obtaining between phenomena, or whether these principles are not rather presupposed in all science and experience? If the latter be true, and *only* if it be true, are we entitled to employ teleology as an explanatory category of our experience. For the ultimate explanatory category of experience must be at the same time its universal presupposition. To work out and justify the connection between presupposition and final category is to complete the circle of experience, and so must mark out for logic the nature of its undertaking.

THE INFLUENCE OF DARWIN ON SOCIOLOGY.

BY PROFESSOR CHARLES A. ELLWOOD,
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It often happens in the history of science that the influence of a great thinker and investigator in one field penetrates to many related fields. It is not often, however, that the influence of such a man comes to dominate in other fields than his own. Yet this is undoubtedly what has happened in the case of Darwin; and perhaps in no field outside of his own is the dominance of Darwin's influence to be seen more clearly to-day than in sociology. John Fiske said that Herbert Spencer was the most eminent thinker that England produced in the nineteenth century: but although Spencer was primarily a sociologist, his influence in sociology is waning, while Darwin's influence is growing. When one reflects upon the immense influence which Darwin's work has had on practically all lines of human thought, and especially on the biological, psychological, and social sciences, one is forced to conclude that Fiske's estimate must be revised, and that Darwin must be given the seat of highest honor as the most fructifying thinker which the nineteenth century produced, not only in England, but in the whole world. And the social significance of Darwin's teachings is even yet only beginning to be apprehended.

Not that Darwin had any theory of his own regarding human society. Outside of a couple of chapters in his *Descent of Man* he says little specifically regarding the problems of human society; and it must be admitted that what little he says is not peculiarly valuable or profound, but only suggestive. In spite of the vast range of his mind and of his scientific labors, Darwin, then, was not especially interested in social problems and made no direct contribution to sociology. On the other hand, Spencer was primarily interested in social problems. His first considerable work, *Social Statics*, was along sociological lines, while his whole synthetic philosophy was confessedly developed to support his social and political theories. Even in his famous controversy with Weismann Spencer admitted that social interests were influential in his defending the doctrine of the inheritance of 'acquired characters.' Thus Spencer's interest in other sciences was subsidiary, while throughout life he remained primarily a sociologist. Nevertheless, as was said above, it has come about that Spencer's influence in sociology is waning, while the influence of Darwin, who was not a sociologist at all and not even greatly interested in social problems, is growing.

The reasons for the decrease of Spencer's influence in sociology and the increase of Darwin's are not far to seek. Spencer sought his principles of social interpretation in the physical sciences, as his work on *First Principles* clearly shows. He aimed at explaining social phenomena in terms of the redistribution of matter and energy. While he found it impossible to carry out an interpretation of social life in these terms, his conception of evolution, and even of social evolution, remained mechanical to the last. Spencer's social interpretations, then, being fundamentally in terms alien to the social life, were fore-doomed to failure. Again, Spencer's social and political theories were largely based upon the ideas and prejudices of the average middle-class Englishman of his time; and his knowledge of biology and psychology did not greatly alter his social theories, but rather the latter powerfully influenced his biological and psychological views. Under these circumstances it is not surprising that many of Spencer's social theories were of a temporary character.

Darwin's methods, on the other hand, were totally different. We find in him no appeal to vague principles borrowed from the physical sciences; but on the contrary he attempts to explain the life-process in terms of its own elements. As is well known Darwin got the key to his natural selection theory of organic development from Malthus, a writer on social and economic problems. Malthus, in his sociologic study of the growth of population, demonstrated that the normal rate of reproduction in man is in some geometric ratio, and consequently, to use Malthus's own metaphor, nature invited more guests to her banquet than she laid covers. Hence arose, according to Malthus, a struggle for existence in human society, in which the weaker succumbed to poverty, disease and death, while the stronger survived. Darwin seized upon this idea and generalized it, applying it to all organic nature and deducing therefrom his famous doctrine of the natural elimination of the inferior and the evolution of higher types through the 'natural selection' of the better adapted. It may be suggested that Darwin's principle of natural selection found ready acceptance in sociology because it was a principle which had already been recognized and applied, though in a negative way, in social theory. However, the deeper reason for the strong influence which Darwin's work has had upon sociology is probably the simple fact that his work was upon a part of sociology's foundations. Sociology, as a body of theory regarding the origin and development, structure and function of human society, could not develop until biology had developed. Spencer worked largely at rearing a sociological superstructure for which the necessary biological and psychological foundations had not been laid, while Darwin worked at these foundations. However much Darwin's selection theory of organic evolution may have to be modified by the biologists of the future, there is no doubt that his work established biology upon a secure scientific basis. The inevitable consequence has been that Darwin's work has reacted to enrich immeasurably all the sciences in any way connected with biology.

The greatest effect of Darwin's work on sociology has been of course in connection with the theory which is particularly associated with his name: the selection theory of evolution. While it is one of the moot points in biology just now whether natural selection operating upon minute variations even through immense periods of time is capable of producing new species, there has never been any doubt since Darwin wrote that selection is a powerful modifying influence upon all forms of life through its 'fixing' certain variations. In this sense Darwin demonstrated that selection is the chief creative force in the biological realm. Sociologists have not been slow to see that this idea had vast possibilities when applied to the interpretation of the forms and movements of human social life. While none has succeeded in showing that natural selection is the key to social evolution, it has been repeatedly shown that natural selection conditions the social evolution process at every step; that natural selection is the basis, though not the moving force, of human progress. The competition between human groups, especially through war, and the resulting elimination of those of inferior organization or of inefficient membership, has been shown to be in past social history one of the chief causes of the continued advance to higher types of social organization. All the higher types of human coöperation may thus be said to be 'fixed' by natural selection quite in the same sense that the higher types of life are. In many other ways also natural selection has been shown to affect human society, especially, for example, in the way in which the death rate affects different classes or elements in complex human groups. So numerous have been the sociological writers who have applied the idea of natural selection to human society that it seems superfluous to mention any, but Gumplowicz, Novicow, Ratzenhofer, Ward and Kidd may be taken as types, though not all of these men have embodied consistently the Darwinian point of view. Indeed, but few sociologists have had with any exactness Darwin's point of view, while not a few, the so-called ultra-Darwinists, by grossly exaggerating certain elements in his doctrine, such as struggle, have brought discredit upon his whole theory. Nevertheless, sociologists are more agreed to-day than ever before that natural selection must be given an important place among the factors of social evolution.

But it is not natural selection alone which has occupied the attention of sociologists, but rather selection in all of its forms; and the impulse to the study of the effects of various forms of selection upon human society may be fairly credited to Darwin, since selection, though long known and practically applied, was first given by him its full theoretic significance in evolutionary science. It is especially social selection which has of late been attracting the attention of sociologists; that is, the effect of social institutions and customs upon the birth and death rates of various classes. Francis Galton, a cousin of Darwin, led in his *Hereditary Genius* (1869) in this study of social selection, showing especially the evil effects of religious celibacy upon various European peoples. Darwin in his *Descent of Man* paid some attention to various forms of social selection, suggesting, among other things, that war produced a 'reversal of selection' (*i. e.*, a breeding from the least fit). This idea has been developed by numerous writers, among the latest of whom is the historian Seeck, who finds in Rome's constant wars, and the resulting elimination of her ablest and strongest men, the chief cause of the decline of Greco-Roman civilization. The selective effects of city life, of economic competition, of standards of living, of marriage customs and laws, of various forms of benevolence, have all received increased attention from students of human society in recent years, though much still remains to be done. Certain it is that in any theory of social evolution in the future the various forms of selection must be given an important place, and especially must mis-selection be emphasized as one of the chief causes of social decadence. It is to be regretted that in a matter of such vital human importance there is still lacking adequate scientific investigation of the working of various selective agencies in human society.

Here must be noted the important practical application of the selection theory which it is proposed to make in bettering social conditions. Francis Galton has spent the latter years of his life in organizing a new division of scientific philanthropy which he calls the science of 'eugenics.' He defines 'eugenics' as 'the study of agencies under social control that may improve or impair the racial qualities of future generations, either physically or mentally.' A 'Eugenics Education Society' has been organized in England, which, together with the British Sociological Society, conducts a vigorous propaganda in behalf of the new science. As yet little similar work has been attempted in the United States. However distant any extensive application of the principle of selection to the improvement of the human breed may seem to be, it is now acknowledged by all scientific students of philanthropy and scientific social workers that there is a biological element in the social problems of crime, pauperism, and other forms of degeneracy which is amenable to control only through selection. The theory of evolution by selection, in other words, has brought a great hope into the world that human misery in its worst forms may itself be subject to control. While sociologists will doubtless continue, as they have done in the past, to emphasize the all-importance of education, the nurture of each individual life, they will in the future have to take into account the possibility of improving nature also through the selective control of heredity. It may well be that future ages will look back to Darwin as marking, not merely a new view of organic nature, but a turning point in the history of the race in its control over human nature and over the problems of collective human life.

Sociology owes much to Darwin also in indirect ways, through the influence which his work has had in developing other sciences than biology, especially psychology. Sociology is not merely an extension of biology, as this paper has, perhaps, thus far seemed to imply ; it is even more a psychological interpretation of the social life. Whatever has contributed to the development of psychology, therefore, has contributed to the development of sociology. Now the influence of Darwin upon psychology, which is discussed in detail in another paper in this number, may perhaps be summed up by saying that it tended toward a functional view of the mental life. Darwin's whole view of life was essentially functional. Everything about an organism, barring perhaps its accidental variations, had a meaning with reference to the whole life-process. The color and form of plants and animals, for example, Darwin sought to show, had a survival value for the species to which they belong. This view he carried over to the mental and moral characteristics of man. Hence has arisen the functional psychology of the present, which regards mental life as a part of the whole life-process and interprets it through its function in that process. This view is now practically dominant in psychology, and is rapidly transforming sociology also. The details of this transformation, which is now going on, cannot be here discussed, but it is evident that a sociology based upon a functional view of human nature will be a very different sort of affair from a sociology based upon a static view of human nature. And all this is undoubtedly a remote effect of Darwin's work.

Finally, the great debt of sociology to Darwin, as of all the sciences, is that he finally established the doctrine of evolution upon a secure foundation. That doctrine, in one form or another, had long been before the intellectual world, but it had failed of general acceptance until Darwin wrote. In the social sciences, it is true, the conception of social evolution had long been common. The idea of progress in human history, first put forth in modern times by Bodin, had been made the central idea in social philosophy by Condorcet. And Comte had even divided sociology into two parts, one treating of the laws of social progress and the other of the laws of social order. Still the idea of evolution, in its broader aspects, was insecurely held in the social sciences and not generally accepted until Darwin wrote. Darwin's work, then, wrought a revolution in the social sciences as well as in other sciences. His influence established in them the genetic point of view, so that sociology came to throw the emphasis, as it does to-day, upon the study of social changes rather than of social structure, making it a science of social evolution rather than merely a science of social organization.

DARWIN AND EVOLUTIONARY ETHICS.

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It is opportune that while we are honoring Darwin for his far-reaching influence in almost every field of modern thought we should consider his relation to ethics. The power of his name is being used in support of policies and doctrines which he certainly did not favor in his writings, and which there is no good reason to think he would approve to-day. Speaking of the general reaction against humanitarianism which shows itself in so many forms to-day Mr. Hobhouse says that "the doctrine that human progress depends upon the forces which condition biological evolution has in fact been the primary cause of the reaction. Darwin himself, indeed, was conscious of the limitations of his own hypothesis . . . " but "what has filtered through into the social and political thought of the time has been the belief that the time-honored doctrine 'might is right' has a scientific foundation in the laws of biology. Progress comes about through a conflict in which the fittest survives. It must, therefore, be unwise in the long run . . . to interfere with the struggle. We must not sympathize with the beaten and the weak, lest we be tempted to preserve them. The best thing that can happen is that they should be utterly cut off, for they are the inferior stock and their blood must not mix with ours." Darwin himself certainly held a very different doctrine.

As has often been pointed out there are two distinct aspects of the relation between ethical theory and evolution, which have been termed respectively the 'evolution of ethics' and the 'ethics of evolution.' But historically, origin and validity have been persistently and almost inseparably connected. To show that a law is not binding, prove that it is a recent, or 'artificial' construction. To give a strong force to custom, say that 'it is not of yesterday or to-day but lives forever, and none knows whence it sprang.' In both ancient and modern times the question as to the origin of law or justice or current moral valuations has been forced to the front in times of conflict over the authority of institutions and customs. Such a situation called out the varying theories of the Greek enlightenment and the serious efforts of Spinoza and Hobbes, Locke and Rousseau. But whereas interest in the ancient world confined itself for the most part to the more objective questions as to the origin of institutions which likewise formed the initial question for modern reflection, the growing importance of the individual has brought increasingly to the front the more subjective problem: How does the moral consciousness arise? Is it an 'intuition' or 'sense' implanted once for all in human nature and incapable of further analysis? Or is it a product of gradual formation which can either be analyzed into simpler elements bound together by association or traced back historically to social forces? These are questions quite analogous to the general alternative between special creation of separate species or that continuity which Darwin maintained as his first premise.

The early evolutionary theories of morals were on their face primarily designed to condemn or approve the existing standards and institutions, and only incidentally as scientific accounts. Polus in the well-known passage argues that Might is Right by nature's law, and that all existing judgments to the contrary are a *Sklavenmoral*, set up by the weak, and gradually accepted by members of other classes who are 'charmed' from early youth by the suggestions emanating from dominant influence. Or again, what is 'stronger, freer, and more masterful,' is admired when it does not infringe too strongly on the interests of others; hence the interest of the stronger is really the basis of all law and 'justice.' Democrats and aristocrats make laws and shape institutions each for their own interest. Our *καλοκἀγαθοί* are 'honorable' and 'excellent' from the standpoint of their own class; but this is because "Nomos is lord of all."

On the other hand, if it is desired to strengthen respect for existing codes, reverence and a sense of justice are attributed to a primeval gift of the gods, designed to make associated life possible and thereby afford man protection against wild beasts — aid in the struggle for existence. Or by Aristotle with a pregnant reversal of standpoint, nature is to be sought not in the beginning, but in the perfected realization of powers. The process of social and moral evolution begins with impulse (*ὁρμή*) to the life in common, but the increasing organization of society gives increasing opportunity for human powers. For though in complete development man is the noblest, yet without the conceptions and the practice of justice and the excellence for which organized society is necessary "no animal is so unscrupulous or savage, none so sensual, none so gluttonous." This doctrine, then, equally with the opposing theories sought a standard in 'reality,' in evolution. But in its intent it looked forward, not backward, to a social intelligence and not to a physical force. Nevertheless, it is obvious that the conception of a law of nature as universal in human institutions and innate in the human soul could easily become in legal doctrine a ground for justifying institutions as they now are.

The reason why 'nature' appealed so strongly to the Greek was not biological. He did not trouble himself particularly as to the future of the race. Professor Dewey has recently stated forcibly why nature was such a word to conjure with:[1]

"What, finally, is this Nature to which the philosophy of society and the individual so bound itself? It is the nature which figures in Greek custom and myth; the nature resplendent and adorned which confronts us in Greek poetry and art: The animism of savage man purged of grossness and generalized by unerring æsthetic taste into beauty and system. The myths had told of the loves and hates, the caprices and desertions of the gods, and, behind them all, inevitable fate. Philosophy translated these tales into formulæ of the brute fluctuation of rapacious change held in bounds by the final and supreme end: the rational good. The animism of the popular mind died to reappear as cosmology."

We find the evolution of morality and the law of nature the center of discussion once more at the opening of modern thought. A Falstaff might flippantly appeal to biology to justify his predatory designs upon Justice Shallow: "If the young dace be a bait for the old pike, I see no reason in the law of

[1] Ethics, Columbia University Lecture, 1908.

nature but I may snap at him." But Hobbes wished to establish a firm basis for government by showing the brutishness of a 'state of nature,' Spinoza to point the way of escape from 'human bondage.' The striking thing about these attempts is the discredit which has now fallen upon the natural. One school of writers, indeed, maintains the rational and social nature of man, and the rational laws of cosmic nature, but the most striking evolutionary theories, those of Hobbes and Spinoza, conceive nature as the realm where force, and the instinct for self-preservation hold sway. This was doubtless due largely to the theological dualism between the 'natural man,' born in sin, totally depraved, with no good instincts, and the spiritual man who must needs be 'born again,' regenerated by special divine grace, before he could be just or good.

In the case of such a writer as Hobbes, very likely a reenforcement to the dualistic attitude came from the horrors of war which seemed to disclose the primitive passions of man when unchecked by the barriers built by law and government against them. In Spinoza's case there was a metaphysical reënforcement. For although it is the very essence of substance (or God) that involves existence and persistence and becomes in man the 'endeavor' for self-preservation, yet as 'the force whereby a man persists in existing is limited,' and as he is thus necessarily 'a part of nature' and 'passive,' "it follows that man is necessarily always a prey to his passions."

The forces adduced by the writers who sought to bridge the chasm without appealing to supernatural agency were various. The view of the world and life *sub specie æternitatis* in which Spinoza saw the only relief from human bondage made the saved as few as the elect of Calvinism. Nevertheless, the measure of reason which men in general have is sufficient to lead them to seek greater power and advantage through union in the civil order. Man perceives his need of his fellow men and in this sense may be called sociable. Hobbes dwelt upon the fear which drove men to political life and legal morality. Mandeville introduced pride and susceptibility to flattery as affording the agencies on which superior classes could work in fastening the 'slave morality' (to borrow Nietzsche's phrase) upon the inferior class — thus "savage man was broke." It was avowedly against the supposedly evil effects of such a nominalistic and selfish theory of morals as that of Hobbes that the evolutionary theories arose which claimed a continuity in moral development.

The 'herding instinct,' the 'seed of a boniform nature,' the instinctive disgust or recoil from what is 'nasty,' the 'moral sense,' of Shaftesbury and his school all reflect this standpoint. The optimism of 'natural religion' (the term itself was an abomination from the previous standpoint as to the wickedness of the natural), the era of comparative peace, the increase of commerce and general intelligence, all favored the spread of the conception of historical and psychological continuity in the moral process. Hume was able to effect a synthesis of the claims of reason and instinct in the rise of society and justice. Sex instinct starts the process and brings pairs together. The advantage of society when once experienced is then consciously appreciated. A civil order which included justice is 'artificial.'

Emancipated from unquestioning acceptance of the authority of the Church and the Leviathan, the individual was moved to examine the nature and origin of the inward authority which was replacing external control. If conscience has the right to govern the world how is such a right derived? The rationalist account of the 'moral faculty' did not lend itself easily to evolutionary treatment. Reason tended to be conceived mathematically or logically. It was 'timeless,' 'universal and necessary.' Kant, indeed, in his essay on political evolution for once seems on the verge of a very different conception. Men's passions and conflicting impulses call out a civil order and evoke a reason to recognize its values. And the later German idealism foreshadowed, at least, if it did not clearly grasp, the conception of an evolution of reason. But it was the 'moral sentiment' which lent itself most easily to genetic treatment whether by the associationist analysis of Hartley or by the brilliant beginnings of social psychology in Adam Smith.

The 'validity' of a moral sentiment was not necessarily threatened by considering it genetically. But when the process was conceived hedonistically, as an association of pleasurable elements, it was difficult to ascribe to the product any greater authority than that of any other pleasurable feeling. If my moral sentiment gives me pleasure in a generous act, well; if I find more pleasure in an egoistic act, who can say me nay? If it is a matter of individual association, why is my liberty judged by another man's conscience? J. S. Mill, as he tells us, felt in his own experience the artificial character of the theory, and in the 'Utilitarianism' took two important steps toward a more adequate conception. On the one hand, the 'social feelings' took on the form of an active 'natural want' rather than of an association of pleasures. On the other hand, he considered that first the social state, so natural, so necessary and habitual, and then the necessity of coöperation with others and of proposing 'a collective, not an individual interest' were agencies in bringing about the social feelings. It wanted but an additional step to disclose the individual as a 'social outcome' rather than as a 'social unit,' but this was a revolution for which the time was not ripe.

The social explanation through Sympathy, begun by Hume in hedonistic terms and developed along broader lines by Adam Smith, cast no discredit upon the product for a generation which valued the social. Not until race collisions, class contrasts, and the clashing of ideals of a new era had set up as morally desirable a sharp antagonism between the 'higher' and 'lower' races, between the 'fit' and the 'masses,' between the 'solitary' and the 'herd,' did sympathy become a synonym for weakness, and come to be regarded as fatally infecting the moral sentiment it had aided in producing.

The great contribution of Spencer was that he placed moral evolution — both moral progress and the formation of moral sentiments — in the sweep of his universal process. We may easily criticize his hedonistic analysis of the 'moral sense,' or, from another point of view, his belief that he has reconciled the empirical and *a priori* schools of thought by his doctrine of the experiences of the race. We may smile at his derivation of the consciousness of duty, and from our present standpoint of social psychology detect the fallacies of his atomistic conception of the individual in group life. We may think that his appeal to evolution in the *Social Statics* is rather to confirm a doctrine of potitical ethics already established on other grounds. The fact remains that he had conceived a world-wide movement. Mental and moral and social evolution gained immensely in their significance and definiteness when placed under a law asserted also of all the inorganic and organic world. And as compared with the great evolutionary conceptions of German idealism, the great advance in the natural sciences and the relative simplicity and clarity of their concepts gave Spencer a great advantage in power of appeal, even if this very simplicity inevitably brought it its own limitations for the explanatory principles so derived. Applied to morality the principle of adaptation makes "moral progress not an accident but a necessity. Instead of civilization being artificial it is a part of nature, all of a piece with the development of an embryo or the unfolding of a flower." For "all evil results from the non-adaptation of constitution to conditions"; but it is an essential principle of life that non-adaptation is ever being rectified until the adaptation is complete. Man's primitive predatory life required sacrifice of the welfare of other beings to his own, and his unfitness for present society is due to a survival of these traits formerly necessary.

The wide-reaching influence of Darwin upon ethical theory was not so much by his own discussion of the moral sentiments in the *Descent of Man*, as by the general biological and logical principles of his *Origin of Species*. The question was soon raised as to the operation of natural selection in the social and moral sphere. No evolutionary theories had brought home so vividly the continuity of the whole organic world. None, therefore, had seemed to immerse man so deeply in nature, and make him merely one link in a chain all forged of one metal and in one fire. Before Darwin's own discussion of morality in the *Descent of Man* numerous important contributions appeared. Among those which Darwin cites as most directly in the line of his problem were those of Wallace, Galton, Bagehot and Greg.

It remained for Darwin to approach the problem 'exclusively from the side of natural history,' and 'as an attempt to see how far the study of the lower animals throws light on one of the highest psychical faculties of man.' The general lines of Darwin's theory are indicated largely by this standpoint and by the fact that the dominating English tradition of his time sought the distinctive character of the moral in the emotional rather than in the rational factor. His proposition is "that any animal whatever, endowed with well-marked social instincts, the parental and filial affections being here included, would inevitably acquire a moral sense or conscience, as soon as its intellectual powers had become as well, or nearly as well developed, as in man."

The four steps in the development are the following: (1) The social instincts lead to pleasure in society, to sympathy, to aid. (2) With the rise of memory, pains due to unsatisfied instinct would arise when the more enduring social instincts had been overcome by some temporarily stronger desire. (3) The common opinion of a group, expressed in language, and appealing to the love of approbation due to sympathy, would become paramount as a guide. (4) These factors would be reënforced by habit.

The weak points in the scheme as worked out are due largely (1) to conceiving the moral consciousness too exclusively in instinctive and emotional terms. There is no reference to the part of choice in building up a moral agent. Thought or reason appears in it chiefly in the guise of memory and there is but a hint at an intelligent forecasting of the future, and weighing of values with reference to a purpose or end. There is thus little thought of a self, and the crux of the problem takes the form of setting 'the more *enduring* social instincts' over against the more transient gratifications of bodily appetite or selfish desire. To throw the whole burden of the consciousness of duty on the single precarious support of the greater 'persistency' in consciousness of the social *instincts* would scarcely be possible for one who had read in ethics as thoroughly as Darwin had studied in the organic field.

The second weakness is of a very different sort, and one which all psychology shared until recently. The individual is conceived to a large degree as the unit, endowed to be sure with social instincts and sympathy which make him responsive to public opinion, but not social in the deeper sense which present psychology is working out and which, it is fair to say, carries out with far more adequate analysis the line of thought which Darwin did much to promote.

For the strong point in Darwin's method of approach was first that it gave to the whole theory of moral evolution a concrete setting in a process which was both broadly conceived and definitely evidenced, and secondly that it gave a much broader basis for the social nature of man than had usually been given by those who had considered man apart from animal life. The examples of mutual aid as well as of instinctive craving for the company of other animals of the species gave a fuller content to the term social, while his long study of animal instincts doubtless kept Darwin from becoming entangled in the hedonistic psychology by which English writers had so often been led astray. It is indeed a striking illustration of Darwin's independence and sagacity that he escaped the common fallacy on this point although, as he says, all the authors whom he had consulted, with a few exceptions, held to the hedonistic theory.

A point of greater present interest because it lies much closer to the question of moral standard is the question how far natural selection is an important factor in the growth of morality and the moral sense. On this point Darwin regards his own discussion as 'imperfect and fragmentary.' As already noted many writers in the period which had elapsed between the *Origin of Species* and the *Descent of Man* had broached this question. Wallace had pointed out that although man would be little liable to bodily modifications through natural selection his intellectual and moral faculties would be both variable and highly important, hence there would be a field for natural selection. Bagehot's *Physics and Politics* originally published in 1867–1869 has as its secondary title, *Thoughts on the Application of the Principles of Natural Selection to Inheritance and to Political Society* and is in many ways the most brilliant discussion of the subject which has appeared. This as is well known had emphasized the necessity of coherence, of obedience and law, of the 'cake of custom,' as fundamental elements of strength. 'The frame of their morals' must be 'set by long ages of transmitted discipline' before there can be individual liberty or general freedom of intercourse. There are also other virtues which are selected by conflict. The military virtues may be said to be the 'preliminary virtues.' On the other hand, Bagehot points out forcibly the defects of the selection which depends upon war. "Humanity, charity, a nice sense of the rights of others, it does not foster." Contempt for physical weakness and for women which mark early society are survivals. So too are the metaphors from law and war which make most of our current moral phrases and frequently vitiate what they illustrate. Military morals exaggerate action and discipline, and place too little value on meditation.

Darwin emphasizes the survival value in primitive life of sympathy, fidelity and courage. He points out, however, that within a specific group natural selection would frequently work to preserve those less virtuous rather than the more faithful and courageous. The primitive instinct would be gradually reënforced by purposive aid performed at first from selfish motives. Habits of performing benevolent actions would strengthen a feeling of sympathy and "habits followed during many generations, probably tend to be inherited." A more powerful stimulus to social virtue, however, is the praise and blame of fellow men, and this also rests ultimately on sympathy. With 'an increase in number of well endowed men and an advancement in the standard of morality,' there will be an 'immense advantage' to one tribe over another. "A tribe including many members who from possessing in a high degree the spirit of patriotism, fidelity, obedience, courage and sympathy, were always ready to aid one another, and to sacrifice themselves for the common good, would be victorious over most other tribes; and this would be natural selection." With civilized nations, on the other hand, "natural selection apparently effects but little." "The causes which lead to the advance of morality are rather the approbation of our fellow men — the strengthening of our sympathies by habit — example and imitation — reason — experience, and even self-interest — instruction during youth, and religious feelings."

Noteworthy because of its significance for the present 'reaction,' and especially in view of Nietzsche's denunciations, is the stress which Darwin lays upon sympathy. "Nor could we check our sympathy, even at the urging of hard reason, without deterioration of the noblest part of our nature. The surgeon may harden himself whilst performing an operation, for he knows that he is acting for the good of his patient; but if we were intentionally to neglect the weak and helpless it could only be for a contingent benefit, with an overwhelming present evil. We must therefore bear the undoubtedly bad effects of the weak surviving and propagating their kind."

As we have said, Darwin's own interpretation of the moral standard is not that currently associated with 'Darwinism.' The conception of a purely mechanical process, excluding all 'norms,' is what some find in the evolutionary process as Darwin conceived it. The supreme value of force or might is the lesson which others read in the same process. This makes strength the only virtue and weakness, of which sympathy is a fellow, the only unpardonable sin. A third conception is derived from the process viewed as a series of advancing types. If each lower type finds its meaning in serving as a means for producing a higher type, then man is no longer to be viewed as 'end in himself.' His end is rather to produce the 'Uebermensch.'

We cannot, of course, discuss these theories within the limits of this paper. As to the first, it is sufficient to remark that values are, of course, not to be sought in a process conceived as 'natural' in a sense which excludes self-conscious valuation. To suppose, on the other hand, that the 'mechanism' which 'governs' in nature excludes the possibility of a consciousness that could be 'normative' would be to interpret the 'continuity' of nature in a way to exclude totally all variation. To appeal to a logical value in urging the truth of the doctrine of mechanical evolution, and to use this appeal to deny all ethical valuation is a thinly disguised contradiction. The fundamental points at issue in the other questions are: (1) Granted the evolution of ethical values, has the process been so uniform and continuous that in seeking guiding principles for life it makes no difference what part of the process we consult? To affirm that such must be the case would be again to give no place to variation. It was the merit of Huxley to point out epigrammatically the difference between the 'ethical,' consciously directed process, and the 'cosmic' process prior to conscious activity. (2) Is the valuation of every man as 'an end,' with the corresponding implication of sympathy, an inherently suicidal moral principle? Will it, if followed, inevitably destroy all moral values by destroying all the more valuable strains and races? That there may be developed a science of eugenics is certainly a consummation devoutly to be wished, but until our civilization corrects some of the gratuitous evils which it now opposes to progress, until it plans dwellings, education, and conditions of work so as to remove the obstacles it now opposes to health and strength, it would seem that the obvious lines of effort were close at hand. For Europe and America to remove the degeneration due to poverty and disease among their own peoples would seem a more hopeful agency of progress than the exploitation of weaker races, and if the 'superior' will not continue their own stock, what will it profit to forbid the inferior to continue theirs? It would indeed be contrary to the implications of the evolutionary method to deny the possibility of new variations, of different standards. But if there is to be any standard at all it must be based on a common good. And if this is abandoned, moral values will not be endangered; they will have already disappeared.

THE INFLUENCE OF DARWIN ON THEORY OF KNOWLEDGE AND PHILOSOPHY.[1]

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I.

Under the headings of 'instrumental' and 'genetic' logic[2] the evolution theory has worked its way into the discussion of the higher processes of thought. The theory that thought is an instrument for dealing with social and practical situations — for solving problems of adjustment and truth — has given to discussions of knowledge and reality a new and vital interest. All knowledge remains experimental until it is confirmed, and it can be confirmed only by a resort to trial in the domain of its appropriate application. This leads up to two very important positions in the newer logic: a view as to the nature of truth on the one hand, and a view on the other hand as to the nature of the 'laws of thought,' the so-called categories or 'schemes,' in which the mind builds up and systematizes its acquisitions.

The theory of truth becomes either one of extreme 'Pragmatism' or one at least of 'Instrumentalism.'

Instrumentalism holds that all truth is tentatively arrived at and experimentally verified. The method of knowledge is the now familiar Darwinian procedure of 'trial and error.' The thinker, whether working in the laboratory with things or among the products of his own imaginative thought, *tries out* hypotheses; and only by trying out hypotheses does he establish truth.

Here Darwinism gives support to the empiricism of Hume and Mill and forwards the sober British philosophical tradition. And no one illustrates better than Darwin, in his own scientific method, the soberness, caution, and soundness of this procedure.

[1] Abstract of part of a paper on 'Darwin and the Mental and Moral Sciences' prepared by request for the Darwin Celebration of the American Philosophical Society.

[2] See Dewey, *Studies in Logical Theory*, and Baldwin, *Thought and Things or Genetic Logic.*

Scientific method, therefore, becomes, when the full implications of the matter are thought out, the exhaustive epistemological method; that is, we must hold that there is no method of reaching results to be called truths, which is not found, when genetically considered, to go back to the fundamental processes of experimentation. There is no royal road to truth; no golden rule of revelation or inspiration by which the philosopher can deduce the 'universe and the contents thereof.' The ambitious *Naturphilosophie* of the last century remained barren and speculative until, through the development of experimental and evolutionary science, it became *Naturwissenschaft*.

But what shall we say of the principles of knowledge itself? Are there no final *a priori* and absolute tests of truth such as we are accustomed to find in 'identity,' 'consistency,' and 'sufficient reason'? Are there no constructive categories which do not themselves owe their establishment to experiment?

As to the categories — here again instrumentalism has its adequate reply; and its reply is strictly Darwinian. These, too, it replies, the categories, are principles which have been saved from numberless possible variations of thought in the course of racial evolution. They represent selections, adjustments to the natural situations which have confronted the mind. They are rules of systematization found useful for thought and experience, for individual knowledge and practice, and for common social belief in the vast stretches of history. The mind has built up a structure, as the body has; and by a similar method: that of tentative and experimental adjustment, followed up by the correlated organic structure fixed by selection.

It is here that Herbert Spencer's most valuable intuition appears—a conception to be placed beside that of Darwin's. The weak point in Spencer's harness, however, was his resort to Lamarckian inheritance for the fixing of the rib-structures of mind. But for the theory of knowledge, the result is the same. The most absolute and universal-seeming principles of knowledge, viewed racially, are 'practical postulates' which have been woven into human thought as presuppositions of consistent and trustworthy experience. They were 'original ideas' at some time, found to be useful for the organization of knowledge and for the conduct of life; and, now, by processes of reflective abstraction, they are set up as schemes or forms divorced from the concrete contents which alone gave them their justification and value, and called 'the Categories.'

So far we may recognize the two great conquests of the instrumental or experimental logic. It holds that *all truth is confirmed hypothesis*, and that all *reason is truth woven into mental structure.* These two great formulations are handed over to philosophy. Both are Darwinian. The first cites the selection of ideas for their utility in the individual's development; the second cites the 'coincident' racial selection that fixes them in the constitution of the mind.

But a more radical point of view is possible. What is now known as pragmatism proceeds out from this point. It is pertinent to notice it here, for it offers a link of transition to the philosophical views with which we must briefly concern ourselves.

Pragmatism[1] turns instrumentalism into a system of metaphysics. It claims that apart from its tentative instrumental value, its value as guide to life, its value as measured by utility seen in the consequences of its following out, truth has no further meaning. Not only is all truth selected for its utility, but apart from its utility *it is not true.* There is no reality then to which, whether humanly discovered or not, truth is still true; on the contrary, reality is just and only the system of beliefs found useful as a guide to life.

I wish to point out that, in such a conclusion, not only is the experimental conception left behind, but the advantages of the Darwinian principle of adjustment to actual situations, physical and social, is lost; and if so interpreted instrumentalism defeats itself. This appears as soon as we analyze any situation involving trial and error. Trial implies a problematical and alternative result: either the success of the assumption put to trial or its failure. When we ask why this is so, we hit upon the presence of some 'controlling' condition or circumstance in

[1] The authoritative exposition is James' *Pragmatism*. I do not hold the author, however, or any other one writer to the statements made in my text in exposition of this chameleon-like theory. My full criticism may be found in the article 'The Limits of Pragmatism,' PSYCHOLOGICAL REVIEW, Vol. XI., pp. 30 ff.

the situation — some stable physical or social fact — whose character renders the hypothesis or suggested solution either adequate or vain, as the case may be. The instrumental idea or thought, then, has its merit in enabling us to find out, to locate, facts and conditions which are to be allowed for thereafter. These constitute a *control* of knowledge, a system of things discovered. Now we may, indeed, say that nothing of what we think can be considered real except what has been actually discovered; but we cannot go on to say that it is the discovery that makes it real. For if that were true what account could we give of this painstaking and often most laborious process of gradual correction and proof? — what account, that is, of the 'control'?

I know there are ways of replying to this criticism — ways of reducing the environment and its controlling facts to the level of postulates of earlier personal or racial experience. But while not finding these replies effective, I may simply say — confining the discussion to the Darwinian text — that the method of selection by trial and error requires that relatively greater stability, fixity and permanence be in the 'control' conditions, in the environment, and finds the genesis of truth in the gradual checking off of hypotheses under this more stable control. This supports instrumentalism, but it does not support pragmatism. I may 'bring about' reality apparently without this external control, by 'willing to believe' in something for which I have no proof or reason, in cases in which the sort of event willed — as for example, some one's else conduct — may be conditioned upon my act of will. But nature does not take to suggestions so kindly. The will of a general may stimulate his troops and so bring to him the victory he believes in; but such an act of the general's will cannot replenish the short supply of powder or shells, on which the issue of the battle perhaps more fundamentally depends.

In one other respect the newer view is transforming the theory of knowledge, a respect in which it shares with political and social science the impulse of Darwinism. I refer to the point of view from which the unit of knowledge, as of practice, is no longer to be found in an isolated and self-regulating individual. Covering both the logical and the political aspects of the topic by the single term 'Community,' I may discuss the topic under that heading.

Community.[1] Work in social psychology has greatly modified the notion of the individual. The individual is found to be a social product, a complex result, having its genetic conditions in actual social life. Individuals act together, not alone — collectively, not singly. In short, the selective processes that have molded the individual, both racially and in his personal development, have turned on collective utilities. When interpreted in the political sciences this discovery shatters, at one blow, the historical theories of individualism, which make such motives as personal contract, individual competition, etc., the fundamental springs of human conduct, in its social relations, and the sources of government. Instead of a social contract, there is a social growth; the only contract is the one-sided one that assigns the too-individualistic thinker or actor to the jail or the asylum. Instead of government only with the 'consent of the governed,' we have government by the few or by the many *with or without* the consent of the rest. In this, and in the more 'socialized' view of social competition and rivalry, and in the new view of social transmission considered as a process which largely replaces physical heredity, both in its content and in its method, we find summed up the enormous debt that political science, together with the other social sciences, owes to researches carried out in the spirit of the selection theory.

In the theory of knowledge the same general truth appears, and it is for this reason that I place the two cases together. In the social sciences and in the theory of knowledge 'community' or some equivalent term is henceforth to be the watchword.

In the theory of knowledge it appears in the social reference that all knowledge implies. It is now the problem to find any knowledge that is psychologically private, not to find knowledge that is common and public. Individual judgment

[1] The two sorts of 'community' indicated in what follows are worked out by the present writer in detail elsewhere; that of the social life in *Social and Ethical Interpretations* (4th ed., 1906) and that of knowledge, in *Thought and Things*, Vol. II.

and sentiment is everywhere rooted in social life — in education, tradition, convention — and it becomes a problem of knowledge, as it is of ethics, to show how it is possible to 'be a Daniel,' and 'to stand alone.' The result is that the subjectivistic theories of knowledge, like the individualistic theories of political science, are soon to be laid away in the attics where old intellectual furniture is stored. The knower does not start out in isolation and then come to some sort of agreement with others by 'matching up' his world of independent sensations and cognitions with theirs. On the contrary, he starts with what his and his neighbor's experience in common verify, and only partially and by degrees does he find himself and prove himself to be a relatively competent independent thinker. The theory of the 'communities' or *common validities* of knowledge, and that of the corresponding 'communities' or *common interests* of society, is our new possession; and we owe them to the genetic researches which the Darwinian spirit and method have inspired.

II.

In coming to a conclusion as to the influence of Darwin's thought on philosophy, we should first sum up the general results of Darwinian views in the different branches of knowledge with which philosophy deals. If we look upon philosophy as many do as simply the broadest and most unified view that we can get of the world as a whole, it is evident that our task will be to set together the results of the more partial disciplines, the results reached, that is, by the sciences of fact and value. This leads to the body of theory embraced by philosophy. Accepting this as a general statement of the problem of the content or matter of philosophy, a second great question remains in the determination of philosophical method. I shall take up the latter question first.

Philosophical Method. In an earlier address, in which the history of psychology was briefly outlined,[1] I took occasion to point out that an epoch in the progress of that science was inaugurated with the absorption of Darwin's point of view; and

[1] Address prepared for the St. Louis Congress of Arts and Science, printed also in the PSYCHOLOGICAL REVIEW, Vol. XII., pp. 144 ff.

this because a revolution was produced in psychological method. Psychology has always been the vestibule, as it were, to philosophy, and advance in the latter never gets far beyond that of the former. So when psychology adopted seriously a naturalistic and positivistic method — the method, that is, of the positive sciences of nature — philosophy had also to recognize the generality of these points of view. Philosophical truth, like all other truth, must be looked upon as truth about nature — the nature of the world and the nature of man — and its progress is secured through reflection exercised under the control of the positive instruments and methods employed in those subjects. Purely deductive, speculative and personal systems of philosophy may be useful as gymnastics and profitable as sources of individual fame; but the genuine progress of philosophy is to be looked for only through those methods of confirmation and proof which control the imagination and permanently satisfy the logical and other demands of common reflection. There may be different philosophies, but like rival scientific hypotheses, each must show the array of facts, aims, motives, values, etc., that it can explain better than any other.

In these directions Darwin has strongly influenced modern philosophical thought; so strongly that the historical issues of philosophy have taken on new forms, which, in the new names now in vogue to describe them, are unfamiliar to the old-school philosophers. Instead of the problem of 'design,' we now have discussions of 'teleology'; instead of the doctrine of 'chance,' we now have the 'theory of probabilities'; instead of 'fatalism' and 'freedom,' we now have 'determinism' and 'indeterminism' variously qualified; instead of 'God,' we hear of 'absolute experience'; instead of 'Providence,' of 'order' and 'law' instead of 'mind and body,' of 'dualism or monism.' Not that all this shifting of emphasis and change of terms are due to Darwin; but that they are incidents of the newer antitheses current since the mind has been considered as subject to 'natural law,' and the world, including God and man, as common material for science to investigate. Scientific naturalism and positivism are methods of unlimited scope; and the question of philosophy is, what does the whole system of things, of external facts and of human values alike — when thus investigated — really turn out to mean?

I may illustrate this by considering in more detail a central problem — one common to biology and psychology alike, and one whose answer colors the whole of one's philosophy. It is the old problem of 'design' debated in biology under theories of 'special creation' and 'chance,' and now discussed, alike in biology and psychology, in the form of questions of 'vitalism' and 'teleology.' In what sense, if any, is the world — and in it, life and mind — an ordered, progressive and intelligible whole? And if it is such in any sense, how did it become so? Is it due to intelligence? — and if so, whose intelligence? The most violent controversies aroused by the publication of the *Origin of Species* were let loose about this question. Darwin's opponents said 'chance,' 'fortuitous or spontaneous variation,' was to take the place of Providence, intelligent creation, God. If there be no rule of selection and survival save that of utility, and no source of the useful save the overproduction of chance cases, where is the Guiding Hand? Does not Natural Selection dispense with a ruling Intelligence altogether?

We have only to realize the present-day statement of this problem to see the enormous range of concession to naturalism the theory of Darwin has forced. Instead of 'chance' in the sense of uncaused [1] accident we now have the notion of 'probability,' a mathematically exact interpretation of what is to superficial observation fortuitous and capricious; and instead of an interfering Providence, we have universal order born of natural law. And it is within such conceptions as these, *now taken as common ground of argument*, that the discussion of teleology is conducted. The world is no longer thought of as a piece of mosaic work put together by skilful artificers — as the old design theory looked upon it — but as a whole, a cosmos of law-abiding and progressive change. A philosopher who knows his calling to-day seeks to interpret natural law, not to discover

[1] Darwin himself described 'spontaneous variation' in these words (*Descent of Man*, ed. cit., p. 49): 'provisionally called spontaneous, for to our ignorance, they appear to arise without any exciting cause.'

violations of it. The violations, if they came, would reduce the world to caprice, chance and chaos, instead of providing a refuge from these things.

So Darwin's view, while giving a 'black eye,' so to speak, to theories of chance and special creation, both equally desultory, capricious and lawless, replaced them once for all with law. It indicated the method of operation by which the progressive forms of nature are evolved in stages more and more fit and reasonable. The operation of such a law is no less and no more 'rational,' no less and no more 'fatalistic,' no less and no more 'atheistic' than that of any other law physical or mental. What law — meaning simply what regular method of change — is operative in nature, and what its range, as compared with other such laws — this is entirely a question of fact, to be determined by scientific investigation. And how far the method or law called by Darwin 'natural selection' goes, what its range really is, we are now beginning to see in its varied applications in the sciences of life and mind. It seems to be — unless future investigations set positive limits to its application — a universal principle; for the intelligence itself, in its procedure of tentative experimentation, seems to operate in accordance with it.

Again, it is in connection with this question that we are beginning to see how intelligence may, and does, work within the limits of law, effectively doing its work without violating the universally natural order. The statistical treatment of cases by newer methods[1] shows that events due to intelligence, on the one hand, and those observed to fulfil law on the other hand, fit into the same curves of distribution, if a sufficiently large number of cases of each be taken for treatment. Events involving social and voluntary factors — as crimes such as suicide,[2] the size of families,[3] each for itself depending upon the intelligent and free choice of individuals — when taken in the mass, follow the same laws of number and variation as do purely physical events in which there is no element of conscious determination. If this is so, we need not suppose any essential difference in

[1] See especially K. Pearson, *The Chances of Death*, Vol. I.
[2] See the works of Morselli and Durkheim, on 'Suicide.'
[3] See Pearson, *loc. cit.*

the results in the long run; but may take our choice as between a purely mechanical interpretation of all the cases, or an interpretation of them all as involving a deeper and more immanent principle which works by both methods. In other words, it is not a teleology of the human type, working individually and tentatively against nature, that our philosophy must recognize, but mind in the larger sense of a principle whose mode of operation is in and through the reign of natural law.

One other instance may be cited to show how the evolution theory is serving to bring about a revision of the older philosophical conceptions. The notion of 'cause,' as held by the earlier more dualistic philosophies, has been transformed with the advent of a broader naturalism.

Cause. — An objection to Darwinism, in the early days, was one that held in effect that natural selection left no place for 'freedom' or intelligent initiation, but reduced all the sequences of nature to the level of 'cause and effect' interpreted, as a mechanical principle of the transfer of physical energy. It was held that all movement, the entire dynamic and genetic aspect of nature, became merely a series of compositions and recompositions, of transformations and retransformations, of a certain physical or energetic stuff. 'Matter in motion' was the formula of 'cause and effect.' On further consideration, however, we begin to see how to make articulate our protest against this most superficial generalization. 'Cause' is a broader conception than 'energy.' Only when quantitatively considered are natural sequences exhausted by merely mechanical change. Qualitative differences are as universal and natural as are quantitative identities. There must be a revision of the notion of causation, to allow for the actual growth processes of life and mind, for the new modes of qualitative appearance that the genetic or developmental series of changes show. All vital, mental and social series of changes are of this sort: they are really dynamic, genetic. A psychological effect is not 'equivalent' to its antecedent conditions, considered as its cause, nor in any way identical with them in a quantitative sense. In what sense can we say — and still be intelligible — that a choice is equivalent or equal in energy to the antecedent motives of the agent? In what intelligible sense can an organic adaptation, upon whose utility the subsequent cause of evolution possibly depends, be said to be a mere transformation, equivalent in energy to the mechanical forces that condition it? We are really dealing here with a different sort of change — with genetic change, with growth and development. We are dealing with qualitative, not quantitative conceptions; with modes of appearance and organization, not with units of energy; and we must recognize the making of new modes of quality in every genetic movement of nature. *Nature achieves novelties;* there is, qualitatively speaking, *more in the effect than there is in the cause.*

This position is forced upon us by the radical acceptance of evolution. Spencer tried to subject the whole evolution movement to the mechanical conception of causation; and he failed most signally. He interpreted all development in terms of successive transformations of energy. Thus life and mind alike were eviscerated of all their richer significance. So soon, however, as we give genetic change a significance as fundamental as mechanical change, we reach a very different result. Every genetic change ushers in a real advance, a progression on the part of nature to a higher mode of reality. *Actually new things — novelties — are daily achieved in life, mind and society.* Mechanical causation, physical energetics — these are the poorest and least interesting facts of nature. They are instrumental conceptions, fruitful in science; but along with the processes which these concepts generalize, go the dynamic, genetic, evolutionary modes of condition and consequent, which are equally actual and, in a comprehensive philosophy, infinitely more far-reaching and significant.[1]

The objection, then, that Darwinism reduces life and mind to physics, is quite beside the mark. On the contrary, the very radicalness of Darwin's conception, in forbidding any compromise with vitalism, accidentalism and all forms of obscurantism, has compelled the recognition of progressive movement, of real

[1] This point of view, developed by the writer under the heading of 'Theory of Genetic Modes' (*Development and Evolution,* Chap. XIX.) is brilliantly and forcefully presented by Professor H. Bergson in his work *Évolution Créatrice.*

evolution, as of the profoundest essence of nature. The reign of physical science and of mechanical law over the scientific and philosophic mind is over now, at the opening of the twentieth century. We have been hypnotized by the term 'energy' long enough.

These illustrations may suffice to show with what stones philosophers are laying the foundations of a new idealism. I may not now develop the matter further, since my topic has its limits in the influence of Darwin. But it is easy to see that with these two conceptions — an immanent principle of change, issuing in modes of reality which are progressively more and more significant for the demands of intelligence and life — the way is open for an interpretation of the world in terms of an organization of which progressive self-integrating experience is the type.

It is sufficient in this place to have shown that, in the working out of such an interpretation, the naturalism of Darwin has been and will be an important factor.

If, in conclusion, a brief statement were called for of the sort of influence Darwin has exercised on modern thought, I should sum it up in somewhat the following terms: Darwin gave the death-blow to uncritical vitalism in biology, to occultism in psychology, and to mysticism and dogmatism in philosophy. Each of these, alike progeny of the obscurantism of dogmatic thought, has in turn yielded before the conception of natural law and order embodied by Darwin in the theory of natural selection. This theory turns out to be not merely a law of biology as such, but a principle of the natural world, which finds appropriate application in all the sciences of life and mind.

N. S. Vol. XVI. No. 4. July, 1909.

THE PSYCHOLOGICAL REVIEW.

VISUAL ILLUSIONS OF DEPTH.

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Visual illusions of movement in a lateral direction, *i. e.*, in some direction at right angles to the line of sight, have often been the subject of psychological description and experimentation. Comparatively speaking, we may say that the number of discussions of such illusions is legion. Illusions of distance are numerous and often commented upon in the literature dealing with the various criteria of visual depth. By 'illusions of distance' are meant those phenomena wherein objects appear to be located nearer to, or farther away from, the observer than they actually are, *e. g.*, the apparent nearness of a mountain peak in a rare and clear atmosphere. Illusions of *movement* in depth, *i. e.*, where the object appears to *move* nearer or farther away, are but rarely met with in the literature. Whether this be due to the fact that such phenomena are rare, have escaped notice, or possess but little psychological value, I do not know. Certain it is, however, that such illusions are rarely mentioned.

In an experimental attempt to evaluate the influence of brightness in the perception of depth, Ashley [1] found that a change in the brightness of an object mediated a consciousness of a third dimensional movement. Increase of brightness caused the fixated object to appear to move toward the observer, while a decrease in brightness produced an apparent movement in the opposite direction.

When two similar objects are binocularly combined and their distance apart is gradually altered while the observer attempts to maintain unity of vision, a pronounced third dimensional

[1] Psych. Rev., Vol. V., p. 595.

motion on the part of the combined image is noticeable. This fact has been known for some time. By using a pair of compass points, one can give a ready demonstration of the influence of convergence and accommodation in the perception of distance. Dr. Bell has recently utilized this principle in studying the relative importance of accommodation and convergence.

While looking at a near object, a faint suggestion of forward and backward movements can be produced by successively intercepting the vision of one eye by a screen. The illusion is supposed to be due to a consequent alteration in the degree of convergent tension. A short account of the phenomenon is given by James.[1]

Third dimensional movements may be produced by either monocular or binocular eye closure, by finger pressure on the eyeballs, by a slight traction on the eyelids, and by forcefully opening the eyes to their widest extent. The presence, direction and extent of the illusory movements due to these causes vary with individuals, the position of the eyes in the socket, etc. The phenomena have been described and discussed by the writer in a previous article.[2]

In fainting spells, receding movements of the visual field occur with some subjects. Just preceding the loss of consciousness, perceived objects are seen to move backward to far distant positions. A similar illusion is said to occur during the loss of consciousness in etherization. James [3] quotes from M. Taine an account of an insane patient describing a similar receding illusion: "Objects grew small and receded to infinite distances — men and things together. I was myself immeasurably far away. I looked about me with terror and astonishment; the world was escaping from me. . . . I remarked at the same time that my voice was extremely far away from me."

Illusory movements in depth are voluntarily produced by some people. The gift is quite rare however. Eight such cases have been described by the writer in previous articles.[4]

[1] James, *Principles of Psychology*, Vol. II., p. 92.

[2] Carr, 'A Visual Illusion of Movement during Eye Closure,' PSYCH. REV., Mon. Sup., Vol. VII., No. 3.

[3] James, *Ibid.*, I., p. 377.

[4] PSYCH. REV., Vol. XIII., p. 258, and Vol. XV., p. 139.

With these people the illusion frequently occurs involuntarily in their normal experiences.

In some experiments involving binocular combination of slightly dissimilar figures, Hyslop[1] found that the depth location of the different parts of the perceived object could be varied relative to each other by changes of the attention. This is similar to the customary illusions of reversible perspective which are also examples of depth illusions.

The above list of illusions represents those cases which the writer remembers having noticed in the literature. It makes no pretence at exhaustiveness, nor at systematization in a general explanatory scheme. Given the possibility of such a variety of these illusions under *special* conditions, it would seem that some of them should also occur with some people in their everyday experiences. With this idea in mind, the writer made inquiries of the members of his classes in psychology for all cases of third dimensional illusions occurring at any time during their life. All persons responding were interviewed and subjected to a thorough cross-examination on the nature and conditions of the phenomena reported. Sometimes it developed that the occurrences described did not belong to the class of illusions desired, or else that the experiences had been so vague, fleeting, or rare, that the observer's memory of the phenomena was too indefinite and hazy in character for a trustworthy account. Such cases have been eliminated; all of the accounts given below represent cases where memory was definite and precise on the points mentioned. Each account represents all the illusions of this general kind which the observer can remember having experienced at any time during his life, with a general expression as to their frequency of occurrence, their nature and conditions.

In a series of classes comprising 350 students, I found 58 persons who have experienced involuntary depth illusions at some time of their lives. Five of these persons also possessed complete voluntary control over the phenomena and their experiences have been described previously. Of the 53 persons with whom the illusion only occurred involuntarily, I

[1] *Mind*, Series I., Vols. XIII. and XIV.

have been able to obtain detailed descriptions from 48, and these cases form the subject-matter of the present paper. Since the illusions have occurred involuntarily, any experimental investigation of the phenomena has been impossible. There is no uniformity in these experiences as regards their nature or conditioning circumstances. Hardly any two are exactly alike in all of their features. An attempt will be made to convey an adequate conception of the illusions by classifying them on the basis of a series of rubrics, giving detailed descriptions for purposes of illustration. In conclusion the significance of the illusions in regard to the perception of depth will be considered.

1. *Character of the Illusion.* — The illusion may belong to one of four types: (*a*) An illusion of pure distance. The objects appear to be located at varying distances from the subject but no movement is perceived. An object is first seen at its true distance, is next perceived close in front of the eyes, and is then seen at a very remote position. Twelve cases belong to this type (see VIII., XIII. and XX.). (*b*) Illusions of pure motion. Objects are perceived moving in a certain direction without any apparent change of location. They move but do not traverse space. This type is represented by two cases (see IX.). (*c*) Illusions of movement involving a change of location. The objects seem to move toward or away from the subject, both the motion and the change of location being distinctly perceived. Twenty-five persons reported this type of experience (I., II., IV. and V.). (*d*) Eight persons reported a combination of the first and third types. The object first *moves* away from its true location and is perceived in some remote position. After a short time the object suddenly *appears* back in its original location but this change of position involves no sense of motion (VI., XI. and XX.). The reverse case occurs in which an illusion of pure distance is succeeded by a return illusion involving the perception of movement (XVII.).

2. *Extent of Visual Field Involved.* — (*a*) Twenty-four persons reported that the illusion involved all objects in the visual field and that no contraction of the field was apparent. (*b*) With five subjects there was an invariable peripheral contraction of the field and the illusion involved all visible objects in

the central portion. The degree of contraction varies with the subject and with the different experiences in the same subject. The periphery is perceived as black, as a homogeneous light gray haze, or it may be a mere void without sense content (XI.). (*c*) Six persons were uncertain as to the periphery; some thought that it remained visible but were uncertain as to its participation in the illusion; others were uncertain as to its visibility. All were merely confident that the fixated objects were subject to the illusion (V. and XXI.). (*d*) With fourteen subjects the peripheral objects remain visible and stationary at their true position, while the central portion of the field participates in the illusion. It may occur that the fixated object moves in relation to other objects in the line of sight. As an example, we may cite an illusion that occurred only in church, wherein the preacher was perceived to move back through the wall and remain visible in this position for some time. The illusion occurred frequently and this striking feature caught the subject's attention (XIX.). With one subject, the central portion of the field remained stationary while only certain parts of the periphery participated in the illusion (VIII.). With one subject the illusion sometimes involved the whole and sometimes only a part of the visual field. This fourth type of experience is illustrated by the following account:

I. The illusion was noticed twice one year ago. It occurred both times under the same circumstances. The observer was looking down a street which ended a block away; a row of houses formed the background at the end of the street. The illusion occurred during day time and the weather was bright and clear. The observer was standing talking to and looking directly at a companion but a short distance away. Soon this person began to move slowly backward down the street until she reached the background of houses at the end, and then slowly came back to her original position. The movement in *both* directions was distinctly perceived. During the illusory movement there was no vagueness of outline or contour, no blurring or confusion of features; the person observed seemed distinct and substantial in character during the illusion. The subject felt that she continued to look directly at the person during the movement; she did not seem to be looking beyond her. The subject has noticed the confused vague appearance presented by persons when one looks beyond them, but in this case the person did not present this appearance. The perceived object moved in relation to surrounding objects; there was no movement of the visual field as a whole. The person decreased in size during the backward movement. She appeared about one half of her normal size when at the end of the street. The size increased during the forward return movement. This change of size was very evident and caught the observer's attention at once. The perspective appearance of the street came out distinctly during the illusory motion, *i. e.*, the houses at the end of the street seemed to be smaller than the houses nearer by in proportion to their distance. The scene looked the way it would need to be drawn.

The illusion was at no time subject to voluntary control in any respect. At my suggestion the observer has since tried to repeat the illusion under similar circumstances, by voluntarily imagining such movements, but she was unsuccessful in obtaining the slightest suggestion of motion (also see VIII., XII. and XIX.).

3. *Kind of Images Involved.* — (*a*) With forty-two people the illusion involved normal perceptual objects. (*b*) There are five cases in which the illusion occurs in dreams. With four people the experiences occur only in dreams. Such a case is described in No. III. (*c*) There are three cases wherein hallucinatory images are involved. The following account represents the type:

II. The subject is slightly neurasthenic and hypochondriacal. During conditions of feverish semi-delirium, indistinct and confused masses of imagery emerge in the darkness and vibrate back and forth rather slowly between the eyes and remote positions. After a time the images disappear.

(*d*) One subject reports that the illusion sometimes refers to visual images under normal conditions. This experience (IV.) is described elsewhere in detail.

4. *Direction of the Illusion.* — Three types occur: (*a*) Illusion of increased distance alone. Objects move to, or appear at, more distant positions and then return to their normal location. Twenty-one illusions belong to this type (I., IV., VI. and VIII.). (*b*) Illusions of decreased distance. This type is reported by twelve persons. The illusion is confined wholly to positions in front of the real location of the object (X.). (*c*) With eleven subjects the illusion involves space on both sides of the real position of the object. The field may move forward close up before the subject's eyes and then back to the apparent distance of the horizon, whence it returns to its normal location (V., VII. and XVII.). (*d*) It is impossible to classify six cases in the above respect, inasmuch as the illusion occurred in dreams or involved hallucinatory images.

5. *Character of the Movements.* — (*a*) With seventeen persons the illusion is always vibratory, *i. e.*, the objects contin-

uously move backwards and forwards between two positions until the illusion is voluntarily destroyed or until it ceases of its own accord. The amplitude of these vibratory movements may vary from a few inches up to the full extent of visible space. The following account is illustrative:

III. The illusion occurs only in dreams. A confused dense mass of imagery slowly takes shape in the surrounding darkness, and begins to vibrate back and forth over an apparent extent of 100 feet. After four or five complete vibrations, the images disappear. The visible mass becomes larger as it approaches and smaller as it recedes. The experience has been repeated quite frequently.

(*b*) With twenty-three subjects the objects move to, or appear at, some definite position and remain there until the illusion is voluntarily destroyed by some means or until it disappears involuntarily. The following description will serve as an illustration:

IV. The illusion with this subject presents several features. All visual objects suddenly recede to the apparent distance of the horizon and remain in that position five to ten minutes. At the end of this period they return to their original position. This return movement is very slow at the beginning, but it gradually increases in rapidity, so that the latter phase of the movement is quite fast. If the subject closes her eyes while the objects are remaining at their distant position, she cannot even *imagine* those objects to be located except at this far distance. The illusion also occurs for visual *images*, when she is thinking of objects in visual terms either with closed eyes or under conditions of a high degree of abstraction from things of sense. These imaged objects behave as do the perceptual objects described above, with the exceptions that the backward movement is much slower, and the objects remain for a longer time at the distant position. The forward return movement is similar in rapidity to the perceptual case. The illusion also occurs in *dreams*, the movements being similar in character to those of the imaged objects.

In all cases the motion in both directions is an actual experienced reality. In no case is there the least voluntary control of the phenomenon. The subject is absolutely helpless as to initiating, stopping, or modifying the course of the illusion in any way. Objects and images decrease in size in proportion to the amount of backward movement and grow larger again on their return movement. The objects do not present any confusion of outline or blurring of features, nor do they become doubled. Persons were generally the objects of attention when the illusion occurred, and the subject maintains that their features remained normally distinct in every respect during the illusion. These experiences have occurred on an average of twice a year ever since she was in the upper grammar grades in school, a period of ten years. She cannot remember their occurring before this time, nor does she know of any sickness or abnormal experience at this time that may have been their cause. The illusion has occurred at all times of the day and with all conditions of illumination, but apparently only under conditions of a rather pronounced fatigue. The experience is always very unpleasant, giving that far-off lonesome feeling of being helpless, and isolated from the world. She generally struggled desperately to bring back the objects to their natural position, but she always failed to move them in the least. With the movements of imaged objects with closed eyes, she can always tell before hand by some vague feeling that the illusion is soon to occur. She could not describe this anticipatory feeling except that it was disagreeable. There was no anticipation of the illusion except in this one case. The subject has never worn glasses nor had her eyes examined, though they seem to be very susceptible to fatigue.

(*c*) The illusion is irregular with five persons. The objects move to, or appear at, a certain position, remain there stationary for a time, undertake another excursion with a stationary period, and so on, until the illusion disappears. As the best description of this type, we give the following account:

V. The illusion has only occurred while reading. The letters suddenly move to some new position and remain there perfectly stationary for a time. They now jump to a new position, remain stationary, and again undergo movement. These irregular transitions in distance may persist during the entire period of reading. The direction of the jumps is irregular and the letters may move either in front of or behind their real location. The letters not only seem to *move* but they also look nearer or farther away. The letters become larger as they approach and decrease in size as they recede. No change in distinctness or vividness is involved. The illusion may occur shortly after beginning to read. Fatigue and steady fixation are not essential to its occurrence. The phenomenon occurred much more frequently in early life than in late years. At first it interfered with reading to a considerable extent but its disturbing influence was soon neglected. The subject has never experienced any trouble with her eyes. The subject could not remember with any degree of confidence as to whether the printed characters alone moved, or the illusion embraced the book and surrounding objects. She is under the impression that the illusion was confined to the letters.

6. The extent of the illusion varies markedly according to the subject, though it is more constant for any one person. With 23 persons the illusion is medium in length — 10 to 50 ft. The extent is less than this with eleven persons and greater with ten. The smallest illusions represent vibratory movements of but a few inches, while often the images move from the apparent position of the horizon clear up to the face. The rapidity of the movements also is subject to wide variations.

7. The frequency of these experiences varies between wide limits. Several persons have experienced the illusion but once. Others have experienced them on an average of three or four times per month throughout their lives. There is practically equal distribution as to frequency and infrequency of occurrence.

8. *Size of Objects.* — The nearest approach to uniformity in these experiences is in regard to the changes of size of the images in relation to the direction of the movement. (*a*) The usual law is that objects grow larger as they approach the observer and decrease in size as they recede. The change of size seems proportionate to the distance according to the law of perspective. Thirty subjects reported this fact. (*b*) Fifteen persons were uncertain upon this point. This is due to the facts that the extent of the illusion was very small, or that the illusion occurred so rarely or so early in life that their memory for details is defective. (*c*) With three subjects the objects became smaller as they approached the observer. This occurred only for those illusions which involved some patterned object. This is the usual result for binocularly combined images of regularly patterned objects, and hence these cases are not to be regarded as an exception to the above rule. (*d*) One subject reported that the images did not change in size. She is very positive in this regard and her statements are to be regarded seriously inasmuch as the illusion has occurred very frequently all her life and the movements were slow, realistic and of great extent. A detailed description follows:

VI. With this subject[1] the whole visual field moves backward until the objects reach the approximate distance of the horizon. The movement varies in rapidity for the different cases; sometimes it is extremely rapid and sometimes very slow, but as a general rule its velocity appears to be that of a brisk walking rate. The objects do not change in size, neither do they become blurred in appearance nor confused in outline. After this receding movement, one of three things occurs: (1) the objects remain visible and stationary at their distant position. This occurs but rarely; (2) the objects seem to move back into a light hazy cloud and disappear from view as though swallowed up by a dim veil-like mist. This distant background of haze remains in view during the continuance of the state; (3) all consciousness of visual space disappears at the termination of the receding movement; the subject becomes temporarily blind. This latter condition obtained in the majority of the experiences. The illusion terminates in either of the three cases by the objects suddenly appearing back in their original positions. They never *move* forward even in the case where they remain continuously visible; they always move away from the observer, but get back again without motion.

This subject is afflicted with hysteria and the illusion is an invariable accompaniment of an incipient trance which has been of very frequent occurrence

[1] This experience has been described more fully in the *Journal of Abnormal Psychology*, Vol. II., p. 260.

from the ages of six to twenty-two years. It has occurred at all times of the day and with all conditions of illumination in the room. The phenomenon has occurred only while the subject has been lying down for rest during a condition of marked fatigue and while the subject is in a state of complete mental and physical relaxation. These conditions, however, do not necessarily produce the phenomenon. The subject possesses no voluntary control over the course of the phenomenon. During the illusion she is always afflicted with a complete paralysis of all voluntary movements. The experience was always intensely frightful.

9. *Distinctness of Visual Objects.* — (*a*) No change in the distinctness of visual objects was reported by nineteen persons. These subjects are usually very positive in this regard. The images generally retain their normal vividness and realistic character. Persons are often the object of attention in these illusions, and it is maintained that every detail of their features remains in distinct view. Cases I., IV., V. and XIII. furnish illustrative examples. (*b*) Fourteen people were unable to give information on this point for various reasons: The mass of imagery was generally indefinite in contour and surface when it was of hallucinatory origin. It was impossible to answer the question in some cases because the illusion occurred during a condition of dizziness. Defective memory was responsible in six cases. (*c*) Fifteen people reported changes of distinctness of varying degrees. Theoretically, these changes may be due to an (1) imperfect ocular adjustment with a consequent blur of surface and contour, or (2) to a decrease of intensity resulting in mere vagueness. (3) An irregular decrease of intensity might result in a confusion of surface and contour which could not be discriminated from that resulting from imperfect ocular adjustment. It was extremely difficult to obtain from the subjects so definite and accurate a description of this aspect of the experiences as to allow a confident opinion in every case as to the essential conditions. Both conditions obtained, though the blur characteristic of defective adjustment seems to be the more frequent. The following cases illustrate each of these types:

VII. With this subject the illusion assumes diverse forms. Sometimes upon suddenly glancing at distant objects, they are seen located only a foot in front of her eyes. She does not first perceive them at their distant position and then see them move nearer; they are immediately perceived in front of her eyes, so close that she feels that she can reach out and touch them. They now begin to move away to their natural position, and they may occasionally move on beyond it, this being followed by a return forward motion. When first seen, the objects are very blurred and the subject judges as to their real position by the degree of distinctness secured.

Again, she may first see the objects at what she regards as their true position, and they begin to move shortly after noticing them. They may also be first seen at their real location but are already in motion when first noticed. This motion may be either forward or backward in direction, will continue for some time, and then become reversed in direction, the objects returning to their true positions. On the return movement, the objects occasionally move beyond their real location for a short distance and thus undergo a second return. If the subject catches the objects on their first movement, she can voluntarily reverse the direction of motion; for example, if the objects are first seen moving forward, she can stop this and send them backward even far beyond their true distance. She knows of no conditioning circumstances which will explain why objects are seen moving forward in one experience and backward in another. She is also unable to describe in any way her method of voluntarily effecting this change of direction.

When objects are moving rapidly forward when first perceived and her visual attention is rather widely dispersed, she feels that the whole world is collapsing from every side toward her as a center, as if to crush her. All objects from above and below, from right and left, as well as those directly in front, are swiftly rushing toward her as a common focus, a condition which is described as being terrifying. Under these conditions she always is afflicted with the unpleasant sense of being crushed and overwhelmed in the onrushing avalanche of the universe.

In all of the illusions the movement refers to the *entire* visual field. The objects always change in distinctness, but she has never noticed any doubling. The maximum of distinctness is the criterion by which the real location of the field is determined. The size of the objects varies in proportion to their apparent distance from the observer. The illusion has occurred at all periods of her life, at any time of the day, and under various conditions of illumination. It has been more frequent out of doors during the daytime, and while looking at relatively distant objects.

VIII. The illusion is one of distance and occurs only during a condition of mental abstraction and steady fixation. The fixated portion of the field remains clear cut and distinct, and at its proper distance, while other objects in the field become faded and vague, and *appear* far away. For example, she has seen the knob of a door remain distinct and at its true position while the remaining portion of the door almost faded away and was perceived far beyond the plane of the knob. The illusion occurs very infrequently and is destroyed by head or eye movements.

The subject also experiences a similar auditory illusion which is rather unique. Sounds vibrate quite rapidly between their true location and some very remote position. The apparent loudness of the sounds varies with the distance, becoming fainter as they recede. The intensity variations are very striking and were described as 'pulsations' and as 'rising and receding swells of sound.' The illusion occurs only during a condition of mental abstraction. Sometimes she can produce the auditory illusion at will by throwing herself into the proper mental condition. The experience occurs involuntarily quite frequently.

10. *Essential Conditions of the Illusions.*—(*a*) With six persons, the illusion is apparently due to external conditions alone. For example, when two persons experience the illusion simultaneously, it is evident that the determining conditions presumably lie in the objective situation.

IX. The following illusion was observed but once, but by two persons simultaneously. It occurred in the hilly country of the Peekskill region. The time was about three o'clock in the afternoon of a bright sunshiny day in the spring, about a half year before this account was related to the writer. There were two parallel ranges of hills, the upper part of the more distant one being seen over the top of the nearer one. The two observers were walking along a valley road which ran parallel to and near the first range. Looking over this first hill they could see the green but hazy top of the second range set off strongly against the bright background of sky. Under these conditions the second range of hills and the sky background were perceived to be continuously moving backward, although the first range appeared stationary. The apparent motion was so real and striking in character and persisted so continuously that they both noted it independently and discussed the illusion at the time. Although the range kept moving backwards continuously, it did not appear to get any farther away; it seemed to remain at the same distance. After turning away their eyes and again fixating the distant hills, the illusory motion still persisted. It continued while walking along the road and persisted while this particular conformation of the landscape obtained. It was judged that the illusion was visible for at least ten minutes. Both observers had been in this particular situation before but had never seen the illusion until this time. My informant has good eyesight, does not wear glasses, and has never experienced any other illusory movements in depth.

(*b*) Internal conditions alone are apparently responsible for the illusion with twenty-four people. As an example the following case is self-evident:

X. The illusion occurs only during an incipient psychic epileptiform seizure generally induced by overeating. The seizure involves a feeling of faintness, dizziness and extreme muscular weakness. The illusion occurs in every such attack. Consciousness is confused, the visual field becomes blurred, hazy and misty so that objects are hardly recognizable. The whole field moves forward from three to five feet and keeps slowly vibrating between this position and its real location throughout the attack. Objects become larger as they approach the subject. Keeping the eyes closed is the only means of getting rid of the illusion. With the exception of the attacks the subject has enjoyed unusually good health. No eye troubles have been experienced.

(*c*) With three persons, the descriptions furnished no clue as to the essential conditions of the phenomenon. (*d*) The necessity of both internal and external conditions was evident with sixteen persons. It may be that the objective conditions are necessary only because they invoke the central conditions which are the immediate causes of the phenomenon. The following account furnishes an illustration:

XI. The illusion occurs *only* while listening to some public speaker in a church or hall. It has been noted most frequently in church. It may occur either at night or during the day, but its frequency has been *greater* in the daytime. All of the peripheral field surrounding the fixated person becomes black. The size of the central visible portion varies in the different experiences. This visible portion now moves back to some remote position and stays there until the illusion is destroyed by rapid winking or eye movement. The fixated object now merely appears back in its natural position and the peripheral objects once more become visible. During the illusion, the visible objects become smaller but remain clear cut and distinct in every way. Often a reddish-yellow flame or halo is perceived to cover and surround the speaker as though radiating out from his body in every direction. (This peculiar effect was present in a similar experience with another subject. Possibly this phenomenon may be similar to the colored aura of theosophy.) This illusion has occurred quite frequently throughout the subject's life. A condition of steady fixation and thorough absorption in the speaker are necessary to effect the illusion.

11. *Nature of Objective Conditions.* — With eleven subjects the illusion occurs *only* while fixating some person. With eight of these, the fixation of some public speaker or singer in church, theater or large hall is an indispensable condition. No. XI. is an illusion of this type. The illusion occurs only while reading with two persons (see V.). Fixation of some checkered or regularly patterned object is necessary with three people. The illusion occurs more readily during the daytime with four persons and artificial lighting is essential with five people. The distance of the fixated object from the observer is of some influence upon the occurrence of the phenomenon in twelve cases, but this factor possesses no influence with fifteen persons. The direction of the illusion occasionally depends upon the distance of the fixated objects (XII., XIV. and XIX.). The 'clothesline illusion' was experienced by three people: While looking up at the line, it is perceived to move forward toward the subject, though the remaining objects in the field are stationary. The subject experiences difficulty in locating the line with her hands. With one subject the illusion was experienced only while observing some person walking; this illusion is of sufficient uniqueness to merit a complete description:

XII. The illusion occurred quite frequently during the period from ten to fifteen years of age. It was first noted while observing a man holding his hands behind his back and walking towards the observer. The subject perceived the motion in the wrong direction, *i. e.*, the person appeared to be walking away from the observer instead of towards him. After this experience, the same illusion occurred involuntarily, although the person observed did not hold his hands in an unusual position. The reversal of direction might occur several times in the same experience. For example, a person was first perceived as walking toward the subject, but suddenly he appeared to be walking away, and this direction of movement was again supplanted by the forward direction, although the subject knew that the person observed was continuously moving in the same direction. Sometimes the conditions were such that the subject was confused as to which was the real direction of movement and which was the illusory one, until the person observed had come into a situation where the direction of motion could be inferred.

The subject developed voluntary control over this illusion, being able to see a man walk in either direction, or to change the perceived direction as often as desired. The subject was unable to describe his method of control except that he merely thought of the direction desired and the perceptual experience was modified accordingly at once.

The illusory movement was just as real and striking in appearance as a similar normal perception. The illusory motion made the person observed appear to be getting nearer or farther away as the case might be. These experiences occurred some ten years ago and the subject's memory was uncertain on many points which might have shed some light upon the phenomenon. The illusion only occurred while observing men walking either directly away from or toward the observer. The person must be from 150 to 400 feet distant and appear against an open background. The subject is under the impression that the illusion occurred either early in the morning or on dull days. He cannot recall whether it was necessary to fixate some definite portion of the body, *e. g.*, the moving legs. Since the period in which the illusion occurred, the subject has tried to initiate the phenomenon, but such attempts have been unsuccessful. With the exception of a slight astigmatism, the subject possesses good eyesight.

12. *Nature of the Subjective Conditions.* — (*a*) Steady fixation was essential with seventeen persons. In these cases eye movements destroy the illusion (see VIII., XI. and XIII.). Six of these people report that the illusion occurs only after a *prolonged* period of fixation. On the other hand steady fixation is not essential in thirteen cases for the illusion persists no matter where they look (see X.). The question of fixation is not pertinent when the illusion occurs in dreams or when the moving objects are hallucinatory images. With the remaining subjects it is impossible to determine from their accounts as to the necessity of steady fixation.

(*b*) Concentration of the attention, complete mental absorption or a dreamy mental abstraction are mentioned as essential conditions by nineteen people. The writer attempted to discriminate between those cases in which there was a mental absorption or concentration of the attention upon the moving visual object, and those in which there was a mental absorption along ideational lines involving an abstraction from the visual experiences. It was generally impossible to be confident that the subjects grasped the distinction, and as a consequence we have grouped these cases together. In all probability the mental abstraction from things of sense is of major importance. Such a condition is illustrated in the following account:

XIII. The phenomenon occurred most frequently when talking to people. All objects in the visual field suddenly appeared much farther away than their actual distance. This apparent distance varied in the different experiences. Objects did not move away, but merely looked farther away. The objects remained clear cut and distinct in outline and detail; there was no vagueness, blurring, or confusion. The subject felt that she still continued to fixate the same object without eye movement in spite of its apparent greater distance from her. All objects looked much smaller when in this distant position. The illusion persisted until the eyes were rotated when the field again appeared in its normal position.

This phenomenon occurred very frequently during youth and its frequency has been gradually decreasing with age. It occurred at night and in daytime, and with all conditions of illumination, though it was more frequent with poor illumination. The subject lacks any direct mental control over the phenomenon; she experiences a feeling of utter helplessness and detachment from the world, a sort of hypnotic fascination, which she can shake off only by a voluntary rotation of the eyes. She was very much frightened at the first of these experiences, before she had learned how to discontinue them at will. The illusion comes on gradually but unexpectedly, and it takes forceful possession of her. A state of dreamy absent-mindedness and steady fixation is favorable to the oncoming of the illusion, and the subject has been able occasionally to produce the experience by voluntarily throwing herself into this mental condition.

The subject has never worn glasses nor had her eyes examined by an oculist. Neither have they ever given her any trouble.

(*c*) Fatigue is mentioned as an essential condition eleven times. The fatigue is generally quite pronounced, occasionally to the point of complete exhaustion. It is general, involving both mind and body (IV. and VI.).

(*d*) *Ocular Defects.* — (1) About 80 per cent. of the subjects do not wear glasses. The ocular conditions of the majority of these are unknown, though no eye troubles have been experienced. Six persons have experienced slight troubles and four have had their eyes examined by oculists who pronounced them *free* from ocular defects (XVIII. and XXII.). (2) Of those wearing glasses, the illusion is as likely to occur with seven people while the glasses are worn as when they are discarded. One person reported that the illusion occurred only after the habit of glasses had been begun. The use of glasses entirely stopped the occurrence of the illusion with one person and largely minimized its frequency in another case (XX.).

(*e*) *Period of Life.* — (1) With twenty-seven people the illusion has occurred all through their life as far back as they can remember with practically the same degree of frequency for all periods (VII., XI. and XIX.). (2) With six persons the phenomenon has occurred too infrequently to allow of any statements as to the possible influence of any special period of life. (3) The influence of special periods is evident in fifteen cases. Six people report that the illusion has occurred in all periods but that its frequency has been much greater at some definite period (XIII. and XIV.). With nine persons the phenomenon occurred only within some definite period of life (XII.). With eight people the illusion ceased entirely or diminished in frequency at the end of childhood (13–14 years of age). Three people report no cessation of frequency until after the adolescent period (20 years). The illusion began after maturity with three subjects and at the beginning of adolescence in one case.

(*f*) Abnormal conditions are essential to the experience with eight people. These conditions include neurasthenia, fevers, attacks of faintness and dizziness, incipient delirium, and three cases of epileptiform seizures involving complete aboulia.

(*g*) *Miscellaneous.*— The illusions occur during a constrained eye position, while lying down, immediately after arising in the morning, and upon opening the eyes after some period of closure. Such conditions are rare and exceptional.

13. *Subjective Attitude toward the Phenomenon.* — Oftentimes the experiences are described as being terrifying or extremely disagreeable. This attitude generally occurs in those cases wherein the illusion is not in the least subject to voluntary control. When the field moves to remote positions, the feeling is one of utter helplessness, lonesomeness and isolated detach-

ment from the world. When the field moves up very close to the subjects, they experience the anticipatory feeling of being crushed and overpowered, or crowded and suffocated. Eleven subjects report that the illusion is always frightful and extremely disagreeable (IV., VI. and VII.). The majority of subjects report no unusual affective or emotional reactions to the illusion.

The feeling of subjective fixation of the objects without eye movement during the illusion is frequently commented upon by the observers (I. and XIII.).

14. *Voluntary Control.* — Cases of complete control over this illusion have been reported. By complete control is meant that the subjects can initiate and destroy the illusion, and alter the direction and the speed of the movement at will. In these involuntary cases partial control of the phenomenon occasionally occurs. (*a*) With twenty people no control at all is possible in either initiating, destroying or modifying the course of the illusion. The phenomenon suddenly occurs and persists for some time in spite of all attempts to escape it. Nos. IV., VI. and X. are illustrative examples. (*b*) Fourteen people can voluntarily destroy the illusion by some means. Of course it may be prevented by keeping the eyes closed until the temporary seizure is over as in No. X., but this can hardly be termed a volitional control. The means employed to destroy the illusion are eye or head movements, rubbing the eyes, or rapid blinking. In these cases steady fixation is an essential condition and the control is indirect, *i. e.*, destruction of the necessary conditions. Nos. VIII., XI. and XIII. are illustrations. (*c*) Five people can sometimes initiate the phenomenon indirectly by voluntarily producing the mental attitudes which constitute its essential conditions. These conditions are steady fixation and mental abstraction while listening to people (see XIII.). In all probability more people could influence the illusion by these means if the attempt had been made. (*d*) Six people can *directly* influence the course of the illusion by mental effort of some sort. Two of these cases have been described (VII. and XII.). The direction of the illusory motion is changed though the subjects could give no adequate account of their volitional method. Three subjects report that they can force the field back to its

true location by an effortful concentration of the attention upon the fixated object. In their own words, they look 'real' hard at the displaced images. The following illustrates this type of experience:

XIV. Backward movements occurred when fixating relatively near objects. The movement referred to the entire visual field. The objects moved away to a position two or three times the distance of their actual location. During the illusion her mind was in a state of abstraction and the objects remained at their distant position during the existence of this mental condition. The field moved back to its normal position as soon as the subject concentrated her mind strongly upon the fixated objects. During the receding movement objects became smaller, blurred and indistinct. She never noted that they became double. The illusion often occurred while reading, the book being the moving object. She was asked to converge behind a printed page and to note the blurring and the doubling of the print. The effect was described as similar to that occurring in the illusion while reading. Consequently, it is possible that doubling did occur in the illusion but that she failed to notice it. These illusions have been of frequent occurrence throughout her life, but they were more frequent during childhood. During the day their greatest frequency was in the evening as twilight came on, though she has experienced them in the bright sunlight.

The subject has also frequently experienced the forward illusion, *i. e.*, the case where the field moves forward to positions nearer than its real location, but she is not certain as to the conditions under which this type of illusion occurred. She related the two following experiences which are illustrative of the class: (1) While a child, she was playing in a barn and ran to an open door in the hay loft and looked down at the ground beneath, some ten or twelve feet distant. Soon the ground moved nearer, became larger and somewhat indistinct, until it appeared to be but a mere step down. The appearance was so realistic that she lightly jumped down with perfect confidence and as a consequence fell and hurt herself severely. She remembers with distinctness her surprise and astonishment during the fall at her disillusionment. (2) She was looking down over a steep precipice some two hundred feet high. The ground beneath at which she was looking was covered with large boulders and occasional shrubbery. These objects moved much nearer, became larger and blurred. She could voluntarily send them back to their proper distance by looking at them 'real' hard. This backward motion was perceived. She judged of the real distance by the clearness and distinctness of the images. This illusion has occurred several times under the same conditions.

The subject has noticed that her eyes become easily fatigued when observing distant objects. She is not conscious of strain or fatigue when observing relatively near objects. She has never worn glasses, nor had her eyes examined.

One subject reports that a receding illusion occurs under a condition of relaxation. The field is brought back to its normal position by a strong effort of will which involves a convergent movement of the eyes. Upon relaxing the effort, the receding illusion again occurs. By voluntarily alternating the effort and the relaxation, the subject can make the field vibrate back and forth at will (see XVIII.).

The extent of movement possible is slight in some of the voluntary illusions previously described. Hence there is no marked line of division between the involuntary and the voluntary illusions. We have cases ranging all the way from no control whatsoever to absolute control.

15. In those experiences in which blurring occurs, in which the movements are large in extent, continuous and irregular in direction, and in which there is some degree of voluntary control, we find that the subjects generally rely upon the criterion of the 'maximum of distinctness' in judgments as to the objective position of the field. Nos. VII. and XIV. are illustrative examples. This feature was reported in one of the voluntary cases previously described.

16. *Causes.*— It is evident that we must assume the existence of several effective causes operating in various combinations in order to explain the diverse results. It is not our purpose to attempt an explanation of every particular illusion from *a priori* grounds. Rather we shall describe several illusions in which the effectiveness of known distance criteria is evident; we shall sketch the theoretical possibilities and limitations of these factors and seek to determine to what extent they may singly or in combination explain the various illusions.

(*a*) *Lenticular Disturbances.*— Lenticular disturbances are apt to be correlated with convergent changes, but we are interested in the effectiveness of this factor irrespective of the results of convergent changes which may or may not accompany it. That adjustments of the lens may be an efficient cause of these illusions is an assumption borne out by the previous studies on the voluntary illusions of depth. The evidence in favor of such a causal factor in the involuntary illusions is most pronounced in the following experience, the facts of which have been kindly furnished by Professor Colvin.

XV. Mr. O. informs me that the illusion occurred only at twilight, while he was resting in a room of average dimensions. At these times, objects at the farther end of the room apparently receded to a position three or four times their actual distance. The illusion was never experienced out of doors or in a bright light. Relaxation seemed to be an essential condition. Objects did not become double. By an effort of will, the objects could be brought back to their normal location, though voluntary initiation of the phenomenon was impossible. The illusion was experienced frequently, in fact every time the above conditions were reproduced. He is under the impression that the convergence tended to change during the illusion.

The subject was increasingly afflicted with cataracts from twelve to twenty-four years of age, when he was successfully operated upon. The series of operations consisted of needling with subsequent absorptions. The illusion occurred only during this period, and most frequently during the three or four years preceding the operations. The phenomenon was less frequent after the first operation; it continued more or less until the lenses were entirely destroyed, but it has not occurred since that time, a period of twelve years.

The above facts are not as definite and conclusive in every particular as one might wish, but they indicate that the lens *in some way* is responsible for the illusion in question.

So far as *a priori* possibilities are concerned, the lenticular principle will explain the illusions with the following exceptions: (1) Those cases in which there is no blurring or confusion of the objects. This limitation is self-evident. (2) Those cases in which some object moves in relation to other objects in the field, when these latter remain in *distinct* view, *e. g.*, Nos. I., VIII., XII. and XIX. It is evident that the illusion should involve the whole visual field, or at least, that part well within the field of attention.

The fact of blur and confusion does not necessarily prove the existence of lenticular disturbances, for these characteristics may be the result of convergent changes or of an irregular decrease of intensity. The fact that the maximum of distinctness is often relied upon in judgments as to the proper location of the field is certainly more consonant with the lenticular hypothesis than with any other (VII. and XIV.).

(*b*) *Intensity Changes.* — It has been experimentally demonstrated that changes of brightness may mediate a sense of third dimensional movement. Backward movements result from a decrease of intensity, and an increase of brightness is interpreted as a forward movement. The influence of such a factor is evident in No. VIII. Here the fixated object is stationary, vivid and distinct. Evidently no defective ocular adjustment occurs. Peripheral objects *fade* away almost to the point of invisibility and recede to remote positions. In the auditory illusion belonging to the same subject, the intensity changes accompanying the illusion are striking. Both illusions are for *increased* distance alone, which facts relieve us from the assumption that there may be an increase of the intensity beyond the normal. The assumption of a decrease of intensity is a logical one, inasmuch as the illusion occurs *only* during a condition of steady fixation and mental abstraction.

Granted that such changes may be an efficient factor, there are several reasons for supposing that these variations are present in many of the experiences. A decrease of brightness during steady fixation of an object is easily demonstrated by experiment. With monocular vision, the fixated object may totally disappear. Therefore there is good reason for suspecting the existence of this factor in every experience wherein steady fixation and mental abstraction are essential conditions. The fact of peripheral contraction of the field in six cases (see XI.) is an evidence of such decrease. In No. VI., the visual field at the end of its receding movement may totally disappear, or the objects may be swallowed up in a dim veil-like mist. Decrease of brightness is a natural result of defective accommodation, and there is a possibility that the blur or confusion so often reported is a result of an irregular decrease of intensity. A number of people use the terms 'hazy' and 'vague' as well as 'blurred' in describing these characteristics. Again it is illogical to suppose that a decrease of intensity does not occur because it is not noticed, inasmuch as it is possible that the intensity changes are not perceived *as such* simply because they are interpreted in distance terms.

So far as possibilities are concerned, this principle may be assumed as an effective factor with the following limitations: (1) It cannot explain those illusions in which the objects move to positions in front of their real location, for this would necessitate the unjustifiable assumption that the objects may become brighter than normal. (2) Such an explanation is not the most probable one in case of the regular vibratory movements, the illusions of patterned objects, and when the distance of the fixated object from the observer possesses some determining influence. (3) The factor possesses the greatest probability when the field recedes and remains stationary at a remote position and when steady fixation, fatigue and abstraction are essential conditions.

(*c*) *Contraction of the Field.* — It has been suggested that the contraction of the field is a causal factor; that the field as a whole looks farther away because it has become smaller, on the principle that changes of size are often interpreted in distance terms. This hypothesis is plausible, but it is open to objection for various reasons: (1) It would be applicable to illusions of increased distance alone, wherein the whole visible field is involved and no confusion of images occurs. Vibrating movements would be difficult of explanation. (2) The peripheral contraction was reported by only six persons, though it may have occurred with a number of the uncertain cases. The application of such a principle is thus very limited on both factual and theoretical grounds. (3) One of the six persons (XVI.) reports that the illusion at first occurred in conjunction with the contraction and that the use of glasses prevented the illusion, although the phenomenon of contraction persisted. This fact disproves the hypothesis for this subject at least. (4) With a second person, the use of glasses greatly minimized the frequency of the illusion. This indicates at least that some other causal factor is present, and there is evidence that convergent changes are the effective agency in this case (XX.). (5) Another of the six cases (XVII.) presents features not compatible with the hypothesis; blurring and confusion of images are present and the objects move to positions in front of their real location. (6) Only three of the six experiences possess characteristics which are in no way *antagonistic* to the theory (see XI. and XXII.). There is some evidence that convergent changes are the effective factor in illusion XXII., and the other two cases *might* be explained by the convergent hypothesis. (7) Several people have informed me that they frequently experience a peripheral contraction during steady fixation, but that the phenomenon has never been accompanied by an illusion of depth.

The effectiveness of the factor has no experimental verification; it possesses factual and theoretical limitations as a general explanatory principle; there are a number of cases which demonstrate its non-effectiveness; there are only two definite cases in which it has a potential validity, and these two illusions can be explained as readily in other terms.

XVI. The illusion with this subject occurs while listening to a speaker in a good-sized room, as a church or lecture hall, and after she has become rather absorbed in the discourse. It never occurs under other conditions. At first all objects in the visual field except those attended to disappear in blackness; these peripheral objects do not move but merely fade away. The speaker and the few surrounding objects well within the focus of attention remain visible, set in the surrounding mass of blackness. The size of this part of the field remaining visible varies with the different experiences. These objects now begin to move backward generally about fifty feet. After a short time the objects appear back in their normal position without movement. During the receding illusion, objects become proportionately smaller and very confused and blurred in appearance and contour. The speaker's voice sounds farther away, becoming weaker and harsh. The experience is decidedly agreeable, giving the subject a feeling of quiet restfulness and impersonal detachment from the world. There is no direct voluntary control over the phenomenon; it can be voluntary initiated to some extent by cultivating the proper mental attitude, an attitude which the subject cannot describe. However, this same feeling can sometimes be induced without the resulting illusion. The phenomenon has occurred in the daytime as well as at night. It began in early life as far back as she can remember. One such illusion per month represents its average frequency. The phenomenon persisted up to six years ago (19 years of age), at which time the subject began to wear glasses. Since then she has occasionally attempted to repeat the phenomenon by throwing herself into the proper mental attitude, but she has never been quite successful, though the illusion has often commenced in an incipient fashion. Even yet all objects in the visual field surrounding the object of attention readily disappear after a few minutes of fixation. The subject possesses good voluntary control of convergence; she can voluntarily converge either in front of, or behind, a wall ten feet distant.

XVII. The experience occurs while listening intently to a speaker. The periphery of the field becomes void of all sense content. The speaker *appears* far away, much smaller, and presents a blurred appearance. After some moments, the person is perceived to move forward, and become larger and more distinct. The forward movement often carries the object to positions in front of its true location. The illusion has occurred very frequently throughout life and under all conditions of illumination. Its frequency has been greater during daylight. A condition of steady fixation and mental absorption is essential, and the experience can be terminated by head or eye movements. The phenomenon can be voluntarily produced by effecting the necessary conditions of fixation and absorption.

(*d*) *Convergent Changes.* — Convergence is a known criterion of depth, so there is no reason for rejecting such an explanation. The presence and effectiveness of convergent changes is indicated by the illusions of patterned objects. Two of the three subjects have noted a tendency for objects to become doubled during steady fixations. One of the persons can voluntarily produce the phenomenon by converging in front of the patterned object. These three cases belong to a special class.

(*e*) *Binocular Parallax.* — This factor is operative in a number of experiences, five of which will be described. It is closely associated with the principle of 'convergent changes' inasmuch as deviating eye movements are a necessary condition for its effectiveness. Consequently, we are concerned in the following illustrations in establishing the presence of convergent changes as a condition of the illusion, though not as the immediately effective factor. The relation of binocular parallax to the convergent changes will be depicted later.

XVIII. The illusion occurs while observing patterned objects and also while observing persons. The following description refers to the latter case. The illusion is one of increased distance alone, and motion in both directions is perceived. She cannot recall as to whether the periphery of the field participated in the illusion, although she is confident that it was vaguely visible. The objects remain stationary for some time at the end of the receding motion. *During* either the receding or the return movement, the images are confused and blurred, but clear up and become *normally distinct* and *definite while they are stationary* at either their real location or at some remote position. Objects become smaller as they recede. The illusion occurs during a condition of steady fixation, mental abstraction and relaxation. Fatigue is not necessary. While the objects are at a remote position, they can be voluntarily brought back to their real location by an effort of will which involves eye movement. This movement is not rotary, for fixation is not disturbed. The subject can distinctly remember the feeling of eye movement, but she cannot recall as to whether it was divergent or convergent in nature. The receding illusion again occurs when the effort is relaxed. By alternating the effort and relaxation, the field has been made to vibrate back and forth. The experience has occurred frequently throughout life. Her eyes have been examined by oculists and pronounced perfect. She has never noted any tendency in normal conditions for objects to become doubled while fixating them.

The subject's statements as to the presence of convergent movements and the blur with the subsequent clearing-up process were made without any suggestive questions on my part and she knows nothing as to theories of space perception. Evidently, no accommodatory disturbances occur because the images become clear-cut even in displaced positions. The fact of blur during the movement with a consequent clearing-up process while the field is stationary is explicable on the convergent hypothesis.

Granted that convergent changes of some sort condition the illusion, it is to be noted that the displaced objects do *not* necessarily become *doubled* or *blurred*. Ordinarily, doubling of images occurs in defective binocular adjustment because disparate retinal areas are stimulated. Thus the convergent theory will necessitate the further assumption that sometimes retinally disparate processes may allow of unitary vision, and it may be that the translocation in depth is due to this very fact, *viz.*, the unitary combination of images due to disparate retinal processes.

XIX. The illusion occurs only in churches, theaters and lecture halls, especially when the distance of the fixated objects is considerable (at least fifty feet). Only the objects of attention move; the periphery remains visible and stationary. The fixated objects also may move in relation to other objects in the line of sight, *e. g.*, a lecturer on the platform moves up to the wall behind him; in fact, sometimes the wall appears blurred and hazily transparent, and the person is perceived to move through the wall and to remain visible for a short time at some distance beyond it. The receding motion is quite slow; the return forward movement is rapid and it occasionally carries the objects past their real positions, this being followed by the necessary return. The moving objects become smaller and blurred during the receding motion. The stationary objects of the periphery remain the same size and are blurred to some extent especially around the edges. The illusion occurs involuntarily during a condition of dreamy abstraction and the objects tend to remain at their distant position during this condition. The illusion may be voluntarily terminated by blinking or eye movements. The moving objects often become *double* at the end of the receding movement; this condition has but a momentary duration; the images snap together and immediately start forward on the return movement.

This doubling always terminates the illusion immediately. Often the doubled images do not remain parallel, *e. g.*, the images of a person may be separated by a space of three feet at the top but only a foot at the bottom. The doubled images are blurred slightly, one always being much more blurred than the other.

The illusion has occurred quite frequently, as far back in life as the subject can remember, and it shows no sign of abatement in frequency of late years. It is experienced in the daytime as well as at night. A condition of dreamy abstraction with a rather pronounced ocular fatigue due to prolonged steady fixation seems to be an essential condition. Her eyes are not strong. Her left eye was forced from its socket when she was a child and for some time thereafter she was afflicted with convergent strabismus. Steady fixation is difficult and fatiguing. All objects more than twenty feet distant *tend to become doubled* homonymously when fixated, especially during conditions of relaxation, *i. e.*, the eyes normally tend to converge in front of the object.

The following facts indicate that the receding illusion is conditioned by a *convergent* movement of the eyes. There is a normal tendency to converge in front of relatively distant objects (over twenty feet) during relaxation and steady fixation, and all these conditions are essential to the illusion. Fixation is difficult and fatiguing. The subject was once afflicted with convergent strabismus. The doubling that often occurs in the illusion must be due to convergence because it would be impossible for objects fifty feet distant to become separated a foot by a *divergent* movement of the eyes. The fixated object may move in relation to other objects in the line of sight, *e. g.*, the speaker moves through the wall behind him; this phenomenon is hardly explicable in other than convergent terms. Accommodatory disturbances are probably present inasmuch as the doubled images are blurred: this result may be due to the fact that they are perceived through the wall, but this is rather improbable in view of the fact that both images are not equally blurred. The stationary periphery is blurred, but this may be due to the doubling. Inasmuch as the doubling invariably destroys the illusion, it may well be argued that convergence can hardly be a cause. This conclusion is possible, but not necessary. As a temporary hypothesis, the following is suggested which is entirely consonant with all of the facts: that both convergent and accommodatory disturbances are present but that convergence is of major importance. The eyes converge in front of the fixated object so that disparate retinal areas are excited. Most of the visual objects become doubled and blurred and are normally located. Owing to the extreme concentration of attention, the objects in the focus are kept combined and a more remote location in depth is necessary to effect this result. The eyes may converge to such a degree that binocular combination is no longer possible and hence the images separate. This separation now stimulates a divergent movement of the eyes, which once more unites the images and brings them back to their normal location.

XX. In the following case, the illusion occurs while listening to speakers at some distance from the subject, and it involves the whole visual field. The field moves backward and remains at some remote position. The phenomenon is destroyed by sudden eye or head movements and the field merely appears back in its normal position. All objects become smaller. The periphery becomes *very* dim and blurred, but it does not totally disappear. The central portion remains clear and distinct. Steady fixation is not a necessary condition. Mental abstraction is essential. The illusion occurs more frequently in the daytime. The period of greatest frequency was between the ages of eight and fourteen years. The subject is astigmatic and near-sighted. The use of glasses has greatly minimized the frequency of the experience. There is a marked tendency for all objects to become double during conditions of relaxation and abstraction. Tests demonstrated the existence of a divergent tendency while fixating near objects (less than ten feet), and a convergent tendency for relatively distant objects. Since these tests were made, the subject reported that she experienced a *forward* illusion while conversing with a person some five feet distant. In other respects the experience was similar to the receding illusion described above.

There is no direct evidence that convergent changes occur during the illusion, but there is proof that such changes tend to occur in the mental conditions essential to the production of the illusion. As in the preceding case, the backward illusion is associated with convergent movements, and in addition we find a *forward* illusion connected with the divergent tendency. Again, the periphery becomes blurred, but the fixated central portion remains clear-cut and distinct. Evidently no accommodatory disturbances are present.

XXI. This illusion is one of *distance* without any perception of motion. As to the periphery of the field, the subject has no memory either of its visibility or its participation in the illusion. The fixated object is always a person *at a distance* of at least ten feet. The illusion is vibratory. Objects appear on both sides of their real position, though the receding illusion is of the greater magnitude. The initial illusion is always a receding one, and the objects appear slightly in front of their real position only in the return illusion. Objects become smaller as they recede. The images are always blurred and generally they are slightly *doubled*. The tendency to double is always present throughout every illusion, and this tendency must always be resisted with effort. This effort is described as a muscular strain in the orbicular region. When the effort to maintain unitary vision is unsuccessful so that marked doubling occurs, the illusion disappears.

The phenomenon has occurred frequently throughout life. The only essential condition is a prolonged steady fixation of some person, involving a high degree of mental abstraction and concentration of the attention. The doubling tendency after a few minutes fixation is present in normal vision, *i. e.*, when the illusion does not occur. Continual effort is necessary to maintain unitary vision. This effort is the same as that described in the illusory experiences. Tests demonstrated that the tendency was convergent in nature, *i. e.*, resulting in homonymous doubling, and that it occurred for objects situated at a distance of more than eight or ten feet.

Again, we have doubling occurring *during* the illusion. Too great a separation of the images destroys the phenomenon. The marked separation possible shows that the eyes converge in front of the objects. In normal experiences the eyes tend to converge in front of the fixated object when it is situated eight to ten feet distant. The illusion occurs only for objects at a distance of at least ten feet.

XXII. The illusion occurs while listening to public speakers or observing a play at the theater. The visual field contracts to about one half its size. The periphery becomes a light gray hazy mass, often suffused with a reddish-yellow light. The illusion is one of pure distance, no movement being perceived. The visible portion of the field alternates continuously between its true location and more distant positions. The change of location is instantaneous, without break of vision. Objects decrease in size in proportion to their apparent distance from the observer, but they remain normally distinct. The illusion has occurred but rarely and only within the last few years. Artificial illumination, a prolonged steady fixation, and a thorough mental absorption are essential conditions. Oculists have pronounced the subject's eye to be *free* from optical defect. Weakness of the external recti is responsible for a slight muscular strain. There is a strong tendency for fixated objects — even relatively near objects — to become doubled. This tendency is oftentimes very difficult to overcome. Tests demonstrated that this tendency is convergent in nature, a result which is consonant with the reported weakness of the external recti.

In the above experience we find associated a weakness of the external recti, a normal convergent tendency, mental absorption as an essential condition, a receding illusion and distinctness of the visual objects.

These five cases offer presumptive evidence in favor of the suggested hypothesis: (1) There is direct evidence as to the presence of eye movements *during* the illusion in three cases. (2) There is also indirect evidence in four cases, inasmuch as there is a natural tendency towards doubling in the mental conditions essential to the illusion. Similar direct and indirect evidence as to the presence of eye movements during the illusion is furnished by one experience, a description of which has not been given. (3) In four cases the convergent tendency is associated with the backward illusion, and the forward illusion is once correlated with a divergent tendency. (4) A marked doubling tends to destroy the illusion in two experiences. (5) The images remain distinct with three persons. (6) Concentration of attention is necessary for the illusion, and it may be assumed that unitary vision resulting from disparate retinal stimulations may occur only under this condition.

This hypothesis makes two assumptions which may be regarded as questionable: (1) Whether unitary vision *may* sometimes result in case an object stimulates non-corresponding retinal areas, and (2) whether this unitary combination ever does involve an unusual depth location. The questions are mooted ones, but probably the best opinion is in favor of the affirmative. The writer subscribes to the affirmative position for two reasons: (1) The main motive underlying the contention that unitary vision involves only corresponding retinal points in a strict mathematical way, seems to be the attempt to standardize visual processes according to mathematico-optical ideals. (2) The assumptions are supported by several experimental facts. The experiments described by Hyslop[1] best serve our purpose. If the two large circles in Figs. 1 or 2 are combined by divergence or convergence so that they fall upon corresponding retinal areas, it is evident that the two smaller circles cannot stimulate corresponding areas because they are not concen-

FIG. 1.

FIG. 2.

tric with the larger circles. Yet it is possible to combine simultaneously in unitary images both the larger and smaller circles, at least so far as casual perceptual results are concerned. Again, it is to be noted that divergent combination of Fig. 1, and convergent combination of Fig. 2, produce the following results: The similar smaller circles stimulate non-corresponding areas in such manner that normally they would be perceived as *homonymous* images, and that the unitary image is located *behind* the plane of the large circle, the degree of its remoteness being conditioned by the extent to which the stimuli deviate from corresponding points. Convergent combination of Fig. 1 and divergent combination of Fig. 2 give these results: The smaller circles, if not combined, would be perceived as *heteronymous* images, and their unitary image is projected *forward*

[1] *Mind*, Series I., Vols. XIII., p. 499, and XIV., p. 393.

in proportion to the degree to which their stimuli deviate from corresponding areas. Thus a forward depth displacement results from uniting heteronymous images, while a backward displacement is correlated with the unitary combination of homonymous images. We are interested for the present in these results merely as *statements of fact*, and not in their explanation. Such factual results in this experiment are exactly identical with the assumptions underlying the explanation of the above illusions: Convergence results in a backward illusion, but convergence produces homonymous doubling. Divergence is associated with the forward illusion, but divergence produces heteronymous doubling. The maintenance of unitary vision, under these conditions which usually result in doubling, is secured by a depth displacement whose direction depends upon the kind of doubling and whose amount corresponds to the size of the deviating eye movement.

It is to be noted that the eye movements *per se* are not the efficient factor in the above explanation. In the ordinary conception as to the influence of eye movements, divergence is correlated with a remote position of the field, and convergence locates the objects nearer to the observer. Either the tactual-kinæsthetic sensations resulting from the movements, or the binocular innervation of the act is supposed to influence the spatial character of the percept. In our explanation the eye movements are essential only because they create the necessary conditions for the operation of the effective principle, *viz.*, the unitary combination of disparate spatial processes. This principle is practically equivalent to what has been termed elsewhere[1] for want of a better expression the 'binocular parallax.' There is no intention of denying the efficacy of the convergent principle as ordinarily understood; it is to be noted, however, that the binocular parallax is effective in the above illusions, although it is spatially *antagonistic* to the supposed effects of convergent movements.

One further possible interpretation must be considered. It may be supposed that the eye movements are so slow as to be unnoticed. Being ignorant of the eye movement, the subject

[1] *Ibid.*, Psych. Rev., Mon. Sup., Vol. VII., no. 3, p. 114.

fails to make allowances therefor, and hence he erroneously judges that the field is moving in a direction opposite to that of the eye movement. This principle has been used in the explanation of some of the lateral illusions of visual motion. I have attempted elsewhere[1] to show the fallaciousness of this explanation of the lateral illusions. Irrespective of its validity in those cases, the principle meets fatal objections in the third dimensional illusions: (1) It cannot account for the relative movement of different parts of the field, especially the relative movement of two objects in the line of sight (XIX.). (2) There is no 'ignorance' of the eye movements, as the theory presupposes, in one illusion (XVIII.). (3) Doubling and confusion of images are not always present, characteristics which must inevitably occur according to the theory. In fact doubling never occurs except in the two cases, and this doubling destroys the illusion. There is no confusion or blurring of the objects in three of the experiences (XVIII., XX. and XXII.).

There are practically no theoretical limitations as to the application of the binocular parallax principle. It is especially adapted to explain certain phenomena to which the other principles are not applicable, *e. g.*, the movement of the fixated objects without any change in their intensity or distinctness, the movement of one part of the field in relation to other objects, especially when the relative movement refers to objects in the line of sight, and a peripheral blur with clearness of images in the central portion of the field. The theory is directly supported by several other experiences which have not been described.

We stated previously that we were interested so far in the principle of the 'binocular parallax' as a mere statement of fact. The prevalent theory of explanation as applied to the perception of solidity — the Hyslop circles — is stated in motor terms: As a matter of fact the combined smaller circles are located at that depth position upon which the eyes must necessarily converge provided the images are normally combined. The eyes are reflexly stimulated so as to combine slightly doubled images. The theory assumes, then, that this constant reflex strain, or tendency, of the eye to converge upon a given

[1] *Ibid.*, PSYCH. REV., Mon. Sup., Vol. VII., no. 3, p. 86.

point in order to combine the displaced images is the cause of their combination and translocation to that position. Several objections may be urged against this theory.

1. The motor strain translocates only the smaller circles. Logically it would seem that such a peripheral factor should effect all objects in the visual field. The assumption that it possesses a selective influence needs further explanatory consideration.

2. The translocation of the smaller circles varies in amount in proportion to the extent of eye movement necessary normally to unite them. The location of the images is constant in any particular case of combination. Hence the theory must assume that the degree of strain varies in different cases in proportion to the deviation of the smaller circles from concentricity, but that it remains constant in any particular case of combination. The latter assumption is extremely questionable.

3. The figures are not only translocated in depth but laterally, *i. e.*, they are moved together until they unite. This lateral movement needs additional explanation.

4. The eyes do not move in the direction of the translocating tendency because of the necessity of maintaining the combination of the larger circles. In fact there are two antagonistic tendencies present — one effective upon the smaller circles and one upon the larger. This fact emphasizes the essentially selective character of these motor tendencies.

Matters may be further complicated. Four pairs of circles may be used. Two pairs may be projected at unequal distances in front of the large circles. The third pair can at the same time be united behind the plane of the large circles. There must needs be three motor tendencies present, a divergent one and two convergent ones. The latter must vary in intensity, for their effects are unequal. Granted that there may be two antagonistic tendencies each effective upon only a certain part of the visual field, yet the assumption of two simultaneous motor impulses of the same directive character, but of unequal intensity and each operating upon only a definite part of the field, is somewhat exacting upon one's credulity.

5. The motor theory meets difficulties in its application to

the illusions described in this paper. The eyes involuntarily diverge beyond an object, and this object remains single and moves forward in proportion to the diverging movement. The theory posits a convergent tendency as the unifying principle. But a divergent tendency is also present greater than the convergent one, because the eyes actually diverge. According to the preceding section, it might be assumed that this divergent tendency is the unifying principle for the larger circles of the Hyslop figures. In these illusions, however, this stronger divergent tendency has no material to unite. The convergent strain unites the images, while the divergent tendency is functionless. How can one possess a combining function and the other not? What determines as to which one is to be functionally active? Logically why should not the stronger tendency prevail?

6. The assumption that the combined images are located at that position to which it would be necessary for the eyes to move in order to combine them normally is true for the Hyslop figure, but not for these illusions. If this were true, the combined images would be located at the actual position of the object and hence no illusion could occur. This is evident from Fig. 3.

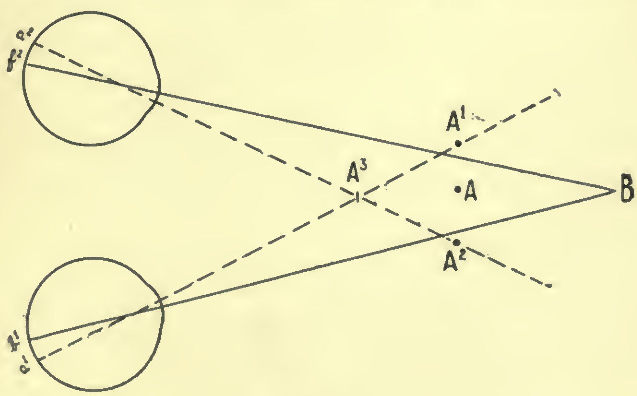

FIG. 3.

The eyes are diverged beyond an object A, so that the lines of sight intersect at B at a distance of six feet from A. A^1 and A^2 represent the heteronymous images of A as perceived in ordinary circumstances. Obviously a convergent movement from B to A will normally unite these images; hence the convergent tendency toward A which is supposed to exert a combining and translocating function, should locate the images at A; the object would thus be perceived in its real position, and no illusion would be possible. As a matter of fact the visual object is located at the position A^3. This objection is fatal to the motor theory.

7. Such a reflex motor tendency to unite doubled images should be universal — occurring for everyone and at all times. This is true for the Hyslop figure, but not for our illusions. The combination of disparate images occurs for but few people and only under certain unusual mental conditions. The universality in the perception of solidity is probably due to habit. My point is this, *viz.*, that the effective principle, whatever it may be, operates only under certain unusual conditions.

In opposition to the motor theory, I wish to suggest a hypothesis which is free from the above objections. It involves two assumptions: (*a*) That the position of monocular images along the line of sight is variable, that an image may be perceived at slightly varying distances from the eye. In support of this contention we know that monocular judgments of distance are much more variable and inaccurate than those of binocular vision. (*b*) Some of these variable determining conditions are mental and central, *i. e.*, are not due to motor adjustments. The nature of these I do not presume to describe. That such mental conditions as extreme mental absorption and concentration of attention should influence monocular depth perceptions is not at all improbable.

The application of these assumptions to the illusions is simple. We have found that the combination of heteronymous images involves a forward displacement. Lines drawn from heteronymous images to their corresponding eyes intersect in front of the position of the object (Fig. 3). Under certain conditions the monocular images A^1 and A^2 are shifted along their lines of projection. If shifted forward sufficiently, they are perforce united. On the other hand, when homonymous images are projected backward, provided the lines of projection do not diverge beyond the parallel, they become spatially combined. No separate uniting mechanism need be postulated.

Inasmuch as the conditions determining the shift along the lines of projection are assumed to be central, they may well be selective upon the visual field, and different parts of the field may be translocated in different directions or for diverse distances. It may be objected that the images may be shifted in the wrong direction along the line of projection. This is true. In other words, the principle is not universal — a characteristic in which it agrees with the facts. The combination will occur only under unusual mental conditions — habit in the case of solidity, and extreme mental concentration upon the object with the above illusions. The fact that both images are shifted simultaneously in the same direction supports the contention that the determining conditions are central and mental.

The projection theory as outlined above readily explains these illusions of depth, a phenomenon which cannot be adequately accounted for in terms of the motor theory (Sec. 5 and 6). The sense of solidity, as typified by the combination of the Hyslop circles, may be explained as readily in terms of one theory as of the other. The projection theory, however, is free from the objections urged against the motor hypothesis, *viz.*, (1) that the effective factors must be spatially selective; (2) the necessity of assuming a number of motor factors working simultaneously, but which may be unequal in strength and antagonistic in direction, and (3) the efficiency of such factors only under unusual mental conditions.

To summarize, we have found direct evidence as to the efficiency of several factors governing depth, and we have sketched the theoretical limitations of each principle. These factors may operate singly or in various combinations. The principles have been derived from a consideration of a few specific cases, but so far as *a priori* possibilities are concerned, any illusion (with a few exceptions) *may* be explained by some one, or some combination, of the above principles. The majority of the illusions furnish no direct and unambiguous evidence as to the nature of the causal factors. If it be granted that all illusions are due to some of the above principles, there is evidence as to the probably effective factor in many specific cases. (1) The binocular parallax seems to possess the greatest potential application. Such illusions as I., V., VI. and XI. are probably to be classed under this heading. (2) Lenticular changes are second in the extent of their application as an explanatory principle. Illusions VII., XIV. and XVI. find their probable explanation on this hypothesis. (3) Intensity changes seem to have some degree of applicability but apparently such a factor is generally operative in conjunction with other factors. (4) Convergence is probably limited to the three illusions of patterned objects. (5) The contraction of the field possesses a doubtful validity but, granted that it has some influence, its application is limited. (6) The illusions occurring in dreams and abnormal psychic attacks, especially when they are rhythmic in character (II., III. and X.), are probably due to motor disturbances and hence may be assumed to be conditioned by convergence and lenticular changes. (7) Those experiences which were classed as due entirely to objective causes (IX.) cannot be explained in any of the above terms. Illusion XII. also presents difficulty to any of these theories.

There is no intention of denying the possible efficacy of other causal factors in many of these illusions. The discussion has been purposefully confined to those factors for whose functional presence there is some direct factual evidence.

17. *Peculiar Phenomena.* — So far we have discussed the illusions from the standpoint of their spatial significance, — the factors determining depth location. Many other characteristics, such as the direction, kind, extent and rapidity of the movements, kind and distinctness of images, extent of visual field involved, etc., are readily explicable and need no further comment. Several features, however, deserve additional explanatory notice.

(*a*) Twelve experiences are illusions of pure distance, no movement being perceived; twenty persons experienced both movement and change of location; with eight persons the first and second types alternate in the same illusion, and two subjects experience pure motion. As a rule the subjects were very positive in regard to these points, the writer took pains to describe and illustrate very carefully these different possibilities, and the distinctions are easily comprehensible. As a consequence there is little doubt of the validity of the distinctions in the main. The differences can hardly be due to the kind of space factor involved. For example, five experiences were explained by the principle of binocular parallax; two of these are illusions of pure distance; movement and change of location are experienced by two persons, and there is an alternation in one case. It may be assumed that the perception of movement and change of location is the normal experience but that the motion is not perceived when the spatial changes are either extremely rapid or extremely slow. When the objects remain at some remote position and the illusion is terminated by eye movements, no motion is perceived in the sudden return illusion. In illusions of pure distance the objects as a rule jump instantaneously from one position to another and remain stationary for a time (XXII.). In a few cases my notes furnish no clue as to the rapidity of the spatial changes. When motion is perceived, however, the movement is generally of a moderate rapidity. On the whole this theory furnishes the best explanation of the phenomena.

(*b*) The relation between the size and distance of visual percepts is a complex and variable one. When after-images, entoptic phenomena and combined images are projected at various distances from the observer, their size varies directly with the distance. This result was obtained in the three illusions of patterned objects, a result that can be obtained by voluntary binocular combination. In the majority of the illusions, the size varied inversely as the distance. This result seems to belong to the principle that when the object is known and the distance is wrongly perceived, a correction of size is made, because habitually distant objects appear small and near objects appear large. The absence of any change of size in the one illusion seems to be an anomalous result.

In conclusion, we are cognizant of the weaknesses of such a method of treatment. The facts are open to suspicion because of possible errors of memory, the incompetency of the subjects for accurate descriptions, and the influence of suggestive questions in eliciting a complete account of the phenomena. The uniqueness and frequency of the illusions should render memory more reliable than in ordinary experiences. Their number and their comparative uniformity in many respects suggest that the general summary of the various characteristics must possess a large basis of fact at the very least. That each description is true in every particular is hardly credible. Necessarily the generalizations as to the space factors can hardly possess a validity equal to those derived from well controlled experiments. The conclusions as to the criteria of depth possess a suggestive and confirmatory value. The experiences do support the general proposition that the relation of the various factors governing judgments of depth especially in respect to their functional efficiency, is very complex, and probably subject to marked individual variations, — a conclusion which has been urged in the previous articles cited. These experiences are also interesting and valuable from the standpoint of human nature. The facts that they are so striking, so real in appearance, so antagonistic to the customary behavior of the visual world, so frequent in early youth — a period of imaginative susceptibility — and often so frightful, lead one to suspect the possible influence of such experiences in the development of more serious mental disorders.

MUSCLE-READING : A METHOD OF INVESTIGATING INVOLUNTARY MOVEMENTS AND MENTAL TYPES.

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I.

Recognition of the existence of involuntary movements, whereby a fit reagent may receive information of various sorts, such, for instance, as the whereabouts of an object thought-of or knowledge of the nature of an action meditated-upon is now widespread. Investigation has shown that the information conveyed by such involuntary movements may be received by the reagent in various ways. He may, for example, receive it through contact as in muscle-reading, or through the ear or the eye as in the so-called mind-reading without contact. An inanimate object may be substituted for the human reagent and involuntary movements may manifest themselves in table-tipping or by the behavior of the divining-rod or of the planchette or they may be accurately recorded by the registration of them by means of the automatograph or other instruments. It has, further, been shown that animals will respond to such involuntary movements, using them as signals for the performance of various acts. Of the possibilities of such response, Herr Pfungst's[1] highly entertaining work on the famous calculating horse of Berlin must remain for some time the classic report.

In the following investigation, the involuntary movements of the subject were interpreted by contact, by the so-called method of muscle-reading. The nearest approach in purpose to the present investigation was found, however — after the tests to be recorded had been completed — in the above-mentioned work of Pfungst, in the section reporting laboratory tests upon involuntary movements.[2] The movements reported by Pfungst

[1] Pfungst, O., 'Das Pferd des Herrn von Osten (der Kluge Hans), ein Beitrag zur Experimentellen Tier- und Menschen-Psychologie,' 1907.

[2] *Op. cit.*, p. 77 f.

were interpreted visually — a method of reaction superior, for experimental purposes, to reaction through contact in that it makes possible graphic registration of the movements of both agent and reagent. The situation dealt with in the following paper was less defined than that reported by Pfungst, a condition which renders the results less capable of precise formulation. They have, none the less, their value. The emphasis in the two tests was different. Pfungst only incidentally touched upon the point wherein lay the special interest of the present investigation, namely, the relation between involuntary movements and the nature of the control process used by the subject in the endeavor to concentrate his attention. Pfungst's observations on himself and his reports from his subjects confirm in part the results obtained in the present tests and constitute by far the most acute analysis of the situation which the present writer has found in the literature of the subject.

The history of muscle-reading runs very briefly as follows: In 1874, under the caption of 'Mind-Reading,' it began its platform career in America spectacularly with the demonstrations of Brown. It yielded, within a few months, its crucial secret — its dependence upon the involuntary movements of the guide — to that acute observer and analyst, Dr. G. M. Beard.[1] In 1881, after a similar career in England, under the auspices of Bishop, it was a second time investigated with similar outcome by a group of English scientists, chiefly Croom Robertson, Romanes, Lankester and Galton. Since that time, skill on the part of operators and knowledge of their *modus operandi* have developed concurrently. A literature on the subject has developed; partly semi-scientific — a reassurance of the public bewildered by the dexterity of the latest platform demonstrator — partly scientific, in the form of reports on specific aspects of the general problem, such as thought-reading without contact.

Apart, however, from the general conclusion that mind-reading by contact or otherwise is possible because of unconscious or involuntary indications given by the guide who concentrates attention on a particular object or action, the following interesting observations have been made.

[1] Beard, G. M., *Trance and Muscle-Reading*, 1882.

Relative to the muscle-reader himself it has been shown that ability in this line is not confined to a few particularly gifted persons, but is a general ability dependent upon practice for development,[1] although Beard questions the possibility of children under fifteen or adults over fifty becoming skillful in the art.[2] Neither do men who are adepts show when tested more than normal acuteness in tactual discrimination.[3] Emotional excitement, incident to public exhibitions if successful at all, and all conditions that induce a semi-hypnotic state in either reader or subject facilitate the reading.[4] The reader is often as unconscious of his method of reading as the guide is of his movements, although an intelligent reader usually grows sophisticated in time.[5] The degree of expertness the mind-reader may acquire is extraordinary. The precision with which he identifies one small object among many — selects, for instance, one pin among a dozen, or identifies a word or letter chosen at random from a large volume — is surprising.[6] Moreover, the action thought-of may be highly complicated without the reader being baffled thereby. Again, the tests may be successfully performed if indirect contact by way of a wire or other rigid connection be substituted for direct contact. Or between the guide and the reader one or more persons ignorant of the object selected may be placed, provided that the human chain thus formed be a rigid one. Moreover, contact may be done away with altogether and the reader be guided by the movements of the whole body of the guide or even by the sound of his footsteps as he moves with him.[7] This last-mentioned method of guidance by way of auditory indications suggests the later work

[1] Beard, *op. cit.*, p. 20, p. 36 (quoting Romane's Report on 'Thought-Reading' in *Nature*, 1881); Laurent, L., 'Les procédés des liseurs de pensées,' *Jour. de Psychol.*, 1905, II., p. 486 f.

[2] Beard, 'Physiology of Mind-Reading,' *Pop. Science Monthly*, X., 1877.

[3] Beard, *Trance and Muscle-Reading*, p. 34 (quoting Romanes's report).

[4] Beard, 'Physiology of Mind-Reading,' *loc. cit.*, p. 472; Laurent, *loc. cit.*, p. 486.

[5] Beard, *Trance and Muscle-Reading*, p. 14; Cumberland, S. A., *Thought-Reader's Thoughts*, 1888, p. 4; Gatchell, C., 'The Methods of Mind-Readers,' *The Forum*, XI., 1891, p. 201.

[6] In Pfungst's experiments, the amplitude of the movement which was the signal for reaction averaged one millimeter.

[7] Beard, 'Mind-Reading by the Ear,' *Pop. Science Monthly*, XI., 1877.

of Hansen and Lehmann, and the still later work of Laurent, on communication through involuntary verbalization.[1] Although the term 'muscle-reading' ceases to be appropriate when methods are so varied as to exclude contact, the principle of thought-reading through involuntary movement, whether interpreted tactually, auditorially, or visually, remains the same.

Relative to the guide in muscle-reading or thought-reading without contact, it has been shown that the value in this capacity of different persons varies greatly. Gatchell estimates that about one person in five among young people and one in ten or twenty among adults satisfy the requirements for a good subject who 'must be capable of mental concentration; he must exert no muscular self-control; he must obey his every impulse.'[2] A difference in the fitness of guides is usually attributed to failure on the part of certain reagents to meet the conditions of the tests Beard remarks that voluntary stiffening of the muscles delay. or renders success impossible and that knowledge on the part of the guide of the *modus operandi* has an inhibitive effect. "The best subjects would appear to be those who have moderate power of concentration and slight control over their muscular movements. Credulous, wonder-loving subjects are sometimes partially entranced through the emotions of reverence and expectation; with subjects in this state operators are quite sure to succeed.[3]

Romanes contents himself with reporting of the reagent in the English test as follows: "It was soon found that he succeeded much better with some of us than with others; so at the second meeting, in order to make a numerical comparison, he was requested to try two experiments with each of the four persons who were present. With Mr. Galton, Professor Robertson and Professor Lankester he failed utterly, while with myself he succeeded once perfectly and the second time approximately."[4] Cumberland, the expert English thought-reader,

[1] Hansen u. Lehmann, 'Ueber unwillkurliches Flüstern,' *Phil. Studien*, XI., 1895; Laurent, 'Les procédés des liseurs de pensées,' *Journal de Psychologie*, II., 1905.

[2] *Loc. cit.*, p. 199 f.

[3] 'Physiology of Mind-Reading,' *loc. cit.*, p. 467.

[4] *Trance and Muscle-Reading*, p. 31.

instances the involuntary or deliberate dishonesty of certain subjects who were either unable or unwilling to concentrate attention. Cumberland objects to the 'nervous' man as a subject. "It is with the determined man, the man with the iron will, the man who can concentrate his thoughts unwaveringly that I can succeed best."[1] And again, "With respect to 'women as subjects' it is quite an error to imagine that I achieve success more readily with women than with men. . . . Who make the best 'subjects'? is another question I am frequently asked, to which I can only reply, that whilst some persons actually are more suitable for experiments of this kind than others, every intelligent, thoughtful man who will act up to the conditions imposed upon him, is sure to be a good 'subject,' and that with such folk, I, in nine cases out of ten, should be successful. . . . Taking all in all, I have found the best 'subjects' among statesmen, diplomatists, mathematicians, literary men and all those engaged in active brain-work." Among distinguished men, Alexander Dumas is named by Cumberland as his worst subject, a fact attributed to Dumas' natural 'self-willedness.' Musicians as a class are cited as poor subjects; artists as somewhat better. Lawyers, physicians, and clergymen are described as furnishing some excellent material for tests but on the whole are found to be susceptible to certain limitations that make against success. Among subjects of different nationalities, the Germans are reported to offer the greatest number of good 'subjects' and Field-Marshal Von Moltke is named as the first among the best 'subjects.'[2]

Laurent[3] insists that success is inevitable if the guide is able to concentrate his attention and submits to the test in good faith. Failure on the part of the muscle-reader results from the involuntary dishonesty of subjects who are unable to concentrate attention for any length of time and from the voluntary trickery of those whose attention is concentrated on the idea of leading the reader away from the correct locality. Success is brilliant with honest guides of hysteric or nervous tendencies, whose obses-

[1] *Op. cit.*, p. 80 f.
[2] *Op. cit.*, Chap. X.
[3] *Loc. cit.*, p. 485 f.

sion by the idea concentrated upon leads to an augmentation of unconscious movements.

Pfungst[1] in his tests in the laboratory experimented upon twenty-five persons of various ages and of both sexes. He noted visually, controlling his observation by a graphic registration, the involuntary jerk of the head by which his subjects indicated the terminal member of a numerical series, thought of by them and tapped by the operator. Out of the twenty-five persons tested, only two (especially abstract thinkers) failed to react in the expected way. In only a few persons, however, was the head-movement very evident, that is, more than a millimeter in extent. Pfungst[2] found that excitement, 'warming up,' practice, fatigue, indisposition, the so-called 'Perseverationstendenz' and faith in results affected the outcome, in so far as these factors affected the subject's power of concentration of attention. If we omit the special conditions required for success when Hans, the horse, served as reagent, we may summarize Pfungst's statement of the conditions necessary for success in the tests tried by him as follows:[3] Capacity for strong concentration of attention for only a pronounced tension of expectation and will issues in such strong relaxation that the innervation-changes lead to an outwardly perceivable movement; lapse of self-control under the conditions of intense concentration; readiness of motor discharge toward the muscles rather than its expenditure in the production of vascular and glandular changes; maintenance of tension a sufficiently long time with relaxation at the right moment. It is concluded that all in all only a very few persons correspond completely to the type described. They were, says Pfungst, characteristically those who were otherwise reputed to be very impulsive and possessed of 'temperament.'

II.

For some years, the present writer has practiced muscle-reading with a deepening conviction that there are possibilities

[1] *Op. cit.*, p. 77 f.
[2] *Op. cit.*, p. 101 f.
[3] *Op. cit.*, p. 145 f.

in its use as a method of investigation which have not yet been exhausted. Her interest centered chiefly in determining if possible by its means a classification of mental types. It is obvious that this problem may be approached from the standpoint of either agent or reagent. The problems presented by the latter are, however, in this connection less interesting than those suggested by the agent or subject, although in mind-reading without contact undue sensitivity of sense-organ or abnormal passivity on the part of the operator must probably be assumed, and in complicated tests with contact there is involved a power of interpretation which may be dependent upon natural facility as well as upon extensive practice. In any case, tests upon many trained operators were out of the question while the writer as operator was herself able to handle many agents. The present investigation was then chiefly concerned with the problems presented by the agent or guide.

The fact that interest centered upon the psychology of the guide accounts for the detailed résumé of the observations that have hitherto been made upon the various types of subjects. A careful consideration of the reports given by experimenters shows two different emphases in explanation of the varying effectiveness of guides, an effectiveness measured by the tendency of subjects to react with involuntary movements and by the accuracy of such movements as an index of the direction of attention. Emphasis is laid, in the first instance, upon the need of concentration of attention and success with a given guide is cited as an evidence of his power to concentrate attention; in the second instance, less stress is laid upon this factor and the bearing of thought-reading tests upon automatic or hypnotic phenomena is emphasized.

Granting the contention of certain experimenters that failure with any subject is due either to the latter's inability to concentrate attention steadily, or to his unwillingness to contribute to the reader's success, and his maintenance, therefore, of self-control with consequent inhibition of natural expression, can muscle-reading be used as a simple device for determining relative to any given individual such temperamental tendencies as inability to concentrate attention steadily, constitutional combativeness, power of control over involuntary movements? If so, muscle-reading has its place as a method in the investigation of mental types. Pfungst would add a third possibility of failure due to a tendency for the nervous energy liberated by concentration of attention to drain itself otherwise than through muscular innervation. He found, it will be recalled, certain abstract thinkers very inappropriate subjects. A varying readiness to motor expression might indeed be expected on theoretical grounds.

The first question then that phrases itself relates to the effect upon the outcome of the test of the subject's attitude toward it. Will scepticism as to the outcome or hostility toward the operator's claims or knowledge of his *modus operandi* result in inhibition of the involuntary movements that otherwise would result from the situation?

A second problem involves a consideration of the relation of concentration of attention to success, a determination, particularly, of the degree of concentration which favors success. An interesting development comes when it is discovered in the course of experimentation that the outcome of tests is varied by a change on the part of the subject in the method of concentration utilized. The question is now phrased: What relation, if any, exists between the sort of mental control exercised by the individual in his effort to concentrate attention and his value or worthlessness as an agent in these experiments? Has any one method of enforcing the attention a constant value? Or does the value of a particular method vary with the individual? If so, is there a constant variation dependent upon the sort of sense control utilized? Again, in that case, will the more or the less habitual method of enforcement prove the more effective? The question we are raising is the differing expressive or inhibitive motor value of differing sense forms of attention, a question that muscle-reading as a method of investigation seems peculiarly adapted to answering.

A third problem formulates itself in view of the fact that voluntary concentration of attention on the part of the subject is by no means as necessary to success as certain reports would lead us to expect. How are we to explain successes with distracted attention and those strange cases in which involuntary movements are an index to the past, not the present, direction of attention?[1] What bearing, if any, do these observations have upon the observations of Beard and Laurent that the most effective subjects for thought-reading tests are those individuals in whom automatic tendencies are increased by the narrowing of the field of consciousness through a trance-like condition brought on through undue suggestibility?

The outline of the report is now clear. After a brief discussion of the method used, in general, the writer will report in detail the course followed in an attempt to answer the above questions.

III.

In the tests, which have been under way for something over a year, the writer has served variously as operator, subject, and spectator. As operator (Dy), she has tested every agent, except herself. As subject — an unusually refractory one — she has introspected carefully her experiences under the test conditions. As spectator, she has been able to conduct certain experiments and make observations otherwise impossible. Her experiences as subject were, on the whole, the most interesting and enlightening.

In the tests in which the writer did not serve as operator, her place was taken by Miss Abby Drew (Dw), a college junior, who as a student of psychology had amused herself by developing skill in muscle-reading. As Miss Drew was, like the writer, a poor reagent, a profitable series of tests was that in which these two reagents worked with each other, tests which only after many weeks ended in success.

In all of the tests to be described, contact between operator and guide was made by way of the hands. The operator with her right hand touched lightly either the wrist or finger-tips of the right hand of the guide, or clasped the guide's right wrist with her right hand and touched the guide's right finger-tips with her own left hand. This is, apparently, the method of contact used by Cumberland, the English expert. Brown,

[1] Downey, J. E., 'Automatic Phenomena of Muscle Reading,' *The Jour. of Phil., Psychol., and Scientific Methods*, Vol. V., p. 650 ff., 1908.

whom Beard tested, pressed the back of his subject's hand against his own forehead and with his other hand touched the palmar side of his subject's hand. Laurent reports[1] that test-variations showed the best method of contact to be that in which the subject placed his hand between the shoulders of the operator. All of these methods were tried during the course of experimentation. Brown's method was discarded because of the fatigue that ensued from the strained position of the arm, a strain particularly evident when there was great difference in the height of operator and subject. Laurent's method proved to be excellent so long as general direction of movement was in question but it was found to be much less precise than contact by the hands when it came to identification of an object.

Most of the experiments reported were tried in the psychological laboratory of the University of Wyoming, a laboratory which consists of a double room, the outer section of which opens on a hall and staircase and is separated from the inner by a partition, the windows of which can be darkened with curtains. The length of the two rooms is about thirty-three feet; the width some fifteen feet. The outer room is equipped with four rows of opera chairs, six in a row, and with writing-desk, book-shelves, a swinging blackboard and radiator. The inner room is furnished with three tables, one extending almost the whole length of the south side of the room; the two other tables, much smaller, occupy the north section of the room. This room also contains two apparatus-cases and wide shelves running the north length of the room. Chairs and radiator complete the equipment.

The general procedure was as follows: The operator would withdraw into the hall and during her absence the subject would select and place in a position either exposed or hidden and in either the outer or the inner room, an object for identification. The operator would then be summoned; contact would be made at the entrance to the outer room; a stop-watch would be started; and the test was on. Sometimes, before movement, the operator would resort to a relaxation of the hand and arm of the subject; occasionally the whole body of the subject would be

[1] *Loc. cit.*, p. 488 f.

swung rapidly from side to side. With 'hard' subjects such relaxation was sometimes repeated during the tests. In the preliminary experiments the operator generally took the lead. Dy moved very rapidly and when successful achieved success, usually, in a very short time. Dw moved more slowly but with great accuracy. In the earlier experiments Dy always blindfolded herself carefully before entering the room and was under the impression that this blindfolding was necessary to success. It certainly contributed to her confidence and shut out distracting impressions. Later, she found it sufficient to close the eyes. Dw was confused by a blindfold and preferred merely to close the eyes.

A report of the experiment was made by the writer immediately at the close of each test. In the more complicated tests, these reports were supplemented by records kept by a spectator during the actual experiment. In many of the tests, for instance, the spectator reproduced on a map of the rooms, previously drawn to the scale, the exact course followed by the subject in placing and by the operator in finding the object.

Variations in method will be described in connection with the discussion of different problems. In general, but little attempt was made to produce brilliant or theatrical effects. It was not possible to estimate the actual expertness of the operators. Dy was, however, able to reproduce all of the feats mentioned by the experts, such as writing out dates of which the subject is thinking (contact by operator's left hand only), or the writing of several syllabled words (contact by right hand of operator); finding a book and identifying therein a word chosen at random; successfully locating an object although several persons ignorant of its whereabouts are placed between operator and subject; operating without contact when the subject moves near her. To succeed in these tests, Dy must work with 'fit' subjects. She had never tested herself in the location of an object at a great distance nor can she succeed without contact when the subject is some distance away. The writer has seen but one platform 'mind-reader.' Stripping his performances of irrelevant and theatrical effects, she could have reproduced his results with great ease. The most difficult part of such an experiment, namely, the identification of the object after its general locality is found, was by this operator overcome by himself selecting or naming the object which was to be hidden. To repeat, for the purposes of the present test, precision was of more consequence than brilliancy.

The great defect of the method was the lack of an objective control of the subjective reports. Of course, in a measure, success furnished evidence of the initiative of the guide and of the expertness of the operator; but in the case of failure, complete or partial, it was impossible to determine absolutely whether the failure was due to defective concentration or defective motor impulse on the part of the subject or, rather, chargeable to the maladroitness of the operator. Objective control by way of registration of the involuntary movements was, however, in the present set of tests, out of the question.

IV.

A preliminary experiment involved the determination of the number of subjects with whom the writer would be able to operate successfully. To estimate the percentage of 'fit' and 'unfit' subjects for such a test, rapid tests of a great number of subjects, taken at random, were tried. Only those subjects upon whom the writer took notes at the time of test are included in the summary. Under such conditions sixty subjects were tested; forty in the laboratory as described above; twenty under slightly different conditions and in other surroundings. In the latter case, there were frequently several or many spectators present; in the former, few or none. In the case of fifteen of these subjects but one test was tried. The other subjects were tested two or more times. Forty-three of the sixty subjects were women or girls; seventeen, men or boys. In age, they varied from nine years to over fifty. The table summarizing results follows. By a partial success is meant a case in which the operator went to the article and then withdrew from it or explored in its vicinity without finally locating it.

Whole number of subjects tested, 60.

Number with whom completely successful first trial, 42 (70 per cent.).

Number with whom wholly or partially successful first or second trial, 56 (93.3 per cent.).

Number with whom failed after repeated trials, 2 (3.3 per cent.).

The subjects grouped according to sex give the following record:

Whole number of women and girls tested, 43.

Number with whom completely successful first trial, 29 (67.4 per cent.).

Number with whom wholly or partially successful first or second trial, 40 (93 per cent.).

Number with whom failed after repeated trials, 1 (2.3 per cent.).

Whole number of men and boys tested, 17.

Number with whom completely successful first trial, 13 (76.4 per cent.).

Number with whom wholly or partially successful first or second trial, 16 (94.1 per cent.).

Number with whom failed after repeated trials, 1 (5.8 per cent.).

The ease with which success was obtained, measured either by the time needed to achieve it or the amount of effort required on the part of the operator, varied greatly even with those subjects with whom success was achieved. About eight of the sixty subjects (including the writer, nine of those tested) would be described as particularly difficult to handle. The striking outcome is the great number of individuals indicating by involuntary movements the direction of attention.

Even in the case of the two subjects with whom the writer failed to succeed, momentary indications were given. With these two subjects six tests each were tried. The first subject was tested at the very beginning of the experimental series, at a time when the writer, unaware as yet of the difference in results introduced by variation in the method of concentration, failed to try the effect of a change in the control. The second subject was tested at a time of intense preoccupation, on the part of both operator and subject, with disturbing matters so that neither was in good condition for the test. It is not probable that these two subjects would have proved, under other circumstances, more difficult to handle than did the others of the eight cited above, with whom the writer ultimately succeeded.

The figures given, namely, that, among sixty subjects, about fifty-two would be fit subjects for muscle-reading tests may be compared with estimates previously made. Gatchell speaks of 'good' subjects, without specification of the degree of effectiveness required to admit one to this rank, and cites as such, 'about one person in five among young people and one in ten or twenty among adults.' The present estimate of fit subjects would be about twenty-five in thirty. This does not mean, of course, that long and complicated experiments would succeed with such a large percentage but that the simple test of locating an article within the compass of a fairly large room would be easily achieved. Pfungst's tests in the laboratory gave complete failure to react with involuntary movements only in the case of two out of twenty-five subjects. Cumberland estimates that he would succeed with nine out of ten persons. Such reports agree well with that given here.

V.

To turn now to a discussion of the questions already formulated. First, as to the effect upon the outcome of the experiment of the subject's attitude toward it. Will scepticism as to the result, or hostility toward the operator's claims, or knowledge of the *modus operandi* result in the inhibition of the involuntary movements that otherwise might result from the situation.

Subjects grouped in respect to their attitude toward the test fall into three classes. There are, first, those subjects who are predisposed to believe in the operator's power 'to read their minds.' Such subjects are charmed when the operator succeeds with them, taking success as a compliment to their strength of will, their magnetic influence, or as evidence of the operator's occult powers. There are, secondly, the sceptical subjects who are inclined to believe that a trick explains any successes they have witnessed, who are angry and ashamed if they prove to be usable subjects. A third class of subjects apparently submits to the test with little predisposition other than to follow the operator's directions with perfect candor.

An attempt to classify one's subjects with reference to their faith or scepticism is, however, obviously open to gross errors.[1] One can only conjecture a subject's attitude from his behavior; to resort to direct questioning is of little value. But the acceptance of success as evidence of some occult influence and an eagerness 'to have one's mind read' bear witness to a high degree of credulity. A sceptical attitude is much harder to discern, although it is sometimes evident from a subject's scornful exclamation, "You couldn't succeed with me!" Such a subject will not believe, except from first-hand experience, that the guidance in such tests is involuntary.

The notes of the writer show that success has often been achieved, and at times with great ease, when the subject's attitude was evidently one of profound scepticism as to the outcome. The chagrin of the guide at the success of the operator was frequently ludicrously apparent. On the other hand, failure occasionally resulted even with a highly suggestible guide, a fact not surprising of course since failure to concentrate attention might be cited as a cause. On the whole, however, the difficult subjects were those who assumed a critical attitude during the course of the experiment. It is customary to assume that the result of such scepticism is to fasten the reagent's attention on the idea of keeping the operator away from the chosen object. That such is frequently the case is shown by the fact that with some subjects it suffices to change failure into success by a shift in tactics, by following the line of most rather than that of least resistance. What is sometimes called 'physiological dishonesty' issues therefore in expression, however

[1] In a semi-public test at the University of Wyoming, the writer once attempted to effect a segregation of 'believers' and 'unbelievers' by asking spectators to seat themselves on the right if they thought they would be good subjects for muscle-reading; if not, to take seats on the left. The majority of those so instructed took seats on the right. Subsequently, however, the statement of the president of the university that he must sit with the 'unbelievers' induced all but a handful of those who were already seated on the right to move over to the left! With one exception, however, the 'unbelievers' who were tested proved as 'readable' as the 'believers.' The president was found to be an excellent subject.

difficult its interpretation, as evidently as does 'physiological candor.'

Pfungst[1] reports that involuntary movements are affected by faith. Even the most 'fit' of the operators with Hans, the horse, were unable to get answers from him when, according to their understanding of the situation, conditions had been so varied as to render success impossible. Pfungst also quotes from records relative to table-tipping and wand-divining to show that with the cessation of expectation of results none came. Pfungst explains this inhibition of expression as the result of diminished tension due to lack of faith in the outcome. Frequently, however, attention is actually concentrated upon something other than it was before. The involuntary expression of the thought, "The table won't tip" may be wholly different from that accompanying the thought, "The table is going to tip." Variation in muscular tension with variation in expectation must, however, be conceded.

Complete passivity in the face of actual concentration of attention is much less frequent than misleading tension; it is, in fact, of such rare occurrence that the writer is ready to assert such only in occasional tests with perhaps four or five subjects.[2] Of course, the absence of all muscular indications could, even in these cases, be charged to failure to concentrate sufficiently. The writer who belongs to this group of subjects and whose arm and hand during visual (though not verbal) concentration of attention may remain perfectly limp, is inclined to believe that muscle-reading actually reveals to some extent the facility with which nervous energy is drained to the motor regions of the cortex. The habitual absence of a high degree of expectation even when attention is concentrated would suggest several interesting questions as to the mental constitution of a person exhibiting such a tendency. Is it not possible that lack of readiness toward a motor discharge might lead to weak expectation as well as the reverse? If so, the relation of weak expectation to a critical or neutral attitude would demand attention. In any

[1] *Op. cit.*, p. 112 f.

[2] Such a statement must of course be taken with the understanding that the operator's skill in perceiving muscular changes was limited.

case, one needs to distinguish between the subject who, through disbelief in the operator's claims, attempts actually, though involuntarily, to thwart him, and the subject who submits to the test without expectation of any sort. The latter subject is the harder one to handle.

In an effort to note what effects would result if the subject were instructed to keep the reader from discovering the object selected, tests of this sort were tried with eight guides. It was found that if the guide tried to 'fool' the operator by actually concentrating on another object rather than the one selected, he could succeed easily. On the other hand, the attempt to keep the reader from success by making the mind a blank, by relaxation or stiffening of the muscles, or by such verbal inhibition as saying, "You can find it," was a failure. These experiments were few in number and, for the most part, tried only upon particularly 'fit' subjects. A series of experiments in which various methods of inhibition should be tried would be of value as a supplement to the tests to be reported later.

The degree to which expectation is excited by an anticipated end and the tension which accompanies such expectation is nicely determined in a series of tests in which expectation is unsatisfied, for the anticipated result fails to occur. A few of such tests were tried in connection with the present investigation. To bring about the desired conditions, it was necessary to blindfold the guide as well as the reader and to instruct a third person, in the absence of both, to remove the selected article after the subject had placed it in position or else to block the pathway to it. The results furnished pretty illustrations as to the motor outcome of baffled expectation. Frequently, under such conditions, the movements of exploration became extensive. Nor did the subject always confine himself to exploration of the immediate neighborhood; he sometimes rambled throughout the whole room. Other subjects in failing to realize their expectation indulged in suppressed or overt exclamations. A nervous fluttering of the hand, very difficult to describe, frequently was noticed. H, one of the most valuable subjects for this test because of the exceeding urgency of her expectation, reported that failure to realize her expectation —

which always takes the form of confirming her visual image of the object to be located — results in a complete blotting-out of the object from memory. With the annihilation of the object, the subject is 'lost,' a distressing experience, accompanied by bewilderment and faintness and comparable to the subject's experience of being lost in a hazel-copse as a child. On at least one occasion it resulted in a peculiar pain in the head. Meanwhile, the operator receives indications of the subject's state of mind through the wild fluttering of the exploring hand, a fluttering perfectly evident to the spectator as well as the operator but of which the subject (when questioned later) reported unawareness.

Pfungst[1] found that certain agents who worked with Hans succeeded in obtaining answers at their first trial, but not thereafter. He explains this result on the ground that attention was in the first trial at a higher tension than at any succeeding trial. On the other hand, too great concentration frequently led to premature relaxation of tension and resulted consequently in errors on the part of the reagent. On the whole, practice was needed in order to achieve the degree of tension required for successful operations. After practice less effort was required than at first. The present writer has noted similar facts, except that after one success with a given subject she has never found it difficult to achieve a second,[2] although in one case several failures intervened between the first and second success. Usually, the first success conciliates the subject; thereafter, success is more and more easily achieved. There was manifestly less effort on the part of the subject in the later tests; there was less tension evident but more initiative.

Pfungst found that 'Hans' was, as percipient, very little

[1] *Op. cit.*, p. 148.

[2] The difference between the muscle-reader and the percipient in Pfungst's experiments, whether Hans or a human reagent, should be noticed. The muscle-reader has the advantage, since by relaxation of the subject's muscles and by such feints as tentative moves — moves made suddenly and sometimes violently — he is able at times to surprise the guide into involuntary indications of the direction of attention. The operator who reacts to a visual perception of an involuntary movement enjoys no such opportunity. The compensating advantages are, of course, the greater precision and simplicity of the latter test.

affected by the presence of spectators, although his human partner in the test might be influenced by the social environment. In the present test the general effect of an audience may be summarized as follows. Frequently, the presence of spectators so embarrasses subjects as to render concentration of attention difficult. Just as frequently, however, the conditions increase expectation and magnify involuntary movements. The writer is as operator rendered somewhat 'nervous' and less sure of herself by an audience. Blindfolding reduces this timidity and a first success puts her at her ease. It is possible that a first success also influences the attitude of later subjects and renders success an easier matter. Moreover, spectators frequently contribute to success by their movements in watching the test, their variations in tension and relaxation made manifest by differences in breathing and the like. No tests, therefore, in which a control of conditions was desired, was tried in the presence of more than one spectator. When it was possible even this spectator was dismissed.

Other investigators, as Beard, have reported that a knowledge on the part of the guide of the *modus operandi* had an inhibitive effect. The writer never made any secret of the explanation of muscle-reading. Frequently, in fact, the explanation was given before the demonstration. It does not follow that the explanation was accepted. One of the surprises of the investigation has been the refusal of many subjects to accept the writer's explanation of her success. One guide (H) insists that she finds by actual experiment that when blindfolded she is unable to move as directly and accurately to the object as the reader does and that, therefore, the reader must be responsible for the guidance. What is emphasized by such facts is the extraordinary difficulty of bringing such involuntary movements to attention. Only a few of the subjects tested by the writer have ever succeeded in observing them even when warned to be on the watch. Certain subjects were, however, put on their guard by the explanation given them and it is unquestionable that in the case of these few knowledge of the explanation rendered success more difficult. When, however, success was achieved these subjects were of all the most mystified. The vital question whether these involuntary movements are merely unobserved or actually unconscious is one which at this point the writer is not prepared to discuss.

VI.

The second division of the discussion involves a consideration of the degree of concentration needed for success. In the present investigation no measurement of this was possible. Pfungst's tests, in which a too intense concentration led to premature relaxation of tension with a resulting error, usually of minus one, in the calculation and an insufficient concentration led to an insufficient relaxation with an error of plus one or more in the calculation, show with great precision the degree of attention attained. Nothing of the sort was possible in the tests reported here except that the operator frequently observed relaxation which was premature and therefore incorrectly identified an object perhaps in the near neighborhood of the correct one.

The interest in the present investigation turned rather upon the effect upon involuntary movements of a shift in the method by which attention was controlled. The experiments had not gone far before the bearing upon success of the control utilized was perceived. Success was achieved with some subjects more easily with their eyes open than with their eyes closed. With others these conditions were reversed. Again, there was frequent report of a verbal control; the guide, for instance, said mentally, "It's the book over there on the radiator," or used similar descriptions. Now the verbal method seemed to Dy and Dw, both of whom concentrated on the object by focusing it visually or by forming a mental picture of it, a strained and artificial method of control. It therefore surprised them greatly to discover that a shift in their own control from visual to verbal assured success in their experiments upon one another, an issue which up to the time of the shift had been an uncertain and sporadic occurrence. It was also found that other difficult subjects became docile when asked to concentrate verbally. Moreover, in the case of effective subjects, whose control was visual, it was found that a shift from the visual to the verbal control frequently resulted in more extensive movements and more pronounced initiative. Evidently the matter of verbal control deserved consideration.

Laurent [1] in the article previously quoted reports that the guide in the muscle-reading tests was asked to form a visual image of the object selected and also to think of the direction in which it was necessary to move in order to get it. A method of dichotomy was effective for the latter. The guide, that is, thought 'left' or 'right.' Laurent makes no comment upon the choice of verbal directions for the tests. In his experiments upon thought-reading without contact, subjects who resort to unconscious verbalization are of course necessary to the success of the experiment if Laurent's explanation of success as dependent upon hyperacousie on the part of the operator be accepted. The interest in such tests is, however, fundamentally different from that of the tests to be recorded in which the effect of mental verbalization upon involuntary movements read through contact is in question.

Pfungst [2] found that commands to 'Hans' spoken aloud were frequently more effective than commands merely represented mentally. This effectiveness was determined by the strong tendency on the part of the experimenter to accompany such spoken commands with involuntary movements, a stronger impulse to such expression being present under such circumstances than under the conditions of mere thought of the command. With practice, however, overt or suppressed articulation could be omitted for mental representation was sufficient to call out the involuntary movement. Again,[3] the observation was made that some experimenters, failing to obtain results on account of fatigue from previous tests, could again achieve success by a shift from abstract calculations to concrete representations. Pfungst's explanation of such facts is, I believe, based upon the greater concentration of attention effected by overt articulation of a command or by concrete perception of an object. Such observations, whatever their explanation, are akin to those that suggested the experiments now to be described.

[1] P. 484.
[2] *Op. cit.*, p. 72 f.
[3] *Op. cit.*, p. 108.

VII.

Before conducting a complete series of tests to determine the relation between different modes of control, a somewhat crude experiment on the efficiency of verbal control to induce involuntary movement was tried. Certain subjects, who had no knowledge of the purpose of the test, were asked, instead of placing an article, to memorize a sentence type-written on a slip of paper and to repeat over and over mentally, while in contact with the operator, the words so memorized. The sentences were so worded as to rule out if possible visual imagery and were purposely rendered as schematic as possible so as to isolate the verbal element. In order that when Dy served as reader she might not be aware of the reading of the slip memorized, seventeen of these slips were prepared some time before the experiment was to be tried and the guide on the occasion of the test drew at random one of them from the bundle. Two tests, under these conditions, are quoted in full. In both of these tests the same slip was drawn which read as follows: "Object is eighteen inches above and six inches to the right of the lower part of second obstruction which is two feet east of first obstruction which is three feet south and fifteen feet north of entrance." The subject made, usually, no attempt to translate such instructions into terms of the surroundings in which the test was tried. No suggestion was made that close adherence to the directions would issue in finding a pin which was stuck into the under side of a book-shelf. Both the operator and guide were blindfolded before contact was established. The notes taken at time of the tests follow.

"Mar. 4, 4 P. M. Reader, Dw. Guide, W. W memorized slip 8. First trial, much initiative, but failure to discover object. Dw went around the room, south, then east, then north, then west, returning to starting point. Tendency noticed for W to swing around Dw. Second trial. Care was taken that W should be placed, by the spectator (the writer) facing the wall so that directions if followed would result in success. This precaution had been overlooked in the first trial. At start, tendency for W to swing around Dw again noticed. Then success in locating article in 90 s. Dw however approached the object from the left instead of the right; W's memory of the slip was found to be faulty in this respect. W reported that he had no visual consciousness during the test; he repeated the words of the slip over and over. His surprise at there being any outcome to the experiment was great."

"May 7, 3 P. M. Reader, Dy. Guide, M. M memorized slip 8. Thinking the test was to be one of distraction and that the words memorized, which she supposed were meaningless, were to be repeated over and over so as to distract attention from the actual object, M hid an object on the ledge in the southeast corner of the inner room. This object was completely blotted out by the repetition of the memorized words. There was much initiative on M's part, as the slip directed. Success in 80 s." The chart, kept by a third person, on which the course taken was mapped out shows a certain amount of circling movement before the article was located.

Both W and M are exceeding automatic in their movements; both show a strong tendency to use verbal imagery which issues at times of difficulty in actual articulation. Other subjects gave different results. H, for instance, immediately translated the words read into visual terms, and remembered them in such terms. She also visualized an object which should be found as an outcome of the test. Such an object was specifically defined as, for instance, "a small black ball about the size of a cherry on a white string that is suspended from a nail above the blackboard." Failing to realize such an expectation, H becomes confused and distressed.

VIII.

The general outcome of the tests showed, however, the potency of verbal imagery in the initiation of involuntary movement even when the words ideated are felt to have little meaning. But further questions arose. For example, would verbalization of the name of the object avail as did verbalization of the direction in which the movement should be made? It seemed, in fact, probable that the potency of the verbal method was due to the enforcement in this manner of attention upon the pathway and that any other method that threw attention upon the pathway as definitely would be as effective.

Throughout the tests, it must be remembered, success is due to the involuntary indication of the path to be followed in going to the object. If this path be indicated, further consciousness of the object is immaterial.

To determine, however, the motor effectiveness, involuntary indication of the path to be followed, of various possible controls, the eight following possibilities were selected for experiment. The guide was required, that is, to concentrate by instruction upon some particular element in the situation. (1) The guide focused his eyes on the chosen object; (2) the guide focused his eyes on the pathway, step by step; (3) with eyes open and roving, the guide 'verbalized' mentally the name of the selected object; (4) with eyes open and roving, the guide 'verbalized' mentally the direction in which the reader should move in order to reach the object; (5) with eyes closed, the guide visualized the object; (6) with eyes closed, the guide visualized the pathway; (7) with eyes closed, the guide mentally 'verbalized' the name of the object; (8) with eyes closed, the guide mentally 'verbalized' the direction in which the reader should move in order to reach the object.

These particular methods were selected so as to determine, if possible, the relative value in the induction of involuntary movements of open versus closed eyes, concentration on the pathway versus concentration on the object, concentration by visual control versus concentration by verbal means. One series of tests consisting of eight separate tests, one test each under the different conditions suggested, would give four tests each with closed and open eyes, with concentration on pathway and on object, with concentration by visual and by verbal means. In the earlier experiments the method of 'control' was suggested to the guide and the pathway traversed in finding the article was carefully mapped; the time taken for the location of the article was also recorded. A comparison was then instituted relative to the precision and rapidity of movement under the several conditions of concentration.

Later, the experiment was rendered more definite by the adoption of the two following methods, the second of which proved the better and was finally used without recourse to the other. The first method attempted to determine the value of a particular method of concentration by measuring the distance traversed by the reader during a given time; the second method sought to measure the relative efficiency of the different methods of concentration by recording the time that it took for the reader to traverse a given distance.

The first method may be described in detail as follows: The guide first chalked on the floor an irregular pathway, indicating by cross-lines each meter-distance. The guide also prepared a list of the eight methods to be used in concentration on the object and then selected and indicated by number the order in which he intended to use these methods. The reader blindfolded was led to the beginning of the chalked path, contact was established and a third person gave to the guide the signal to begin concentration by gently touching him on the arm by means of a long pointer, at the same time starting a stop-watch. At the end of every twenty seconds, when the guide was signaled to in similar fashion, he changed his method of concentration. During the intervals the third person charted carefully on a map that had previously been drawn to scale to correspond to the chalked pathway, the pathway the reader followed. Thus every deviation from the correct pathway, or, if none, the exact space traversed in any given twenty seconds could be determined. The value of this method lay in its throwing into sharp relief the variations in muscular tension effected by a shift in mental control.

In the second method, simple irregular pathways, always three meters in length but varying in form, were chalked by the guide and the time needed for the reader to traverse these pathways under the conditions of the several tests was recorded by the use of a stop-watch. In these tests the object was in every case the same, namely, a piece of chalk placed on a chair at the end of the pathway. The short pathway and the easily identified object were purposely chosen in order to shorten the time needed for controlled concentration and to simplify the identification of the object. As before, the reader was blindfolded and then led to the beginning of the pathway.

It was essential throughout that the reader be in complete ignorance of the particular method of concentration used in a specific test and that she refrain from making tentative moves, waiting passively in each case to receive the initiative from the guide. It was also desirable that the guide have no idea of the particular purpose of the test, for suggestion as to the results anticipated would no doubt affect the outcome. In the case of Dw and Dy as guides this last condition could not be fulfilled. As, however, the course of the previous experiments had unexpectedly revealed the varying effectiveness of different methods of concentration, these tests served to put into more precise form conclusions that had already been reached in the course of the preliminary experiments.

In general, the following sources of error were present. On the part of the guide, failure to control attention in the way desired on account of lack of practice or, at times, on account of fatigue; on the part of the reader, variations in skill due to fatigue or anticipation. Moreover, the reader's observation of muscular conditions was defective since any attempt to throw attention upon such during the course of the experiment was apt to interfere with the passivity so essential to success. With increasing practice, such observations interfered less and less with skillful reading. The reader, as was said above, refrained from tentative moves; to assert absolute absence of initiative would, however, be impossible.

An important preliminary test was that tried in order to ascertain the method used naturally by the guide when asked to concentrate on an object. By reference to this test it was attempted to determine whether or not the natural method of control were the most effective one from the reader's standpoint. After practice with any particular subject, the reader found it possible to name with considerable accuracy the method of concentration that the subject was using in a particular test. The constancy with which variations in muscular tension ensued upon changed conditions was surprising, although such changes became less noticeable after the series of tests had been repeated many times, with a given subject.

IX.

In discussing results, the tests upon Dy and Dw will be first considered. Each it will be recalled served as reader or guide for the other. Both were difficult subjects to handle. Although verbalization occurred at times, both concentrated on the object, for the most part, visually. With Dw serving as guide there was considerable initiative, apparently in the direction of the eye-movements. The reader frequently went directly toward the chosen object at the beginning of the test; then withdrew and rambled aimlessly, although apparently in obedience to Dw's initiative. Identification of the object was exceedingly difficult, even when it was touched. Both premature and insufficient relaxation were noticed. When Dy attempted to read slowly instead of rapidly and rendered herself unusually passive, waiting in every instance for the motor impulse to begin with the head, success was more apt to be achieved. If Dy attempted tentative movements, Dw responded with the suggested movement. Dy, on the contrary, was as guide absolutely passive; her hand hung limp. Dw reported that it was necessary to pull her forcibly if movement were to be initiated at all. It should, however, be stated that frequently Dy and Dw would each insist that in a particular instance the other had taken the initiative. With Dy as guide, distraction of attention rendered success more likely to occur. This result did not occur in the case of Dw. In the earlier experiments, up to May 15, before, that is, any attempt was made to control the method of concentration, Dw as reader worked with Dy as guide some fifteen different times. In five of these tests Dy's attention was distracted from the object by counting aloud. In the ten experiments without distraction, success or partial success occurred four times; in the five tests with distraction, three times; seven successes in all. With Dy as reader and Dw as guide, seventeen tests were tried; twelve without and five with distraction. Seven successes or partial successes occurred when there was no distraction; one, when there was distraction; eight successes in all. By a partial success is meant the approach towards and perhaps selection of the chosen article without confident identification of it.

The notes on the tests tried May 15 include the following statement: "Today's success may be due to the guide's method of concentration of attention on the object or may be due to the reader's unfatigued condition." In any case, on that day, for the first time, attention was controlled according to instruction, the 'controls' suggested including one in which verbal suggestions were mentally given as to the proper direction of movement. This 'control' resulted, both when the eyes were open and when they were closed, in rapid and easy success. On this occasion, Dy served as reader and Dw as guide. May 20, the rôles were reversed and again the mental giving of verbal directions proved successful, although success was more slowly achieved than on the previous occasion. The deliberate attempt to control attention, whatever the method used, probably resulted, on the whole, in actual increase of attention to the object with less consciousness of inhibition of the guide's movements. The tests that followed introduced systematic control of attention and attempted to determine the value of each different 'control' by a determination of the extent of movement during a given interval, the course followed by the reader being mapped out carefully as described in the first method.

Although there was considerable variation in detail, it became evident from eight series of tests in which Dy served as

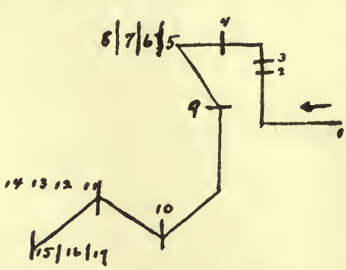


FIG. 1. 1, 9, eyes open, 'verbalized' pathway; 2, 10, eyes closed, visualized pathway; 3, 11, eyes open, fixated pathway; 4, 12, eyes open, fixated object; 5, 13, eyes closed, visualized object; 6, 14, eyes open, 'verbalized' object; 7, 15, eyes closed, 'verbalized' object; 8, 16, eyes closed, 'verbalized' pathway.

guide, that concentration on the path induced much more movement than did concentration on the object. Moreover, a verbal concentration on the path was a more reliable method than was control by visual means. Verbal concentration on the object had little effect. Holding a visual image for any length of time requires great effort on Dy's part, although chalking the pathway lessens the effort required to visualize the path. Fig. 1 reproduces the chalked pathway used in the test of July 15. The diagram is drawn to scale, a centimeter for a meter. The cross-lines indicate the places at which the control of attention was shifted, such a shift occurring every twenty seconds. The numbers on the figure indicate the sort of 'control' utilized by the guide as interpreted in the legend below the figure. The distance moved by the reader during any particular twenty-second interval can be determined by the distance between any two cross-lines; the method used by the guide in concentrating attention can be seen by reference to legend. Where numbers or cross-lines appear to fall off the path, hesitation (without advance) is indicated. In this test the reader never left the pathway, although during several intervals there was no apparent progress.

Dw when serving as guide gave somewhat different and, on the whole, less constant results. As suggested before, Dw's attention is apt to be diverted by visual stimuli when her eyes are open. This observation accords with the fact that in the tests now being described concentration on the path, by whatever means, issued in success if the eyes were closed. Thus verbal concentration with the eyes closed proved more effective than the same method of control when the eyes were open. Visualizing the pathway, with closed eyes, was also an effective method of control.

At this point in the experiment the method was shifted to that described as the second method (see section VIII.). The value of each method of concentration was now estimated by the time taken by the reader in traversing a three-meter pathway. The results of this test confirmed those obtained in the preceding test, so far as Dy was concerned. The results from Dw were ambiguous and unfortunately circumstances made it necessary to bring the investigation on this guide to a premature close. The full notes on a test with Dy are given; Dw served as reader.

"Dy when serving as guide notices a strong tendency to close the eyes when focusing the eyes on the object or on the pathway. This tendency is perhaps due to the fact that meaning evaporates from a visual perception after a few seconds steady concentration upon it. On the other hand, distinct visualization (mental) affords intense concentration for a few seconds, after which the visual image fades completely and there is a strong desire to open the eyes and get a new picture. While controlling attention by the use of mental verbalization, there is little need of inhibiting visual control. During visual concentration, all sort of irrelevant verbalization occurs. Strangely, Dy serving as operator notices no such tendency to verbalization; at frequent intervals, a stray visual image of the pathway or of surroundings enters consciousness. The specific tests resulted as follows:

"1. Dy gave, mentally, with eyes roving, verbal directions to Dw. Dy said, 'To right'; then 'Towards telephone'; then, 'Straight ahead.' Strong tendency noticed to look in direction named. Dy deliberately kept eyes from direction named. After words were once determined upon, Dy could repeat them mechanically. Success, 72 seconds.

"2. Dy closed eyes and formed a mental image of the object (a piece of chalk). Muscles were very tense. The reader got off the path, but brushed terminal chair, which caused chalk to rattle. This shortened discovery. Dy took no initiative. There was long hesitation before the reader moved. After the first seconds Dy found visualization very difficult. She opened her eyes occasionally to get a new picture of the chalk. There was also some difficulty experienced in ruling out the visual picture of the chalked pathway. Strong tendency to turn head and closed eyes toward object. When they were deliberately turned aside there was great tension in the neck-muscles. Dy found herself saying mentally, 'I can see it! I can see it!' Success, 126 seconds.

"3. Dy visualized the pathway, which, to assist the process, had been chalked in the form of an equilateral triangle. Muscles were reported to be less tense than in the preceding experiment. Dy found no difficulty in shutting out an image of the object.

There was a good mental picture of the triangle, but suddenly the triangle shifted its position in the room, which induced confusion. Dy was obliged to open her eyes and fixate the triangle again in order to get it in proper position. She verbalized involuntarily, 'I've got the triangle!' and as Dw advanced, 'That's right!' Success, 67 seconds. Test to be tried again.

"4. Dy repeated over and over again with eyes closed, the word 'Chalk,' actually innervating tongue and lips. There was no overt articulation. The verbalization was largely automatic; there was no meaning to it; the object was forgotten; attention was actually on the movements of the jaw. Once, there was a flash in Dy's consciousness of a picture of the pathway, accompanied by a picture of the whole floor of the laboratory. Once, the piece of chalk was seen mentally. Dw reported that the muscles of the wrist were tenser in the earlier than in the later part of the test. Pathway was not followed accurately. Success, 102 seconds.

"5. Dy watched the pathway step by step. Dw followed the pathway accurately. Dy found it hard to inhibit verbalization of thoughts on a topic foreign to the experiment. Unless Dw moved so as to change the point of visual fixation, the whole process lost meaning for Dy. Success, 133 seconds.

"6. Dy focused her eyes on the object. It was 40 seconds before the first movement was made. Dy verbalized mentally, 'It's no trouble to look at it!' Then her attention wandered and the object became unmeaning. Dy said mentally 'Dw should be blindfolded; it would be easy for her to open her eyes,' then, as Dw left the pathway, 'I must remember to enter in the notes that she got off the path and rambled.' At last, Dy feared failure; she said mentally, 'I must concentrate!' With great effort she inhibited verbalization. Success, 245 seconds.

"7. Test 3 was repeated (visualization of pathway). Good control; visualization excellent; verbalization inhibited most of the time, although once Dy said, 'I mustn't open my eyes!' and again, 'If I don't talk to myself, I won't have to write such a long report.' Pathway was followed pretty accurately. Success, 148 seconds.

"8. With eyes closed, Dy told the reader, mentally, in what direction she should move. She said 'Straight ahead; straight ahead.' Muscles were relaxed. There was no visual imagery; no effort. Success, 60 seconds.

"9. With eyes roving, Dy 'verbalized' mentally the word 'chalk'! It was ninety seconds before Dw moved. Dw then rambled over a big part of the room. The locating of the chair was perhaps accidental. Success, 235 seconds.

Arranging the tests in the order of time required for location of the object and beginning with the test requiring the least time, we get the following arrangement: (1) Eyes closed, verbalization of direction of movement, 60 s.; (2) eyes open, verbalization of direction of movement, 72 s.; (3) eyes closed, verbalization of the name of the object, 102 s.; (4) eyes closed, visualization of the object, 126 s.; (5) eyes opened, focused on the pathway, 133 s.; (6) eyes closed, visualization of the pathway, 138 s.; (7) eyes open, verbalization of the name of the object, 235 s.; (8) eyes open, focused on object, 245 s. It was unfortunate that the series could not be repeated many times as was done in the case of other guides. Dw's help was needed as reader and, as was stated above, circumstances prohibited Dw's further assistance.

How very slowly the reading went when Dy served as guide may be realized by a comparison of the time-reading in her case and in the case of H, whose record remains to be discussed. In H's case, the maximum time-reading in nine complete series of eight tests each, was 25 s.; the minimum time-requirement was 4.5 s.

X.

H, as a guide in the test, affords, in almost every respect, a complete contrast to Dy. H has served so frequently as a subject for the writer that before beginning the series on muscle-reading, the writer was aware of many of the features of her mental make-up. She knew, for instance, that H gives evidence of automatic tendencies; that, for H, to think and to act are almost synchronous; that long mental hesitation is for H distasteful. H's mental stuff is visual to a higher degree perhaps than that of any other person the writer has ever tested. It is not only visual but concretely visual and circumstantial to the last detail.

In anticipating a committee meeting, for instance, H sees each individual member of the committee in the proper environment. The mental picture includes the least details, even to the shoestrings that lace A's and B's shoes. Moreover, in her visualization, H never departs from the dictates of experience. A description of a bird occurs in her reading; immediately, she illustrates the text with a mental photograph of a little gray bird that she saw, a year ago, sitting on the lower branch of the cottonwood tree that stands at the corner of M and N streets. Proof-reading with the writer one day, H stumbled over the accent of the word 'automatic.' Finally she remarked, "I've got it now. I've put a little picture of Tom H. over the letters 'tom.'"

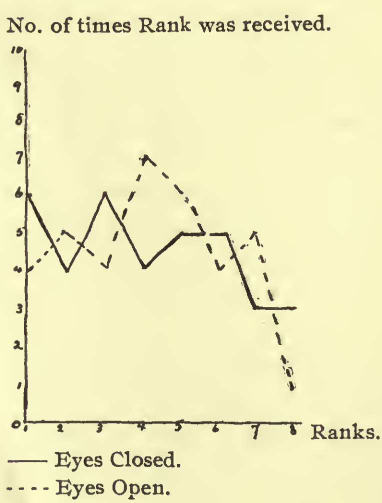
H was a most effective subject in the present tests. The results she gave were constant; her introspections were of high value. Her only difficulty lay in the inhibition of visual control when it was desired to isolate verbal control. To do this completely is painful; as said before, H under such conditions feels 'lost.'

The writer has record, with full notes, of some ninety muscle-reading tests carried on, by Dy, with H as subject. From nine series of three-meter tests (eight tests in each series, eight different 'controls' being used as described in section VIII.), the following curves were prepared. The value of each 'control' in its own series was determined by the length of time taken to traverse the three-meter path, previously chalked. Each test was given a rank in its own series, the possible ranks numbering from one to eight. When two tests took the same time, they were given the same rank. From the seventy-two tests the following curves were obtained. The horizontal numerals represent the possible ranks in the series; the vertical numerals represent the actual number of times each rank was received by the control in question. Six curves were plotted: the first (Curve I.) shows the comparative effectiveness of 'control' when attention is on the pathway and when it is on the object; the second (Curve II.) shows the comparative effectiveness of visual and verbal 'control'; the third (Curve III.) shows the comparative effectiveness of the 'control' with the eyes open and with the eyes closed.

From these curves it is evident that success was achieved in shorter time when the object was focused and the attention with-

Curve I. Subject H.

Curve II. Subject H.



Curve III. Subject H.

drawn from the pathway. Mental verbal control, on the whole, induced slightly more rapid movement than did visual control. It made very little difference in rapidity of the test whether the eyes were open or were closed, although, if the results permit generalization, the 'control' was slightly more effective when the eyes were closed. Adding together the ranks received in the nine series by the different 'controls,' the lower numeral indicating the more rapid initiative, results as follows: Attention on the subject, 123; attention on the pathway, 176; verbal attention, 144; visual attention, 155; 'control' with eyes closed, 148; 'control' with eyes open, 151. If one sums the ranks in the nine series for each of the eight 'controls, the following ranking occurs: Eyes closed, visual image of object at focus of attention, 27; eyes closed, attention on verbal naming of object, 31; eyes opened, focused on object, 31; eyes open and roving, attention on mental 'verbalization' of name of object, 34; eyes closed, attention on 'verbalization' of direction of movement 38; eyes open, attention on 'verbalization' of direction of movement, 41; eyes open, visual focusing of pathway, step by step, 45; eyes closed, visualization of pathway, step by step, 52. In time, the tests took from 4.5 seconds to 25 seconds. In 65 per cent. of the seventy-two tests, the time-reading is below 8 seconds.

Following the pathway is such an automatic process for H that any attempt to focus it voluntarily involves effort of attention. The notes show that when attention was thrown upon the pathway, the muscles of the wrist and arm stiffened, the initiative was very slow and very precise, and there was every indication of effort. Although H very rarely succeeded in wholly ruling out visual imagery — a mental picture of the chalked path in whole or part was usually present more or less clearly — any attempt to throw visual imagery into the foreground resulted in increased tension. A visual concentration on the pathway led to the most accurate and precise tracing of the pathway. Concentration on the object reduced the muscular tension and quickened the process of reading. Mental naming of the object proved very effective. When allowed to select her own method of 'control,' H resorted naturally to visual concentration on the object, which, as the results of the tests show, was a very effective method.

H's guidance in muscle-reading is pronounced. Frequently all that is necessary is to place one's self in advance of H, establish contact (although contact is often unnecessary, probably), and then to move forward with her. Verbal control, especially, issues in extensive movements on the part of the whole body; there is a very free swinging movement on the part of the arms. The change to the muscular tension that accompanied a visual focusing of the pathway and the insistence upon the precise tracing of this pathway was very noticeable. Of her initiative, H is unaware, and as stated before, argues that the reader must be responsible for the direct location of the object since she herself finds it impossible, when *blindfolded*, to locate it voluntarily as accurately and directly as the reader does in the test. This statement led to trial of the time that it took H voluntarily to pace a three-meter path, similar to those used in the tests. The time-readings ran from 4 seconds to 8.5 seconds, with this curious result that, with her eyes closed, H actually did find it difficult to move directly to the terminal chair. If she actually ruled out of consciousness a picture of the chalked pathway, there was much facial contortion and an incoördination of movement that contrasted strangely with the quickness and precision of the involuntary movements.

XI.

Besides the tests on H, the three-meter tests were repeated on J, C and R.

J was, like H, a good subject for muscle-reading. The time-readings in eight series of tests (eight tests to a series) ran from 4 seconds to 36.2 seconds, with 68 per cent. of the time-readings below 8 seconds. It was found with J, as with H, that concentration on the object gave a shorter time-reading than did concentration on the pathway. The curves plotted to show the comparative effectiveness of such 'controls' in the case of J closely resemble those found for H. Voluntary concentration of attention upon the pathway resulted as in the former case in effort, greater muscular tension, and slower initiative. In J's case, however, and this in contrast to that of H, a verbal 'control' induced quicker initiative than did a visual 'control.' Naming the object mentally was by far the most effective method for J. Visualizing at first required distinct effort, although during the course of the experiments this effort lessened and the muscular tension under such conditions became less noticeable. Closing the eyes raised the time-readings over those found when the eyes were open. When uninstructed, J uses verbal imagery of some sort.

The results with C were less well defined than those obtained with J and H. The time-readings ran from 7.4 seconds to 215 seconds, with 50 per cent. of the readings below 20 seconds. C showed a strong tendency to move in the direction of his eye-movements. Visual fixation of the object resulted in cutting of the pathway. When the eyes were roving, this tendency led to rambling, particularly if attention was maintained by verbalization of the name of the object. The closing of C's eyes made identification of the object a difficult matter for the operator. In general, success was more rapidly achieved when the object, rather than the path, was fixated, and much more rapidly achieved with a visual than with a verbal 'control.' Closing the eyes and giving verbal directions retarded the movement but issued in a precise retracing of the pathway. Focusing the eyes on the object proved to be the quickest method.

R (two series, of eight tests each) gave results very similar to those found with Dy. R was an exceedingly difficult subject to handle. Concentration on the pathway, particularly by verbal means resulted in success, although the reading was a severe drain upon the operator's attention. Visual fixation of the pathway also permitted success, so also, on one occasion, did a visual fixation of the object. Mental visualization on R's part resulted in failure on the part of the operator. As a matter of fact, R reports that he cannot be said actually to visualize at all. He knows how an object looks but cannot see it mentally. The time-readings ran from 11.4 seconds (verbal concentration on the direction of movement) to 223 seconds (visual fixation of the path). It should perhaps be noted that in the first few experiments, R gave evidence of much more involuntary movement than he did afterwards. He became conscious of his initiative and thereafter inhibited it deliberately. Dy's success under such conditions surprised him greatly. R showed a strong tendency to lead to the chair and object of the test just preceding the one actually in progress.

XII.

Besides these tests on the subjects named, Dy tried one series each on nine other subjects. It was hoped to determine in this way whether or not any general tendencies were to be observed. One series each was probably insufficient for the purpose. The fourteen subjects taken together show that in the first test at least concentration on the pathway was on the whole very much more effective than concentration on the object; concentration by verbal means slightly more effective than concentration by visual means; and concentration with closed eyes more effective than concentration with the eyes open.

The individual, and not the group, results are of the greater interest. In a few cases, verbal concentration alone induced involuntary movements. Frequently, even with subjects in whom visual 'control' issued in involuntary motor impulsion, the verbal concentration induced freer and, apparently, more automatic movements. With one guide this shift from precise to free rambling movements occurred with the change from concentration with eyes closed to concentration with eyes open. With his eyes open, this guide (T) moved in a free rambling way toward the object upon which his eyes fell, the direction of movement shifting frequently.

The results obtained in the series of experiments under consideration may be summarized as follows. *Those guides in whom the motor impulse is strong, under all conditions, indicate the direction of attention by motor initiative. This initiative is, however, retarded, although frequently rendered more precise, by concentration on the pathway, that is, on the direction of movement.*

Those guides in whom the motor impulse is less insistent, if the experiment be a long one, frequently find their attention weakened by the conditions of the test. If, however, attention be thrown on the pathway (direction of movement), the motor impulse is increased, and attention is maintained by the shift of the point of fixation.

Verbal control produces, in general, a freer and usually less accurate initiative than does a visual control. The actual innervation of the vocal musculature may possibly have general motor accompaniments. Verbal control frequently rendered success possible with subjects otherwise refractory. Verbalization merely of the name of the object selected occasionally caused extensive rambling, at times in the direction of the eye-movements; for others, it induced a cutting of the pathway.

In the case of certain subjects, who moved in the direction of eye-fixation and that too without conscious direction of attention toward any particular object in the field of vision, blindfolding was contributory to success.

XIII.

The third question phrased for discussion related to the automatic tendencies revealed in muscle-reading with reference to Beard's and Laurent's observation that the guide's obsession by the suggested idea not only induces very extensive and free movements but also increases the probability of automatic activities of various sorts. The present tests gave frequent evidence of such automatism; the freedom and extent of the guide's initiative were, moreover, very surprising under such conditions.

The peculiar automatic tendencies observed in connection with muscle-reading have been rehearsed by the writer in another article.[1] To list them briefly, they include success in the location of an object although the attention of the guide is distracted from the object; recapitulation of the pathway followed by the guide in hiding or selecting the object of the test; a return to the chosen article of a preceding test, or to one thought-of as a possible object for the present test; an early indication of the level at which the article is actually hidden. The three-meter tests revealed a tendency on the part of the guide to return to the beginning of the chalked pathway if the operator went 'astray,' with, as before, a tendency to locate the chair and object of the preceding test rather than the object of the present test.

[1] 'Automatic Phenomena of Muscle-Reading,' *Jour. of Phil., Psychol. and Scientific Methods*, Vol. V., p. 650.

Such occurrences as those mentioned in the preceding paragraph could be artificially induced by the experimenter. The suggestion of a spectator that an object selected and placed in position by the guide should be rejected for another chosen by the spectator often induced a location of both articles. An experiment, called for brevity the 'either-or' test was also interesting in its outcome, although this varied with the individual tested. In this experiment, a third person selected and hid the object, stating to the guide, who was blindfolded, that it had been placed 'either' in one described locality 'or' in a second. To cite a particular case. J was told that the object had been placed 'either' on a high cabinet at the west side of the inner room 'or' on a low table at the north end. J decided to concentrate on the low table north and guided the operator thither, but all the movements of exploration were high up at a level with the cabinet. For a more detailed description of the conditions under which similar automatic activities occurred, the reader is referred to the article cited above.

The point of interest is the possible revelation of the 'mental set' even when attention is not concentrated rigorously or is concentrated upon something other than is revealed by the involuntary movements. Success in locating an object with the guide's attention distracted from it and the tendency to reveal involuntarily the location of an object previously thought-of, although at the moment of the test the guide's attention is concentrated upon something else are interesting features of the experiment. The persistence of an idea even after it has been dismissed from the field of attention is cited by Pfungst[1] as a source of error in his experiments. In his numerical tests such persistence of an idea, called by him after Müller and Pilzecker, 'die Perseverationstendenz,' resulted in the operator tapping a number corresponding to one previously thought-of rather than the number thought-of at the moment of test. The point here to be emphasized is that in such an instance, at least, the concentration of attention upon the desired idea is less effectual in the induction of involuntary movements, than is the subconscious or co-conscious second idea.

[1] *Op. cit.*, p. 106 f.

The results obtained in the three-meter tests, in which attention was voluntarily controlled, were similar in a way to the observation of the great effectiveness of subconscious ideas in revealing the direction of attention. The fact that the most impulsive subjects gave freer initiative than usual when attention was withdrawn from the pathway, although following this pathway accurately, is a case in point. The difference in muscular tension accompanying a labored concentration and an automatic initiative is perfectly evident to the operator. As has been stated, the latter was frequently able to tell the subject at the close of a test, the method utilized in the concentration of attention.

It is evident then that the most 'fit' subjects for muscle-reading tests will frequently exhibit automatic activities. Success will also be possible with subjects who are able to hold their attention steadily to a desired object. For brilliant success a certain amount of automatism on the part of the guide is required.

Laurent's[1] experiments upon muscle-reading without contact emphasized the fact that those subjects who were most obsessed by the suggested idea resorted unconsciously to suppressed articulation and that the operator, if also automatic in his tendencies, was brought by the test into an abnormally passive state of mind in which he heard unconsciously the verbalization of the guide.

In the present test the writer was on the outlook for any cases of pronounced verbal automatism. One or two striking instances of it occurred. Thus W when baffled would speak aloud and report, when questioned, that he was unaware of having said anything. The movements of M's lips became noticeable whenever she became absorbed in an experiment. H, when voluntarily controlling attention by mental verbalization, actually whispered at times to herself. So far, however, as she is aware, Dy was in no case guided by audition. In the two or three cases where she heard the suppressed whispering she misunderstood the words; in none of these cases did she permit such direction to supersede guidance by touch. Operating without contact, Dy is still dependent upon her perception

[1] *Loc. cit.*, p. 489 f.

of variation in the guide's movements; at most, she guides herself auditorially by noting variations in breathing and footstep. Very little attention, on the whole, was given, in the present investigation, to the auditory factor.

XIV.

The main interest in the present investigation was, as stated in the early part of the paper, to discover if possible a method of studying mental types. This purpose has been in part achieved, although a more rigid control of conditions would have been desirable.

Apart from limitations of this sort, the acquaintance that the muscle-reader gets with the expressive side of mental situations and with the individual variations in such expression is most striking.

First of all, the varying impulsiveness of subjects is noticeable. The extent of this variation was, as the foregoing report shows, very great. One would wish to determine the bearing of such varying impulsiveness upon the whole character of the subject. The writer's general knowledge of the subjects of her muscle-reading tests has led her to expect that the 'fit' subjects for the test will, on the whole, be those who in daily life exhibit few inhibitions either in judgment or action, who are hopeful and confident in their attitude toward things. The opposing type includes those more hesitant in act and judgment, more critical and reserved. How far the temperament is an expression of variation in the readiness of the motor discharge is a question worth detailed consideration. How far also the nervous energy released by concentration of attention is drained otherwise than through involuntary movements is of interest. No doubt this draining may take place in various ways, with consequent effect upon temperament.[1]

Eight subjects of the muscle-reading tests were also tested in their ability to maintain writing under distracted attention.[2]

[1] Manouvrier, L., 'Mouvements divers et sueur palmaire consécutifs à des images mentales,' *Revue philosophique*, 1886, 22, p. 204 ff.

[2] Downey, J. E., 'Control Processes in Modified Hand-Writing,' Part II., Monograph Supplement, PSYCHOL. REV., Vol. IX., No. 37.

The writing of the impulsive subject, under distraction, issued in a large free hand with frequent unawareness of the writing and repetitionary or persistence lapses. For the more self-controlled subject, writing under such conditions issued in writing small and labored, controlled consciously and with effort.

Varying impulsiveness was not, however, the only individual difference thrown into relief by the tests under consideration. Variations in the conditions under which the muscle-reading took place could be introduced almost without end and every such variation showed further possibilities as to the revelation of character by such means. Volitional tendencies so-called came out distinctly in the tests in which the subject found himself, unexpectedly, baffled. This baffling was effected by removing the object after the guide had placed it in position or by blocking the pathway by which the subject supposed it could be reached. Certain subjects were resolute and unfaltering in their insistence that the operator surmount any obstacle in her path; others, after a moment's hesitation, tried another way round, still others gave up at the first hint of a difficulty. The momentary 'pause' with which certain subjects reacted to the difficulty before initiating other movement contrasted strangely with the 'wild' explorations of others. The movements, it should be understood, were usually involuntary variations in muscular tension, not overt movements.

The 'either-or' test described a few pages back introduced an instructive variation in the conditions. The vacillation of attention with which certain guides met the situation threw into relief the quick decision of others.

It is difficult to comprehend without first-hand experience the wonderful accuracy of the operator's response to the slightest variation in the guide's muscular tension. H, for instance, has as guide placed a clock on a ledge above a long table which is three and a half feet wide and flat against the wall. On Dy's arrival at the table, H begins to chuckle mentally, thinking, "She can never reach it!" Dy raises herself on tiptoe and leans over the table, exclaiming "I can never reach it!" H sees in a visual flash Dy climbing the table and Dy actually pulls herself over the table and gets the clock. Such delicacy in reaction gives the operator a unique acquaintance with the guide's mental processes, his uncertainty, his timidity, the course of his deliberations. The awkwardness of one of the writer's friends was thus realized in a new and unexpected manner when with this friend serving as her guide, she felt herself 'backing-up' to the object in a most ludicrous manner.

Muscle-reading affords a new method for investigating certain features of bodily orientation. The blindfolding of guides issued in instructive results. Not merely the timidity of some, under such limitations, and the confident orientation of others was noticed but also certain peculiarities of adjustment. A tendency on the part of the operator to indicate a position symmetrically homologous to that actually thought of by the guide was noticed by Romanes in the early English investigation and has also been recorded once or twice in the present investigation. Whirling a subject around several times rapidly after he has been blindfolded and before the test is on serves to complicate the latter peculiarly. Turning the guide adrift blindfolded in an unfamiliar room in which the object to be located has been placed for him by a third person and the place of location carefully described is an interesting test especially if the guide be a very impulsive one. The notes the writer has collected on this topic she is reserving for fuller treatment at some later time.

One of the most interesting traits that the muscle-reading tests revealed was the tendency to revert automatically to a past condition. This tendency described a few pages back has been extensively treated by Müller and Pilzecker [1] in connection with their experiments on memory. The persistence in the present tests was a motor rather than a sensory persistence. The authors mentioned above dilate at some length upon the general effect of such a tendency upon character as a whole. They admit, however, that the tendency may show itself in a particular situation without being a common feature in all the reactions of a particular subject. The tendency is one meriting more elaborate investigation in all its various forms.

[1] Müller, G. E., und Pilzecker, A., 'Experimentelle Beiträge zur Lehre vom Gedächtnis,' *Zeitschrift für Psychol. und Physiol. der Sinnesorgane*, 1900, Ergänzungsband, I., p. 53 ff.

That muscle-reading threw into relief certain peculiarities in imagery type is apparent from the course of the paper and needs no further elaboration at this point.

On the whole, the writer has been greatly impressed with the possibilities muscle-reading affords for certain sorts of investigation. She has been impressed with the exceeding delicacy of the expressive side of the mental life and, above all, impressed with the minimal awareness of the subject as to the nature of such expression.

ANNOUNCEMENT.

During Professor Baldwin's temporary absence in Europe, MSS. for this section of the Review may be sent to Professor John B. Watson, The Johns Hopkins University, Baltimore, Md.

Professor Watson from now on becomes one of the editors of the Review.

N. S. Vol. XVI. No. 5. September, 1909.

The Psychological Review.

TIME-RELATIONS OF THE AFFECTIVE PROCESSES.[1]

BY TAIZO NAKASHIMA, Ph.D.

I. Study by the Direct Reaction Method.

Experiments with Color Combinations.

The problem of the present investigation was twofold, — first, the determination of the time necessary for the arousal of an affection; and secondly, that of the dependence of affective intensity upon the duration of stimulus. The apparatus used was Hering's modification of Ludwig's kymograph. A long roll of paper, on which thirty-two color combinations were pasted, was stretched around the drum and the cylinder. The observer sat at a distance of about 1 m. from the screen, which stood close to the drum, and had an oblong window (3 × 5 cm.). Two colored papers combined horizontally served as stimulus, and the line of junction was so adjusted that it came just on the middle line of the window. Thus the vertical length of each colored paper actually seen by the observer was 2.5 cm. The horizontal length of each color combination was 4 cm., and the distance between one color combination and another was also 4 cm. The length of the roll of paper was 256 cm. By dividing the time necessary for a complete revolution of the paper by this total length, we obtain the amount of time necessary to pass through 1 cm. The rapidity of the drum was graded in 7 degrees above and below 0 at the center. The exposure-distance of each stimulus was thus 7 cm. (complete and partial exposure), and

[1] The experimental work in Section I. was done in the Harvard laboratory during 1906–7; that in Sections II. and III. was done in the Cornell laboratory during 1908–9.

the interval between the stimuli, *i. e.*, the length of the background was 1 cm. With speed 0, the time necessary for a complete revolution of the drum was 3′ 50″; therefore, the time of exposure of stimulus was 6.3″. With the other six speeds, the times in seconds are as follows:

Speed.	Time of Complete Revolution.	Time of Exposure.
1	110	3.01
2	75	2.03
3	55	1.47
4	45	1.26
5	35	0.98
6	30	0.84

The 32 (Milton-Bradley) color combinations selected were as follows: I., BV and GS1 (green shade no. 1); II., YOS1 and VT1 (violet tint no. 1); III., VRS2 and RS2; IV., GT1 and VRT2; V., Y and BV; VI., ROT1 and VRT1; VII., ROS1 and VRS2; VIII., RS1 and VRS2; IX., Y and VR; X., O and VT2; XI., RS2 and Black; XII., B and OYS2; XIII., V and YG; XIV., Warm Gray No. 2 and A-Red, Dark; XV., GY and BV; XVI., ORT2 and A-Red Violet, Light; XVII., VRT2 and Neutral Gray 1; XVIII., VRS2 and BVS2; XIX., YG and ORT2; XX., GBS2 and A-Red, Dark; XXI., V and GS1; XXII., OS2 and RVS2; XXIII., GY and VT2; XXIV., YG and VT2; XXV., BV and YG; XXVI., VS2 and RS2; XXVII., VRT2 and A-Green, Light; XXVIII., BVS2 and RS1; XXIX., Warm Gray 2 and RVS2; XXX., VRS2 and GS2; XXXI., OR and G; XXXII., Y and BS2.

Every judgment on the first stimulus of a series was stricken out, since the speed of the drum was at first slow; and that reaction was supplemented by one taken later, with normal and uniform speed. The first color of a series was 1, 32 or 15. The change of the speed-pointer above or below zero, caused the drum to reverse its direction, and this gave the opportunity of arranging series in ascending and descending order. Usually, one complete series was made in two experimental hours, to avoid too frequent repetition of the same stimuli. The observers were instructed to pass affective judgments upon the color impressions in terms of P–U, and in the seven steps: 1 very pleasant; 2 moderately pleasant; 3 just pleasant; 4 indifferent; 5 just unpleasant; 6 moderately unpleasant; 7 very unpleasant. The judgment in these steps was to be passed on the basis of the affection aroused during the exposure of the stimulus, and it was to be registered as '4' when there was no pleasantness or unpleasantness during the time of exposure. The actual procedure of the experiment was as follows. About 3 sec. after 'Ready,' and at the signal 'Now,' the experimenter started the drum, and the observer gave his judgment, '2,' '5' or '4,' for instance, which a third person registered on the prearranged laboratory tablet. After the thirty-second judgment, the experimenter stopped the drum and received from the observer any remarks on the experiences that suggested themselves. Then the recorder took the place of the observer, and the previous observer kept the record. The experiments were made during the months of October, November, and December, 1906. They were performed in ordinary diffuse daylight; every care was taken to keep the light as constant as possible, by the adjustment of curtains at the windows of the room, The observers were Professor E. B. Holt (*H*), Miss E. L. Davis (*D*), Messrs. C. A. Barnes (*B*), C. S. Berry (*Br*), E. P. Frost (*F*), A. Mitchell (*M*), C. A. Pierce (*P*), H. Reverdin (*R*), H. M. Sheffer (*S*), H. Taylor (*T*), K. T. Waugh (*W*).[1] The results of eight observers only are given here, since the other three could not complete the series. The following tables show the results which were obtained after three complete series (672 reactions) of preliminary experiments with each observer, made for the purpose of learning and standardizing the scale of arbitrary affective values. All invalid series have been eliminated. Immediately before the preliminary series, a view of the whole set of stimuli was given to the observer for some time, to furnish him with a rough estimation of the relative affective position of each stimulus in the series.

In these tables, the figures in the first horizontal row indicate the seven times of exposure 0 to 6; the figures in the first vertical row show the number of affective judgments of the same grade between the limits 1 and 7. In Table I. the figures

[1] All except *H* and *T* were graduate students and members of the laboratory.

Table I.

Observer H.

	0	1	2	3	4	5	6
5	1.0	1.6	1.4	1.6	1.6	1.8	1.9
7	2.0	2.7	2.5	2.7	2.8	3.0	2.9
14	3.0	3.7	3.5	3.8	3.8	4.1	4.0
12	4.0	4.2	4.0	4.1	4.0	4.0	4.0
14	5.0	4.7	4.6	3.9	4.0	4.2	4.0
9	6.0	4.8	4.7	5.0	4.3	4.3	4.4
3	7.0	5.7	6.0	4.7	4.0	4.4	4.4
	12	15	16	19	26	30	34

Table II.

Observer Br.

	0	1	2	3	4	5	6
13	1.0	0.3	0.2	0.4	0.3	0.4	0.5
7	2.0	0.3	0.1	1.0	0.9	1.0	1.7
7	3.0	0.6	0.9	0.8	0.8	1.0	1.0
2	4.0	0	0	0	0	0	0
15	5.0	—.4	—.6	—.4	—.6	— .8	— .8
14	6.0	—.2	—.7	—.8	—.8	—1.0	—1.0
6	7.0	—.2	—.6	—.5	—.9	—1.0	—1.0
	2	4	5	6	6	8	10

Table III.

Observer D.

	0	1	2	3	4	5	6
	—	—	—	—	—	—	—
9	2.0	0.5	0.4	0.4	0.4	0.4	0.5
14	3.0	0.5	0.4	0.3	0.4	0.4	0.5
27	4.0	—.1	0.1	0	0	0	0
14	5.0	—.6	—.4	—.4	—.8	—.8	1.0
	—	—	—	—	—	—	—
	—	—	—	—	—	—	—
	27	27	27	28	32	33	37

Table IV.

Observer F.

	0	1	2	3	4	5	6
2	1.0	0	0	0	0	0	0
6	2.0	0.3	0.3	0.4	0.4	0.6	0.5
11	3.0	0.3	0.2	0.3	0.4	0.6	0.8
11	4.0	—.2	—.2	—.1	—.1	0	0
8	5.0	0	0	—.2	—.2	—.4	—.4
12	6.0	—.1	—.4	—.4	—.3	—.4	—.5
14	7.0	—.1	—.1	—.2	—.2	—.4	—.8
	11	8	11	11	12	13	16

Table V.

Observer M.

	0	1	2	3	4	5	6
4	1.0	0	0	0	0	0	0
1	2.0	3	3	3	3	4	4
12	3.0	0.4	1.0	1.1	0.9	0.8	0.8
5	4.0	0.2	0.1	0.2	0.2	0	0
17	5.0	—.2	0	—.2	—.3	—.4	— .6
16	6.0	—.1	—.2	—.2	—.5	—.4	—1.2
9	7.0	—.4	—.5	—.8	—.9	—.9	—1.2
	5	12	13	13	14	18	33

Table VI.

Observer S.

	0	1	2	3	4	5	6
13	1.0	0.3	0.2	0.2	0.4	0.4	0.6
4	2.0	0.5	0.3	0.3	0.3	1.3	1.5
11	3.0	0.7	0.5	0.5	0.6	0.5	0.6
6	4.0	—.2	—.2	— .1	— .1	0	0
10	5.0	—.7	—.8	—1.2	— .8	— .7	— .8
9	6.0	—.6	—.5	— .4	—1.0	—1.0	—1.4
11	7.0	—.5	—.4	— .4	— .8	— .9	—1.7
	6	10	6	8	12	17	20

Table VII.

Observer W.

	0	1	2	3	4	5	6
11	1.0	0 .4	0.3	0.3	0.3	0.3	0.4
20	2.0	0 .7	0.6	1.0	0.9	1.2	1.2
16	3.0	0 .1	0	0	0.4	0.6	0.8
8	4.0	— .1	0	0.1	0	0	0
6	5.0	— .9	— .8	— .8	—1.0	0.5	0.8
3	6.0	—1.0	—1.1	—1.3	—1.0	—1.9	—2.0
	—	—	—	—	—	—	—
	8	8	10	12	13	15	18

Table VIII.

Observer T.

	0	1	2	3	4	5	6
	—	—	—	—	—	—	—
10	2.0	0.1	0.2	0.3	0.3	0.4	0.5
25	3.0	0	0	0.1	0.1	0.3	0.6
11	4.0	0.1	0	—.1	0	0	0
18	5.0	—.1	—.2	—.2	—.3	—.3	—.5
	—	—	—	—	—	—	—
	—	—	—	—	—	—	—
	11	11	12	12	13	16	18

under the exposure times are averages, obtained by the following mode of calculation. (1) Count up the number of times a given affective judgment (1, 2, 3, . . . 7) occurs under heading 0. (2) Sum up the judged values for the same stimuli under the other time-headings 1 to 6. Divide by the number of occurrences found in (1). If judgment 3, for instance, occurs 5 times under 0 (for stimuli 1, 7, 10, 13, 20), then sum up the judged-values for these same stimuli (1, 7, 10, 13, 20) under each time-heading 1–6, and divide by 5. — In the remaining 7 tables, the figures under the grades 1–8 are the differences between the standard values under the 0 exposure, and the values under those other grades which were obtained in the mode explained above. The figures in the last horizontal row of each table indicate the number of the stimulus judged '4' under the respective grades of exposure, out of the two complete series, 448 judgments, which are the total data of the 'averages of the judgments.' The tables show clearly that *affective intensity decreases with decrease of time of exposure*. There are, however, disturbing factors. In Table I. the figures 4.2 and 4.1 in judgment 4 are certainly the effect of such factors. These deviations from the 4 show that the indifferent stimuli at the 0 exposure are occasionally judged for other steps. Affective variations due to unknown disturbing factors are noticed in other steps and in all other tables. In Table III. the figures — .1 and 0.1 in judgment 4 indicate that there are judgments passed which deviate from the 4 towards opposite directions. With *F* and *M* the variations in judgment 4 are slightly greater than those in the case of *D*, and the tendency of variation is decidedly in one direction. *S* shows a stronger tendency of deviation from the 5 than from the 3. With *Br*, *T* and *W* the deviations from the 4 are very slight. All these and similar variations we admit. But they are very slight as compared with the general effect of the time of exposure, as can be seen from examination of the original records. In all the tables, the increase of the number of the judgment 4, and the convergence towards the 4 of all other grades of judgment with decrease of the time of exposure, are definite beyond doubt.

M and *S* had at first much difficulty in passing an affective judgment in the seven steps under our experimental conditions, although later the difficulty disappeared, except during the exposures 5 and 6. Other observers also had more or less difficulty, but soon learned to adapt themselves to the required conditions. *M* and *S* said that, with the exposures 5 and 6, the shortness of exposure and the need of quick judgment seemed to oblige them to pass judgment from memory; and in so far the judgment was intellectual. *Br* and *F* had no such difficulty, but also thought that their judgments might be purely intellectual. The experimenter then explained the nature of affective habituation, and asked them strictly to observe the rules laid down for judgment, and to say '4' whenever they found no affection during the exposure. The results were much the same as before, and no observer could be positively sure that his judgments were purely intellectual. *D*, *T* and *W* seem to have had more or less difficulty, but did not express themselves definitely on the question. *H* remarked that a few stimuli might possibly have been dropped without being included in the affective judgments; *i. e.*, might have been passed as '4,' while they had in reality some affective tone. All observers were unanimous in their experience of definite affections during exposure time 4. We may, then, take the times of exposures 5 and 6 as dubious, and conclude that *the shortest time necessary for an affection to arise varies from 0.84 to 0.98 sec.*

Experiments with Geometrical Figures.

The stimuli used in this experiment were such geometrical figures as stars, triangles, dots, circles, crosses, Japanese chrysanthemum patterns, oblique and straight lines, angles, etc. From three to five such figures were drawn in certain spatial relations within a rectangular frame (2 × 7.5 cm.) of black lines. The whole figure with the frame was pasted on another larger rectangular piece (4 × 10 cm.) of white paper. The distance between one piece and another was 4 cm., while the distance between one frame and another was 6 cm. Thirty-two such pieces were pasted on a roll of paper like that used in the preceding experiment. The window in the screen was 4 × 10 cm.; the horizontal distance of exposure of each stimulus was, therefore, 6 cm. (complete and partial exposure), and the interval, *i. e.*, the distance of the background between two frames, was 2 cm. One complete series consisted as before of 224 reactions. The observers were instructed to give their judgments, if possible, on the total affection aroused by the figure with its frame. All other requirements and experimental conditions were exactly the same as in the experiment with color combinations. *H*, *D*, *B*, *Br*, *F*, *M*, *R*, *S*, *T* and *W* again served as observers. The work was done during the months of January, February, and March, 1907. The results to be stated below were obtained on the basis of two complete series, after two complete series of preliminary experiments.

The general result of the present experiment is practically the same as that of the preceding. In this experiment, however, the influence of disturbing factors is much less, while that of the decrease of exposure time is considerably greater, as is shown both by the original records and by their statistical examination as in the last experiment. All the observers, except *M* and *S*, found that during exposure 6 most of the stimuli aroused no affection, and a few remaining stimuli had hardly time enough to develop an affective reaction during the exposure. *M* and *S* observed that during exposure 6 there was absolutely no affection, and that the judgments with the next exposure might be purely intellectual; only at the end of exposure 4 there were a few stimuli which aroused an affective tone. We may, then, conclude that *the shortest time necessary for the affection to arise varies from 0.72 to 1.08 sec.*

We are now to discuss the method and the results obtained by the method. One might question whether the times determined in the above two experiments do not include the time for judgment. But that is not necessarily the case; for the observers were instructed carefully to note the relation of the time-limit of exposure to the moment of appearance of the affection, not its relation to the completion of their judgment, and to record on the basis of that observation only. The time obtained may, however, be somewhat longer than it would have been if we had asked for a record of the first bare experience of affection. On the other hand, if we had required this simplest form of reaction, the response would have been impulsive only, and the result must have been quite dubious. At any rate, all our observers were certain that there was no single case in which affection appeared simultaneously with, or earlier than, the sensation; it always appeared distinctly later than the cognition of the impression.

In the ordinary chronometric reaction, what is registered is a certain conscious moment and the time of the physical movement of response. Consequently, the rapidity of the reaction movement, and every influence on that rapidity, cause a difference in the time measured. The time determined by this method never corresponds to a real mental time. It is a time known through the intermediation of a certain movement; a time of indirect reaction. In our reaction, the time registered is a certain conscious moment, which we seek to determine, and the observer's task is to note the time-relation of the stimulus to this conscious point; while the expression of his observation or reaction might be quicker or slower, without any consequence for the determination of the time. Thus the reaction by our method is direct, and the time found represents an actual mental point in its relation to the objective time of stimulation. The present direct reaction method, therefore, avoids the whole problem of the physiological time of the reaction movement, and eliminates the possible error that arises from the variation of that movement. Further, the method saves the time and toil necessary for the graphic and chronoscopic reaction methods, since it needs no objective device for registering reaction-times. It should also be remarked that the new method has no essential connection with the serial presentation of stimuli, and with passing judgment in seven steps, although we combined these procedures with the method in the present study. We resorted to serial presentation, because it seemed desirable to prevent the possible persistence of the affective process, aroused by a stimulus, by the inhibition through a following stimulus, so that there could not be any confusion of an actual affection with the affection attaching to the memorial after-image of a precedent stimulus. The scheme proved efficient, as was reported by the observers. Moreover, the need of quick successive judgments gave the least chance for disturbing association and reflection, and required uniform concentration of attention.

The passing of judgment in seven steps is purely accidental to the method; it was required simply because we sought to determine the relation of affective intensity to time of exposure. The main issue as concerns the method is that the direct reaction method is feasible and reliable, and has important advantages, even when it is applied to the affective processes pleasantness and unpleasantness.

Experiments with Complex Visual Impressions.

The experience of the preceding experiments encouraged us to attempt a further study of the time-relations of affective process with the same direct reaction method. In this experiment, however, a single stimulus was presented, instead of the serial presentation of a number of stimuli. The apparatus consisted of two parts: the one was Meumann's time-sense apparatus, and the other was an exposure box, 32 cm. by 21 cm. by 18 cm. This was painted black. The front of the box was open, as far down as 14.5 cm. from the top. The lower part of the left side of the box, as far up as 16 cm. from the bottom, was covered by a door which was shut during the experiment, but could be opened for the introduction and removal of the stimulus. Two screens, just large enough to cover the front, were held up to the top of the box, at equal distances from each other, by two pairs of electromagnets; a third was held by a support. The screen was made of a light black wooden frame and a black cardboard inserted in the frame. On the second or middle screen, a stimulus was fastened, and the third or last screen with a check stimulus formed the background. The first pair of electromagnets, which held up the front screen, were connected by wires to the first contact on the scaled disc of the time-sense apparatus, and with a pole changer formed the first circuit. The second pair of electromagnets, which held up the middle screen, were connected to the second contact on the disc, and with wires and another pole changer formed the second circuit. When the contact at the first key was broken by one of the revolving arms, the front screen fell, and at the same time the stimulus on the second screen appeared. The second screen fell, after a certain interval of time, when the second contact was broken by the other revolving arm. The observers were requested to record their observations after the screen had fallen, and, after a glance at the stimulus on the third screen, to check or destroy the affective process possibly remaining as an after-effect of the stimulus. The time for adjustment to the stimulus, before its appearance, was about 7 seconds, the commencement of the whirring noise of the wheel-work serving as the signal for attention. However, the actual duration of attention was, according to the report of the observers, about 4 seconds. The initial 7 seconds was found necessary for the introduction of uniformity in the rate of the revolving arms. The distance between the exposure box and the observer was 70–80 cm. The time of exposure, *i. e.*, the time between the fall of the first screen and that of the second, could be varied widely by fine steps, and exactly determined by measuring the distance passed by the arm between the first and the second break. The experiments were made during the months of March, April and May, 1907. They were performed in a completely dark room, except that an electric light of 5 c.p. at the front of the upper board of the box illuminated the stimulus. The same nine persons and the writer served as observers. The stimuli used were forty illustrative photographic pictures on postal cards (Detroit Photographic Co.), five Japanese postal cards, and ten plates of surgical diseases of the chest. The stimuli on the third screen were illustrative postal cards similar in their nature to those used as stimuli on the second screen. They were changed in every other experiment, keeping a constant relation to the stimuli on the middle screen. The observers were called upon to pay special attention to (1) the time-relation between the moment of the appearance of affection and the exposure of the stimulus, *i. e.*, whether it was aroused during or after the exposure; (2) the quality, intensity, vividness and duration of the affection; (3) the relation of the affection to the background, with especial reference to inhibition, reinforcement, and coexistence, if there were such processes; (4) associations, bodily reactions, etc. They were also told that the quality and intensity might be recorded in the seven steps. Above all, they were carefully to observe the time-relations of the affective processes.

In the actual experimental procedure, if there were two cases in which no affection arose with one and the same exposure time, a further experiment was tried with the next longer exposure time; and if P or U was aroused, then the experiment was stopped, and the time was taken with another stimulus. If an exposure time was found in which an affection was aroused in the first or second experiment, the next shorter exposure was tried, until a time was found in which there occurred two cases of 'no affection.' The final time for 4 was determined by the same procedure as that for P–U, but after it was found we went on further to find the final time for P–U. Usually, in the next exposure grade P–U was aroused, and 2 trials were the average for all observers except *T* and *N* (4 trials). In other cases, 7 trials were the average for determining the final time of P–U or 4, except again for *T* and *N* (10 trials). There were only a few cases in which P–U after 4 did not arise even in an exposure time of 141.5 sec. In such cases, the trial was stopped, and the original time was taken as final for 4. The grade, *i. e.*, the interval between one exposure and the next, was .05 sec. In calculation of the times, therefore, .025 sec. was subtracted from the time in which an affection or a feeling was aroused, on the assumption that it must have been aroused between the exposure time in which there occurred two cases of no affection, and the time in which an affection was aroused. The following Table IX. shows the results of the time measurements.

Table IX.

Observer.	Direct Reaction Times of P–U.						Times of 'Indifferent.'					
	Range.	Median.	MV.	Av.	MV.	No.	Range.	Median.	MV.	Av.	MV.	No.
Barnes.......	31.5– 86.5	41.5	5.9	47.0	6.9	112	26.5–126.5	36.5	5.3	39.0	6.9	140
Berry.........	31.5– 66.5	39.0	6.0	39.0	6.0	172	31.5– 91.5	41.5	6.2	40.0	6.0	39
Davis.........	31.5–111.5	39.0	5.8	41.5	5.9	162	26.5–136.5	41.5	6.4	41.5	6.4	41
Frost.........	31.5–111.5	40.0	5.4	41.2	5.9	172	31.5– 96.5	42.0	6.0	42.6	5.9	36
Holt..........	36.5–141.5	49.0	6.9	51.5	6.9	183	41.5– 66.5	50.0	7.1	49.0	6.9	20
Mitchell.....	31.5–126.5	42.5	5.4	42.0	5.7	179	41.5–141.5	52.0	7.1	47.0	6.4	49
Nakashima	36.5– 86.5	40.5	5.4	41.5	5.6	346	31.5– 86.5	36.5	5.0	37.5	5.6	24
Sheffer	31.5–131.5	42.5	5.6	46.0	5.4	174	31.5–126.5	41.0	5.3	43.6	5.5	52
Taylor.......	31.5–111.5	39.0	6.0	40.0	7.0	225	31.5– 96.5	39.0	5.9	41.5	6.2	126
Waugh......	36.5–141.5	48.5	6.8	50.0	6.9	178	41.5– 91.5	48.0	7.2	50.0	6.9	28

The figures under 'Number' indicate the total number of experiments. The stimuli were 55 in all, but the necessity of finishing the experiment within the academic year obliged us to reduce the number of the stimuli, and experiments on 30 stimuli only out of 55 were completed, these 30 including the stimuli that required the longest and the shortest times. The 25 stimuli eliminated were those which required intermediate times. The number with observer *T* is exceptionally large, because many extra hours were obtained from this observer in advance of the others, when the experimenter had not the adequate experience for reducing the number of the experiments. The experience obtained from the work with *T* and the writer's own observations enabled the experimenter to guess that such and such stimuli would require about such and such times; and thus he could try from the beginning the approximate exposure time in which an affection would be aroused. This is the reason why the numbers for the other observers are smaller. In the table, the 'indifferent' (4) means 'indifferent in regard to pleasantness or unpleasantness.' Thus it covers such things as the feelings of strain, excitement, curiosity, interest, surprise or shudder, wonder (strangeness), familiarity, recognition, a certain indescribable feeling, etc.[1] The observers used the word feeling in the ordinary way, and the 'indifferent feeling' is by no means a pure elementary affection. It is noteworthy, however, that the times of these indifferent feelings are practically the same as those of P–U, and in the case of *B* and *D* are even shorter than the latter. The times of both P–U and the indifferent feelings are also remarkably uniform. This is partly due to the fact that .05 sec. was taken as the unit of the grades, so that possible finer differences could not be brought out. However, even if we take the interval of the grades into account, the individual ranges and the ranges among the different observers are much smaller than might have been expected; for the possible maximum interval according to our mode of calculation is only .025 sec. More-

[1] These feelings are really Orth and Marbe's 'Bewusstseinslagen,' Ach's 'Wissen' or 'Bewusstheiten,' James' 'fringe of consciousness,' and as such may be a part of the consciousness of understanding; otherwise, they are nothing but organic sensations. Whether in the first case they are sensorial or nonsensorial is still a matter of dispute.

over, it should be remarked (1) that in the calculation of the times, no single time value was eliminated (except the extremely isolated times mentioned below), and that even in the simple reaction a range from the shortest time to its 2 or 2.5 times is not exceptional; (2) that the reactions were performed in the condition of ordinary naïve experience, without trying to eliminate anything which might be regarded as a disturbing factor from the standpoint of psychophysics; (3) that the greater variations are only exceptional cases; reference to the ranges in the table shows that the times 126.5, 131.5, and 141.5 were found only once with *B*, *D*, *H*, *S* and *W*; and finally (4) that the ranges in the table were obtained from a large variety of complex stimuli.

It seems, then, to follow from these considerations that the mode of variation of affective reaction-times is of the same order as that of sensory reaction-times. Even the absolute values of the averages and the medians in the table are practically the same as those of cognitive reactions, which are already known from reaction experiments on visual impressions made by various investigators. All the observers agreed that, although feeling appeared only after a more or less clear perception of the stimulus, the temporal disjunction was in most cases very slight. The introspective records of the observers are quite homogeneous, except for *H*, so that they may be presented later in a summary way. The following are *H*'s records taken from each last one of 55 stimuli:

Stimulus 1. Feeling of recognition and it was perhaps pleasant no. 3. But I cannot be sure that there was more than one quality.

Stimulus 2. Some mild feeling which I cannot describe.

Stimulus 3. Feeling no. '5,' slightly disagreeable owing to *color* (simply). Inhibited by background. No association. Feeling of dejection.

Stimulus 4. Clear perception, but no feeling. There was feeling from memory image.

Stimulus 5. No feeling during exposure. Feeling no. 2 *now* as I recall the picture. Perception clear during exposure.

Stimulus 6. Feeling no. 1 (*very* faint) inhibited at once by background. Association with Japanese art.

Stimulus 7. Clear perception, feeling no. 3, mildly restful.

Stimulus 8. Clear perception but no feeling during exposure. After exposure had feeling no. 5.

Stimulus 9. No pleasantness or unpleasantness. Surprise, and, I should think, horripilation to a slight degree, during exposure.

Stimulus 10. No feeling during exposure. But first picture and background together were no. 7, and gave me a sort of shudder.

Stimulus 11. Feeling no. 3, coming long after exposure, attached to memory of stimulus.

Stimulus 12. Clear percept but no feeling during exposure. Feeling no. 5 afterward. No relation between picture's feeling and background.

Stimulus 13. Feeling no. 6 at time of exposure. Fairly intense: one of irritating nervousness. Inhibited by background. No association. Bodily feeling of (my) withdrawal from stimulus.

Stimulus 14. Feeling no. 1 aroused faintly during exposure; became stronger after exposure and inhibited background for all but its (the background's) first moment of exposure.

Stimulus 15. Simply not time for a feeling although a pretty clear perception of stimulus.

Stimulus 16. No feeling during exposure. Memory image inhibited by background. Two or 3 sec. later feeling no. 5 attaching to original picture's memory image.

Stimulus 17. No feeling. If (as here) background is agreeable it has a strong tendency to inhibit picture and feeling-tone altogether.

Stimulus 18. Feeling no. 2 very faint. Inhibited by agreeable background.

Stimulus 19. Feeling no. 6, during exposure. Feeling of 'mussiness' perhaps of 'tension.'

Stimulus 20. Feeling no. 3 rather strong; inhibited by background.

Stimulus 21. No feeling during exposure. Background seemed to come as a feeling-tone, no. 6, was surging up. This inhibited background for a moment (after it had an instant appeared).

Stimulus 22. Mild intensity of feeling no. 1, coming after I had seen background and looked away from it.

Stimulus 23. No feeling for picture itself. Contents thereof not well apprehended: effort (feeling) to comprehend what I saw.

Stimulus 24. Feeling no. 5; mild intensity; lasted as long as exposure; inhibited by background. Association with Buffalo Bill, and picture of him that I had in childhood (these came up mostly after card was removed from view).

Stimulus 25. No feeling during exposure. Perception clear. A feeling has arisen from memory of stimulus.

Stimulus 26. Feeling mixed of 3 and 5. Almost simultaneous with exposure. Sharp contrast with feeling of background which inhibited that of 26.

Stimulus 27. Vivid perception (color) and yet no feeling until after the exposure.

Stimulus 28. Feeling no. 3 intense (or vivid) inhibited background for a short time.

Stimulus 29. Clear vision but no feeling.

Stimulus 30. Clear vision, feeling no. 6 (depression). Inhibited instantly by background.

Stimulus 31. Not clear vision and no feeling (except, as always, in recall).

Stimulus 32. Clear vision, feeling no. 3.

Stimulus 33. Clear vision, feeling no. 4 (remarkably indifferent sensation).

Stimulus 34. Feeling no. 1, rather intense, arose instantaneously and seemed to persist, coloring the background with feeling-tone.

Stimulus 35. Feeling no. 3, moderately intense, arose as background was being exposed and inhibited feeling of latter for a moment.

Stimulus 36. Feeling no. 5, mild, but it inhibited background for a short time.

Stimulus 37. Feeling no. 2 (weak) arose after background had been seen.

Stimulus 38. Feeling no. 6, brief, not intense, and inhibited by background.

Stimulus 39. Feeling no. 2, of mild intensity, arose slowly, but exposure was long and feeling came during exposure. Was inhibited at once by background.

Stimulus 40. Feeling no. 5 (depression?). In all these cases the background cuts off the previous perception and feeling.

Stimulus 41. Clear vision; feeling no. 6, with pleasant associations.

Stimulus 42. Perception not clear: feeling-tone no. 1 attaching to memory image.

Stimulus 43. Feeling no. 2, relaxation, moderately strong, inhibited by background.

Stimulus 44. Mild feeling of tension.

Stimulus 45. No feeling during exposure. From memory image a mild no. 3, alternating with feeling from background.

Stimulus 46. No feeling during exposure, but faint perception: fairly strong but indescribable feeling attached to memory image.

Stimulus 47. *Loathing*, very strong, but not the same as feeling no. 7. Arose instantaneously and persisted after exposure ceased.

Stimulus 48. Feeling no. 5 during exposure and increase to 6 and 7 after exposure (attached to memory image). A slight bodily tendency to shudder.

Stimulus 49. Disgust, intense: not same as feeling no. 7. Instantly and persistently felt. Contraction of the diaphragm.

Stimulus 50. Feeling no. 6, with a rather strong sensation of revolt in the stomach. Inhibited background for a moment and persisted for a considerable time longer.

Stimulus 51. Feeling no. 2 on first glancing, changing to no. 5 as (during rather long exposure) my attention rested on lesions of the skin.

Stimulus 52. Feeling no. 6 moderately strong. Disgust besides; organic sensations in lower part of trunk. All pretty quickly inhibited by background.

Stimulus 53. Moderately strong, instantaneous, and inhibiting background, cringing feeling; reaction in throat.

Stimulus 54. Feeling no. 7 strong, instantaneous and inhibiting background; reaction in throat.

Stimulus 55. The disagreeable fact not obvious on first glance; it developed later (from memory image), after background shown, and had a feeling-tone no. 7, rather mild.

Besides simple pleasantness and unpleasantness, *H* records various feelings; as, dejection, depression, massiveness, tension, relaxation, restfulness, loathing, a certain indescribable feeling. These are, however, rather emotions than simple affections, such as we should expect to arise from the nature of some of our stimuli, and as we infer from accompanying reactions in throat and chest, contraction of diaphragm, revolt in stomach, contraction in back of mouth, etc. Or they may, perhaps, be simply complexes of organic or kinæsthetic sensations. Other observers also recorded these feelings and reactions, but far less frequently, except *S*, who is very liable to bodily reactions. *S* recorded, besides the reactions stated above, such things as a tendency of the whole body to a forward motion with involuntary 'ah!' — surprise; opening mouth, as if to speak or about to ask a question — wonder; shrinking — depression; shrinking of entire body — utter disgust; wrinkling or knitting brow and twitching mouth — shudder; expanding of whole body — moderately pleasant; closing of mouth and deep wrinkling of forehead — very disagreeable; tendency to raise left arm at very moment of exposure — distinct surprise at the blackness of the general impression; wide opening of eyes — after excitement; backward and contracting motions of entire body — very unpleasant and disgusting; slight forward movement of body — slightly pleasant; opening of eyes and exclaiming 'ah!' — very pleasant; tendency to smile — moderately pleasant; 'sour' reaction all over — very unpleasant and disgusting; reaction by certain finger movements — curiosity; raising of eyebrows to give close attention — wonder; etc. There are a few cases of mixed feeling mentioned in the records of *H*, *F*, *D* and *T*. Cases of affective change with change of the exposure time are noted in all the observers' records and with nearly every stimulus. More particularly, all agree that the transition from the state of no feeling to nos. 3 and 5, and thence to other steps in either direction, is definitely observable, and that at the region near no feeling and the next steps our gradations are not fine enough to express affective discrimination. Thus they often speak of 4.5, 3.5, 3.2, 3.7, etc. Finer discrimination of this sort was not recorded by *H* and *W* because they thought that the graduation should be given in the seven steps as prescribed. *T* and *B* observed that in most cases a certain process of mental excitement intervened between the stage of no feeling and the next affective stage; this they called a 'purely indifferent feeling.' The period of this feeling was usually very short, and it was liable to be overlooked.

H seems to believe that each of the seven steps has its own intensity and vividness; he speaks, for instance, of 'Feeling no. 1 aroused faintly'—st. 14; 'Feeling no. 3 rather strong' —st. 20; 'Mild intensity of feeling no. 1'—st. 22; 'Feeling no. 2 very faint'—st. 18. But the observer did not seem sufficiently to pay attention to the influence of organic sensations and other sensational and apperceptive complexes upon the affective judgment. The differences in these factors are too often confused with alleged qualitative differences in simple affections, while in reality the latter may be quite homogeneous. Moreover, only three similar cases are recorded in all the reports of the other observers. At any rate, the evidence is not conclusive.

What all observers became surprisingly certain of is that our time-sense for affection is exceedingly definite. They could clearly trace the manner of the appearance and cessation of affection in terms of quickness, duration, and the temporal relation of the affection aroused by the stimulus to that of the background; thus they tell how it quickly appeared, or how it slowly disappeared, or how long it lasted, or they observed "the feeling came long after exposure," or "it came just at the end of the exposure," or "it came after the background was exposed," or "the feeling was inhibited for a time by the background," or "it instantaneously inhibited the feeling from the background," or "it arose instantaneously and persisted after exposure ceased," etc. Observer *T* noted that the feeling had time to reach a constant level, and that there was more or less interval during which inhibition existed. These observations seem to warrant the conclusion that affection or feeling is identical in temporal definiteness with sensory experience; and in this connection we may repeat our previous conclusion that the affective reaction-times and their variations are of the same order as those of the sensory reaction. The affective times are, however, always more or less longer than those of the cognitive reaction.

II. Reaction Experiments with Simple Colors.

The experiment reported in the first part of Section I. was made with combined color impressions. It seemed desirable to supplement this by an experiment with simple color impressions. Out of the Milton-Bradley color series, 28 colors were selected, and from these there were rechosen, as the permanent stimuli, the six: I., red (R); II., green (G); III., violet blue (VB); IV., blue violet (BV); V., violet shade 1 (VS1); VI., engine colored paper 7, with the view that colors very pleasant, very unpleasant, and somewhat variable in affective quality, as well as the two colors most widely separated in the range of reaction-time, should be included in these 6 stimuli. The selection was made on the basis of preliminary experiments with the 28 colors, performed with three observers who did not take part in the regular experiments. The other 22 colors were employed to introduce a variety in the stimuli, and with a view to the avoidance of any possible habituation. The times of reaction to these colors were not counted in the result. With each of the 6 colors, 20 affective reactions, and the same number of cognitive reactions were taken with the same stimuli for the observers *P* and *G*. For another observer *R*, 15 affective reactions and an equal number of cognitive reactions were taken with each of the 6 colors. A set of experiments comprised 26 colors, 6 of which were selected from the extra 22 colors, and distributed among the rest. The order of presentation, and the choice of extra stimuli were determined by chance.

The apparatus used was the vernier chronoscope,[1] model II., and the Wundt tachistoscope.[2] The whole frame of the tachistoscope was covered in front by a sheet of gray cardboard. The cardboard had a window (4 × 4 cm.), the lower end of which was 1 m. 16 cm. distant from the floor. A strong cardboard was inserted between the two pillars to stop the fall-screen, in which a colored paper mounted on a cardboard was placed. The screen was suspended by a pair of electromagnets at such a height that the color appeared at the moment it began to fall. The electromagnets and the chronoscope formed a circuit through

[1] E. C. Sanford, *Am. Jour. Psy.*, XII., 1901, 592.
[2] J. Zeitler, *Philos. Studien*, XVI., 1900, 381.

wires and a storage battery, which on the depression of the key was broken at the same time that the long pendulum was released.

The observer was instructed to react at the moment of the appearance of the affection, and to record his judgment in terms of P–U. He was also given general directions for the performance of the cognitive reaction to the colors. In the actual experiment, the observer sat at a distance of 1.5 m. from the window, and the experimenter standing by the tachistoscope said 'Ready,' whereupon the observer adjusted his hands to the keys. After about 3 seconds, and at the signal 'Now,' the observer pressed the key with the forefinger of the left hand, to start the long pendulum, and after the required experience pressed the key of the short pendulum with the forefinger of the right hand. The experimenter counted off the swings of the two pendulums in the usual way. The experiments were made during the months of March, April and May, 1908. They were performed in ordinary diffuse daylight; every care was taken to keep the light as constant as possible, by the adjustment of white curtains at the windows of the room.

The observers were Miss M. G. Rand (*R*), Dr. W. H. Pyle (*P*), and Dr. L. R. Geissler (*G*). *R* had had two years of laboratory training in psychology; *P* and *G* are assistants in the psychological laboratory. *G* had already taken part in extended affective studies. Only a few preliminary experiments were made for all observers, just enough to let the observers adapt themselves to the experimental conditions, with a view to the comparison of the variations due to the influence of practice upon affective and sensory reactions.

In the following Tables I., II. and III., the Roman figures in the first vertical column indicate the order of the series, and the average in each series is based upon 5 reaction-times with one and the same stimulus. The reaction-times in series IV. for *P* and *G* were taken under entirely different instructions. In this series the observers were requested to take a perceptive attitude, an attitude favorable to the determination of the quality and intensity of the stimulus, and to react when the affection naturally appeared in spite of that sensory attitude (which

TABLE I.

OBSERVER G. UNIT, $\frac{1}{100}$ SEC.

Series.		Affective Reaction Times.								Cognitive Reaction Times.							
		R.	G.	VB.	BV.	Vs1.	7	Wrong.	Av.	R.	G.	VB.	BV.	Vs1.	7	Wrong.	Av.
	Range	126–190	112–128	90–122	96–140	108–126	100–142			40–49	40–54	41–54	46–54	46–54	43–48		
I.	AV.	149	118	109	119	117	117	1	121.5	46	47	46	50	50	45	2	47.3
	MV.	21	7.0	10	14	8.0	14			3.7	5.0	5.0	2.7	2.7	1.8		
	Range	108–128	84–118	88–110	98–114	92–110	90–108			42–50	36–50	40–50	43–51	40–48	40–44		
II.	AV.	115	101	102	107	103	95	1	103.8	45	41	45	47	43	42	1	43.8
	MV.	5.3	8.0	6.1	5.6	5.0	6.0			2.3	4.2	3.7	3.0	2.7	1.3		
	Range	105–126	84–115	89–110	98–112	94–113	92–110			42–51	38–53	42–52	42–49	42–50	40–46		
III.	AV.	114	103	101	107	104	99	1	104.7	45	42	45	46	45	43	0	44.3
	MV.	6.1	8.2	6.3	6.5	5.3	7.0			2.4	3.4	3.2	2.9	2.3	1.8		
	Range	98–103	96–110	110–114	100–118	94–113	88–93			42–50	39–54	41–51	43–50	41–51	42–48		
IV.	AV.	100	103	112	109	104	90	1	103.0	44	42	44	46	47	45	1	44.7
	MV.	1.2	5.2	1.1	6.5	5.2	1.1			2.3	3.5	3.3	3.0	2.8	2.2		

Table II.

Observer P. Unit, $\frac{1}{100}$ Sec.

Series.		Affective Reaction Times. R.	G.	VB.	BV.	Vsı.	7	Wrong.	AV.	Cognitive Reaction Times. R.	G.	VB.	BV.	Vsı.	7	Wrong.	AV.
I.	Range	84–120	92–130	90–110	70–120	98–144	84–130	2	103.7	44–57	42–52	41–52	40–52	41–56	44–51		
	AV.	96	107	101	100	117	101			49	48	48	48	47	48	2	48.0
	MV.	8.7	12	6.0	17	15	17			5.0	4.0	4.3	4.0	6.0	3.0		
II.	Range	62–86	53–76	60–66	60–84	63–90	58–80	1	70.0	41–57	40–52	47–52	39–52	41–56	44–50		
	AV.	78	65	64	68	79	66			48	45	49	46	48	47	0	47.2
	MV.	8.0	7.2	2.4	6.8	7.8	6.0			6.3	5.0	1.5	5.0	4.0	2.3		
III.	Range	61–86	53–74	60–67	61–82	64–90	58–72			41–55	40–54	46–51	39–52	42–56	42–48		
	AV.	76	64	64	69	79	64	1	69.3	47	45	48	46	49	46	1	46.8
	MV.	7.8	7.1	2.6	7.0	8.0	5.1			5.6	4.8	1.7	4.8	4.4	3.2		
IV.	Range	92–104	84–88	90–94	92–99	92–118	92–98			42–55	41–54	44–50	41–55	41–54	42–49		
	AV.	95	86	91	95	103	95	1	94.2	47	46	47	47	49	46	1	47.0
	MV.	6.3	1.3	1.7	2.7	9.0	2.3			5.5	5.0	3.0	5.0	4.6	3.0		

TABLE III.

OBSERVER R. UNIT, $\frac{1}{100}$ SEC.

Series.		Affective Reaction Times. R.	G.	VB.	BV.	Vsi.	7	Wrong.	Av.	Cognitive Reaction Times. R.	G.	VB.	BV.	Vsi.	7	Wrong.	Av.
I.	Range	200–348	126–210	152–174	102–342	200–270	180–220			33–40	26–36	35–41	32–36	35–40	27–34		
	AV.	274	168	163	222	235	200	3	210.3	36	33	37	34	38	31	2	34.8
	MV.	49	14	10	56	23	13			2.3	4.3	2.7	1.3	1.3	2.7		
II.	Range	44–56	41–49	45–50	46–50	40–50	46–53			32–40	27–32	34–38	30–34	36–45	27–32		
	AV.	50	44	47	48	45	50	1	47.3	35	32	36	34	38	33	1	34.3
	MV.	4.0	3.7	2.0	1.3	3.3	2.3			2.5	2.6	1.4	1.4	3.7	1.6		
III.	Range	44–54	41–50	43–48	44–48	42–54	44–50			32–41	28–33	33–38	32–36	36–44	28–32		
	AV.	49	45	45	47	47	48	1	46.8	36	32	35	35	38	32	1	34.3
	MV.	3.9	3.8	1.5	1.8	3.6	2.3			2.9	2.8	1.3	1.8	3.5	1.8		

seemed to represent the more common state of affairs in ordinary life); while in all other series for *P* and *G* the attitude was purely affective, *i. e.*, a receptive attitude towards the affective quality.

Inspection of these tables shows that the times of affective reaction are decidedly longer than those of cognitive reaction. The MV of the affective reactions is also larger than the MV of the sensory, except in series IV. The times in IV. for *G* are practically the same as those in II. and III.; while the times in IV. for *P* are much larger than those in II. and III., and stand between the times in these series and in series I. In general, however, the mean variations in IV. are for both observers much smaller than those in the other series. If we take the percentages of the mean variations in terms of the averages, they are smaller even than those of the corresponding series of cognitive reactions. The ranges in IV. for both *P* and *G* are very irregular, and are similar in their mode of irregularity to the ranges in series I. The averages in I. are larger than those in the other series, especially for *R*. The longer times and the irregularity in I. are probably due to the observer's comparative helplessness in passing affective judgments, and to the fact that she availed herself of various secondary criteria, — a fact clearly shown in the introspective records of the first 30 affective judgments. General introspective remarks which were made at the end of series I. are as follow: "In general, judgment was difficult, and it was hard to come to a final conclusion. I hardly ever pass judgment immediately, and am usually uncertain of final judgment." Between series I. and II. there were 6 reaction times, one for each stimulus, which were isolated from those of both I. and II. Taking the red first they are: 94, 82, 88, 108, 92, 92; and when *R* was asked to tell the nature of the judgment in these reactions, she stated: "Judgment was easy and quite immediate, but not satisfactory, as in many cases it had changed while I was reacting." At the beginning of series II., the observer seemed to have learned entirely to give up her reflective attitude, and the very same stimuli which were formerly judged by their associates are now ranked in terms of their intrinsic pleasantness or unpleasantness. The sudden decrease in the times in II. is the effect of this radical change. The times in III. do not greatly differ from those in II. At the end of series II. the observer made the following general observation: "Judgment was very easy and perfectly immediate. There was distinct pleasantness or unpleasantness accompanied sometimes by organic sensation as color was presented. This is the first time I have been able to react on the feeling before first naming the color that appeared." At the end of the last series, when the observer was asked to describe her affective reaction in comparison with the cognitive reactions during the series II. and III., she stated: "When reacting for perception, my attitude is more attentive. I am actively looking to see something and expecting to do something. During the affective reaction, my attitude is more quiet. I am waiting to receive the stimulus and note its effect. In neither case is there any attempt to speculate on what is going to appear. In the perception reaction, I press the key as soon as I have noted the color, and there is absolutely no feeling of *P* or *U* unless I deliberately look again at the color. In the affective reaction, when the color is pleasant, there is a distinct feeling of 'lightening' of the eyes; when unpleasant, a kind of sinking feeling and a bodily relaxation."

The times in II. and III. for all three observers are much shorter and are quite constant. One might be tempted to explain this by a possible habitual association, but there was nothing of this kind in evidence. For when *G* was requested to give an account of the sudden decrease of the times from his introspective observation, he stated: "Times of reaction seemed to be faster because I could more easily recognize my own state of affection, could tell better which way I felt about certain colors. It was not, it seems to me, a matter of associating a certain color with a certain previous affection or rather with the idea of such a previous experience. The greater ease of telling my affection showed itself also in making finer distinctions in the degrees of pleasantness or unpleasantness. Besides, the reaction with the second pendulum is much more automatic, takes no conscious effort and no innervation. In some of the older series I had sometimes to remind myself while looking at the color that I must now react to it. Thus practice seems to have entered in two different ways: greater familiarity with certain states of feeling and hence faster recognition, and more automatic functioning of the reacting finger. I don't know of anything else that might have entered in."

The other two observers agreed with the above statements.

The absolute times of the affective reaction, even in II. and III., are longer than those of the cognitive reaction not only of the corresponding series but also of the other two series. If, however, we take the relative percentages of the individual ranges and of the mean variations, the figures for the affective reaction are of the same order as those for the cognitive reaction, as is seen in the following table:

	Percentages in Affective Reaction.						Percentages in Cognitive Reaction.					
	R.	G.	VB.	BV.	Vsi.	7	R.	G.	VB.	BV.	Vsi.	7
R	12	22	9.0	14	23	28	15	18	12	14	28	22
	3.3	8.0	3.8	4.8	8.0	7.7	3.7	8.7	5.1	5.9	8.0	9.2
P	12	40	34	24	41	41	11	35	33	14	34	33
	4.1	11	10	8.0	10	10	3.5	16	10	7.0	12	9.0
G	24	37	14	20	20	20	24	40	17	15	21	19
	6.2	7.9	6.1	7.1	5.4	5.1	7.1	8.0	6.3	4.2	5.3	5.1

In this table, the figures in the first horizontal row for each of the three observers give the percentages of the relative variation of the individual ranges, and those in the next row give the percentages of the averages to the mean variations, in series III. The corresponding percentages to be obtained from series II. also show relative variations of the same order. If we average the averages of series II. and III., for the sake of a rough comparison, we find:

	Sensory.	Affective.
R	34.3	47.1
P	47.0	69.7
G	44.1	104.3

The cognitive reaction-times of all three observers, especially of the latter two, are longer as compared with the typical times [1] of earlier studies. This is probably due to the instruc-

[1] E. B. Titchener, 'Zur Chronometrie des Erkennungsactes,' *Philos. Stuaien*, VIII., 1893, 141.

tion given as to the stage of the cognitive process at which the reaction should be made; the observers were told to react at such a cognitive moment that they should be able to report the general quality of color after the reaction movement was made, although actually the report was not required. It seemed fair, for comparison with the affective times, to give their 'Aufgabe' for the cognitive reaction, since in the affective reaction the observers were required to record their judgments in terms of P–U. The affective reaction-times of *G* are strikingly longer than those of the other two observers. This is probably partly due to individual difference, and partly to *G*'s extremely passive attitude in the reaction; for when asked to describe his affective reaction, he stated: "When I am to make an affective reaction I must be unconscious of my body and even of the purpose of the experiment, the idea of the motive has to be as vague as possible. I rely on the very vague pressure sensations coming from the fingers touching the keys to remind me of the reaction. Really, how the second reaction ever takes place I am not able to say, because the motive is not conscious. Hence I catch myself sometimes suddenly arousing to the consciousness 'now I must move that key.' . . . I could not really tell what my attention is on before reacting, probably on the color more or less, but I never thought of this fact until this moment."

There was no single case, in the reaction experiments, when the affection appeared first in consciousness, as the herald of the connected sensory quality. This result is contrary to Wundt's recent opinion[1] and to that of certain others. That the formation of an affective consciousness requires a longer time than that of a sensory consciousness is not only indirectly proved by the reaction experiments, but directly by the introspective evidence given by our observers, and by the evidence of the direct reaction method with which expertments were made in the Harvard laboratory (Section I.). Another result which has been brought out in the present experiment is that the relative variability of the affective reaction-times follows the same rules as that of the cognitive reaction-times, so that the affective proc-

[1] Cf. the writer's article: 'Contributions to the Study of the Affective Processes,' *Am. Jour. Psy.*, XX., 1909, 181.

ess is undoubtedly identical with the sensory in that respect, in spite of its difference from the latter in the need of longer times. Moreover, the affective times of *R* are of almost the same order as the cognitive reaction-times, and it is possible that, as the observer became more familiar with affective experience, the times might more nearly approach those of the sensory reaction.

III. Reaction Experiments with Tones.

In the field of sensory reaction, it is already established that the times differ with different sense departments. Whether the same thing holds for the affective reaction is not yet known, except in the cases of affective reaction with visual and cutaneous impressions.[1] The time-relations of affection to sensation are also known in those two senses. The desirability of systematic completion led us naturally to further study of the same problem with tones; we assumed the applicability of the reaction method in this field. Meanwhile we had also another part-problem in view, that is, the time-relation of the affective processes to their physiological expressions. We therefore resorted to the graphic method. However, a full description of the experiments and presentation of their results would deviate too far from the main aim of the present study; they are, therefore, omitted in this article.

The apparatus used in the main work were a chronometric interrupter manufactured by G. Hasler, Bern; a clock-work kymograph made by C. H. Stoelting; and a piano as the source of stimulus. An electromagnetic time-marker formed a circuit with the interrupter through wires, a storage battery, needles attached to the interrupter, and the mercury contained in small cylindrical glass vessels under the needles. To maintain the vibration of the interrupter, another circuit was made between a pair of larger electromagnets above the interrupter and the direct-current switch in the wall-box of the experimenting room. A Nichols tinned iron rheostat was inserted in the circuit to regulate the voltage. The distance between the needles and the mercury surfaces, and the distance between the electromagnets and the interrupter can be so adjusted that the circuit

[1] *Ibid.*, pp. 187–193; and Sections I. and II. of this paper.

through the time-marker closes, while the circuit through the larger electromagnets is broken, and conversely. The vibration rate of the interrupter ranges between 10 and 60 in a second. In the present experiment, the point of 25 vibrations was selected, but with the highest speed of the drum it was possible to count .02 sec. without fractionating the curve. The stimuli were tones from the piano, under whose keyboard was laid a rubber tube. The one end of the tube was connected with a Marey tambour, and its writing lever marked a signal on the smoked paper of the drum as soon as a key was struck, so that the moment of stimulation was known. The observer's reaction to the stimulus was registered by an electromagnetic signal, which formed another separate circuit through wires, a storage battery, and a double contact electric key. Occasionally, tests were made to determine possible time-errors in the signal of stimulation, by bringing the electric key near and parallel to the piano key, and by striking the note and pressing the key button at the same time, in order to make a comparison of the time-relation of the two signals. There was, however, no definite time error; and if there was any error at all, it was a negligible quantity. Every care was taken to make the ends of these three pointers lie in one and the same vertical line on the smoked paper.

The observer was given general directions for the performance of the cognitive reaction[1] to tones. In the affective reaction he was required to react at the moment of the appearance of the affection, and to record his judgment in terms of P–U. He was also told to make another reaction, at the cessation of the tone or the affection as the case might be. In a concrete experiment, the experimenter sat before the piano, and at the 'Ready' the observer closed his eyes and adjusted the forefinger of the right hand to the button of the key. As soon as the observer seemed ready, the experimenter called 'Now,' and struck a note. After the second reaction, he stopped the drum, and numbered the smoked paper. The experiments were made during the months of June, 1908, to January, 1909, with the intermission of a greater part of the summer vacation. The following observers served in the experiments: Miss M. C. West (*W*),

[1] E. B. Titchener, *Exp. Psychol.*, II., i., 1905, 187.

Dr. Geissler, Dr. Pyle and Miss E. M. Kitch (K), a graduate of Oberlin College. W had already taken part in extended affective studies.

Out of the 46 notes, striking out the first 5 low tones, C_1, D_1, E_1, F_1, G_1, and the last 6 high tones, a^3, b^3, c^4, d^4, e^4, f^4, we selected 7 low tones, A_1, B_1, C, D, E, F, G, 7 high tones, a^2, b^2, c^3, d^3, e^3, f^3, g^3, and the 7 tones of the middle part, a, b, c^1, d^1, e^1, f^1, g^1. Out of these 21 tones, the 9 tones A_1, D, G, a, d^1, g^1, a^2, d^3, g^3 were rechosen as the permanent stimuli; the other twelve tones were employed to introduce a variety of stimuli, with a view to the avoidance of possible habituation. With each of the 9 tones, 20 affective reactions and an equal number of cognitive reactions were taken with the same stimuli. A set of experiments comprised 22 stimuli, 4 of which were selected from among the extra 12 tones, and distributed among the set. The reaction-times of these extra stimuli were not counted in the result. The order of presentation, and the choice of extra stimuli were determined by chance. This rule was kept throughout all the experiments with W, K and G. With P, 30 affective and 40 cognitive reactions were taken with each of the 9 tones. A complete set of experiments comprised 50 stimuli, 30 of which were presented for sensible, and 20 for affective reaction. Care was taken to distribute the 9 tones evenly, and to utilize them all in the affective work. The 50 stimuli were so arranged that the same tone was never presented twice in succession, for the two kinds of reaction; and the order of presentation was reversed from set to set. A half set, 25 reactions, was taken in the experimental hour. Since all the observers had already had considerable experience in reaction experiments, we did not give more than one set of 22 reactions for preliminary practice. With observer P, only a few preliminary experiments were made for each of the two kinds of reaction, to let the observer adapt himself to the new experimental conditions with a view to the comparison of any possible variations due to the effect of practice upon affective and cognitive reactions.

Tables I., II. and III. give the results of 360 reaction-times obtained from the observers G, W and K; 180 times of cognitive, and an equal number of affective reactions for each observer.

Table I.

Observer G. Unit, $\frac{1}{100}$ Sec.

Stimulus.	Affective Reaction Times.				Cognitive Reaction Times.			
	Range.	Average.	MV.	Wrong.	Range.	Average.	MV.	Wrong.
A_1	73–96	89	8.2	1	34–42	38	3.0	0
D	68–94	85	11.0	0	35–44	40	3.1	1
G	60–87	82	10.7	0	26–36	32	3.0	0
a	59–89	73	8.5	0	26–36	30	3.6	1
d^1	57–78	68	8.8	1	27–42	36	4.0	0
g^1	60–78	69	6.3	1	31–38	35	3.5	0
a^2	76–94	82	8.0	1	27–38	31	4.0	0
d^3	54–80	72	9.3	0	25–34	30	3.1	0
g^3	80–92	85	3.2	1	35–42	38	2.7	1

Table II.

Observer W. Unit, $\frac{1}{100}$ Sec.

Stimulus.	Affective Reaction Times.				Cognitive Reaction Times.			
	Range.	Average.	MV.	Wrong.	Range.	Average.	MV.	Wrong.
A_1	56–76	65	7.0	1	32–47	41	4.9	2
D	58–74	67	5.0	1	35–45	40	4.0	1
G	59–87	77	7.4	0	32–44	38	4.2	1
a	47–63	53	6.0	1	30–44	36	4.0	0
d^1	48–58	53	5.0	2	37–44	41	3.5	1
g^1	58–89	76	9.9	1	30–44	37	4.0	1
a^2	54–70	63	5.7	1	37–41	39	2.0	1
d^3	59–64	62	2.0	0	38–48	42	3.5	2
g^3	49–57	53	5.1	1	36–42	39	2.3	1

Table III.

Observer K. Unit, $\frac{1}{100}$ Sec.

Stimulus.	Affective Reaction Times.				Cognitive Reaction Times.			
	Range.	Average.	MV.	Wrong.	Range.	Average.	MV.	Wrong.
A_1	52–73	63	7.0	1	30–42	37	4.0	1
D	66–86	76	8.1	2	35–45	42	4.0	1
G	60–86	76	7.5	1	26–42	33	3.9	1
a	60–80	69	8.2	1	38–46	40	3.4	1
d^1	64–76	71	4.0	0	25–40	33	3.9	1
g^1	45–76	61	7.9	0	29–94	38	4.3	1
a^2	56–80	69	8.4	0	38–43	41	2.5	0
d^3	58–74	68	6.7	1	30–45	41	5.2	1
g^3	54–70	58	6.2	1	28–38	33	2.0	1

The tables show clearly that the times of affective reaction are decidedly and uniformly longer than the times of cognitive reaction.[1] The MV of the affective reactions is also larger than the MV of the cognitive reactions; a single exception occurs for W with the stimulus d^3. In general, however, the individual ranges of the affective reaction-times are of the same order as those of the cognitive reaction-times in their relative variation; and the total range of cognitive variations, from 20 per cent. with g^3 to 41 per cent. with a^2 for G, from 11 per cent. with a^2 to 47 per cent. with a for W, and from 13 per cent. with a^2 to 61 per cent. with g for K, nearly coincides with the affective limits 15 per cent. (unpleasant) and 50 per cent. (pleasant) for G, 9 per cent (distinctly pleasant) and 53 per cent. (slightly pleasant) for W, 19 per cent. (very pleasant) and 68 per cent. (pleasant) for K. The individual relative values of the mean variations to the averages are also of the same order; and the mean variations corresponding to the above percentages are as follows: 7 and 13 per cent. for G, 5 and 11 per cent. for W, 6 and 12 per cent. for K, in the cognitive reactions; 4 and 12 per cent., 3 and 13 per cent., 6 and 13 per cent., in the affective reactions.

Table IV. gives the results of 270 affective reaction-times and 360 cognitive reaction-times. The Roman figures in the second row indicate the order of the series, and the figures under each series show the range, average and its mean variation of 10 reaction-times with the same stimulus. The average in the third row from the last is the average of averages in each of three series in successive order.

The general results are the same as those of Tables I., II. and III. Taking the figures in the third series, the total range of cognitive variations, from 35 per cent. with G to 64 per cent. with g^1, stands nearly on the same level as that of the affective limits 26 per cent. (very pleasant) and 53 per cent. (barely pleasant). The corresponding percentages of the mean variations are 9 and 11 on the one hand, 8 and 13 on the other. The results in the first and second series do not greatly differ from those of the third series. The average of the first series is

[1] G. Martius states that the reaction-times for high tones are shorter than those for low tones (*Philos. Studien*, VII., 1891–92, 470–480). The times in the above tables show no trace of such difference; probably it has been swamped in our larger cognitive times.

larger than those of the two other series; the decrease of the time is probably due to the influence of practice. It is, however, so slight that the times in I. show no trace of isolation from

TABLE IV.

OBSERVER P. UNIT, $\frac{1}{100}$ SEC.

		Affective Times			Cognitive Times.			
		I.	II.	III.	I.	II.	III.	IV.
A_1	Range	48–58	49–58	47–59	27–40	28–40	25–39	25–39
	AV.	52	51	52	30	29	30	30
	MV.	4.0	3.8	4.0	3.6	3.4	3.1	3.1
D	Range	44–65	45–65	44–64	26–40	27–42	27–40	25–40
	AV.	56	54	55	31	30	31	29
	MV.	6.1	6.0	6.0	3.5	3.5	3.0	2.6
G	Range	40–62	40–60	40–60	27–38	26–36	26–35	26–36
	AV.	50	49	49	29	29	28	28
	MV.	5.1	4.8	5.0	2.6	2.4	2.5	2.6
a	Range	55–82	56–79	58–80	26–40	28–41	26–40	26–39
	AV.	65	63	63	33	32	31	31
	MV.	7.0	6.8	6.8	3.6	3.4	3.2	3.2
d^1	Range	50–76	51–77	49–74	24–38	25–39	24–37	25–36
	AV.	63	63	61	30	29	29	28
	MV.	7.4	7.0	7.0	3.0	3.2	3.0	2.7
g^1	Range	44–64	45–65	44–63	24–38	24–40	25–41	24–38
	AV.	54	54	52	28	29	28	27
	MV.	6.5	6.6	6.2	3.0	3.1	3.0	2.8
a^2	Range	54–82	52–79	51–76	25–39	26–38	24–35	24–37
	AV.	66	64	63	31	30	30	29
	MV.	8.6	7.9	7.8	3.4	3.2	3.2	3.2
d^3	Range	50–78	50–75	51–78	24–34	26–35	25–34	25–36
	AV.	62	60	60	29	28	28	27
	MV.	8.1	7.9	7.8	3.0	2.8	2.7	3.0
g^3	Range	52–76	53–75	53–76	28–42	29–42	28–41	24–40
	AV.	63	61	60	36	35	35	34
	MV.	6.0	6.0	6.2	3.9	3.7	3.8	3.5
Average		59.0	57.7	57.0	31.0	30.1	30.0	29.0
P.E. of AV.		±4.01	±3.80	±3.54	±1.66	±1.42	±1.49	±1.49
Wrong		3	2	2	3	2	1	1

those in the other series. A like constancy was found with the three other observers. There is no indication of an effect of fatigue, which would show in loss of regularity and in a lengthening of time in the later series.

For the sake of a general grasp of the essential result, we give the average of averages for all four observers:

	Sensory.	Affective.
G	34.4	78.3
P	30.4	57.9
W	39.2	63.2
K	37.5	67.9

The reaction at the cessation of the tone or of the affection was required not in all, but in the greater part of the experiments. The total number of experiments in which the second reaction was made is 216: 12 cognitive reactions with each of the 9 tones, and the same number of affective reactions, for each observer. The following table gives the result:

Observer.	Sensory Duration.				Affective Duration.			
	P	*G*	*W*	*K*	*P*	*G*	*W*	*K*
A_1	337	159	246	237	218	119	237	250
D	192	118	224	218	121	114	169	173
G	190	114	189	186	112	110	89	156
a	190	101	137	132	90	100	79	189
d^1	180	93	120	120	90	97	80	100
g^1	178	93	160	171	86	96	72	246
a^2	188	90	120	140	92	95	70	188
d^3	175	91	230	252	85	90	267	316
g^3	137	92	210	215	78	94	225	233

The table shows that in general the duration decreases from the low tones to the high. The tones d^3 and g^3 are marked exceptions with *W* and *K*. However, what we are concerned with is not the relation of duration to pitch, but the relation of sensory to affective duration. For *P* the sensory duration is always longer than the affective, and in his introspective records there is no mention of a case in which affection lasted longer than sensation. For *K*, *W* and *G*, such cases are by no means exceptional, and in the introspective records they often and definitely stated their occurrence. They noted, however, that the affection attached to a kind of memorial after-image of the tone. For *W* bodily attitude seemed also to have something to do with it: "I noticed in exp. 3 and 4 that after the tone ceased I was still enjoying it, but as I noticed this the *P* seemed to resolve itself into a physical attitude — very still, breath indrawn, a sort of tickle in the chest, eyes closed, chest lifted, and a sort of judgment without words that it was nice. It seemed to me that as long as I kept myself perfectly still and in that position I should continue to enjoy it, and then I began to be uncertain whether it was real feeling or not — I thought 'this might as well end — I'll never know — I can keep this up indefinitely' — and so I struck the key. In both cases I felt dissatisfied afterwards, and thought that the next time it happened I would keep it up, and see if I had the feeling as long as I had the attitude. So in no. 9 I held on to the feeling and attitude until I was sure the feeling was gone, but I wasn't sure when I lost the attitude, it may have been before or after or with the feeling. But the difference between this state after the feeling is gone, and the other which I wasn't sure of as *P*, makes me now certain that it was *P*, and that I could not hold it indefinitely; yet I could and did prolong it, I think, by holding the attitude — for the other times, when I gave it up, *P* ended as I struck, when it might have lasted longer, I think."

W also recorded many cases of affective change, and in such cases there was usually an indifferent state at the point of change; but the time-relations of the state could not be determined; its duration was very short, and was not registered.

Turning now to our original problem, we should say in summary that the main results of the present experiments are essentially the same as those of the last section. It remains only to remind the reader of the fact that in the affective reactions with colors, the initial irregularity in variation and the subsequent shortening of the times were striking, while in the affective reactions with tones there were no such heterogeneous variations or sudden decrease of the times.

Conclusions.

The essential conclusions to be drawn from this study and from the earlier researches connected with it are (1) that affective judgments may be and usually are as direct and immediate as the sensory judgments of psychophysics; (2) that the formation of an affective consciousness requires a longer time than that of a sensory consciousness; (3) that affective times and their variability are either absolutely or relatively of the same order as sensory times and their variability; (4) that the method of reaction, when applied to the affective processes pleasantness and unpleasantness, has a like scope and validity as for sensory processes. More particularly, the direct reaction-method is feasible and reliable. However, the time-relation of affective to sensory process varies with the different sensory fields. The relation is most intimate in the case of cutaneous impressions, as was shown in our work with the discriminative reaction; the averages of the sensory and the affective times for an average observer being 56.8 and 73.5. There was an indication that the relation would be still closer, with olfactory stimuli, as we judged by the result of our experiments with odors. The time-relation in the experiments with colors and tones is complicated, and it is difficult to make a fair comparison (the average affective times of tones and colors are respectively 67.9 and 69.7); but in general, the affective reaction to color impressions seems most remote from the sensory in its time-relations. The times, however, vary with variation of stimulus; sensory intensity is the most important factor. This is best shown in the experiments made with illustrative cards in the dark room. The cards were adequately illuminated, and were very favorable for the arousal of affective processes, as was shown both by the observers' introspective evidence that the quality, intensity, and temporal course of the affective processes were in their case surprisingly definite, and also by the objective evidence that the affective times were so far shortened that the median, for an average observer, amounted only to 42.5. In regard to the interpretation of the results of our reaction experiments, two opinions are *a priori* possible. The one is that pleasantness and unpleasantness are the resultant of a sensation-complex or of an apperceptive combination, the range of the complex process being variable through three grades at least: sensational complex, apperceptive combination of sensation and ideas, and fusion of apperceptive combination with organic sensations. Affective arousal should then, as Münsterberg would maintain, take a longer time than sensory, since these complex processes must be formed before affection appears as a definite resultant in consciousness. The other view regards lack of clearness as the principal criterion of affection. The delayed appearance of an affection is then due to this characteristic. The results of our experiments tell, we believe, for the second hypothesis; for affective judgments could be passed directly and immediately on the basis of the stimuli and of these only, as is proved in our earlier studies on the mechanism of the affective judgment. In the experiments with illustrative cards, cases were frequent in which the mature stage of complex apperceptive fusion had been reached in the affective judgments, and yet these did not require longer times; the averages and the medians were, on the contrary, shorter than the times of affective reaction with simpler processes.

It is, however, a question if lack of clearness in affection is absolute. It may be only relative; it need not imply the impossibility of affective attention. Discriminations in seven or even more than seven grades; the definite determination of rise, cessation, and details of temporal course; observations of intensive change with change of exposure time, and of phenomena of inhibition; and finally, the similar or identical time-relations of affective and sensory process — all these introspective descriptions and objective results may perhaps be direct measures of affective clearness and affective attention. The whole issue must, however, be left open, so far as the present studies are concerned, since an affective consciousness always includes sensory or ideal components, and what appear without analysis to be affective discrimination and affective attention may be based upon and guaranteed by these sensory or ideal concomitants.

Further light on the relation of affection to sensation can be hoped for only after serious researches have been carried out on the other criteria of affection proposed by various investigators, which have recently received review and criticism by Titchener in his *Psychology of Feeling and Attention.* What we think we have proved is that affection is different from sensation in its need of a longer time of arousal; but that it is akin to sensation in so far as affective judgments are direct and immediate, and affective times and their variability are of the same order as those of their sensory correlates.

A NOTE ON THE ACCURACY OF DISCRIMINATION OF WEIGHTS AND LENGTHS.

By PROFESSOR EDWARD L. THORNDIKE,

Teachers College, Columbia University.

It is the purpose of this note to present some new data upon the very old question referred to by the title in the particular case of weights of 100 and 200 grams and lines of 50, 75 and 100 mm.

In spite of the repeated failure of experimental researches to verify the doctrine that the larger magnitude requires a proportionately larger difference for equal discriminability, that doctrine reappears so persistently in our text-books that it may be supposed to require further refutation.

The measurements with weights comprise 16 tests by the error method with each of 72 subjects. Eight were with a 100-gram standard and 8 with a 200-gram standard. The method was to give the subject the standards and also some boxes identical with the standards, except that of course they were empty, and also a supply of lead scraps and shot. The subject filled an empty box, comparing it with the standard by lifting both simultaneously or successively as often as he chose, adding or taking out shot until he was satisfied. The experiments were conducted by Dr. Wilfrid Lay, a trained psychologist, and Mr. P. R. Dean, a student and teacher of physics. Counting of the shot was not done by the subjects and, of course, would not have been allowed. They worked in ignorance of the amounts of the errors they made. The subjects were 37 young women students of psychology and 25 high school boys.

In the nature of the case there were no important general tendencies to constant error since the subject compared his weight with the standard back and forth several times. With the 200-gram weight there was a slight tendency to a minus error because the subjects filled up *to* the weight oftener than they over-filled and corrected. The deviation from the standard is the measure (inverse) of accuracy of discrimination.

The general tendency is for the 200-gram weight to show an error 1.585 times that of the 100. The probable Median Deviation of this result from the true general tendency is *.04*, Taking the 37 young women separately we have 1.65 (*.07*). The figures are 1.52 and (*.14*) for the 25 high school boys.

Table I. gives for the 34 women the sum of the deviations from the standard for the 100-and 200-gram weights, the ratio

TABLE I.

Individual.	Sum of Deviations.		Ratio of Error with 200 g. to that with 100 g.	Apparent Median, Constant Errors, with:	
	From 100 g.	From 200 g.		100 g.	200 g.
1	96	58	.60	2	4
2	64	93	1.45	9	— 3
3	45	94	2.09	5	0
4	37	73	1.97	3	0
5	57	88	1.54	— 6	— 4
6	48	76	1.58	— 5	0
7	21	47	2.24	0	.5
8	45	27	.60	— 6	.5
9	36	191	5.31	1.5	— 7.5
10	79	103	1.30	7.5	2.5
11	24	96	4.00	— 1.5	— 3
12	55	91	1.65	2.5	— 6.5
13	21	43	2.05	0	— 2
14	15	82	5.47	0	— 8
15	43	85	1.98	0	1.5
16	41	65	1.59	3	9
17	52	103	1.98	— 6	—12.5
18	21	46	2.19	— 1.5	— 3.5
19	28	110	3.93	1.5	— 3.5
20	75	89	1.19	3.5	— 8
21	52	160	3.08	— 2	—19
22	48	76	1.58	— 6	— 8.5
23	104	74	.71	— 3	1
24	25	72	2.88	2	— 5
25	111	118	1.06	16	— 8
26	33	55	1.67	0	— 5.5
27	48	78	1.63	2	10.5
28	37	56	1.51	3	— 1.5
29	52	77	1.48	7.5	.5
30	56	73	1.31	1.5	3.5
31	93	58	.62	.5	2
32	28	50	1.79	0	.5
33	100	214	2.14	—12	—29
34	34	58	1.71	1.5	— 1
35	68	148	2.18	3	4
36	157	169	1.08	29	— 8
37	42	25	.60	4	— 2

Median of ratios = 1.65.

of the latter to the former and the apparent constant errors for the two sets, all for each individual. The last facts are given to show the impropriety of using the deviation from an individual's own general plus or minus tendency as a measure of discrimination.

Table II. gives similar facts for the 25 boys, except that here the sums of deviations were taken from the general tendency of 198 g. instead of from 200 g. Two hundred grams would have been better to use, but as it makes no difference in the general results, I have not recalculated all the deviations.

TABLE II.

Individual.	Sum of Deviations.		Ratio of Error with 200 g. to that with 100 g.	Apparent Average, Constant Errors, with:	
	From 100 g.	From 198 g.		100 g.	200 g.
38	40	60	1.50	— 2	4.5
39	40	66	1.65	2	6.5
40	37	24	.65	— 4.5	— 4
41	35	53	1.51	1	1.5
42	65	73	1.12	7	7
43	53	264	4.98	— 6	—15
44	90	161	1.79	3	—19.5
45	103	91	.88	— 8.5	—13.5
46	44	80	1.82	— 4	— 8.5
47	63	96	1.52	6.5	—13
48	28	40	1.43	2.5	1.5
49	33	84	2.55	— 4	— 7.5
50	55	53	.96	7	0
51	34	36	1.06	— 1	— 1.5
52	113	108	.96	13	—14
53	33	61	1.85	— 1	— 1
54	46	72	1.57	— 2	— 4
55	33	80	2.43	0	1
56	62	94	1.52	7.5	5
57	36	45	1.25	— .5	— 5.5
58	49	109	2.22	0	9
59	65	138	2.12	— 7	—11
60	62	72	1.16	8	— 2
61	23	73	3.17	.5	6.5
62	79	97	1.23	— 8	— 5

Median of ratios = 1.52.

My measurements with lines comprise 60 tests with each of 37 individuals — 30 in drawing a line at one side of a 100-mm. standard as nearly as possible equal to it, and 30 in equalling in the same way a 50 mm. standard. The individuals tested were the students of psychology mentioned above, and the tests were conducted by Dr. Lay.

Table III. presents the results in the shape of (1) the sum of the deviations from the standard, (2) the general tendency to draw too long or too short lines (the so-called constant error) and (3) the so-called variable error, that is, the general tendency to deviate from the individual's own general tendency to draw too long or too short lines (the measure used for this variable error is the distance between the limits which include 50 per

TABLE III.

Individual.	A. Sums of Deviations from the Standards.		B. So-called Constant Errors.		C. So-called Variable Errors (2 × Q).		D. Ratios 100/50 from A.	E. Ratios 100/50 from C.
	From 50 mm.	From 100 mm.	From 50 mm.	From 100 mm.	In case of 50 mm.	In case of 100 mm.		
1	166	159	0	3.5	8	8	1.37	1.00
2	91	232	1.5	8	6	5	2.55	1.00
3	51	138	0	4	4	8	2.71	2.00
4	57	77	1	2	4	4	1.35	2.00
5	138	170	4	—2	4	6.5	1.23	1.63
6	108	359	2	12	5	10	3.32	2.00
7	90	79	—2	1	2	5	.88	2.50
8	56	130	0	3	4	6	2.32	1.50
9	110	289	2	6	5	16	2.63	3.20
10	115	242	3.5	8.5	4	5	2.10	1.25
11	80	204	2	6	5	6	2.55	1.20
12	111	234	1.5	8	5	6	2.11	1.20
13	75	124	2.5	4	4	2	1.65	.50
14	115	122	—3	2.5	4	8	1.06	2.00
15	81	86	—3	0	3	7	1.06	2.33
16	94	151	1	4	7	6	1.61	.86
17	118	135	—4	5	3	4	1.14	1.33
18	80	198	—1.5	0	4	15	2.47	3.75
19	66	148	1	4	4	6	2.24	1.50
20	79	203	.5	6	6	8	2.57	1.33
21	122	198	—1.5	—5	10	7	1.62	.70
22	45	97	2	2	2	4	1.02	2.00
23	89	113	3	3	3	4	1.27	1.33
24	253	459	8	15	5	5	1.81	1.00
25	181	220	6	7.5	5	5	1.22	1.00
26	90	193	—1	5	6	7	2.16	1.17
27	138	110	—4.5	— .5	5	8	.80	1.60
28	69	66	2	2	4	4	.96	1.00
29	76	297	—1	9.5	4	7	3.91	1.75
30	167	154	—5	1.5	10	12	.92	1.20
31	83	183	0	6	6	6	2.21	1.00
32	74	141	2	2	2	5	1.91	2.50
33	135	202	4	5	5	8	1.50	1.60
34	112	222	3	7	3	6	1.98	2.00
35	171	331	6	11.5	3	4	1.94	1.33
36	245	329	8.5	10	5	11	1.34	2.20
37	63	160	1	5.5	5	5	2.54	1.00

Medians of ratios are 1.81 and 1.33.
Averages of ratios = 1.79 and 1.42.

cent. of his records), (4) the 100/50 ratio in the case of the deviations from the standard, and (5) the 100/50 ratio in the case of the deviations from the so-called 'constant error.'

As has been pointed out by Professor Cattell, the so-called constant error is really extremely variable in the case of equalling lines. It is a result of short-lived motor or perceptual habits as well as of some persistent tendency. It is sensitive to practice. It differs enormously with individuals. The practice of disregarding it in measuring the accuracy of sense discrimination is therefore dubious. When we disregard it, we do not have left a measure of accuracy in any intelligible sense, but strictly only a measure of the *variability* of an individual in responding to the same situation.

It is not, however, my purpose at this time to interpret the errors made, but only to point out that, whatever measure one chooses to take of inaccuracy of sense discrimination, the inaccuracy is not proportional to the magnitude used as a standard. The deviations from the standard are not twice as great for the 100-mm. line as for the 50, but only one and three fourths times as great [1.8 ± .1 (P.E.)]. And the so-called variable error in the case of the 100-mm. line is only one and one third times that with the 40-mm. line [1.4 ± .08 (P.E.)]. If each individual's deviations from the constant error of the entire 37 are used, the ratio is still less than one and a half.

The variations of the individuals from the central tendency of the group are very wide in the case of both weights and lines. There is nothing like close clustering of the individual ratios about 1.6 in the former, or 1.8 and 1.4 in the latter case.

I attribute the results in general not to one main cause, in the shape of some one psycho-physic law, varied by minor disturbing causes, but to the influence of many specialized tendencies to response. The individual scores for the weights, coming each from only 8 measures, are subject to much variation from the totals of which they are samplings. Tests covering hundreds of trials spread over many days would reduce the individual differences in the 200/100 ratios markedly. But it is hard to believe that such would reduce them to a range much less than 190 to 130. The individual scores for the lines are more reliable and rough calculation of their reliabilities shows the practical certainty that with complete measures there would remain a range of variation, around the 1.8, of from 1.5 to 2.3 and a range, around the 1.4, of 1.15 to 1.75.

In general the determinations of the so-called psycho-physic law have failed to find close correspondence in the different individuals measured or in the different divisions of the magnitude-series used. It is chiefly the speculative doctrine that *some* one simple equating of sensations or judgments with magnitudes judged must exist and be the main cause of our powers to judge them that has led psychologists to neglect these failures. But against such a doctrine stand (1) the facts of the changes in these powers with practice, (2) their specialization with content, and (3) the difficulty of connecting the doctrine with the known facts about the action of the nervous system, as well as (4) the direct measurement of the variations amongst individuals. The present writer believes not only that the experimental data do not give proof of the existence of any one psycho-physic law, but also that they do give proof that there is *not* any such one law.

An objection may be made to the original measurements themselves: namely, that they are extremely complex measures of discrimination resulting from all sorts of conditions. This is true, in the sense that the judgment of weight was allowed to rest upon liftings in any number, at any rate, with either hand, and that the judgments of length were allowed to rest upon data from the movements made in drawing the lines or from the sight of them at varying distances while being drawn and after completion. But it is not necessarily true if complexity means elaborateness from the point of view of the individual making the judgment or with respect to the behavior of the nervous system in making such responses. To have to lift weights at the same rate through the same distance by a time schedule may be a more complex act, because of the inhibitions involved, than the act in the case of my experiments.

But in either event the method is not really objectionable. If the law in question held for the responses when made to each single constant kind of sense datum, it would necessarily hold for any responses to any random combination of such. The particular accuracies might vary, but the general law would appear as before. So, unless the critic can give evidence that the subjects chose specially accurate data from which to judge the 200 g. weights and specially inaccurate data from which to judge the 100 g. weights, the objection is futile.

The writer chose the method deliberately because it seems to him sure that accuracy in sense-discriminations has developed as a function of responses to concrete objects with all the available means at the animal's disposal, and that the laws regulating it will relate more closely to such instinctive responses and the habits they easily grow into than they will to the rare and artificial responses we cultivate by restricting the situation ostensibly to some one element, but really to that element in an elaborate context of distracting suggestions and inhibitions.

A RANGE OF INFORMATION TEST.

BY PROFESSOR GUY MONTROSE WHIPPLE,
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In Professor Kirkpatrick's vocabulary test, the application of which I discussed last year,[1] the list of one hundred test-words is intentionally selected by chance: some of the words, like *page*, happen to be very ordinary, every-day terms; others, like *lanuginose*, are unusual, technical terms. Knowledge of the ordinary words is, of course, common to almost all examinees: knowledge of the more unusual terms, however, depends almost entirely upon the examinee's erudition — upon the nature of his school training, his professional interests, and the quantity and type of his general reading.

I have endeavored to extend, or rather to supplement, the vocabulary test by devising a list of words that shall serve in its entirety as a measure of erudition or range of information. For this purpose the hundred test-words have been selected, not by chance, but by careful consideration, and in such a manner that each shall be representative of some specific field of knowledge or activity, in the sense that if the examinee has made himself familiar with a given field of knowledge or activity, he will almost certainly know the word selected from that field, whereas, if he has not made himself familiar with the field, he will almost certainly not know the term, or at least will not have such knowledge of it as to enable him to define it exactly. Thus, general knowledge of American history is tested by the name *Anthony Wayne*, knowledge of French by *aujourd'hui*, of chemistry by *chlorine*, of golf by *midiron*, of social usages by *R. S. V. P.*, of the technique of photography by *f-64*, etc.

Nature and Method of the Test. — For conducting the range of information test, each examinee is supplied with a printed blank[2] as reproduced herewith: he is asked to read the direc-

[1] 'Vocabulary and Word-building Tests,' PSYCH. REV., 15, March, 1908, 94–105.

[2] These blanks may be had of C. H. Stoelting, 12 S. Green St., Chicago.

tions through twice before marking the words, and his attention is called to the request for definitions that follows the test-words. There is no time restriction.

Name.. Date...

INFORMATION TEST.

Below are 100 words, phrases, or abbreviations, largely technical, which are designed to test the range of your information. Consider each one carefully, and place *before* it one of these four marks:

(1) The mark **D** if you could *define* it as exactly as words are ordinarily defined in the dictionary.

(2) The mark **E** if you could *explain* it well enough to give some idea of its meaning to one who is not familiar with it, though you could not give an exact definition that would satisfy an expert.

(3) The mark **F** if the word is merely roughly *familiar*, so that you have only an indefinite idea of its meaning and could not use it intelligently.

(4) The mark **N** if the word is entirely *new* or unknown to you.

When you have finished, count the marks, and fill out these blanks:

D .... **E**......... **F**......... **N**.........

ageratum	cleistogamous	infusoria	puer
amphioxus	cosmogony	intaglio	pyramidal tract
amphora	cotangent	Kepler's law	quadratics
annealed	dibble	kilogram	rococo
Anthony Wayne	dietetics	kinesthetic	R. S. V. P.
apocalypse	dryad	kinetic	scherzo
architrave	electrolysis	Les Misérables	semaphore
aujourd'hui	Elohim	linotype	simony
Babcock test	entrée	logos	spoils system
base-hit	Eocene	luff	Stoicism
Bernard Shaw	Euclid	Malthus' law	synecdoche
Bokhara	*f*—64	metacarpal	testudo
Braille	f. o. b.	midiron	tort
call-loan	gambit	Millet	trephine
calorie	gasket	mitosis	triangulation
cantilever	glycogen	morgen	trilobite
Cædmon	gneiss	nada	triple-expansion
catalepsy	golden section	natural selection	undistributed middle
cephalic index	guimpe	noi	Utopia
ceramics	hedonism	ohm	vantage-in
chamfer	hemiptera	parallax	way-bill
Chartism	homiletics	peneplain	Weismannism
chlorine	hydraulic press	Pestalozzi	wigwag
chromosome	impetigo	Polonius	X-ray
clearing-house	impressionism	pomology	Zionism

On the reverse side of this sheet define or explain the first five words that you have marked **D** and the first five words that you have marked **E**.

Results. — 1. Typical quantitative results, as obtained by the writer from some hundred cases are embodied in Table I. Inspection of this table makes it evident that advance in school training (coupled with increased maturity) is accompanied by an increase in the number of technical terms that can be defined or explained, or that are at least 'roughly familiar.'

TABLE I.

DEPENDENCE OF RANGE OF INFORMATION ON AGE AND SCHOOL STATUS.

Academic Status.	Number.	*D*	*E*	*F*	*N*
Graduates	4	39.0	21.0	12.2	27.8
Seniors	5	20.6	17.2	25.2	37.0
Juniors	10	24.8	12.0	23.7	39.5
Sophomores	30	17.7	12.7	17.3	52.2
High-school	52	6.8	7.6	16.3	69.3

2. *Comparison of Sexes*, both in college and high-school students has indicated the superiority of men over women and of boys over girls. When both grades of students are combined the sex difference appears in the averages shown in Table II.

TABLE II.

DEPENDENCE OF RANGE OF INFORMATION ON SEX.

	Number.	*D*	*E*	*F*	*N*
Men	44	15.79	11.98	18.22	54.02
Women	57	12.21	9.42	17.19	61.17

3. The *definition-test* which is required serves to render the examinee more cautious in his marking: it also affords the examiner some index of the reliability of the marks obtained. Since, in the majority of the papers, both of high-school and of college students, there appeared one or more errors or inaccuracies in the ten definitions, it is evident that the results just figured must be discounted. For exact results, the examinee should be required, preferably orally, to define every word that he has marked *D*, and to explain or attempt to explain every word that he has marked *E* or *F*. In practice, especially when testing by the group method, such careful checking may prove too onerous: erroneous definitions may be neglected, or the quantitative data may be revised by discounting on the basis of the percentage of error revealed in the definitions. Or, again, the examiner may, after the test is concluded, define the 100-

words, and let each member of the group revise his own paper by placing a second series of marks *after* each word to indicate the manner in which he should have marked it. A comparison of the sums of the *D*'s, *E*'s, *F*'s and *N*'s of the first and of the second series will then show approximately the extent and nature of the error due to ignorance or misunderstanding of the real meanings. In general, the sum for *D* and for *E* will be reduced, but there are in most groups a few persons who are overcautious in their first marking.

The nature of these errors in definition is sufficiently indicated by the following illustrations: the assumed source of confusion is indicated by the terms in parentheses after the definitions:

ageratum — an aggregation of objects: the aggragate (sic) amount.

annealed — pressed or rolled out thin: molded together.

Anthony Wayne — a historic character who was hung in the cause of freedom for the blacks: a man who fought in the Revolution on the English side.

Babcock test — a device to ascertain whether or not cattle have tuberculosis.

Base-hit — when the ball is hit and strikes a base or is caught there: a ball batted over a base: when the striker bats the ball into the pitcher's hands.

Bokhara — name of a place in Austria.

cantilever — a bar with a hook in one end by which lumbermen roll logs (cant-hook).

catalepsy — a form of disorder of the nervous system which causes fits or convulsions (epilepsy). (Similar statements given by 15 persons.)

chamfer — the tree from which camphor gum is obtained: this is the simplified spelling of it (!). (The confusion with camphor was found in 4 papers.)

clearing-house — a sale that takes place when a store wishes to dispose of its stock (clearing sale): a place where clearing papers are given to vessels to enable them to leave the harbor (customs house + clearing of vessels): picking up everything to move; taking everything out of the house: a place used by express companies to sell uncalled-for goods: a house where goods are made ready to be delivered.

cotangent — name of one of two tangents drawn to a circle from the same point without the circle: one lying alongside of (contingent): straight line drawn to touch a circle at one point (tangent).

dibble — to get just a smattering of some subject, as to dibble in medicine or politics (dabble): to do with divided interest (dawdle).

dryad — a priest of early English times (druid).

entrée — first course at a banquet, usually soup: something in the way of food, new and out of season: when the waiter brings in a new course it is called an entree: French for 'to-day': French for 'between' (*entre*).

Eocene — the term applied to one of the early ages of civilization.

Euclid — a book written by Vergil (Æneid): name given to certain trees (eucalyptus): an ancient Egyptian who studied geometry: name of an avenue in Cleveland, Ohio.

f-64 — means the temperature is 64 degrees above zero, Fahrenheit.
f. o. b. — cash on delivery (c. o. d.) : forward on board.
hydraulic press — a kind of air-pump, rather complicated, operated by suction and pressure : a machine for washing dirt from gold or from steep slopes (hydraulic mining) : the force with which water flows upon or against a thing, as a paddle wheel.
impressionism — when a man imitates the looks or actions of another : the art of exciting an impression.
infusoria — a chemical herb (infusion ?).
kilogram — the greatest quantity in the metric system : French measure of distance (kilometer) : French unit of liquid measure : the weight of a cube of water whose dimensions are a kilometer.
Les Misérables — a French tragedy written about the last part of the 17th century by Racine, one of the famous French writers : French work written by George Sand, author of Le Diable.
linotype — the product of a certain method of making prints from photographs.
Millet — a blind poet (Milton).
natural selection — in nature each animal selects its mate, a device for building up a stronger race.
ohm — German word for uncle (*Oheim*).
Polonius — a prominent character in Julius Cæsar.
pomology — the study of the palm of the hand, used by fortune tellers (palmistry).
tort — French word for ugly (*tors ?*).
triple expansion — the expanding of anything three times its normal size.
Utopia — a silk factory.
way-bill — a bill that is being considered
Zionism — same as Dowieism.

RESISTANCE OF KEYS AS A FACTOR IN REACTION TIMES.[1]

BY J. V. BREITWIESER, M.A.,

Assistant in Psychology, 1906–8, Indiana University.

The results tabulated in this paper are from records made in the psychological laboratory at Indiana University during the academic year of 1907–8.

A few records with varying resistances of the reaction key were taken in the summer of 1906. These showed an increment in the reaction time as the resistance of the reacting key was increased. With a view to further investigate this problem special keys were made and a program for more experimental data was arranged.

As finally formulated, the purpose of the experiment was, (1) To find what difference in the reaction time would be caused by increasing the resistance of the reacting key from 50 to 500, 1,000 and 1,500 grams; (2) to find the variation in the number of taps that could be made in five seconds with the same series of resistances; (3) to ascertain the changes in the reaction time for an isolated movement, namely, that of the last joint of the index finger with a series of varying resistances, beginning at 1,000 grams, and increasing 500 each time until it went beyond the lifting ability of the muscles involved.

As will be shown more fully below, the measurements for the records in tables 1, 2 and 3 were made with keys which required a movement like that of the ordinary telegraph key, while for those in No. 4 an ergograph was used.

Apparatus.

The apparatus used for measuring the reaction time was an improved type of pendulum chronoscope with accessories as designed by Professor Bergström. The original form of the

[1] From the Psychological Laboratory of Indiana University, J. A. Bergström, director till August 1, 1908.

chronoscope is described in the PSYCHOLOGICAL REVIEW, Vol. VII., No. 5, and that of the improved type with accessories was reported at the meeting of the American Psychological Association at Chicago in 1907.

The key, which was of the break circuit type, was so arranged that the tension of a spring could be brought to bear upon it so that various pressures could be required for the breaking of the circuit.

The signal for the reaction was given by a spring sounder with a scale showing the height of the hammer stroke, thus making it possible to make the strength of stroke perfectly uniform.

To count the number of the tapping movements a recorder like that described by W. L. Bryan in the *American Journal of Psychology*, November, 1892, was employed. The time for the tapping experiments was kept by a metronome which was checked with a stop watch.

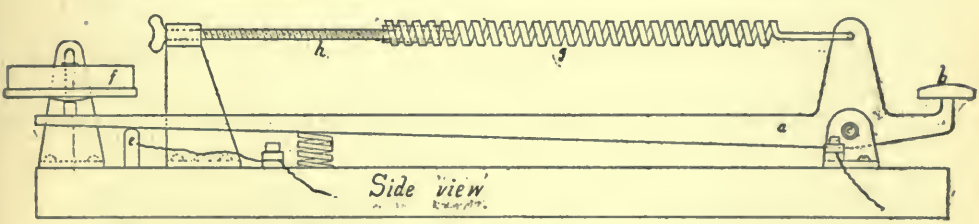

FIG. 1.

The key for recording the excess of pressure consisted of a long steel blade (*a*) (see Fig. 1) so mounted that the pressing button (*b*) was on the short arm of the lever, which was mounted on an axis at (*c*). Under the long arm of the lever was fastened a short spring which allowed only a small movement for a considerable increase in pressure. This spring did not exert any pressure when the lever was at rest, and served as a resistance for the excess pressure which was measured, as will be described below, by the height of stroke on a kymograph drum. A brass post (*e*) stood under the long end of the lever where the current was made or broken with the platinum contacts. It was the breaking of this current which stopped the chronoscope in the reaction experiments. The end of the lever was in connection with a tambour (*f*), a tube from which ran to the recording tambour writing on a kymograph drum which recorded the movement of the lever after the connection at (*e*) was broken. The varying resistances were introduced by the spring (*g*), the tension of which was varied by the screw (*h*).

The up stroke of the recording tambour therefore drew a line on the revolving drum which was proportional to the excess pressure exerted on the key. Various measured pressures were then put on the button (*b*) of the key and the height of the stroke of the recording tambour measured on a scale. With this scale the tambour strokes could be measured and their value recorded in terms of grams of pressure on the key.

The records given in the tables were taken throughout a period of 46 weeks. Usually a full set was taken at a sitting with rests between trials to avoid fatigue which seemed to have much influence especially with the heavier resistances. All records are recorded in thousandths of a second. A few records were discarded where it was known with certainty that the subject had been disturbed by outside influences. Subjects were kept as free as possible from distracting disturbances and were asked in every instance to give as nearly as possible a uniform concentration of effort on work at hand. An attempt was made to have all records made with the same kind of movement so as to have as nearly as possible the same muscles to deal with.

Tables of the results show in each instance the amount of resistance of the key and the corresponding record, also the mean variation. A 'ready' signal was given for every reaction and then the sounder was snapped and the time interval between snap of key and subject's pressing of key recorded. All reaction records are to auditory stimuli and of the motor type.

Program for Daily Experiment.

In the first series of experiments the resistance of the reaction key was set at 50, 500, 1,000, and 1,500 grams. A record of ten reactions was taken with each of the respective resistances and then the order of reacting so that ten records were taken again for each resistance, but beginning at 1,500 grams and going back to the 50 grams. This programme was followed by all the subjects. They were never allowed to quit without completing this program; thus there were never less than 80 reaction records taken at a sitting. The purpose of this program was to equalize the effect of practice and fatigue on the reactions for the respective resistances.

In the second series of experiments, an ergograph [1] was employed as a reacting key in connection with the pendulum chronoscope; and so arranged that whenever a certain amount was lifted on the ergograph it recorded the reaction time on the chronoscope in much the same manner as the keys used. This reacting movement had the advantage of isolation and uniformity. In this experiment the index fingers were used and only the end joint allowed to move, the finger being so clamped as to isolate this movement from the rest of the hand.

When the ergograph and the chronoscope were used together the following method was employed. Ten reaction records were taken for every resistance, beginning with 1,000 grams as the lightest and increasing the amount by 500 grams each time. This increase was continued up to a point where the subject was unable to lift the weight, then the reverse order was taken with ten readings for every 500-gram variation in weight running from heavy to light. The ergograph resistance is counted as if applied 31.8 mm. from the center of rotation of the joint.

The subjects who served in the experiments were chosen from the regular students working in the psychological laboratory. For the beginning experiments they were Mr. Smith and Mr. Durgee. The remaining two thirds of the school year Messrs. Miller and Harris acted as subjects. They will be referred to by their respective initials. The writer also acted as a subject in all experiments except where the ergograph was used as a reacting key; he will be designated by the letter B.

All subjects had done over twelve weeks of experimental work in psychology. H. and M. were especially strong men and had shown themselves to be very steady in experimental work. H. was about 27 years old, M. 25. None of the subjects had any preconceived notions as to what the results of the

[1] J. A. Bergström, 'A New Type of Ergograph with a Discussion of Ergograph Experimentation,' *Am. Jour. of Psych.*, Vol. 14, 1903.

experiments would be and their effort in all the reactions was to make them as quickly as possible.

From the experiments thus performed we have the following results:

The averages for 125 reactions each for 50, 500, 1,000, and 1,500 grams resistance were as follows, the reaction records being made by S., D. and B. on the first type of key used, which did not record the excess pressure. We also have the averages of thirty-five records of the number of taps made in five seconds on the same key. The resistance for the tapping records being the same series as in the reaction records. The records of this table are regarded as preliminary, and are therefore given merely as simple averages.

Table I.

Resistance of Key.	50 Grams.	500 Grams.	1,000 Grams.	1,500 Grams.
Av. reaction time for all records of S., D. and B.	112.8	127.5	141.8	149.7
Av. No. of taps in 5 sec. S.	57.8	56.5	54.8	52.3
Av. No. taps in 5 sec. B.	52.6	48.8	44.2	40.8

In Table II., in which the results are given more in detail, will be found averages for 180 reaction records for each resistance made by H., M. and B. on the second key, described above, which recorded the excess of pressure in the reaction. A few tapping records are also reported. The same resistances were used as in Table I.

Table II.

Resistance of Key.	50 Grams.		500 Grams.		1,000 Grams.		1,500 Grams.	
		M. V.		M. V.		M. V.		M. V
Average reaction time for H.	71.3	12.6	92.2	12.6	102.3	12.	108.6	13.
" " " " M.	98.1	11.8	116.5	8.4	130.5	11.5	139.3	10.2
" " " " B.	75.	12.	89.6	10.4	102.9	10.4	119.8	9.2
" " " all.	81.7		99.3		111.9		123.2	
Mean variation for all.	14.23		10.49		11.22		10.86	
Average No. of taps in 5 seconds for all.	49.6		49.2		44.8		41.8	

In Table III. will be found averages of the measurements of the excess pressure used. Excess pressure is that above what was necessary to break the circuit which stopped the chronoscope index. The results which are given in grams are as follows:

TABLE III.

Resistance of Key.	50 Grams.	500 Grams.	1,000 Grams.	1,500 Grams.
Average excess for H.	711.2	864.1	1,081.2	1,171.6
" " " M.	854.0	970.5	802.0	891.6
" " " B.	814.1	1,327.9	1,202.9	935.3
" of all readings	793.2	1,051.1	1,028.7	998.8

To measure the effect of practice on the excess pressure used, the average excess for each successive day was found, the subject going through the program as stated above. The results are given in Table IV.

TABLE IV.

Successive days.	B.	M.	H.
1	1187.5	1493.0	1869.37
2	880.0	1321.25	1333.67
3	1120.0	655.62	921.25
4	911.2	605.0	661.25
5	758.7	480.62	827.5
6	653.7	694.37	
7	884.3		

While there are two exceptions in B's averages and one each in M's and H's there appears to be a decrease in the amount of excess on successive days. Table V. gives the averages of reactions with the ergograph, used as the reacting key, in which the last joint of the index finger was used to produce the movement. This table shows the average of twenty

TABLE V.

Resistance in Grams. 31.8 mm. from Center of Rotation of Joint.	Reaction Time. M.	Reaction Time. H.
1,000	85.0	88.3
1,500	85.5	109.6
2,000	94.1	117.3
2,500	101.8	121.6
3,000	103.3	136.5
3,500	112.3	147.3
4,000	112.8	157.6
4,500	124.9	164.2
5,000	154.0	183.3
5,500		209.3
6,000		216.8

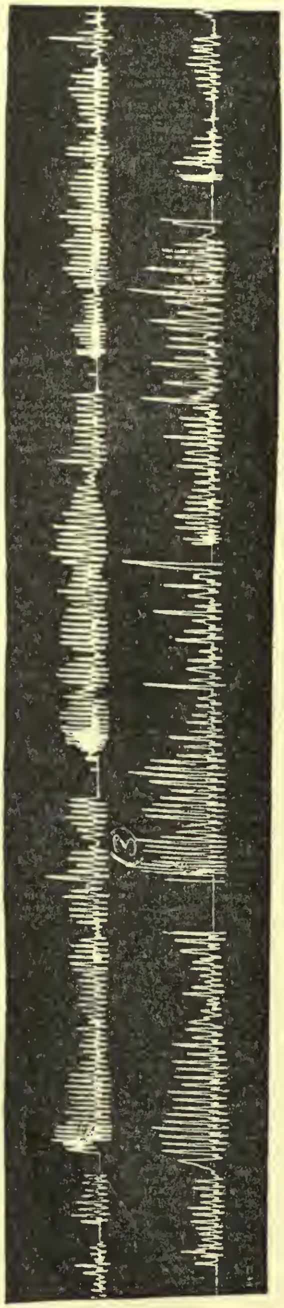

FIG. 2.

records for each weight. This series was the last taken, the subjects having thus had the practice of all the previous experiments.

Near the limit of the muscle ability to move the weight, the reaction time is evidently nearly double what it is at the beginning.

In Fig. 2 is given a part of the kymograph record of the excess pressure used in tapping. It shows that the excess, or surplus force, was expended in rhythmic or pulse-like beats for the curve runs in a wave form. One curve for five seconds began at an excess of five hundred grams which increased in about five taps to an excess of from 800 to 1,200 grams, then lowered to 50 grams and then rose again to the same excess. The number of taps between the greatest excesses varied from 10 to 20. The increase or decrease of a series of excess averages is usually regular, yet isolated high or low records also occur.

From the above tables of results we may draw the following conclusions:

1. Reaction time is lengthened or shortened, respectively, when the resistance of the reacting key is increased or decreased within the limits employed. The resistance of the reaction key should therefore be made definite, and should be stated in reaction experiments.

2. The rate of tapping is greates

with the minimal resistance employed, and decreases as the resistance is increased.

3. The excess force used in a reaction movement does not seem to vary in a marked or definite way with the resistance, in other words, it is largely independent of it.

4. The graphic records for the excess show a tendency towards rhythm, especially in the tapping records.

5. The excess diminishes (more or less regularly) with practice. (Table IV.)

A peculiar fact (perhaps worth noting) is that with the ordinary reaction key M.'s records were longer, while with the ergograph his records were shorter than H.'s. The evidence in detail will be found in Tables II. and V.

Some work on this and closely related problems has already been done. In 1892, M. Ch. Féré[1] arrived at the following conclusions:

For one and the same subject the reaction time is longer according as the weight to be lifted is heavier — provided that the weight is not known beforehand. When, however, the weight to be lifted is known to the subject beforehand the length of reaction time does not vary regularly with the weight, but with the capability of the subject to adapt his attention.

It was found however in our experiment, that even though the subject learned the resistances, he still had an increment in his reaction time that increased as the resistance was increased.

Helmholtz found that the total muscular force was not developed instantaneously. Haycraft[2] of the University of Wales, working upon this problem found that if a muscle be lightly loaded, the muscular force sufficient to raise the weight will be developed say in $\frac{1}{100}$ of a second; if it be loaded with a heavier weight, the greater muscular force requisite to raise it in this case will not be developed say for three or four hundredths of a second.

The amount of pressure was measured by Delabarre[3] in his experiment on the force and rapidity of reaction movement.

[1] *Comptes Rendus de la Société de Biologie*, 9th Series, Vol. IV., 1892, pp. 432–435.

[2] *Journal of Physiol.*, Vol. 23.

[3] PSYCHOL. REV., Vol. IV.

He made a study of temperament by taking the reaction time itself, the degree of pressure used by the subject, and the rapidity with which he contracted his reacting muscles. The degree of pressure in this case was measured by the height of a mercury column forced up by the reacting movement. In this experiment as well as the one reported by Féré, we have the force of inertia to overcome at the beginning of the reaction movement while in the experiment reported in this paper all resistances were made by varying tensions of springs, thus reducing the effect of inertia.

If we attempt an explanation of the effects observed one of the possible explanations is that the tip of the finger is capable of quite a good deal of compression and that perhaps some little time was consumed in bringing about this compression before the key was actually moved, which would make the heavy reactions longer than the light. To a slight extent this must be true, but not to a very great extent, especially in the ergograph reactions which followed the same law of increment, for here the finger was placed snugly in a thimble where the compression on all sides was great enough to move the resistances with very little compression.

It has been shown in physiological experiments that muscles have a certain amount of elasticity. Lombard in speaking of the effect of different weights on the gastrocnemius muscle of a frog says: "There can be no movement of the lever until the inertia of the weight has been overcome and the first effect of the contraction is to stretch the muscles, a part of the energy of contraction being changed to elastic force, which on the recoil assists in raising the weight. Thus the myogram may fail to reveal the instant that the contraction process starts.

"Inasmuch as tension increases the activity of muscle protoplasm it is probable that the presence of the weight really hastens the liberation of energy at the same time that it delays the recording of the contraction." This seems to be a very probable explanation for the increase of the reaction time with the increase of the resistance, and may be regarded as one of the chief factors in producing the results recorded above.

A further explanation may be that the nervous impulse itself is a thing of volume and requires time in formation and conduction, and the greater the strength of impulse required the longer the time required to get the requisite amount of stimulation to the nerve ending. The subjects felt the constantly increasing sense of effort the greater the resistance. This may however have been the feeling arising at least in part from the external movements resulting from the effects of the effort put forth. This also is a possible partial explanation of the results.

ANNOUNCEMENT.

The seventh International Congress for Psychology will be held in 1913, in the United States, the city to be determined later by the committee in charge. The following officers have been appointed: Honorary President, William James; President, J. Mark Baldwin; Vice-Presidents, E. B. Titchener, J. McK. Cattell; General Secretary, John B. Watson. A report of the sixth Congress, held in Geneva last month, will appear in an early number of the Bulletin.

N. S. VOL. XVI. No. 6. November, 1909.

THE PSYCHOLOGICAL REVIEW.

SOME EXPERIMENTS ON THE COLOR PERCEPTIONS OF AN INFANT AND THEIR INTERPRETATION.

BY HELEN THOMPSON WOOLLEY.

While observing the development of a normal healthy infant, I became convinced that her interest in colors, and probably her perception of them, developed during the latter part of the sixth month. The ability to grasp objects was not gained until the beginning of the fifth month. During the latter part of the fifth and early part of the sixth month, I made several rough attempts to discover whether she displayed any preference for bright colored objects, but could detect none. She had two celluloid rattles, alike except for color. One was a dull blue, and the other a brilliant rose pink. The brightness difference was slightly in favor of the blue. When the two rattles were held out for her to grasp, she took the easier one if there was any difference in the difficulty of obtaining them. If no such difference existed, her choice seemed to depend on chance. About the middle of the sixth month I thought I noticed a dawning preference for the bright pink rattle. By the end of the month, the pink rattle was so decidedly the favorite that she would reach for it when it was placed behind other toys, overlooking the blue one entirely. The color preference in this case seemed so marked that I was tempted to try a series of tests to corroborate my observation of color vision at so early a period.

The method to be used in making the tests was suggested by the behavior of the infant herself. She seemed to be passing through a stage of sense comparison. I frequently saw her looking back and forth from one to the other of two similar objects — two faces, two chandeliers, or what not. It occurred to me accordingly to try the method of paired comparison, rather than that used by Baldwin with an older child, that of recording the number of times the child reached, or failed to reach, for a colored paper. My preliminary tests convinced me that the method was applicable. When offered two pieces of colored paper of the same size and shape the child looked back and forth from one to the other, and then grasped one of them, often with a comical appearance of mature deliberation. After grasping the paper, she turned it back and forth, looking carefully at both sides.

In making the tests, the usual precautions for securing favorable conditions were observed. Tests were made only when the child was feeling well and rested; and when the light was good. The child was placed sitting in as easy a position as possible, with both arms free to move. The colors were presented in the form of discs of colored paper four and one half inches in diameter. In each test two discs laid side by side were moved up to the child directly in front of her. Care was taken to see that the two were equally illuminated. The tests were made in series of ten, or in a few cases twelve, choices, half in each of the two positions. The influence of position, and of the hand used, was thus eliminated from the results, though both factors were recorded, and will be reported upon in a separate communication. The background upon which the discs were laid was a medium gray in all cases except those in which one of the discs to be compared was gray. In those cases the background was white.

There are two sources of error to be considered. The first one is the imperfect eye-hand coördination of the child. Occasionally it was evident that while intent on one piece of paper, she grasped the other by mistake. When this occurred, she usually held the paper a short time, with her eyes still on the other one, and then dropped it for a second trial for the preferred color. Sometimes the feeling of the first disc in her hand diverted her attention to the new sensation, and made her forget her original intention. It was easy to tell from observing the child whether she had grasped the paper she intended to take or not. The mistakes were not of frequent occurrence, and were excluded from the results if there was the slightest ambiguity of interpretation. This source of error, therefore, seemed to me negligible. The other difficulty was somewhat more serious. It was due to the fact that the series of discs with which I began the tests was not of uniform texture. Some of them were the Hering tissue paper discs, some were the later washed papers of Hering, and some were from the Milton Bradley papers. The black was the black velvet paper of the Hering series. I soon saw that the rattle of the tissue paper, and the rough feeling of the black velvet paper were interesting to the child, and associations which began to influence choice were formed with the black velvet paper, and possibly with the tissue papers. As soon as I had noticed the fact, I made a complete series of discs from the Milton Bradley papers, and the greater part of the tests were made with these.[1]

The tables of results show the comparisons made. Each of the four colors blue, yellow, red and green was compared with each of the other three colors, and with black, white and medium gray. The tables of results fail to show the numbers demanded by uniform series of ten or twelve, because it often happened that some tests had to be discarded for various reasons, and in a few cases series were not completed because of the introduction of some disturbing element. The number of series for the various pairs is also quite uneven. The experimenter intended to complete the series which are brief, but the untimely failure of the method, about to be chronicled, made it impossible.

The experiments were begun when the child was just six months old, and were continued for a month. By the end of that time she seemed to have passed beyond the intense interest in mere sense comparison which had dominated at the start. The act of grasping, which at first had been a mere means for obtaining clearer sense impressions, had developed into the ability to manipulate objects, and that became her one desire. Instead of comparing the discs, and then carefully and laboriously grasping one or the other, she grabbed at them without

[1] I am indebted for all the discs to Professor James R. Angell and Professor John B. Watson, of the University of Chicago.

appearing to care which one she got, and often took one in each hand, a feat which had been very difficult in the early tests. When she got the discs, her whole desire was to crumple and shake them, not to look at them as at first. When this stage was reached, the tests were of course discontinued, but a sufficient mass of material had been collected to throw some light on the question of the existence of color vision at so early a period, and even on the order of preference of the four primary colors.

TABLE I.

RED.

yellow	blue	green	black	white	gray
15–17	14–4	27–5	11–4	5–2	17–3

YELLOW.

red	blue	green	black	white	gray
17–15	12–12	13–7	4–6	15–5	14–6

BLUE.

red	yellow	green	black	white	gray
4–14	12–12	19–12	6–3	8–2	7–3

GREEN.

red	yellow	blue	black	white	gray
5–27	7–13	12–19	7–4	6–4	10–10

Black, 17 ; white, 3.

In the tables of results, the first one of each pair of numbers indicates the number of choices of the color heading the table, and the second, of the color heading the section, when those two colors were compared. Thus in the table headed red, and he section headed blue, the first number, 14, shows that when red and blue were compared, red was chosen 14 times ; and the second number, 4, that under the same circumstances blue was chosen 4 times. The total number of comparisons made between the two colors is thus indicated by the sum of the two numbers in the section.

The conclusions which it seems to me can fairly be drawn from the tables of results are as follows. The child perceived red, blue and yellow as colors. It is uncertain whether or not she perceived green as a color. Her preference for red, and her indifference to green are striking. Blue and yellow occupy an intermediate place, with yellow somewhat in the lead, though no stress can be laid on the difference.

Let us consider first the outcome of the comparison of each of the colors with the colorless discs. Out of 42 choices in which red and a colorless disc were compared, 33 are for red and 9 for the colorless discs; or red 79 per cent. and colorless discs 21 per cent. There are 50 choices in which yellow and the colorless discs were compared. Of these 33 are for the yellow, and 17 for the colorless discs; or yellow 66 per cent., colorless discs 34 per cent. There are only 29 cases in which blue was compared with the black-white series. Of these 21 choices are for the blue and only 8 for the colorless discs, or blue 72 per cent., colorless discs 28 per cent. Green and the black-white series were compared 41 times. Of these 23 choices are for the green, and 18 for the colorless discs, or green 56 per cent., colorless discs 44 per cent.

If estimated by the preponderance of choice of colored over uncolored papers, the order is therefore red 79 per cent., blue 72 per cent., yellow 66 per cent., and green 56 per cent. The percentage of choices for green (56) is too small to serve as the basis of any conclusion. The others are large enough, in my estimation, to justify the inference of color vision.

In estimating the order of preference of the colors by means of the comparison of one color with the others, the irregularities to be expected in such a series of tests are disturbing, but the general trend of the tables shows differences in choice marked enough to be significant. For instance, in the direct comparison of red and yellow, the choices for yellow are slightly in excess, and yet the preference for red when compared with the other colors is so much greater than that for yellow, that it seems clear that red is the preferred color. When the preference for color is estimated by a comparison of the number of choices for each color with the number for the other three with which it was compared, the outcome is as follows. Red was compared with the other colors 83 times. Of these 56 choices are for the red and 26 for the other colors; or red 68 per cent., other colors 32 per cent. Yellow and the other colors were compared 76 times, of which 42 choices are for the yellow, and 34 for the other colors, or yellow 55 per cent., other colors 45 per cent. There were 73 comparisons of blue with the other colors, of which 35 are for the blue and 38 for the other colors, or blue 48 per cent., other colors 52 per cent. Green was compared with the other colors 83 times, of which 24 choices are for the green and 59 for the other colors; or green 29 per cent., other colors 71 per cent. In this case the order of preference is accordingly red, yellow, blue and green.

When the total series is summed up by estimating the percentage of choices for each of the colors from the total number of pairs in which each one appears, the result is as follows. Out of 124 pairs in which red is one member, red is chosen 89 times, or 72 per cent. Out of 126 pairs in which yellow is one member, yellow is chosen 75 times, or 60 per cent. Out of 102 choices in which blue is one member, blue is chosen 56 times, or 54 per cent.; and out of 124 choices in which green is a member, green is chosen 47 times, or 38 per cent.

The positions of blue and yellow when the preference for color is inferred from the comparison of the colors with the uncolored discs are not the same as when the basis of judgment is the comparison of each color with the other colors, or with all the other discs used. My reason for thinking the latter two formulations the more significant is that the number of comparisons of blue with the uncolored discs happens to be much smaller than the corresponding series for the other colors. Moreover, as I shall point out presently, the child had a preference for black, which, in so far as it tended to influence the choice of colors, would enhance the value of blue fictitiously.

The question of the influence of brightness differences on color choice must of course be considered. In the series of papers used, yellow is the brightest color, green next, red third, and blue darkest. It is obvious that the child's color preferences do not coincide with the brightness series, either ascending or descending. There is very little difference in her liking for yellow and for blue, the lightest and the darkest colors; and both hold a place intermediate between red and green, which are the intermediate members in the brightness scale. If the order of preference for the brightness is figured from the tables of results in the same way as that for the colors, it appears that black ranks first, gray next and white last. Out of 45 choices in which black was a member, it is preferred 17 times, or 38 per cent. Gray was taken 22 times out of 70 choices, or 31 per cent. White was chosen 13 times out of 47 presentations, or 28 per cent. The preference for black is further shown by a series of choices between black and white on a medium gray ground. Out of 20 presentations, black was chosen 17 times, or 85 per cent. The child's interest in black had been noticed before the experiments were begun. Black dresses, and still more black hats, aroused her enthusiasm, and she displayed a passion for black shoes. The only explanation of the fact I could make was to trace it back to an incident which had happened about six weeks before the beginning of the experiments. The child was sitting in my lap with her back toward me; and with the intention of keeping her amused, I was putting up first one foot and then the other rhythmically. She was very quiet, and I was not conscious that a deep impression was being made until a friend who was sitting in the room exclaimed that the infant was very much frightened. She was staring at the appearing and disappearing feet with every expression of intense fear on her face. I at once tried to soothe her and show her what the object was. She is not subject to fears, in fact that was the only instance of it during the first year. From that time on for several months she displayed an intense interest in black shoes, and secondarily in any black object. When placed on the floor at our feet with toys about her, she invariably neglected the toys to reach for our shoes. She cared nothing for white shoes. Whatever the explanation for her interest in black, the fact is undoubted. In so far as brightness differences influenced choice, they must have tended to enhance the value of the darker colors; but since black itself ranks considerably lower in the scale than any one of the colors, there is no reason to suppose that brightness was of great importance in determining color preferences.

As a sort of control test for the colors, I conducted during the same period a similar series of experiments with material toward which I thought the child would be indifferent. I chose for this purpose the large sized square and circle of the kindergarten gifts. The method and conditions of the experiment were the same as those of the color tests. Out of 70 choices, 34 were for the square and 36 for the circle; a result which is in marked contrast to the reactions to brightness and color differences, and indicates that mere form was in fact of no interest to the child.

The results obtained in this series of color tests are not in accord with the best accepted opinion up to the present time. Miss Shinn[1] in her excellent summary of the material at hand, comes to the conclusion that color vision probably does not develop until the last quarter of the first year, and that red is the only color perceived until the second year. She finds no evidence of the perception of either blue or green until the latter half of the second year, a time when most children learn color words.

If it were not for the series of tests which I have reported; if I had depended on mere observation, I should certainly be of the same opinion as Miss Shinn, except that I should be doubtful of the perception of even red during the first year and a half. In observing the child on whom these tests were made, I have up to the present time (16½ months) seen no further convincing evidence of color vision. I am confident that the series of experiments I have reported, if performed any time between seven and seventeen months, would have yielded negative, or at least ambiguous, results with regard to the perception of color. My infant has within the last two months displayed the usual interest in learning words, particularly the names of objects in which she is interested. She cannot herself say the words, but she shows her understanding of them plainly by her responses. She also understands a few qualitative words, though no effort to teach them has been made. Thinking that the time was ripe for learning the color names, I have again given her the colored discs used in the tests, and have attempted to teach the word

[1] *Notes on the Development of a Child*, 1907, II., p. 159 ff.

red, but so far without success. The amount of time and effort I have expended on it would be more than sufficient to teach the name of an object in which she was interested. In this point my experience coincides with that of other observers. Miss Shinn reports that all the children of whose vocabularies she has records learned not only other words, but other qualitative words before those for color.

This series of facts, if the facts can be regarded as established, needs further interpretation. Let me summarize. Color vision has been shown to exist in the case of at least one infant at six months. Neither in the case of this infant, nor in that of any other on record is there convincing evidence of color vision in the period between seven and sixteen to eighteen months, except a few cases of interest in red and more doubtfully yellow. Experiments conducted during this period have yielded ambiguous results.[1] The next conclusive evidence of color vision coincides with the period of learning color names, a stage which is reached by most children some time between the sixteenth month and two years. Words for color are always acquired later than some other descriptive adjectives.

There are three problems presented by this series of facts for which I would like to offer a tentative solution: first, why after becoming capable of color vision, the child should give so little evidence of it for so long a time; second, why the child's interest in color vision should display itself just when it does; and third, why other descriptive adjectives should be understood by the child before those for color.

All of these problems seem to me capable of explanation by the laws of interest and attention. In observing the infant on whom these tests were made, I have been very much impressed by the extent to which the young infant displays certain dominant stages of development of interest in his world, which determine not the possibilities of perception, but the actual discriminations made. My first tests were made at a time when I felt sure that the child was absorbed in sensory experiences, or, if the word sensory involves the psychological fallacy, in a

[1] Miss Shinn's criticisms of Professor Baldwin's tests (*loc. cit.*, p. 155) seem to me valid.

largely passive experiencing of the world. She was intent on listening to sounds, looking at objects, and comparing them visually, or alternately looking at and feeling of objects. In all these activities it is of course true that her attitude was not entirely passive. She was making accommodations to her world, and motor coördinations were being developed. But what seemed to be uppermost in her consciousness was not the movements she was making with eyes or hands, but the sense impressions she was receiving. The movements were a mere means for obtaining a clearer sense impression. In my experiments she first compared the two discs visually, and then grasped the one she wanted and brought it nearer her eyes for a more careful look. At about the end of the sixth month, when the act of grasping had become precise and easy, and had begun to develop into the ability to manipulate objects, the center of interest seemed to shift quite rapidly from the passive to the active aspect of experience. It was no longer a question of what sense impressions were being received from an object, but rather of what manipulations could be made with an object. She had discovered her capacity to act and had ceased to be a mere spectator. Only objects that could be handled held the child's attention for any length of time and she became endlessly eager to get hold of new objects. In my experiments, it was as though she ceased to care whether the paper was red or gray, but cared only that it was something she could crumple and tear. The stage of absorption in the process of manipulating objects has lasted unimpaired up to the present time. The child's life has been devoted to such activities as putting the cover on to a box and taking it off again, turning over the leaves of books; putting her toys into a basket and taking them out again, trying to put on her own clothes, and taking off the doll's shoes and stockings. When I give her colored discs to play with, her whole desire is to do something with them; to put them into a box and take them out again, or bring them to me and take them away again, or put her finger and mine through the hole in the disc. For all these purposes one disc is as good as another. When I demand that she shall bring me only the red one, or put only the red one into the box, I am interfering with the normal course of her activities, and she only gets angry and impatient. She does not understand, not because she is incapable of experiencing the colors, but because nothing in her activities at the time hangs upon their discrimination. The child, like the adult, attends to and discriminates only those aspects of experience which are of importance in carrying out his purposes.

But why, to take up my second point, at a somewhat later period does the child suddenly find that color is of importance to him? The child whom I have under observation has not yet reached that point, and in attempting an interpretation of the fact I feel on less certain ground. It seems to me, however, that it marks another shift of attention, conditioned by the acquisition of a degree of mastery over the process of manipulation. It is the general law of attention, that to the extent to which the process which has been absorbing it has become habitual, to that extent does attention shift to some new phase. As soon as one problem is solved, another arises. By the time a child is about eighteen months of age, he has become familiar with the common objects about him, and knows with some accuracy what can be done with them. The mere handling of objects is therefore no longer so absorbing as to occupy the field of attention exclusively. On the other hand it is a sufficiently well coördinated activity to serve as a means to some further end. The child's attention shifts to making further sensory discriminations because it is free to do so, and because he has become capable of a more differentiated response to his world, a response which demands more discrimination.

Miss Shinn's account of the development of her niece's interest in color offers an excellent illustration. She began to exercise herself in color discriminations by pulling the books out of the bookcase, and calling the color of each one as she did so. At an earlier period the mere act of pulling out the books had been in itself an absorbing activity. When it had, through practice, become a well coördinated and partly habitual act, her attention was free to pass on to new aspects of the experience, and she began to discriminate among the books. The act of pulling out books had became sufficiently habitual so that it could be used as a means of obtaining this book or that, and distinctions among the books became of importance accordingly. Though, as I have said, my own infant has not yet arrived at an interest in color, I can see signs that her attention has begun to shift from the process of manipulation. Until very recently, for instance, all she cared to do with a book was to open and close it, or turn over the leaves. She was impatient of any attempt to interfere with this activity by trying to call her attention to the pictures, though she had shown her ability to interpret pictures under favorable conditions. Now she voluntarily stops to look at the pictures.

The third point which I wished to discuss is the interpretation of the fact that most children learn other descriptive adjectives before those for color, and even the words dark or black before color words. To Miss Shinn the facts point toward a late development of color vision itself. To quote her argument: "Can we consider the wave of attention and discrimination as due only to the new power to *name* the perceptions? — a power that is always intensely interesting to the child, and that leads him often to discriminating observation of things he had scarcely noticed before? It would really beg the question to say so; why should the power to name the perceptions be delayed to this period, when other concepts, which seem to us much more abstract and less obvious, are rapidly coming to expression? . . . And I cannot see that there would have been any more advanced analysis in pointing at a red ribbon, or blot of red ink, and crying 'red,' than in pointing at a blot of black ink, or a coal smutch, and crying 'black.' Yet the *red* identification seemed impossible to my niece for two months after the *black* one was easy; to Mrs. Hall's boy for at least four months. Both these children, moreover, used several other descriptive adjectives before color names appeared; and while no other record corroborates mine as Mrs. Hall's does with regard to the early appearance of *black* (dark, however, often appears early), all the vocabularies in my hands show that other adjectives precede those of color, by a considerable interval — two or three months, even up to eight months, in all cases where I can fix the dates" (*loc. cit.*, p. 163).

It is doubtless true that pointing at a red object and crying 'red' is no more difficult a piece of analysis than pointing at a black one and crying 'black'; and yet there may be a reason why the wave of attention and discrimination should, as a matter of fact, take one direction rather than the other. That is to say, there may be circumstances in the conditions surrounding the average child which lead him to make certain kinds of discrimination earlier than others of apparently equal ease. In fact every child makes an enormous number of sensory discriminations whose temporal order must be determined by some other factor than that of difficulty. Now it seems to me that there are circumstances in the surroundings of the average normal child which should lead him to single out some other qualitative aspects of experience earlier than color. To apply the laws of attention once more, the child attends to those aspects of experience which are of importance in directing his activities. So long as the child's activities are still in the stage of mastering the simple, gross manipulation of the objects about him, color is a factor which is rarely of crucial importance. The quality hot, on the other hand, is sure to be early learned because of its obvious practical import. Hot objects are objects which must not be handled under penalty of immediate and certain pain. Hot food is food which must not be eaten. My own infant, though as I have said, she does not as yet understand color words, knows the meaning of the words 'another,' or 'the other' perfectly, concepts which at first sight seem more abstract than color. Her mastery of them has come about simply enough in the course of her activities. She is very fond of trying to put on her own shoes or my slippers. After she has succeeded with one, I say "Where is the other?" and she understands because the word denotes something that is of importance to her at the moment. Again she is putting her blocks into a basket, and I keep asking for another block to put in. The words now have a generalized meaning for her. But in trying to teach the color words, I have failed to create a situation where the color is of any inherent value in controlling her activities. What she can do with one disc, she can with another. I have tried to throw an artificial stress on the red by arbitrarily refusing to accept any other to put into the basket, or shut up in the box. In time I think even this arbitrary condition will bring about the desired discrimination, but the child knows as well as I do that my restriction is arbitrary, and that as far as her interests are concerned one disc has no advantage over the others. What I have called out so far is indignation at my arbitrary interference, not discrimination of the red. The child recognizes the spoken word perfectly well, but wishes to apply it to all the discs.

The same kind of difficulty has confronted me in trying to teach the words thumb and finger. She recognizes both words, but wishes to apply them both to all the appendages of the hand. There can be no doubt, of course, that she perceives the thumb as a separate object from the fingers, but there is no situation where it is important to her to single out that appendage and set it over against the others. The words square and circle, too, with as much effort, have had as little success as the color words.

The fact that most children learn the word dark early, and that some of them learn black before the colors seems to me capable of a similar pragmatic interpretation. The experience 'dark' is of obvious practical importance to the young child. A dark room is one in which all his activities are impeded, and, moreover, usually means going to bed. One would have to know the circumstances under which the word black was learned by the children to whom Miss Shinn refers to know whether the same principle applies, but a very probable way for the word to become of early importance is with reference to dirty, black hands which have to be washed, or a black dress which must be changed.

ON OCULAR NYSTAGMUS AND THE LOCALIZATION OF SENSORY DATA DURING DIZZINESS.

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If a person sitting with his eyes closed and his head in an upright position is slowly set in rotation about a vertical axis lying within or near his head, he meets with a considerable variety of sensory experience. All these sensory data contribute more or less toward the perception of motion, but in different ways. And these data fall into three general groups which it takes no extremely subtle introspection to distinguish.

1. Firstly, one distinguishes a group of sensations which proceed from extra-peripheral stimuli: currents of air are felt by their impact or their temperature, on any uncovered surfaces of skin; any source of light which can be dimly perceived through the eyelids or eye-bandages, will be seen intermittently as with each rotation the face is brought opposite the light; any source of sound will be heard alternately loud and faint as with each rotation an ear is twice presented to it. The air currents would of themselves give no clue to the movement of the person's own body, and they are ordinarily felt as somewhat irrelevant data, which hardly even tend to fuse with the other sensations of motion. The visual and the (doubly rapid) auditory intermittences become, with increasing speed of rotation, rhythms of which the spatial or temporal significations are subject to considerable individual differences. Thus for my own introspection the visual intermittence becomes a temporal rhythm, while the auditory sensations become a hoop of sound lying horizontally about the head as a center, and having two spots of maximum loudness opposite each other, and two of minimum loudness midway between them. I have sometimes, though seldom, had from the visual intermittence a comparable hoop of light. In any case the person is aware of a relative motion

between his body and the hoop of sound, or light, but which of these is at rest and which in rotation is so far ambiguous and is determined by the factors given in group 3. Both the visual and the auditory phenomena are readily isolated in introspection, and both are felt to be distinctly 'secondary criteria' of motion.

The three sorts of sensation so far mentioned may all be eliminated by fairly simple precautions (though they are also readily ignored by the observer), but another kind of sensation is neither so readily elimated, nor ignored, nor distinguished from the sensations of group 2. This kind comprises the tactual sensations mainly of the hands, back, thighs and soles of the feet, which vary with the inertia of the body and with the centrifugal moment induced by the rotation. These sensations, while less clearly a secondary criterion of motion, while more intimately associated, that is, with the sensations of group 2 and even of group 3, can still after some practice be distinguished as tactual sensations of varying strength.

2. Secondly, there are the sensations from proprio-ceptive organs (Sherrington, '06, p. 130) in joints, muscles and other tissues, which are stimulated (similarly to the last-named class of group 1) by the inertia of the trunk, limbs, internal organs, and even perhaps of the blood, and by their centrifugal moment. Sensations supposed to be stimulated in this way in the cerebellar mass or its sensitive coatings were originally adduced by Purkinje (Aubert, '88, S. 119–120) to explain dizziness; and in connection with movements of translation Delage ('86, p. 623) has referred to sensations seemingly "produced by a sort of internal tidal movement in which all the liquids and such solid organs as have any mobility, participate." While such factors are hypothetical, certain sensations from proprio-ceptive organs in joints, tendons and muscles undoubtedly play a part in the perception of the motion of one's own body (Schäfer, '87; Breuer, '90, S. 204; Mach, '73, S. 127; Abels, '06, S. 382). It is difficult even after practice to distinguish introspectively these sensations from those of group 3, except when, with a high speed of rotation, they become intense, whereupon they are readily distinguishable as secondary criteria, from the true

sensations of movement of group 3. How much these proprio-ceptive stimulations when not intense contribute to the perception of movement is hard to determine. Some writers, as Abels ('06 and '07), have wished to find in them the very basis of that perception, but this is an unwarrantable view for as Mach ('74, S. 130) has said: "One can scarcely explain feelings of motion in terms of skin or muscle sensations, in view of the feelings in the head, the enormous influence of the head position, and Flourens's experiment:" and indeed Mach might have added, in view of well-nigh every fact that experiment has yielded regarding the canals and sacs of the ear (cf. also Breuer, '07). Or, as (Crum) Brown has said ('95, p. 15), "A few experiments . . . will convince any one that we have here to do with a perfectly definite sense, and not with any vague sensations caused by the inertia of the soft parts of the body." Nevertheless the proprio-ceptive sensations are of interest here, and deserve more experimental notice than they have so far had.

3. Thirdly, there are the true sensations of motion which are in some way dependent on the semicircular canals, and probably the sacs, of the ear. These present a remarkable complication of phenomena, with which we shall have chiefly to deal in the present paper. And firstly introspectively. If a person sitting with his eyes closed and head upright is rotated about a vertical axis within or near his head, and if the speed of rotation increases continuously, the person feels his body to be rotating in the direction of the actual motion, and he also generally feels objects in the space around him (by as much as he is aware of them) to be moving more or less rapidly in the opposite direction. Two things, in short, the body and the objects around it, are felt to be in relative motion.

Problem I. — What organs yield the sensation of rotation?

I believe that it has not so far been noticed that unless the rotation is very rapid, the *direction of the attention* is able to determine which of these shall be felt to be the more involved in motion and which to be almost or quite at rest. If, namely, the person 'directs his attention' to his own person, *this* will seem to be in rapid motion while then the environment may seem to be quite at rest: but if the attention is directed to the environment sweeping by, if, that is, the sensations of groups 1 and 2 occupy the focus of attention, the objects about the person will seem to whirl rapidly to the rear while his own body will seem to be nearly or quite at rest. I find also in this case a faint suggestion in consciousness of a space far behind these dimly presented objects, which is, like my body, at rest. When the rotation is rapid, however, it is much more difficult and often impossible to achieve such a 'setting' of the attention. Also, as we shall later see, some individual differences are to be expected in this field. This influence of the direction of the attention on the perception of motion during rotation is doubtless analogous with the effect mentioned by Hering of the same factor, on the apparent position of objects.

Problem II. — What is it which is involved in what we introspectively call 'setting the attention,' which in the case of rotation can shift the appearance of motion from one object to another?

If the rotation is long protracted at an ever-increasing rate of speed, the person becomes sick. This phase is no part of dizziness proper, and does not here concern us. The remaining introspective phenomena which interest us occur when the speed of rotation is decreasing, and after the rotation has stopped. Now when the acceleration of motion changes from positive, or zero, to negative, the person feels without appreciable latency *both* himself and the objects around him to be rotating in the contrary direction. Here, too, the direction of the attention is of influence, but here the attention is to be directed *against* the illusory motion, stemming the tide as it were, and then the movement *both* of the body and of such sensations as one has of the outer objects (group 1) is alike diminished or annulled.

Problem III. — How does setting the attention against the illusory post-rotary movement reduce the apparent motion of both the body and the enviroment?

If now the eyes are opened, the apparent rotation persists (although cf. Báràny, '06, S. 223) save that the visual field is far more prominent than before; and it is still whirling contrariwise to the original rotation. As Mach ('73, S. 127) has described it: "As soon as the apparatus is slowed down one has the feeling of making a contrary rotation together with the box [in which one is enclosed]. If now the box is opened, entire visual space with its contents rotates. *It is as if all visible space were turning within a second space which one believes to be motionless, although it is identified by no visible cue.* One might almost believe that there exists behind visual space another space to which the visual is always referred."

Furthermore, during the decrease or immediately after the end of the rotation, objects presented to the tactual sense are felt to be in contrary rotation, similar to that of the visual field. This tactual dizziness is far less pronounced than the visual, but is sufficiently attested by Purkinje ('20), Mach ('00, S. 100), Wundt ('02, Bd. 2, S. 586) and others. There is likewise an auditory dizziness quite analogous to the preceding, whereby sources of (continuous) sound appear, after the rotation, to rotate contrariwise. As Münsterberg and Pierce ('94, p. 475) have described it: "If after the rotation, but while the eyes were still closed, the sound was given continuously for a time, it seemed to make the illusory movement too: it remained, that is, in constant orientation with the body." These visual and auditory phenomena experienced *after* rotation are not, of course, to be confused with the 'hoops' and other phenomena of group 1 experienced *during* rotation.

The foregoing phenomena immediately suggest the following problems:

Problem IV. — Why does the body appear to reverse its motion and to rotate contrariwise when the acceleration becomes negative and after the actual rotation has ceased?

Problem V. — Why after the rotation has ceased does the visual field continue to rotate contrariwise?

Problem VI. — Why do tactual and auditory impressions likewise continue to rotate contrariwise?

While the behavoir of the visual field has since the early seventies been referred in a general way to the ocular nystagmus which is normally induced by rotation, the connection between the two is still susceptible of elucidation: and I am not aware that any specific explanation has been offered for the tactual and auditory dizziness. It was with these problems in mind that I undertook in the fall of 1908 some experiments, on which the following discussion is partly based. It is also based in part on some experiments with dizziness previously described by me ('06), and on the literature of the subject.

I. What Organs Yield the Sensation of Rotation?

On this question a very large number of investigators have come, although in a very general way, to some appearance of agreement. In general nearly all assent to the hydrokinetic theory of Mach, Breuer and (Crum) Brown,[1] and I believe that in the main this theory is established beyond all question while some of its details will bear further examination. The experimental facts show that the semicircular canals of the ear are stimulated by circular and rotary motions, and that the utricle and saccule are (almost certainly) stimulated by motion of translation. (Some writers dispute the latter point, although, as it seems to me, on hardly sufficient grounds.) And from this the conclusion is commonly drawn, that these organs yield *directly* the sensations of rotation and of translation. The argument is —(1) sensations of motion depend on the position of the head; (2) the only receptor organs situated in the head, which are stimulated by motion, are the ampullæ and sacs of the ear; (3) therefore the sensations of motion are sensations from the ampullæ and sacs. Now clearly the two premises warrant the conclusion that — therefore the sensations of motion *result* from stimulation of the ampullæ and sacs. But these sensations need not result *directly*, as the first conclusion affirms. Nor have I so far discovered any experiments adduced explicitly to show that sensations of motion result directly rather than indirectly, from labyrinthine stimulation, although the former is commonly assumed. Bárány ('06, S. 265, S. 275–6) touches on this point and declares: "It is an error to say that excitations thereof [*i. e.*, of the canals and sacs] do not come to consciousness; *strong excitations of them do come to consciousness*, either as

[1] This theory has been admirably summarized by Nagel ('05, S. 790) and by Peters ('05).

such or in combination with [unter Mithilfe d.] the accompanying eye-movements" (eye-movement sensations?). But the ground of this affirmation is neither here nor elsewhere made clear. The only other reference to just this point, which I have found, is in Ewald ('92, S. 133): "There are not merely special physiological movements, which take place during and after rotation, but also these are accompanied by special sensations. But the relation existing between the two has never been made clear. It has been taken for granted that the abnormal movement ensues on an abnormal sensation, and is in a way its visible expression. But I do not believe that the relation is such a simple one." And in another place (S. 141) Ewald finds the movements following rotation to be 'reflex,' by which he clearly means that they are produced directly by the labyrinthine excitations *without* these latter having come to consciousness. There is, then, ground for debate in this matter.

On the other hand it has been generally granted that the nystagmic movements of the eyes are closely connected with the visual dizziness. Thus at the very outset Purkinje attributed visual dizziness to 'unconscious' eye-movements carried over [übertragen] to outer objects (Aubert, '88, S. 117). Delage, too, while deeming the labyrinth an organ of sensation in the strict sense, ascribes features of visual dizziness to movements of the eyes ('86, p. 610–11). Mach ('00, S. 98–100), Breuer ('98, S. 499) and Kreidl (*ibidem*) also attribute visual dizziness and illusions as to the vertical, to nystagmic and compensatory eye-movements; although these writers too believe in direct labyrinthine sensations. The accepted view should seem to be, then, although I do not know of an explicit statement to this effect, that movements of one's own body in rotation and translation are perceived by means of sensations coming directly from the ampullæ and sacs, while visual illusions of rotation and many of motion and position are due to reflex (and 'unconscious') eye-movements. Motion of translation of one's own body would be perceived by means of sensations from the sacs, of rotation by sensations from the semicircular canals.

Now there stands in somewhat surprising contrast to this view the experimental fact that both during and after rotation the sensation of rotation of one's own body is instantly inhibited if the ocular nystagmus is inhibited. This observation was first made by Bárány ('06, S. 224): "*The direction of one's line of regard is also of influence on the illusory sensation of rotation. If I have nystagmus horizontalis to the right* [*i. e.*, the rapid eye jerk toward the right and slow movement toward the left], *which I can inhibit by looking toward the left, and if with my eyes closed I do look toward the left, the sensation of apparent rotation stops at once—just as the apparent motion of outer objects had* [previously and for the same reason] *stopped. If I look again to the right, the illusory motion commences again. One can observe several such disappearances and reappearances of the sensation of rotation. A considerable number of physicians was able to observe this phenomenon, which I am the first to describe, and I have had the same reports from enquiries among patients.*"

I have previously reported ('06, p. 72) that the slow phase of the ocular nystagmus can "not voluntarily be inhibited; whereas the swift movement is so far voluntary that it can be *inhibited* at pleasure. It is possible, that is, to fix the eyes on that side of the field toward which the slow movements are directed, but not on any point at the other side of the field." And this inhibition of the nystagmus always inhibited visual dizziness; but I had not at that time noticed that it also inhibits the apparent rotation of one's own body. Now conflicting statements are to be found on this point, and the most emphatic are those of Mach, who states ('74, S. 123) "that a person can have very marked subjective phenomena of rotation with demonstrably fast fixation and no eye-movements. If on the inside of the paper box described in the previous communication [the observer was inside the box, and both rotated] there is fastened a black cross on a white ground, so that when one fixates the crossing every deviation of the line of regard is betrayed by an after-image, then one observes no such after-image when dizziness starts up. One can fixate and still feel dizzy. I have also convinced myself by direct observation of the eyes of a second observer, that the eyes can remain at rest when the experiment is carried out in the way I have described."

And twenty-six years later Mach again wrote ('00, S. 101–2) to the same effect, although here it is *visual* dizziness rather than the sensation of bodily rotation which is not inhibited by voluntary fixation of the eyes. For Mach both kinds of dizziness undoubtedly persist.

It chanced that a few weeks before learning about the observations of Bárány, two other observers, Dr. Tait and Mr. Ricker, and myself noticed (accidentally, for we were then interested in visual dizziness) that voluntarily inhibiting the nystagmus does away with the sense of bodily rotation not merely after the rotation has stopped, but *during the actual rotation itself*. We were all three able repeatedly to undergo a lively passive rotation (axis of rotation vertical, head erect and over axis, eyes closed) of one to two minutes without *at any time* having the sensation of bodily rotation.

Such an inhibition of nystagmus *throughout* the experiment can be accomplished only in this way: Before the chair is set in motion (by a second person) the observer directs his fixation as far as possible in the direction *contrary* to the coming rotation, and holds his eyes in this position as long as the acceleration remains positive. With the eyes closed, as here, this requires some practice and we found that it could be facilitated by securing a fairly durable after-image on the retinæ immediately before the experiment. When the acceleration has nearly reached zero, *i. e.*, when the speed has become nearly constant the observer relaxes his fixation and lets his eyes do as they will. They wander slowly toward the primary position of regard and remain there as long as the acceleration stays at zero. No motion of the body is felt if the voluntary control of the eyes is relinquished at the right moment. As soon, now, as the motion begins to be reduced (the acceleration is negative) the eyes wander involuntarily to the other side, *i. e.*, *with* the actual rotation, and here they must voluntarily again be fixed until several seconds after the rotation has actually ceased. The experiment requires that the observer shall not actively assist to rotate himself.

If this is successfully accomplished all sensations belonging to group 3 are inhibited leaving, however, those of groups 1 and 2. The 'hoops' (group 1) continue to rotate contrariwise as long as the actual movement lasts, but no longer (although they would continue contrariwise still longer if the nystagmus were not inhibited), and the centrifugal sensations are distinct in consciousness; and yet so insignificant are these secondary criteria of motion as compared with the primary sensations thereof, that the subject feels himself to be at rest in a somewhat remote though whirling entourage. This motion of surrounding objects is far from being adequately realized, as we ascertained occasionally by opening the eyes during rotation, whereupon the sudden realization of the rapid movement (backward and contrary to the actual movement) of visible objects came as a shock. But even this does not reinstate the sense of one's body being in motion provided that the nystagmus is still inhibited. It startles one sometimes into relaxing the hold on one's chair, so that we found it to be very disagreeable and somewhat risky to open the eyes while the nystagmus (and therewith the feeling of rotation) were being suppressed *and* the rate of rotation was rapid. The three observers above mentioned were well trained in the observation of dizziness; another subject, with less training and rather easily nauseated by dizziness, underwent the rotation without a sense of being himself in motion: and two women, quite untrained in the matter of dizziness, suppressed the post-rotary feelings of bodily rotation on the first trial, by inhibiting the post-rotary nystagmus; and on second trial succeeded in feeling no motion during as well as after the rotation. In all the experiments I observed nothing which would lead to any other conclusion than that voluntary inhibition of the ocular nystagmus *directly* inhibits the sensation of the rotation of one's own body.

These experiments go wholly to confirm the observation of Bárány which was given above. And we must now consider the precisely contradictory testimony of Mach. Since there is not the slightest ambiguity in the form of his statements, there remain three conceivable ways of reconciling them with the other observations above reported. It is possible that with Mach and his subjects the nystagmus was not really inhibited, for not all who try to inhibit it succeed. This is rendered plausible by the fact that Mach nowhere speaks of inhibiting the nystagmus by fixating toward the *side* contrary to motion, and in the absence of such a statement one must suppose that the inhibition of nystagmus was attempted by trying to hold the eyes voluntarily in the primary position, *i. e.*, straight forward (cf. the above quotation from Mach). Now I am personally quite unable to inhibit the nystagmus, either during or after rotation, in this way, nor have I seen another person who was able to do this; and it is clear from Bárány's observations ('06, S. 215–17) that such an attempted fixation straight ahead might actually augment rather than decrease the nystagmus. Nor can I attach much importance to the after-image test above quoted, with the black cross on a white ground, since there is no vision during the quick phase of the nystagmus, as we shall see later, and since the slow phase is too slow to leave a perceptible after-image streak unless the stimulus (here the 'white ground') is very intense.

Yet aside from this Mach says that he examined the eyes of another observer during the voluntary inhibition. And while this is very difficult, since Mach himself must also rotate, while also nystagmic movements sufficient to produce dizziness can be so minute as to need a reading telescope for their discovery (Bárány, '06, S. 214), I cannot think it probable that Mach would have convinced himself, as he says, that this observer had inhibited the nystagmus if such had not really been the case.

A second possibility would be that Mach and his subjects mistook centrifugal sensations (group 2) for the true movement sensations of group 3. (Sensations of group 1 give no feeling of *bodily* motion so long as the nystagmus is inhibited.) This would be very probable with observers of little experience, but it can hardly have happened with Mach; and furthermore he says that visual dizziness also continues after the inhibition of nystagmus, and centrifugal sensations could scarcely have been mistaken for visual dizziness.

The third alternative remains, that we have here a true case of individual difference. And one must be the more willing to admit this here since it is not more remarkable than other manifest discrepancies among the observations of careful experimenters in this same field. Thus Bárány, for instance, gets post-rotary dizziness of his body so long as his eyes are closed, but this is *supplanted* by visual dizziness when he opens his eyes: whereas Mach, and most other observers, feel with the eyes open both kinds of post-rotary dizziness *at once* (Bárány, '06, S. 223). Or again, in post-rotary visual dizziness Bárány sees the visual field oscillate from side to side in both directions ('06, S. 221); whereas Mach, Breuer, Delage and most other observers see it whirl contrariwise to the preceding actual rotation (Nagel, '05; Peters, '05); and Helmholtz ('67, S. 603; '96, S. 747) saw it whirl sometimes with and sometimes contrary to, the direction of the preceding rotation. Still more extraordinary are the different observations as to the localization of a visual after-image with the eyes closed, during voluntary and involuntary eye-movements. Indeed there are few branches of psychology where entirely credible observers more widely disagree regarding simple matters of fact. And I should designate this branch as the one comprising the following four things and their interrelations — motion, muscular contraction, the voluntary innervation to contraction, and the perception of movement.

Granted, then, the fact of unusual individual or typical differences, it remains to study the several types in and for themselves, in the anticipation that in the end some explanation will be found which will reconcile all discrepancies. I have not so far seen a subject who, like Mach, experiences bodily and visual dizziness after he has inhibited his ocular nystagmus, but I shall look for such persons, and meanwhile return to the discussion of such cases as Bárány and I have met. For some things are inevitably true of these subjects, whatsoever else may be true of the members of other types. Now we have seen that for the subjects who are at present in question, voluntary inhibition of the nystagmus inhibits the sensation of bodily rotation. Bárány ('06, S. 275–6) has sought to interpret this fact: "We have further seen that inhibition of the nystagmus eliminates the sensation of rotation. Since the voluntary direction of the regard can scarcely effect an inhibition of impulses coming over the vestibular nerve, we seem bound to conclude that the nystagmus as such, the involuntary and unconscious movement of the eyes, is of influence on the production of the rotary sensation; perhaps, indeed, that it is the nystagmus center in which the abovementioned integration [Verarbeitung] of the vestibular impulses and the excitations occasioned by changes of the head position, takes place. I perhaps went too far in an earlier paper, where I said that inhibition of the rotary sensation through inhibition of the nystagmus, proves that the nystagmus and not the vestibular impulses cause the rotary sensation. *It is sufficient to assume that for the production of rotary sensation such impulses are necessary as, owing to the nystagmus, are delivered to the center involved, and that these form so large a component in the integration of the subcortical impulses [?] that the inhibition of them suffices to prevent the sensation of rotation from being produced. Nothing but a case of total, bilateral, oculomotor paralysis of central origin could really decide the point: in such a case there ought to be no sensation of rotation.*" I quote this passage in full (with italics as in the original) because it bears so explicity on our theme. I understand Bárány's conception to be that afferent vestibular impulses and afferent impulses from eye-movements are combined in a subcortical center, from which they emerge in consciousness *as* the sensation of rotation: and that the latter components (which would ordinarily be called 'sensations of eye-movement') at least are indispensable to the production of rotary sensations. Presumably he would hold the vestibular impulses to be indispensable as well.

But there are alternative possibilities. Is it true, as he declares, that "the voluntary direction of the regard can scarcely effect an inhibition of impulses coming over the vestibular nerve"? The voluntary direction of the regard certainly inhibits whatever impulses those are which produce the *rapid* phase of the nystagmic movement, and I see nothing to warrant a statement on one side or the other as to the relation between the vestibular and the voluntary impulses. It *might* be that the vestibular sensations are the sensations of rotation, but that these are inhibited when the rapid eye jerks are inhibited. And yet on the one hand professed ignorance is better than so farfetched and mysterious an assumption as this latter; while on the other hand, regarding the former assumption, we must remember the many cases in which one's body is felt to move, with sensations distinctly like those of group 3, in which neither the semicircular canals nor the sacs can be supposed to be stimulated. A person who stands on a bridge and watches the water flow beneath, from time to time feels himself moving contrary to the flow of the water (Mach, '00, S. 104). Helmholtz ('96, S. 763–4) mentions that when the dome of an astronomical observatory is turned about, a person standing beneath it is apt to feel the floor and himself rotating contrariwise. And there are many other such illusions of bodily translation or rotation, not distinguishable from the sensations of group 3, in all of which the stimulus is purely visual and *there are no* afferent vestibular impulses. These considerations, I believe, quite shut out the vestibular impulses from being essential to the sensation of motion of one's own body.

We have next to examine Bárány's second and indispensable component — the afferent impulses occasioned by the nystagmic movements. Before we can suppose such impulses to be essential, or even in any wise contributory to the perception of motion, we must answer satisfactorily the arguments so cogently, and one might almost say savagely, stated by Hering ('61, S. 30–32): They are, he says, "proof enough that only the displacement of retinal images, and not sensations of tensions in the muscles, acquaint me with changes in the position of my eyes." In short, Hering allows no share at all to eye-muscle sensations in the perception of eye-movement; and he is disinclined to allow even their existence. Is it, then, conceivable that they afford sensations of movements of the whole body? We must also bear in mind that the famous discussion between Plateau, Oppel, Helmholtz, Dvořák and others did not confirm the belief in the existence of eye-muscle sensations. We have also had recently, from Dodge ('04 and '07), Judd ('05) and other investigators in the Yale Laboratory, fresh evidence that sensations of eye-movement play little or no part in the perception of space. Personally I am unable at present to dispense with 'eye-movement sensations' as a part of my psychological furniture, and yet in the present case I must admit that several facts seem to exclude them from assuming any importance. It is, apparently, the efferent motor impulses to the eyes rather than afferent impulses yielded by eye-movements that have taken place, that most closely parallel, or as some persons might say, *are*, the sensations of bodily movement.

These facts are, firstly, that of the two phases exhibited by the nystagmus, the rapid and the slow, one phase but not the other seems to cause sensations of bodily movement. This is the rapid phase. Under positive acceleration the rapid eye-jerk is *with* the rotation, and in this same direction the body is *felt* to turn; and as soon as the acceleration becomes negative the rapid jerk takes place contrary to rotation, and the body is felt to reverse its motion although it is actually rotating in the same direction. At high but *uniform* speed, of course, there is no nystagmus and likewise no sensation of one's body being in motion. As Bárány has said ('06, S. 225), "*the apparent rotation of one's own body is always in the same direction as the rapid nystagmic movement.*" Now on the sensory side there is nothing, so far as we know at present, to distinguish these movements so sharply from each other; for of course the circumstance that one is fast and the other slow would not account for one of them being 'sensed' and the other not. But on the efferent side there is a prime distinction — *the motor impulses to the rapid movement can be voluntarily inhibited*, while those to the slow cannot be checked (Holt, '06, p. 72). The voluntary attempt to inhibit them, which has to be made by trying to fix the regard in a direction wholly or partly opposed to the direction of the slow movement, results only in *increasing* the nystagmus (Bárány, '06, S. 215–17). (This is why Mach is trying to fixate a point straight in front, is unusually fortunate if he succeeded in inhibiting the nystagmus.) Now certainly the rapid movement cannot be called a voluntary movement, since the whole nystagmus arises involuntarily; but since the rapid phase is amenable to voluntary inhibition, it may properly be called semi-voluntary. Now this circumstance that the rapid phase which alone counts toward the sensation of bodily movement is more nearly related to voluntary effort than the other phase, is directly in line with those facts already referred to, which Hering so emphasized ('61, S. 30). "A position of the eyes which I have not voluntarily induced, which therefore I did not already know pretty exactly before it took place, and even more a movement which takes place without my special intention — I am totally unable to estimate. . . . [S. 31] If I have voluntarily brought about a position of the eyes, then of course I know beforehand the direction and approximate extent of movement [involved], since otherwise I should not have been able to will just this movement; and both the direction and approximately the amount of force which is necessary for a given movement, are decided [bestimmt] by the will." This is coming, perhaps, very close to 'innervation feelings.' The empirical data to which Hering refers, may be summarized as the general lack of relation between the position of the eyes and the subjective localization of optical impressions. The eye-movement sensations, supposing them to exist, in many cases so inadequately register the eye-movements that a false localization is assigned to the objective sources of visual data. An example will sufficiently illustrate this general phenomenon.

In a previous paper ('06, p. 72) I observed, "it is well known that after-images move with every involuntary eye-movement." Here I relied chiefly on the introspection during dizziness (with eyelids closed) of myself and four other subjects (*ibid.*, p. 70–1), on some observations of my own on the vision of after-images during 'pursuit movements' (Dodge, '03), and I *think* on some printed statements which, however, I can no longer identify. My own observations were, and on retrial still are, unequivocal. But I have since discovered that other observers equally 'well know' that for them after-images do *not* move with involuntary eye-movements: on this point both Hering ('61, S. 30–31) and Bárány ('06, S. 221–2) are perfectly explicit, and several other authors imply the same view. On the other hand Hering's account distinctly implies that after-images *do* move with *voluntary* eye-movements even when the eyelids are closed. Bárány ('06, S. 222) gives this as Hering's view and confirms it himself (S. 221), Exner ('90, S. 50) and A. Nagel ('71, S. 256) affirm the very same. Whereas

Purkinje (Aubert, '88, S. 118) recounts and apparently confirms an experiment of Charles Bell's in which "a blindingly bright image left on the eye after gazing on a shining object, always appears at rest during eye-movements executed in total darkness, and starts into motion only when the eyes are open and the [after-]image can be compared with external objects which are at rest." Now the observations in question are singularly easy to make, even for a novice, and I believe that such extraordinary discrepancies again rest on true differences between observers. (A colleague of long experience in the study of vision tells me that he gets both of the last-mentioned phenomena, but more commonly the latter of these.) Now these discrepancies, if we accept them, prove that there is no direct relation between eye-movements and the localization of after-images, and such a thing is scarcely possible if visual data in general are localized by means of eye-movement sensations. But the main differentia, of which we know, between the above cases lies in the manner of *innervation* — whether this is voluntary, semi-voluntary, or involuntary: the first alone affecting the localization of after-images for Hering and Bárány. And with this view Mach ('00, S. 93–105) also concurs. It is true that this does not remove all disagreement, for I find on myself and four subjects, that localization of after-images shifts with both phases (semi-voluntary and voluntary) of labyrinthine nystagmus, while Hering and Bárány deny this; and Purkinje and Bell find that even voluntary innervation does not affect the localization of after-images. Yet it is, to my mind, simpler to suppose that these divergencies rest on peculiarities of innervation than of eye-movement sensation. And in this Bárány, even more emphatically Hering, and also Mach would agree.

One can of course assume the inhibition, under given circumstances, of the supposed eye-movement sensations and hence their failure to govern localization; but there is nothing to inhibit them in our examples above save the several modes of innervation: so that again the explanation of the phenomenon would lie in peculiarities of innervation. Furthermore the observation of Mach, already described, that the feeling of both bodily and visual dizziness (rotary localization) *can* persist when there are no nystagmic movements could not be explained in terms of sensation of these non-occurring movements. And if recourse is then had to reproduced sensations of eye-movement, it again appears that only the labyrinthine or voluntary innervations could be effecting such reproduction. The fact that Lotze, Münsterberg and others have declared 'innervation feelings' *to be* reproduced sensations of movement does not affect the present case for the issue here is between afferent and efferent process,— whether incoming or outgoing impulses are more nearly parallel to feelings of motion. To resort, then, to *re*produced kinæsthetic sensations is to yield the point at once and to grant that it is outgoing impulses and not impulses coming in from the eye-movements that govern the consciousness of movement. And while in Mach's experiment his feeling of bodily and visual dizziness cannot have come from sensations of eye-movement, since he says that the eyes were not moving, it may well have come from innervations to eye-movement, innervations which became inhibited at some level lower than their point of origin. It is worth noting that this view, and so far as I know no other view, accounts for the familiar pathological cases in which the innervation to contract a muscle which is paralyzed, produces the feeling of *the intended* movement although the muscle is not actually contracted by the innervation.

If now visual localization is not explicable in terms of eye-movement sensations, the localization of one's body is of course even less so. And thus this latter depends neither on afferent impulses from the eye-balls nor, as we saw before, on afferent impulses from the labyrinth; and yet the voluntary inhibition of eye nystagmus *inhibits* the feeling of movement (changing localization) of one's body. Only one conclusion remains — the voluntary innervation to inhibit the nystagmus, which is really directed as we have seen to inhibiting the *rapid phase* of the nystagmus, suppresses the feeling of bodily motion by inhibiting the (semi-voluntary) *innervation* of that rapid phase. And it will be recalled that the feeling of motion of one's own body is always in the same direction as the rapid phase. Hence it is the semi-voluntary innervation to the rapid nystagmic phase which is the process most closely parallel to the feeling of bodily rotation. Our first problem was: "*What organs yield the sensation of rotation?*" *And the answer would be that this is not a sensation in the ordinary meaning of that word, but that the process most nearly parallel to the feeling of rotation is one kind of innervation process.* And I believe that this proposition applies to more than rotation, applies at least to all feelings of motion of one's body that are supposed to be given by the semicircular canals.

This result is nearly in line with a view long supported by Mach ('00, S. 95), who says: "The will to execute movements of regard [Blickbewegungen] or the innervation (?) to these, is the very sensation of space itself." The question-mark after the word 'innervation' is presumably out of deference to those who oppose 'innervation feelings.' I should not care to say innervations *are* sensations of space nor, for reasons too general to be discussed here, that innervations *are* the feelings of bodily movement. The phrasing as italicized above is, I think, a somewhat securer statement. But it is clear that our argument, based in several places on observations at variance with those of Mach, comes out to a position not so far removed from his.

It is true that I have not explained all the conflicting observations given above, nor have I, by-the-way, begun to exhaust the anomalies that stand on record in this field. But I believe that the cases which we have considered, if they are facts and not errors of observation on the part of one person or another, necessitate the conclusion to which we have arrived. And whatsoever conclusion other facts lead to, it will not be contradictory to this of ours. We have found empirical evidence of three grades of innervation — voluntary, semi-voluntary, and involuntary — and that these exert a different influence over the inhibition of the feeling of movement. It is therefore probable that different grades of innervation are of different value in producing the feeling of movement. The three grades of innervation doubtless emerge at higher and lower neural levels; but the neural levels are many and hence the grades of innervation may be many. On this, I think, we may base a reasonable hope of explaining the so-far complicated and seemingly conflicting mass of observations.

The further problems raised in the first part of this paper must be discussed in a sequel, and it here remains to say only this: The conclusion that *innervations* of one kind or another are the process most nearly parallel to feelings of bodily motion, does not, of course, imply that such efferent impulses are created from nothing, as say on the 'psychic plane.' The nervous energy that constitutes these innervations is released by impulses coming more or less immediately from the periphery. For the physiological unit of the nervous system is the reflex arc. And the issue raised by our empirical data between afferent (from the eye-muscles or orbit) and efferent (to the same) is, I apprehend, more precisely stated as follows. Is the nervous process which runs parallel to the consciousness of the rotation or translation of one's body, one in which the afferent or sensory impulses come wholly or mainly from eye-muscles (or orbit), and diverge in the central nervous system, passing out as diffuse innervations to various and so far unidentified members; or is the process one in which the afferent impulses come from various and so far unidentified members, and converge in the central nervous system, passing out as a unified and definite innervation to eye-movement? The latter alternative is the conclusion that we have reached.

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MENTAL DIAGNOSIS BY THE ASSOCIATION REACTION METHOD.

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The object of this series of experiments was to verify the validity of the association reaction method in a number of dissimilar experiments conducted with normal subjects. The first two experiments were so planned that they could be carried out before a psychology class within an hour's time. The entire series was so arranged that we gradually restricted any advantage that the operator had and gave the subjects greater opportunity to conceal their relations to the experiments. This was most successfully achieved in the third experiment which we describe. A further object of the first two experiments was to learn what mental diagnosis a class would be led to make from observations during the progress of the experiment, the point of view being to estimate the feasibility of using this method in the presence of a jury. We also wished to discover, if possible, whether knowledge on the part of the subject of the methods used would invalidate the results, as has been asserted.

The instruments used were a chronoscope, measuring time in one-hundredths of a second, a lip-key used by the operator to start a pendulum on the chronoscope at the time of giving the stimulus word, and a mouthpiece into which the subject spoke the associated words, thereby releasing a circuit-breaker, which records on the chronoscope the time elapsing between the giving of the stimulus word and the reaction word.

Experiment I.

Mr. H. and Mr. O. acted as subjects, Mr. H. being a junior and Mr. O. a graduate student. In one corner of a dark-room under a gas-jet stood a child's desk on which were placed Joseph Jastrow's book *The Subconscious*, a bottle of red ink, pen and paper, and a child's Christmas story book. A hammer was tied to the gas fixture. On a table in another corner of the room were an old dusty crushed derby, and a few other things the nature of which will appear in the progress of the experiment. Mr. S. assisted by handing the instructions to Mr. H. and Mr. O. and seeing that they were fulfilled. One of the men was not to enter the room, the other was to follow out the typewritten directions, which were as follows:

"1. Sit down at the desk. Observe that it is a child's desk. Take up the child's book which is at your right on the desk and read it. Is the poem familiar to you? Are you able to get a mental image of Santa Claus? (See front cover.)"

"2. Pen, paper and ink are on the desk in front of you. Write the first page of 'The Night Before Christmas,' using the material at hand."

"3. Pick up the book at your left and take note of the following: (1) Its author. (2) The title. Write the name of the author and the title three times with the red ink."

"4. Untie the hammer which is tied to the gas fixture, and knock on the desk with it three times moderately hard. Then await further orders." [1]

"5. The building in which you now are is fifty years old. Owing to its age and the fact that it is constructed entirely of wood, there is the greatest danger of fire at any time. In fact the danger is considered so great that the rooms on this fourth story have been abandoned. The building and its contents are heavily insured. The university does not believe it wise to assume any risk. A fire might break out just now. There is a carpenter shop in the basement with wood and oil. What would you do in event of fire? Spend the next few minutes until the clock strikes in devising a plan of escape, if access to the stairs were shut off because of fire. When the bell strikes turn to No. 6."

"6. Back of you there is a door partly open and near you there is a rope. This rope is long enough to reach the fifty feet to the ground. Take up this rope and follow it hand over hand for fifty feet from the door, [2] then come back the same way and leave the room."

[1] At this juncture the assistant came into the room with an interval clock set to ring in seven minutes and told him to proceed with No. 5.

[2] This rope led fifty feet back into a dark attic.

"Please do not talk with anybody about the room or what you did in it."

These instructions having been carried out, the assistant brought one of the men into the lecture room before the class and we proceeded to take the associated reactions, the results being as follows:

TABLE I.

RESULTS OF EXPERIMENT I.

No. of Word.	Stimulus Word.	Results for Mr. H—.		Results for Mr. O—.	
		Association Word.	Association Time.	Association Word.	Association Time.
1	Tree.	Tree.	1.36		
2	Wood.	Log.	1.50	Tree.	.87
3	Sun.	Boy.	1.30		
4	Sky.	Air.	1.82	Story.	
5	Father.			Mother.	1.80
6	To speak.			Radiator.	3.25
7	Grass.	Lawn.	.90	Brown.	.90
8	Sweet.	Sugar.	.97	Bitter.	1.30
9	Ten.	Twenty.	1.45	Men.	1.67
10	Blue.	Sky.	1.20	Seat.	1.70
11	Chair.	Desk.	1.37		
12	Girl.	Boy.	1.10	Boy.	1.60
13	Supply.	Breakfast.	1.47	Tonight.	2.40
14	To fly.	Bird.	1.07	Bird.	1.17
15*	Christmas.	New Year.	1.70	Snow.	1.67
16*	Interval.	Minute.	1.67	One Minute.	3.25
17*	Skull.	Skeleton.	1.52	Neandertal.	3.70
18*	Sleepy.	Man.	1.00	Boy.	2.40
19*	Joseph.	Boy.	1.00	Egypt.	3.15
20*	Ink.	Black.	1.40	Black.	1.05
21*	Creature.	Man.	1.80	Mouse.	1.70
22*	Black.	Ink.	.85	Night.	2.85
23*	Hammer.	Anvil.	1.60	Nail.	2.75
24*	Old College.	Building.	1.40		
25*	Fourth Story.	Building.	2.65	Fifth.	2.55
26*	Rope.			Ground.	4.00
27*	Fire.	Sherman Ave.	2.05	Engine.	1.80
28*	Danger.	Fire.	1.80	Escape.	3.70
29*	Minutes.	Hours.	1.55	Fire.	2.50
30*	Night.	Day.	.90	Dark.	3.07
31*	Before.	After.	1.27	Fisk.	3.60
32*	To write.	Speak.	3.37		
33*	Conceal.	Plain.	1.23	Knowledge.	2.30
34*	Hat.	Coat.	1.03	Black.	2.40
35*	Dread.	Fear.	1.55		
36*	Conceal.			Knowledge.	2.10
37*	Subconscious.	Conscious.	1.10	Stale.	1.80
38*	Ink.	Black.	1.30	Black.	1.40

In this table the significant stimulus words are marked with an asterisk, fourteen words having been first introduced to gain the normal association time. Mr. H. was first tested and we soon came to the conclusion that he had not been in the room; this was further substantiated when Mr. O. was brought before the class and the length of his association time and the associations to the significant words were noted. At the conclusion of the experiment, before announcing the results to the class, every member was requested to indicate on a slip of paper which one of the two men had been in the room. The entire class, thirty-eight in all, was unanimous in its judgment that Mr. O. had been in the room. Only one of the thirty-eight thought that Mr. H. had also been in the room. We believe that the class drew its conclusions partly from the appearance of emotion in Mr. O., as well as from the manifestly delayed association time, when some of the significant words were given and by the nature of the associations themselves.

Subjoined will be found a table of the quantitative results of the experiments.

Table II.

Quantitative Results of Experiment I.

	Results for Mr. H—.		Results for Mr. O—.	
	Irrelevant Words.	Significant Words.	Irrelevant Words.	Significant Words.
Mean.	1.25	1.62	1.77	2.54
Mean Variation.	.29	.26	.56	.63
Difference in Means.	+ .37		+ .77	
Extremes: Long.	.85	.90	.87	1.05
Extremes: Short.	1.82	3.37	3.25	4.00
Maximum Range.	2.52		3.13	

The mean variation of Mr. H. on the irrelevant words was more than on the significant. While in the case of Mr. O. the mean variation for the irrelevant words was less than for the significant words, this being precisely what we expected.

We may sum up the grounds for our conclusion that Mr. O. had been in the room and had attempted to conceal as follows: (1) The association time for Mr. H. was not sufficiently lengthened in the case of the significant words to indicate a voluntary change of association. Mr. O.'s associations were manifestly delayed in the case of some of the significant words. Take, for instance, the two words *night* and *before*. These are from the poem, ''Twas the Night before Christmas.' *Night* suggested the word *dark*, which was long in coming, possibly on account of the highly suggestible words *fire*, *danger* and *minute* just preceding, and when the stimulus word *before*, which naturally suggested *Christmas*, was given, Mr. O. changed this to *fisk*, thereby lengthening the time to 3.60 seconds. The same situation becomes apparent in the case of other significant words which can easily be selected by the reader. (2) Mr. O. gave a number of very significant associations in response to certain stimulus words. This, taken in connection with the highly delayed reactions, forms another basis for our conclusions. The word *rope* brought up the association *ground* in 4.00 seconds. An examination of the typewritten directions of this experiment under Number 6 where the sentence occurs, "This rope is long enough to reach 50 feet to the ground," will show just why this word was selected. (3) As we have already shown, Mr. O.'s mean variation for the significant words was considerably higher than for the irrelevant words, while the reverse was the case with Mr. H.

Experiment II.

In Experiment two, our object was three-fold: First, to discover which one of three subjects had performed a series of acts and was trying to conceal his relation to them; second, which one had performed the acts, and did not try to conceal; and third, which one knew nothing about them. Mr. J., Mr. S. and Mr. U. acted as subjects. We used the same room and the same directions as in Experiment Number I. Mr. W. was assistant and handed the subjects who were entirely ignorant of the nature of the experiment, the typewritten directions. When the subjects had fulfilled their parts, they returned to the lecture room one at a time and their associations were taken as is indicated in the following table:

The reader will readily see that this experiment was much more involved than the previous one, thereby increasing the difficulty of accurate diagnosis. The significant words caused a manifest delay in the association reaction time of Mr. S. This was not the case with Mr. U., while the slight difference

Table III.

Results of Experiment II.

No. of Word.	Stimulus Word.	Results for Mr. J.		Results for Mr. S.		Results for Mr. U.	
		Association Word.	Association Time.	Association Word.	Association Time.	Association Word.	Association Time.
1	Father.	Mother.	.92	Mother.	1.51	Papa.	1.02
2	To speak	To say.	1.42	To talk.		To say.	1.05
3	Grass.	Dog.		Green.	2.25	Green.	1.60
4	Sweet.	Sour.	.85	Sugar.	1.61	Sour.	1.23
5	Ten.	Twelve.	1.32	Twenty.	1.80	Twenty.	.77
6	Blue.	Black.	.97	Green.	1.80	Green.	1.37
7	Chair.	Table.	.93	Black.	2.00	Arm chair.	2.95
8	Girl.	Boy.	.82	Boy.	1.70	Light.	1.16
9	Supper.	Dinner.	.81	Dinner.	1.81	Dinner.	.75
10	To fly.	To see.	1.02	Bind.	2.15	To sail.	.85
11*	Christmas.	Thanksgiving.	1.35	Christmas Tree.	2.00	New Year.	1.02
12*	Interval.	Space.	1.56	Time.	2.55	Between.	1.14
13*	Skull.	Head.	1.37	White.	2.00	Bone.	.97
14*	Sleepy.	Sink.	1.08	Bed.	2.10	Tired.	.90
15*	Joseph.	James.	1.28	Jacob.	2.45		
16*	Ink.	Black.	1.39	Black.		Ink.	1.45
17*	Creature.	Man.	1.70	Bug.	2.00	Thing.	1.10
18*	Black.	Blue.	1.06	White.	1.20	White.	1.12
19*	Hammer.	Nail.	1.65	Black.	1.70	Nail.	1.25
20*	Old College.	Y. M. C. A.	1.80	University Hall.	2.76	New College.	1.15
21*	Fourth Story.	Fourth Floor.	1.12	" "	3.80	Third Story.	1.11
22*	Rope.	String.	.97	White Cord Line.	1.17	Cord.	1.16
23*	Fire.	Water.	1.32	Match.	1.67	Burn.	1.10
24*	Danger.	Signal.	1.21	River.	2.37	Tired.	.80
25*	Minutes.	Seconds.	.88	Sixty.	1.19	Seconds.	1.37
26*	Night.	Day.	.84	Black.	1.13	Rain.	1.38
27*	Before.	After.	.78	After.	1.47	After.	1.14
28*	To write.	To sing.	.79	Paper.	2.00	To left.	.70
29*	Conceal.	Hide.	1.10	Weapon.	2.25	Hide.	1.80
30*	Hat.	Cage.	1.50	Black.	1.57	Cob-web.	.90
31*	Dread.	Hate.	.89	Fear.	2.25	Tired	.99
32*	Conceal.	Hid.	1.17	Weapon.	1.29	Hide.	.96
33*	Jastrow.	Anything.	1.75	James.	2.70	Jastrow.	2.00
34*	Subconscious.	Ill.		Psychology.	2.25	Untie.	2.25
35*	Ink.	Black.		Black.	1.04	Red.	

* The significant words are indicated by an asterisk.

in time of Mr. J. could easily be accounted for on account of his unfamiliarity with the strange words. Further the reaction time of Mr. S. is on the whole greater than for Mr. J. or Mr. U., and finally, the variability in the reaction time of the significant words is greater for Mr. S. than for the other two. If we examine the association words of Mr. U. we find two very significant associations. The word *hat* brought up the word *cobweb*. Why? Was it not because the old crushed hat in the room was dusty and covered with cobwebs? The word *ink* brought the response *red*, probably because the ink on the desk was red. When the word *Jastrow* was given to Mr. J. he gave the associated word *anything*, and the tone in which it was said indicated that he was unfamiliar with the word.

Before giving our decision to the class, each member was requested to write on a slip of paper what he believed to be the relation of each of the subjects to the experiment. Eight members were unanimous in their belief that Mr. U. had been in the room and was not trying to conceal it. Six thought Mr. S. had been in the room and was trying to conceal it, and six that Mr. J. had not been in the room.

Our conclusions were as follows: (1) Mr. J. had not been in the room; (2) Mr. S. had been in the room and tried hard to conceal it; and (3) Mr. U. performed the series of acts and did not try to conceal it. We were correct in our judgment. Below are given the quantitative results of Experiment II.; in this the mean variation in the significant words stands out very prominently in the case of Mr. S.

Table IV.

Quantitative Results of Experiment II.

	Results for Mr. J.		Results for Mr. S.		Results for Mr. U.	
	Irrelevant Words.	Significant Words.	Irrelevant Words.	Significant Words.	Irrelevant Words.	Significant Words.
Mean.	1.01	1.24	1.85	1.95	1.23	1.21
Mean Variation.	.16+	.26+	.19+	.47+	.41	.26+
Difference in Means.	+0.23		+0.10		+0.02	
Extremes { Short.	.81	.78	1.61	1.04	.75	.70
Extremes { Long.	1.42	1.80	2.25	3.80	2.95	2.25
Maximum Range.	1.02		2.76		2.25	

Experiment III.

Having been successful in the previous enumerated trials and others, we determined to further restrict any advantage which the experimenters might have over the subject. Two men, Mr. S. and Mr. O., both of whom had knowledge as to the nature of the association reaction method, were selected, and they were given the following options: (1) both might perform the series of acts according to instructions; (2) either one could perform them; (3) neither need necessarily perform them; (4) after having chosen what they would do they were allowed to conceal, or not to conceal, their individual relation to the experiment.

In a drawer of a table in the psychological laboratory, the following things were placed; three bottles of ink (carmine, green and violet), two pieces of glass (red and blue), a one-pound iron weight, a handkerchief scented with asafœtida, a copy of *The Psychology of Advertising*, by Walter D. Scott, and *The Native Tribes of Central Australia*, by Spencer and Gillen.

The written instructions were as follows:

"1. Take the book, *The Psychology of Advertising*. Who is the author? Turn to page 44. Read the advertisement on that page carefully."

"2. There are three bottles of ink in the drawer. Notice carefully the color. Are the bottles full or empty?"

"3. Take the book by Spencer and Gillen, *The Native Tribes of Central Australia*." This is a large book. How many pages has it? Turn to page 33 and notice the old man. Also turn to page 47.

Is not she a winsome lass? It is too bad that she has lost one tooth!"

Mr. S. was first examined and in order to increase the probability that our conclusions were correct the list of words was given a second time. Mr. O. was then examined in the same way. The results are indicated in Table V.

A comparison of Mr. O.'s figures in Table V. with those of Table I. shows that, while his figures are somewhat higher than those of some other normal subjects, yet the difference between the means in Experiment III. is much less than in the first experiment, in which he tried to conceal. Moreover, the mean variation both for the irrelevant and significant words is in general much lower than in the previous experiment. It will also be seen by a comparison of the quantitative results of these experiments that his maximum range is much higher in the first than in the last. The variability, however, of his reactions was not a sufficient basis for concluding that he was trying to con-

TABLE V.

RESULTS OF EXPERIMENT III.

No. of Word.	Stimulus Word.	Results for Mr. S.				Results for Mr. O.			
		Association Word (1st Time).	Ass'n Time (1st).	Association Word (2d Time).	Ass'n Time (2d).	Association Word (1st Time).	Ass'n Time (1st).	Association Word (2d Time).	Ass'n Time (2d).
1	House.	You.	.65	Dinner.	.50	Barn.	1.60	Barn.	3.50
2	*Man.	Old.	.99	Five.	.45	Woman.	1.60	Woman.	1.70
3	Boy.	Bugs.	.70	You.	.85	Girl.	1.40	Girl.	1.32
4	Weber.	This.	1.19	Heavy.	.53	Cold.	1.10	Cold.	1.60
5	Law.	This.	1.41	Long.	.56	School.	2.95	School.	1.80
6	Hypnotism.	This.	1.65	You.	1.08	Eddy.	2.75	Eddy.	1.70
7	*Girl.	Yes.	1.25	Outside.	.87			Barn.	1.50
8	University.	Northwestern.	1.12	Caught.	.75	Ground.	2.83	Ground.	1.70
9	April.	May.	1.14	Northwestern.	.90	Second.	1.80	Second.	
10	*Ink.	Yes.	1.40	Late.	.60	Black.	1.54	Black.	1.60
11	*Book.	Black.	1.62	Old.	.25	Black.	2.75	Black.	1.24
12	*Psychology.	Thorndike.	1.65	This.	1.12	Class.	1.80	Class.	2.70
13	*Woman.	Old.	.77	Cold.	.76	Man.	1.80	Man.	1.60
14	*Hairy.	Hairy.	1.37	Forget.	.83			Man.	1.80
15	*Winsome.	No.	1.43	Metric.	.56	Man.	2.65	Man.	
16	External.	Yes.	1.50	Lass.	.90	Skin.	2.60	Skin.	1.17
17	*Green.	Fisk.	.60	Blue.	1.00	Grass.	1.80	Grass.	1.42
18	*Violet.	Yes.	1.47	Any.	1.00	Pansy.	1.70	Pansy.	1.66
19	*Red.	Carmine.	.87	Carmine.	.95	Black.	1.70	Black.	1.60
20	*Odor.	Carmine.	.79	Caterpillar.	1.00	Stink.	2.25	Stink.	1.50
21	Library.	Russell.	.72	Coxcomb.	.76	Lunt.	2.85	Lunt.	1.50
22	*Central.	This.	.83	Switch.	.45	Street.		Street.	1.27
23	*Native.	This.	.80	You.	1.10	Africa.	1.70	Africa.	1.62
24	*Man.	Nice.	.97	Old.	.77			Woman.	1.50
25	*Old.	Yes.	1.20	Woman.	.80	Man.	1.80	Man.	1.70
26	*Skin.	Black.	.90	Drawer.	.52	Black.	1.33	Man.	1.80
27	Six Hundred.	Yes.	1.07		1.15			Seven Hundred.	1.80
28	*Skin.	Ride.	.60	Wry.	.65	Black.	1.70	Man.	1.70
29	*Hair.	Black.	.80	This.	.64	Brown.	1.80	Brown.	1.57
30	*Tooth.	Buds.	.71	Buds.	1.10	White.		White.	1.80
31	*Page.	Three.	.85	Yes.	.47	Book.	1.80	Book.	1.80
32	*Weight.	Heavy.	.89			Iron.	1.80	Iron.	1.33
33	*Glass.	Yes.	1.04	Heavy.	.76	Bottle.		Bottle.	1.80
34	*Pound.	Heavy.	.96	Kilo.	.80	Ounce.	1.80	Ounce.	1.80
35	*Fat.	Kilo.	.76	Yes.	1.00	Man.	1.80	Man.	1.80
36	*Cook.	Meter.	.72	Quick.	.42	Mrs. Rhadge.	1.80	Mrs. Rhadge.	1.80

ceal. It will be remembered that there was a handkerchief scented with asafœdita in the drawer. When the word *odor* was given, O. gave the association *stink*. Again, when the stimulus word *weight* was given, he answered, *iron*. This, perhaps, because of the iron weight in the drawer. And when the word *hairy* was given the reply was *man*. This would appear to be on account of the picture of a hairy man on page 33 of the book, *The Native Tribes of Central Australia.*

In the case of Mr. S. it is at once apparent that for some reason he repeated words in giving associations which were manifestly not prompted by the stimulus word. This is shown by the short reaction time. The word *red* called up the word *carmine*, which was the name on one of the bottles of ink. When *winsome* was given he gave the word *metric* in .56 of a

TABLE VI.

QUANTITATIVE RESULTS OF EXPERIMENT III.

	Results Mr. S.				Results Mr. O.			
	(1st Time) Irrelevant Words.	(1st Time) Significant Words.	Reproduction Irrelevant Words.	Reproduction Significant Words.	(1st Time) Irrelevant Words	(1st Time) Significant Words.	Reproduction Irrelevant Words.	Reproduction Significant Words.
Mean.	1.01	1.11	.75	.76	1.84	2.21	1.66	1.78
Mean Variation.	.26	.20	.26	.15	.22	.65	.18	.38
Difference in Means.	+.10		+.01		+.37		+.12	
Extremes { Long.	1.65	1.65	1.08	1.12	2.95	2.75	3.50	2.70
Extremes { Short.	.65	.60	.50	.25	1.10	1.33	1.17	1.24
Maximum Range.	1.05		.87		1.85		2.33	

second. This word he evidently had in mind as his next association word. *Winsome* was followed by the stimulus word *external*, the association given for this being *lass*, we had reason to believe that this was the actual association called up by the word *winsome.* By turning to the written directions the phrase, 'winsome lass' will be found. An examination of the quantitative results of the experiment shows that Mr. S. had association words in mind before the stimulus word was given. This has greatly reduced the average time, both for the irrelevant and the significant words. The difference in the association time of the irrelevant and the significant words of Mr. O. indicates the emotional tendency of a significant stimulus word, whether or not the subject tried to alter his first associations.

Our judgment on this last experiment was (1) that both Mr. O. and Mr. S. had been in the room and had performed the series of acts, (2) that Mr. O. did not try to conceal, (3) that Mr. S. made an attempt to conceal.

Our conclusions regarding the general feasibility of the association reaction method in mental diagnosis are as follows:

I. Accurate judgment is reasonably certain, in event the situation is so controlled that the diagnosis take the simple form of determining between two possible alternatives, the exact motive of which is definitely known by the experimenter; our results in this regard have been in substantial agreement with those given by Messrs. Yerkes and Berry.[1]

II. In such cases it is probable that the diagnosis will be so simple that a third party who has observed the experiment will be in a position to draw right conclusions.

III. Knowledge of the association reaction method on the part of the subject, though he attempts to utilize it in concealing his relation to the experiment, does not make a correct diagnosis impossible.

IV. The difficulty of accurate diagnosis increases in proportion as the advantage which the experimenter has over the subject is gradually restricted, and the number of possible diagnoses increased. We see no reason why the situation might not conceivably be so complicated that accurate diagnosis would *ipso facto* be impossible.

[1] Cf. *The American Journal of Psychology*, January, 1909, p. 226.

BINOCULAR RIVALRY.

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In a former report on binocular rivalry[1] the average length of the normal rivalry phases for 10 mm. squares was reported to be 1.89 seconds with an average variation of .5 second. In the experiments upon which this report was based the stimuli — red and green squares with black lines running diagonally across them — were mounted upon a stereoscopic slide and placed in the stereoscope so that the red square was presented to the right eye and the green square to the left eye. The background of both squares was a uniform black. The length of time each field remained in consciousness was registered by means of electric keys connected with the recording pens of a kymograph drum. The observations were made upon myself ten years ago.

Within the last year I have repeated the experiments under the same conditions except that the squares were crossed by vertical and horizontal instead of by diagonal lines; vertical on the green and horizontal on the red squares. These squares were mounted as before on a uniformly black stereoscopic slide. Registration of the length of the phases of the rivalry was made upon a revolving drum by means of keys electrically connected with pens, one for each hand. The right key was pressed down when the right (red) field was in consciousness and the left key when the left (green) field was in consciousness. The kymograph drum carried a tuning fork marker which registered the time in hundredths of seconds.

Three hundred changes in the rivalry showed an average phase length of 1.84 seconds with an average variation of .5[2]

[1] 'On Inhibition,' Vol. III., No. 1, Monograph Supplement, PSYCHOLOGICAL REVIEW.

[2] There is considerable variation in the lengths of the rivalry phases for different individuals under the same conditions. This was found to be true in previous experiments. See also 'A Study of Retinal Rivalry in the After-image,' by Alma de Vries and Margaret F. Washburn, in the January number of the *American Journal of Psychology*, p. 131.

second. Between 54 and 55 changes occurred in the red and green fields during every 100 seconds, practically the same rate of fluctuation in the rivalry as that of ten years ago. The following account gives the time of the rivalry phases and rate of fluctuation under new conditions not yet reported upon.

The Effect of Variation in the Size of the Stimuli.

Five slides were prepared like the one used in the previous experiment except that they varied in size from 3 mm. to 30 mm. One hundred changes were recorded in each case. Tables I. and II. give the results

Table I.

Size of Squares.	Length of Time of 100 Changes.	Average Length of Phase.
3 mm. squares.	317 seconds.	3.17 seconds.
8 " "	240 "	2.40 "
10 " "	184 "	1.84 "
20 " "	152 "	1.52 "
30 " "	130 "	1.30 "

The rate of fluctuation per 100 seconds is given in Table II.

Table II.

3 mm. squares.	31–32 changes in 100 seconds.
8 " "	41–42 " " " "
10 " "	54–55 " " " "
20 " "	65–66 " " " "
30 " "	76–77 " " " "

In one of the experiments already reported the stimuli were of different sizes for the two eyes. A 10 mm. square was presented to the right eye, a 5 mm. square to the left eye. This change in the size of the stimuli resulted in lengthening the average phase for the smaller square by .34 of a second. The normal rivalry, when 10 mm. squares were used for both eyes, showed 53–54 changes in 100 seconds and an average phase length of 1.89. But when one of the squares was reduced to 5 mm., the rivalry was reduced to 44–45 changes in 100 seconds and the phase corresponding to the smaller square was lengthened to 2.23 seconds while the phase for the larger square remained the same.

The effect of increasing the size of the squares is analogous to that which resulted from increasing the light intensities of the stimulating squares. Experiments with the 10 mm. squares showed that the rate of fluctuation in the rivalry increased and decreased with the increase and decrease of the intensity of the lights used to illuminate the squares.[1] These lights varied from a very dim light just sufficient to make the lines upon the squares clearly perceptible to that of a 100 c.p. arc light. Table III. is compiled from the data of previous experiments.

TABLE III.

Light Intensities.	Rate per 100 Seconds.	Phase Length.
Dim light.	24–25	4.24 seconds.
16 c.p. at 400 cm. (Inc.).	46–47	2.15 "
16 c.p. " 50 " "	58–59	1.70 "
80 c.p. " " " "	67–68	1.48 "
100 c.p. " " " (Arc).	83–84	1.20 "

In the data given above the light intensities for each eye were equal in every case. But if the squares were unequally lighted, it was found that the phase length corresponding to the brighter square was lengthened instead of shortened. In every case where there was an equal increase in the light intensities of both squares there was an increase in the fluctuation of the rivalry and a decrease in the phase lengths, but when one square only was increased in light intensity, the rate of fluctuation was decreased, due to the increase in the phase length of the brighter square.

The Effect of Distinctness of the Images upon Rivalry.

In the above experiments the stereoscopic slides were so adjusted that the retinal images were brought to a sharp focus on the retinæ. In order to determine what effect a change in the distinctness of the images would have upon the rivalry 400 changes were measured, 200 in focus and 200 out of focus. The slide with the 10 mm. squares was used.

In Focus.

200 changes in 363 seconds.
Average length of phase 1.81 seconds.
Average variation .5 second.
55–56 changes in 100 seconds.

[1] 'On Inhibition,' page 39, Sec. 9.

Pushing the slide out 70 mm. from its position of sharpest focus, or until the black lines on the squares were just distinguishable, the following results were obtained:

OUT OF FOCUS.

200 changes in 748 seconds.
Average length of phase 3.74 seconds.
Average variation .5 + second.
26–27 changes in 100 seconds.

PERIPHERAL RIVALRY.

So far, only the rivalry of the central parts of the retinæ has been considered. For comparison of central with that of peripheral rivalry the slide with the 8 mm. squares was used. When placed in position in the stereoscope for clearest vision the slide was found to be 200 mm. from the principal foci of the eyes. In order to keep the distances from all parts of the slide to the retinæ uniform when the fixation points were changed, each half of the slide was mounted upon sections of spheres whose radii were 200 mm. The 8 mm. squares were each placed in the central parts of the sections and around each were placed eight fixation points, two to the right, two above, two to the left, and two below. All the points to the right and left of the squares were in the horizontal plane which cuts the eyes into upper and lower halves, and all the points above and below were placed in the median planes of each eye when they fixated the centers of the squares. The arrangement was such that there was a fixation point directly to the right, directly above, directly to the left and directly below the center of each square and at an angular distance of 3.6° from the centers. Similarly a second set of fixation points was placed at a distance of 7.2° from the centers.

When placed in the stereoscope the red square and the eight fixation points of the right field and the green square and eight fixation points of the left field were superimposed. Fixation of the points to the right resulted in projecting the images of the squares upon the temporal half of the right retina and upon the nasal half of the left retina, the right square upon the right eye and the left square upon the left eye respectively. If the first point were fixated the images of the squares were approximately 3.6° from the foveæ and upon corresponding areas of the retinal surfaces. The rivalry then took place upon the peripheral parts of the retinæ. If the second point to the right were fixated then the images of the squares were approximately 7.2° from the foveæ. When the points to the left were fixated the images were thrown upon the left halves of the eyes; when the points above were fixated the images were thrown upon the upper halves; when the points below were fixated the images were on the lower halves of the eyes and at the distances from the foveæ represented by the distances of the fixation points from the centers of the squares. This gave an opportunity to measure eight sets of rivalry images upon the peripheral parts of the retinæ, and to compare the rates of rivalry and the lengths of the phases with that upon the central parts of the eyes. The rivalry was found in every case to be very much slower on the periphery and consequently the phases of the fluctuation were very much lengthened. The phase length was more than doubled while the rapidity of the fluctuations was less than one half that of normal central rivalry. The zones further from the foveæ gave a slower rivalry rate than the zones nearer the fovea. Table IV. gives the results of this experiment.

TABLE IV.

Central Rivalry, 8 mm. Squares.

		Number of Changes Recorded.	Total Time of Changes.	Average Length of Phases.
		62	149 seconds.	2.44 seconds.
Peripheral Rivalry, 8 mm. Squares.				
Fixation points 3.6° from center of squares.	Right.	28	134.8 seconds.	4.80 seconds.
	Up.	40	236.9 "	5.92 "
	Left.	28	120.2 "	4.30 "
	Down.	40	208.7 "	5.22 "
Fixation points 7.2° from center of squares.	Right.	24	121.5 seconds.	5.06 seconds.
	Up.	22	131.6 "	5.98 "
	Left.	22	125.7 "	5.71 "
	Down.	22	127.2 "	5.78 "

The average variation for the central rivalry was .5 second; for peripheral rivalry it was 1.1 seconds. The general average of the length of the phases for the zones 3.6° from the foveæ was 5.05 seconds, while that for the zones 7.2° from the foveæ was 5.64 seconds. The rate of rivalry per 100 seconds was as follows:

TABLE V.

Central Rivalry.			40–41 changes in 100 seconds.
Peripheral Rivalry.	Fixation point 3.6° from center of squares.	Right.	20–21 " " " "
		Up.	16–17 " " " "
		Left.	20–21 " " " "
		Down.	19–20 " " " "
	Fixation point 7.2° from center of squares.	Right.	19–20 " " " "
		Up.	15–16 " " " "
		Left.	17–18 " " " "
		Down.	17–18 " " " "

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF WELLESLEY COLLEGE.

(COMMUNICATED BY PROFESSOR ELEANOR A. McC. GAMBLE.)

I.

Intensity as a Criterion in Estimating the Distance of Sounds.

BY E. A. McC. GAMBLE.

The purpose of this study was to find evidence for or against the ordinary assertion that the distance of a sound is estimated mainly on the basis of its intensity. The investigation falls into two divisions. In the first division the evidence was sought by an indirect method. The experiments constituted an attempt to determine the just noticeable divergences from a number of standard distances. The argument on which the work was based is as follows:

1. The intensity of a sound varies inversely as the square of the distance. If the relative distance of two sounds is expressed by the ratio 9:16, then, other things being equal, their relative intensity must be expressed by the ratio 4:3.

2. If the relative distance of sounds is judged in terms of their intensity, then a just noticeable difference in distance may be expected to imply a just noticeable difference in intensity. This means that if a sound at a distance of 9 feet is just noticeably nearer than the same sound — *i. e.*, a sound produced by the same stimulus — at a distance of 16 feet, then the just noticeable difference in intensity must be one third of the intensity of the weaker stimulus. It should be noted, however, that it is conceivable that intensity is the main criterion in judging only gross differences in distance, and that variation of overtones is highly important in judging liminal differences.

3. If Weber's law holds for sounds in general, then a just noticeable variation from the intensity of two or more standard sounds produced by the same stimulus must be approximately

the same fraction of the total intensity of these standard sounds. If we grant all these premises, then we must infer that whenever we find a just perceptible difference between distances of the same sound, we shall find that the intensities as determined by these distances always bear about the same ratio to one another. Several investigators have found that one third is the 'Weber's law fraction' for the noise of small falling bodies. Let us suppose that this fraction holds for sounds in general, whether noises, tones of different pitches, or clangs. Then one sound will always be just noticeably nearer and louder than another when the intensity ratio is approximately 4 : 3. Whenever we find a just noticeable difference in distance, we shall find that the distances are as 9 is to 16 and that the intensities are as 4 is to 3.

The fraction one third has been taken only for purposes of illustration. It is more than probable that it does not apply to sounds at large. Let us then represent two just noticeably different distances of the same sound by m and n. Now on the assumptions of the foregoing argument, one will find that the ratio which holds between m^2 and n^2 always holds between the squares of any two just noticeably different distances of this sound. Therefore, if in various instances of just perceptible difference in distance, we find no such equality of ratios, then either Weber's law does not apply to the sound-stimulus used, or else intensity is not the main criterion in estimating liminal differences in its distance, or else the validity both of Weber's law and of the intensity-criterion are ruled out in the particular case. If, on the other hand, the equality of ratios is found to hold repeatedly, then there is a strong presumption both that Weber's law does apply to the stimulus in question, and that intensity is indeed the main criterion in estimating liminal differences in the distance of this particular sound. Of course, a negative conclusion will be warranted only if the experimental conditions are good or if the results are so numerous that the effects of accidental variations in the conditions may be supposed to cancel one another.

In the second division of the experiments the method was more direct. The subjects, who knew little or nothing of the object of the experiments, were required to describe repeatedly the difference in two sounds, which varied sometimes in initial intensity and sometimes in distance, or which were altered in intensity when they were supposed to be altered in distance. The purpose of these experiments was to determine the degree to which the subjects were likely to confuse one difference with the other. For brevity, the experiments of the first division will be called the 'Weber's law experiments,' and those of the second division will be called the 'confusion experiments.'

Experiments of both divisions were made in the academic years 1897–1898, 1898–1899 and 1899–1900.[1] Throughout the experiments the sounds were given with a telephone receiver and the distance from the subject's ear was measured upon a board supported at such a height that the opening of the receiver when held close above it — with only the experimenter's fifth finger inserted between receiver and board — could be approximately on a level with the opening of the ear of the subject whose chair could be raised or lowered according to her height.

In the first year of the experiments, this board was about two inches wide and was raised on supports about 16 inches from a table 36 inches wide. In the last two years the measuring-board was only half an inch thick, had a bevelled edge graduated in half-centimeters, and was held, edge-up, by slender supports which rose from the floor. The room is which the experiments were made is 47 feet long and 15½ feet wide. The subject's end of the board was about 10 feet from one end of the room and the board ran parallel with the longer walls of the room and practically in the center crosswise. From this end of the room all furniture unnecessary to the experiments was cleared away. The room was reasonably but not ideally quiet. Rude as the conditions thus indicated may seem, by far the most serious drawback to the experimental conditions consisted in the nature of the sound itself. The sound used was not a telephone-click; the click was considered too weak and irregular ('sputtering') for the purpose. The sound employed

[1] The experiments were made in the three successive years by Miss Louise S. McDowell, Miss Amy G. Whitney and Miss Inez Mathews, who were all students in a second-year course in psychology. The work was directed at different times by Professor Calkins, by Dr. James E. Lough and by the writer.

was a 'musical tone' produced by passing the alternating lighting-current of the college, or a secondary current induced by this current, through the telephone receiver. The coil on the magnet of a discarded piece of apparatus was thrown into the circuit by way of resistance. When the primary current was used, the opening of the receiver was plugged with cotton to reduce the loudness of the sound. The secondary current produced a sound which erred in the direction of being too weak, but the intensity could be further reduced at will by sliding out the induction-coil. The great defect in the experimental arrangements consisted in the fact that the intensity of the sound varied considerably from one sitting to another according to the number of electric lamps through which the current was passing. A minor difficulty consisted in the 'humming' of the induction-coil. In the first year of the experiments no induction-coil was used; diminution in the initial intensity of the sound (*i. e.*, diminution in its loudness near its source and not as determined by distance) were produced by screening the receiver with the hand. In the second year the induction-coil was used only in the confusion experiments. In half of these experiments the initial intensity of the sound was altered by sliding out the coil; in the other half the screening-method was used. In the third year, the induction-coil was used in all the experiments because in consequence of a change in the dynamo supplying the alternating current, the sound produced by the primary current had altered to a harsh bray. In all cases, the experimenter made and broke the circuit by means of a push-button on a shunt.

Throughout the experiments reasonable precautions were taken to cancel the effect of the time-error, the expectation-error, and the like. (The experiments were scarcely elaborate enough to merit a detailed account of program.) At least in the last two years, the sounds to be compared were given two seconds apart and the subject was required always to judge the sound with reference to the first as a standard. The subjects with one exception were all students in a first-year course in psychology. In the confusion experiments they were blindfolded, but in the other experiments they were simply required 'not to look.'

The Weber's-law experiments of the first two years led to no very definite outcome. The work of these two years consisted in skirmishing to hit upon the divergence from a number of standard distances which would give 80 per cent. of right cases in comparing the two distances. Perforce, a number of different subjects were used — three in the first year and four in the second and only one of them trained — because no one subject was available for extended work. With each subject several different distances were used as standards, ten different distances were compared with each standard, and only ten comparisons were made with each pair of distances. (Comparisons of the same standard with different distances were interspersed with one another.) In view, on the one hand, of the variations which must arise under experimental conditions of so rough a nature, and in view, on the other, of the scattering of the experiments over so many subjects and distances, it is scarcely surprising that little regularity appears in the figures obtained. The results of the second year are rather less regular than those of the first. The latter may be summarized as follows, if one averages the results of the three subjects and if one assumes that about 80 per cent. of right judgments indicates a liminal difference between two stimuli:

Standard distances in cm.:	20	30	40	60	80	100	120	140	200	300
Ratio between intensity at standard and distance just noticeably greater:	$\frac{100}{69}$	$\frac{100}{74}$	$\frac{100}{86}$	$\frac{100}{76}$	$\frac{100}{61}$	$\frac{100}{82}$	$\frac{100}{82}$	$\frac{100}{72}$	$\frac{100}{86}$	$\frac{100}{80}$
Distance just noticeably less:	$\frac{100}{111}$	$\frac{100}{133}$	$\frac{100}{111}$	$\frac{100}{111}$	$\frac{100}{120}$	$\frac{100}{145}$	$\frac{100}{110}$	$\frac{100}{160}$	$\frac{100}{125}$	$\frac{100}{123}$

These figures look very much like the sort of results which might very well be obtained from unpracticed subjects, under rough conditions, if Weber's law applied to the stimulus and the fraction were about one fifth.[1]

In the third year only two subjects were employed — L., a student in a second-year course in psychology, and G., the writer. The plan of the experiments was to find a pair of distances which would give, when compared, from about 75 to 80 per cent. of right cases, and then to work with another or other

[1] M. Wien found the fraction one fifth to hold good for the tone *a* at about 220 vibrations. For the corresponding *e′*, he found the fraction to be one sixth and for the corresponding *a′*, he found the fraction to be one eighth. This statement is made on the authority of Ebbinghaus, *Grundzüge der Psychologie*, 1905, p. 302.

pairs of distances which would involve the same ratio between the intensities of the sounds. The results of these experiments are given in the following table:

Results of Third Year of Experiments in Determining Liminal Differences in Sound Distances.

Subject.	Set of Experiments.	Distances Compared, cm.	Ratio of Corresponding Intensities.	Number of Comparisons.	Right Cases, Per Cent.
G.	1	20 and 25	156 : 100	224	91
	2	20 " 22.5	127 : 100	250	89
	3	30 " 35	136 : 100	425	89
	4	30 " 33	121 : 100	475	86
	5	30 " 32	114 : 100	400	66
	6	30 " 32.5	117 : 100	475	76
L.	1	36 and 43	143 : 100	825	71
	2	36 " 46	163 : 100	150	99
	3	36 " 43.5	146 : 100	825	74
	4	26 " 31	142 : 100	725	72
	5	46 " 55.5	146 : 100	325	77

In the case of G., the effect of practice made the '80 per cent. point' hard to find. A difference in distance which at first promised to give far less than 80 per cent. of right judgments would toward the end of a set of comparisons yield far more. Thus, only one difference was finally demonstrated to be liminal, viz., the difference between 30 and 32.5 cm. That this difference was really liminal is shown by the fact that either an excess or a lack of half a centimeter made a great difference in the number of right judgments obtained. It is noteworthy that the intensity-difference implied by this liminal distance-difference is about one fifth of the smaller stimulus-intensity.

The results obtained from L. certainly seem, in so far as they go, to prove the point at issue. In two cases in which the intensity-ratio between the sounds compared was approximately the same, the percentage of right cases was almost exactly the same, and in two other cases in which the intensity-ratio was exactly the same the percentage of right cases was approximately the same. Moreover, the number of comparisons in each case was respectably large. The fraction which measured the just noticeable difference was, however, very large, amounting to two fifths of the smaller intensity.

On the whole the results of the first division of the experiments suggest although they do not prove, that the estimation of sound-intensities in general follows Weber's law, and also forms the basis of the estimation of liminal differences in distance.

The confusion experiments were both simpler and much more fruitful of results. Their conduct may conveniently be described in connection with the following table which summarizes the data obtained:

Results of 'Confusion Experiments.'

Year 1.

Difference in Stimuli.	Cases.	Distance in Cm.											
		30			60			120			240		
		Judgments Per Cent.			Judgments Per Cent.			Judgments Per Cent.			Judgments Per Cent.		
		N	*F*	*E*	*N*	*F*	*E*	*N*	*F*	*E*	*N*	*F*	*E*
Second sound louder.	30	87	7	7	80	7	13	73	13	13	40	37	23
Second sound softer.	30	—	100	—	—	97	3	17	83	10	37	53	10

Year 2.

Relation of Stimuli.	Method.																			
	Screening.										Use of Induction Coil.									
	Distance.										Distance.									
	15 cm.					30 cm.					15 cm.					30 cm.				
	Cases.	Judgments Per Cent.				Cases.	Judgments Per Cent.				Cases.	Judgments Per Cent.				Cases.	Judgments Per Cent.			
		N	*F*	*E*	*U*		*N*	*F*	*E*	*U*		*N*	*F*	*E*	*U*		*N*	*F*	*E*	*U*
Second sound louder.	152	73	5	22	—	153	72	9	19	—	162	65	7	27	1	142	65	8	26	1
Second sound softer.	125	6	78	14	1	124	6	80	12	2	132	9	77	14	—	116	5	84	10	—
Both sounds loud.	31	26	3	71	—	32	16	3	81	—	36	19	8	72	—	27	11	15	74	—
Both sounds soft.	41	15	5	81	—	42	12	10	79	—	45	4	20	73	2	139	10	21	69	—

Year 3.

Difference in Stimuli.	Cases.	Judgments Per Cent.					
		N	*F*	*L*	*S*	*E*	*U*
Second sound nearer.	893	22	5	29	8	32	4
Second sound farther.	1159	5	28	6	29	27	5
Second sound louder.	509	21	2	35	8	30	4
Second sound softer.	712	1	28	2	52	13	3

The only abbreviations which need explanation are the initials in the columns under 'judgments per cent.' *N* means that in a certain percentage of a given set of comparisons (of which the number is given under 'cases'), the second sound was judged to be the 'nearer' of the two. In the same way, *F* stands for 'farther,' *L* for 'louder,' *S* for 'softer,' *E* for 'equal' or 'same,' and *U* for 'uncertain.' All cases in which for any reason the subject failed to pass judgment are gathered under *U*. In the figures for each of the three years, the results obtained from all the subjects are massed (not averaged) as if they had been obtained from one subject. In the first year, the subjects numbered three, and sixty cases — twenty for each subject — were obtained with each distance — thirty with the second sound louder and thirty with it softer. In the second year, the subjects numbered fifteen and each subject made about twenty-five comparisons with each method of altering the initial intensity of the sound (screening and use of the induction-coil) at each distance — about one hundred comparisons in all. In the third year of work, thirty-two subjects were used, and each subject made about one hundred comparisons. In this year no attempt was made to compare the results which might be obtained at different standard distances. The subjects of the confusion experiments were all first-year students of psychology, but those of the first year of work were the same three who had served as subjects in the experiments of the Weber's law division.

In the first two years of the experiments, the subjects were led to think that only the distance of the sound would be varied, whereas, if any difference at all were made in the stimulus, only the initial intensity of the sound was actually varied. As important a point as any which the figures bring out is that the subjects did not detect the imposition which was practiced upon them. (To this rule there are one or two exceptions which are of little practical importance since the subjects' misgivings, which never amounted to more than suspicion, were due to same carelessness or misadventure on the part of the experimenter.) The figures show that in the great majority of cases, difference in intensity produced the impression of difference in distance — in such wise that the louder sounds were interpreted as the nearer — and that equality of intensity produced the impression of equality in distance. Although the sounds were all really equal in the respect in which the subjects judged some of them to be different, and although in the second year some pairs of sounds were given which were really equal in all respects and were judged to be equal, yet no preponderating tendency appears to err in the direction of passing too many equality-judgments. As regards the experiments of the second year, it should be noted incidentally that a greater change of intensity seems to have been produced by screening the telephone than by sliding out the coil. The coil was moved from 3 to 5 cm. according to the strength of the current on the particular day. Since the subjects did not detect the very simple ruse employed, the greater number of right cases obtained with the screening-method can scarcely be laid to any peculiar muffling of the sound.

In the first two years the subjects were under the influence of suggestion when they interpreted differences in intensity as differences in distance. The effect of suggestion might conceivably be great enough to make the subject's image different 'distance-qualities,' if such marks ever exist, with different sound-intensities. In the third year, the subjects were entirely free from the effect of suggestion, as regards the point at issue. They were required simply to tell *how* the sounds in the pairs given them for comparison differed. It was suggested merely that these sounds might differ in distance, in loudness, or in pitch. The statement was purposely made in such a form that the unreflecting subject could think that 'the same' sound might be nearer without being louder. As a matter of fact, the difference was sometimes one of distance, and sometimes one of initial intensity, as controlled by the use of the induction-coil. The extent to which the coil was pulled out and the distances at which the sounds were given differed somewhat from one sitting to another according to the strength of the primary current, but at the same sitting only two distances and two intensities were compared. The one hundred comparisons demanded of each subject were ordinarily made at one sitting. The distance at which the nearer sound was given rarely exceeded 30 cm. The experimenter meant to work with superliminal differences both of distance and of intensity, but the figures indicate that the differences were in general not more than liminal.

The third part of the table shows that the judgments of 'nearer' and 'louder,' and of 'farther' and 'softer' were practically interchangeable. The subjects showed a marked tendency, however, to say more often that a sound was louder when it was louder only in virtue of being nearer, than to say that it was nearer when it was merely louder, and so also, *mutatis mutandis*, with the judgments of 'softer' and 'farther.' This fact may be interpreted in three different ways: (1) If one beg the question at issue in this investigation as a whole, one may say that when the initial intensity and the distance of a sound are both unknown, one's attention dwells upon intensity just because one is more accustomed to making intensity the clue to distance than to making distance the clue to intensity. In view of the whole trend of the confusion experiments this seems to the writer the natural explanation of the tendency towards judgments of 'louder' and 'softer,' and the tendency itself seems to be a striking confirmation of the ordinary belief which is here in question. The fact that there were any judgments at all of 'nearer' and 'farther' is, in view of the results of the first two years, sufficiently explained by suggestion. (2) If, however, one believes in distance-qualities, one may say that the subject is more likely to overlook the difference in such qualities than to imagine one. (3) Finally, the tendency in question may (conceivably) be due to the fact that the subjects were reflecting enough to realize, at least dimly, that nearness implies loudness in a way that loudness does not imply nearness, so that the intensity-judgment has a double chance of being right. There are, however, few recorded remarks or other data which lead one to believe that the subjects at large clearly distinguished between the loudness of a sound as determined by its distance and its initial loudness. The failure of our subjects to make this distinction must not be interpreted as telling against the practical value of intensity as a clue to distance. One may associate place-images with intensities for practical purposes in ordinary life — as, for example, when one is estimating the distances of a railway-train which one wishes to catch — and yet, in spite of these serviceable associations, one may fail to think clearly about the two conditions of intensity on occasions when intensity and distance are alike unknown and are equally uninteresting to the natural man.

Three additional remarks must be made: (1) In these records — for the confusion experiments of the last year — there is a sprinkling of cases in which the same sound was judged to be both nearer and louder or farther and softer, and a still smaller number of cases — about a dozen out of 3,273 — in which the same sound was called both nearer and softer or farther and louder. These double judgments are reckoned in the table as if the first judgment expressed had been the only one. They may be interpreted either for or against the assumption of a sharp distinction on the part of the subjects between the two conditions of loudness.

(2) Differences in pitch or musical quality were very rarely alleged by the subjects — not nearly so often indeed as differences in duration, which were purely accidental. No correlation can be made out between the pitch-differences mentioned and differences in distance.

(3) Curiously enough, throughout the confusion experiments of the three years, the number of right and of pseudo-right cases was noticeably greater when the second sound was the weaker of the two compared. Thus the ordinary time-error was consistently reversed. The writer cannot explain this fact.

The results as a whole offer considerable evidence for, and little or no evidence against, the ordinary belief that intensity is the main criterion in estimating the distance of a sound. The writer is not prepared to explain the divergence between the results of these experiments and the results obtained by Professor Von Kries, in support of a distance-quality or mark, but must be content to point out that the results here presented are the more numerous and that they were obtained by a method which was scarcely less precise than the method of Von Kries.[1]

[1] See Von Kries, 'Ueber das Erkennen der Schallrichtung,' *Zeit. f. Psych. u. Phys. der Sinnesorgane*, I. (1890), especially pp. 246–247.

II.

The Perception of the Distance of Sound.

BY DANIEL STARCH, Ph.D.,
With the Assistance of ANNE L. CRAWFORD, B.A.

The object of this experiment was to determine the accuracy of perceiving the distance of a sound in a representative group of directions, to discover whether this accuracy varies in different directions, and to find the factors on which the perception of distance is based.

It was necessary for this purpose to employ as constant a stimulus as possible. The telephone click had been used in some preliminary experiments but it was not sufficiently uniform. A small wooden drop hammer, 3 cm. long by 2 cm. in diameter, was devised which produced a satisfactory stimulus. The handle of the hammer, 20 cm. long, was set in a pivot so that it could be swung freely up and down. The hammer was held by a catch-spring from which it could be released easily and quietly and dropped nine centimeters upon a wooden block covered with chamois. The block, the pivot of the hammer and the catch-spring were all mounted on a small bar of wood which served as a convenient handle in operating the hammer. The sound thus produced was constant and of sufficient intensity to be readily perceived.

As a guide in determining the distances of the stimuli, a narrow bracket bearing a centimeter scale and projecting toward the center of the room, was fastened to the wall at the level of the observer's head. The room was an unceiled eight-sided steeple room, two and a half meters in diameter.

The method of the experiment was as follows: The observer sat with closed eyes on a stool in the center of the room so that his ears were on level with the hammer. The stimulus was first given at the standard distance, one meter from the center of the observer's head, and then approximately two seconds later at a certain interval, say fifteen centimeters, nearer or farther. The observer then gave his judgment of 'nearer' or 'farther' comparing the second sound with the first. Two of the observers, G. and S., made after each trial a brief introspective statement of the basis of judgment. The other observers did so only occasionally. In this manner twenty-five judgments were obtained in succession for a given direction. If more than 84 per cent. were correct the next smaller interval, in this case ten centimeters, was used and if less than 68 per cent. were correct the next larger interval, twenty centimeters, was tried for the same direction. These percentages were empirically found to be the widest limits on the basis of which to calculate safely, by Fullerton and Cattell's table, the threshold of difference necessary to have 75 per cent. of the judgments correct. The series of distance intervals was three, five, ten, fifteen, . . . forty centimeters. The total number of cases for a given position in which the second sound was nearer, and of those in which it was farther, were equal, but the cases followed in irregular succession.

Thirteen directions, all in the right half of the horizontal plane, were tested: 0°f (*i. e.* straight in front), 15°rf, 30°rf, 45°rf, 60°rf, 75°rf, 90°r, 75°rb, 60°rb, 45°rb, 30°rb, 15°rb and 0°b. The stimulus hammer always remained in the same position, and, in order to test the different directions, the observers turned to the required positions. These were determined by means of Titchener's sound cage which was suspended at the center of the room. The direction were tested in succession in the double fatigue order, taking twenty-five judgments at a time for one direction. The sittings were about forty-five minutes long.

The results are presented in Tables I. and II. The figures in the tables represent in centimeters the distances which the second sound was required to be nearer or farther than the standard in order that it might be perceptibly nearer or farther. For example, the first figure in Table I. means that for G. the second sound had to be 20.8 cm., nearer or farther than the first or standard sound in order to be noticeably nearer or farther. Table I. contains the measurements obtained from G. and S., the two experienced observers. G. is associate professor of psychology and S. is the writer. The former had no acquaintance with the problem whereas the latter had planned the investigation. Each gave 100 judgments for each direction, in all 2,600 judgments. Table II. contains the data obtained from the six untrained observers, each giving 50 judgments for each direction, altogether 3,900 judgments.

TABLE I.

	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
G.	20.8	17.8	19.7	16.6	20.0	16.1	16.3	21.5	20.0	19.6	18.7	16.2	19.1
S.	11.7	12.4	11.5	14.5	10.9	15.0	9.8	12.8	9.6	11.6	12.7	9.7	8.2
Av.	16.2	15.1	15.6	15.5	15.4	15.5	13.0	17.1	14.8	15.6	15.7	12.9	13.6

TABLE II.

	0°f	15°rf	30°rf	45°rf	60°rf	75°rf	90°r	75°rb	60°rb	45°rb	30°rb	15°rb	0°b
R.	13.5	12.7	14.3	16.3	13.2	13.0	16.4	13.0	15.6	11.2	16.8	11.9	12.9
Su.	18.2	19.3	15.9	18.7	18.7	15.5	12.2	11.1	12.2	10.5	16.0	9.8	12.0
J.	7.9	11.0	6.8	7.1	8.8	3.7	3.5	2.7	3.2	5.0	3.3	5.3	2.7
K.	16.7	16.6	22.4	15.1	25.2	26.1	16.8	23.2	26.3	21.6	30.0	17.0	24.5
C.	18.8	22.4	15.5	22.4	20.9	21.4	26.9	18.0	21.3	28.9	16.3	18.4	16.0
W.	7.6	10.0	21.7	14.7	12.2	10.2	15.9	11.0	14.7	18.1	10.9	13.4	11.6
Av.	13.8	15.3	16.1	15.7	16.5	15.0	15.3	13.2	15.5	15.9	15.5	12.6	13.3

These figures give a definite answer to the questions in whose interest these experiments were made.

First, in regard to the accuracy of perceiving the distance of sound, they show that the least perceptible difference between the distances of sounds is approximately 15 cm. when the sounds are a meter away. The averages in both tables are in the neighborhood of 15 cm. The individual records agree quite closely with the exception of the unusually accurate record of J.

The second aim was to discover whether the accuracy of perceiving the distance of sound varies for different directions in the same way in which the accuracy of the perception of direction varies for different regions. The results plainly demonstrate that the accuracy of the perception of distance is the same for all the directions tested. The averages all lie within the range of 13 and 17 cm. without indicating any uniform tendency toward greater accuracy in one region than in another.

Third, in reference to the factors on which the auditory perception of distance is based, the introspections recorded in connection with each judgment of G. and S. revealed several elements, namely, differences in intensity, in pitch, and in quality, for the different differences. The introspections accompanying each one of the 2,600 judgments of G. and S. were tabulated in order to determine the relative significance of these factors. By far the most important one is intensity. With G. 95 per cent. and with S. 92 per cent. of the judgments were said to be based wholly upon intensity. A sound was judged nearer when it seemed to be more intense, and farther when it seemed less intense than the standard. The remaining judgments were based partly or entirely upon differences in pitch and quality. But these factors did not seem to be used consistently. Sometimes the farther sound seemed higher in pitch and sometimes the nearer one. There was, however, considerable uniformity among the judgments taken at one sitting.

Visual imagery of the position of the sound was mentioned a few times and was probably only a concomitant process. The occasional introspections of the untrained observers indicated the same factors, as those mentioned by the trained observers, giving the greatest importance to intensity.

DISCUSSION.

DARWINISM AND LOGIC: A REPLY TO PROFESSOR CREIGHTON.

In his interesting paper, having the same title as this note, published in the Darwin Number, May, 1909, of this REVIEW, Professor J. E. Creighton cites my work, *Thought and Things*, as representative of the Darwinian point of view in logic, and criticises it in some detail. I am, of course, gratified that the work is honored in this way. I find, however, that Professor Creighton's criticisms are not altogether valid, and I will accordingly suggest certain considerations which in my opinion show this.

Professor Creighton has no difficulty in showing by quotations from my different publications, that I am a Darwinian, and that Darwinian conceptions have had frequent application in my work; this I am making explicit enough in a little book on *Darwin and the Humanities* now in press.[1] Nor has he greater difficulty in showing that I often take the standpoint from which experience is looked upon as an immanent self-integrating movement. But he considers these two points of view inconsistent with each other: one interprets experience 'biologically' — as a relation of organism and mind to environment — the other 'logically' or 'teleologically' (so Professor Creighton) — as a principle of internal organization and movement. The question then is this: can both of these points of view be held at once? — or does either commit us to a philosophy which excludes the other?

Evidently the first, the method and view-point of biological science, must be upheld if we are to have a theory of mental development and evolution at all. Each mind grows up in a body, and both mind and body are in environments. Experience requires things and situations: its own movement establishes and utilizes what we call the 'trans-subjective reference.' Is the recognition of this consistent with a theory which interprets experience as a progressive organization having its own 'logic'?

Professor Creighton thinks that the latter point of view commits one to a 'teleology' which — though somewhat vague to me — seems to require the denial of the validity of a Darwinian conception of

[1] Review Publishing Co., Baltimore.

adaptation, considered as a necessary factor in the development of experience.[1]

Proceeding then to the criticism of my views, made by Professor Creighton, I may say that it is in my last work alone, the 'Genetic Logic,' that I have taken exclusively the point of view of experience. It should not be compared with the other more biological books and papers except as this difference is recognized.

In the *Genetic Logic* the attempt is made to trace out the actual movement of experience from mode to mode, all of these modes being equally 'psychic.' The result is reached that a dualism of controls, due to segregation of contents, is come upon *in experience itself*. This dualism is not injected by our interpretation, nor read in from an external point of view: *it is found by and in the process*. The important point is that by its own immanental movement into the logical mode, experience *establishes just the dualism that science adopts and employs*. In the discussion of the relation of the 'psychic' and 'objective' points of view (*Thought and Things*, I., chap. II., §§ 3, 4), I show that the latter is simply the explicit outcome of the dualism normally established when the mode of judgment or reflection is reached.[3] The scientific is simply the logical point of view made use of as deliberate method. It involves the self judging or thinking and objects judged about or observed — objects known to it as 'things.' This very dualism is the presupposition of the logical as such; and scientific method — whether its results issue in Darwinism, Lamarckism, vitalism, mechanism, teleology or any other type of theory — is

[1] He uses the expression 'genetic or teleological' as if these two terms were synonymous (p. 185).

[2] It is a conscious and deliberate difference, and cannot be looked upon as a contradiction unless it can be shown that one of the points of view is rendered invalid when one takes the other. In the *Social Interpretations* both methods are used on occasion, to supplement and confirm each other, the biological however having a very subordinate place. In the *Genetic Logic*, the standpoint of experience, the 'psychic' point of view, is consistently maintained. It is erroneous, therefore, to say (Creighton, p. 180), "Professor Baldwin's account professes to show, not how the mind becomes conscious of its own logical nature, but how that logical nature is engendered in it through the motor adjustments of the organism to material conditions." How the mind becomes [grows to be] conscious of its logical nature [or processes] is just what the *Genetic Logic* does profess to show.

[3] Instead of allowing Professor Creighton's interpretation to the effect that the 'inner and outer controls' are in my hands 'a translation into other terms of the organism and environment,' I hold that the relation of organism and environment is *a logical transformation of the dualism of inner and outer controls*.

thinking, no more and no less than thinking. In the more refined operations of thought upon ideas, the ideas are symbols of the things into which they are at any time convertible. The sciences of observation go directly to the things, to perceptions and sensations; but in both cases the control of the context, whether it be one of ideas or of things, is the same — that of a sphere *taken by the process to be foreign to itself.*

So far then from finding a contradiction between the point of view of evolution — dualistic as it is — and that of a truly psychic account of the genesis of knowledge, I find that *the latter issues in and justifies the former.* Any adequate tracing out of the progression of knowledge, within experience itself, shows it to issue in a system of judgments in which the two controls — things as 'outer' and the self as 'inner' — are found confronting each other. Reflection sublimates this dualism by erecting a mediating context of ideas; but all validities in the context and all truthful references beyond it, rest upon the fact that this mediation is dual.

What then I would insist upon is the radically unreal character of the supposed contradiction. The observation, experimentation, analysis, etc., of biological science, as of all science, are processes proper and vital to the logical mode of experience. Science is logical process proceeding under its normal and necessary presuppositions. In recognizing the externality of things — the environment — it is only following the essential movement of psychic process, which although presupposing externality, still finds it to be a meaning of contrast with the internality of the inner control, of the self. Accordingly, one may freely use the biological method and point of view (as I have done in the paper on 'selective thinking' which Professor Creighton considers very reprehensible in this respect); for this procedure only recognizes as valid, for purposes of deliberate observation, the dualism that logical experience itself establishes for all the processes of thought.[1]

Of course, the further question will be asked: Is one's final philosophical view then to be dualistic? — is logical experience to be taken at its word and as the final word? Professor Creighton, as just cited, says that I recognize only two alternatives, mechanism and apriorism; and he suggests the third, teleology. But my recognition of these two modes of interpretation is merely to cite them as horns of a

[1] It is clear then that the following statement of my view is not correct (Creighton, p. 184), "here as elsewhere the alternative for Professor Baldwin is between deriving logical principles mechanically and *finding them existing a priori*" (italics his).

dilemma both of which are to be avoided.[1] The teleological interpretation, also, taken in its ordinary sense — barring its excessive ambiguity — is also to be questioned, and for much the same reasons. These reasons I may now briefly state.[2]

1. We are only remaining true to the standpoint of experience itself in seeking to trace out the rise and development of such categories as mechanism and teleology. They arise as meanings attaching to different sorts of experience; and by them objects and situations are consistently and profitably apprehended and treated. Some experiences have a certain regularity and lawfulness: these, thus apprehended, come to mean the mechanical. In the case of other experiences, developing conation shapes the contents toward personal ends: these, so apprehended, mean the teleological. In the logical mode, these two meanings become general ways of assimilating events of one type or the other. Each is valid for its purpose, and each is restricted in its use: one means to experience just the dominance of external, the other that of internal control.

Now to use either of these as an exclusive or universal mode of interpretation is to abolish the other in its own province, and so to falsify our report of the progression of experience in which they have together arisen. The mechanical would not be mechanical but for the possession of those characters which show it to be bare of teleological meaning; it represents knowledge formed under a control which evidences itself as foreign. The teleological, on the other hand, would not be teleological but for its character as embodying the agent's control exercised in the pursuit of personal ends. Teleological processes as such are for consciousness not mechanical, and mechanical are not teleological.

I have contrasted the results of these two modes of process by using the two expressions 'knowledge through (external) control' — issuing in sequences which are mechanical in their meaning — and '(internal) control through knowledge' — issuing in sequences with which personal interest and conation are identified (*Thought and Things*, II., chap. XIV.). Unless the teleologists can show, from the movement of further experience, that there is positive justification

[1] I do not accept the term 'mechanism' as applicable to a genetic movement proper; it denotes only one of the possible naturalistic interpretations of this movement. My own interpretation, embodied in the theory of 'genetic modes,' combats the mechanical view.

[2] The following has reference also to Professor Creighton's paper read at Baltimore to which I listened. It may suggest to him some revision of that paper, since this discussion is new.

for the step,[1] they may not employ as a universal solvent the partial meaning which they favor.

2. But even if we allow the category of teleology to apply universally, it also issues in a characteristic dualism from which there is no logical escape. Ends are attained *through the mediation of ideas or facts.* Facts and ideas are not ends: 'what a man hath why doth he yet hope for?' — it is a further realization, beyond the idea or fact, that he hopes for. A conscious end is always meditated — furthered or hindered — by some fact or idea. To any teleology which involves genuine purpose, the dualism of 'fact-idea and end' — taking the form of 'means and end' or of 'hindrance to end' — is as stubborn as that of 'thinker and thing' in the domain of cognition.

To escape this difficulty, the intellectual idealist goes over to a teleology which does not involve purpose in any concrete or actual sense, while he still retains vaguely the principle of 'means and ends.' But what 'means and ends' can mean apart from an agent who adopts the means (facts or ideas) to attain the ends (results), it is difficult to see. What is really present is the actual flow of genetic process, with its great dualisms of knowledge and purpose. If we take this process for what it is, it discovers itself to experience in the two modes of organization called teleological or mechanical according as the situations of actual life present contents of one sort or the other.[2]

[1] Actually the progress of experience, both personal and racial, is away from animistic and anthropomorphic teleological interpretations of nature. Science has had gradually to achieve its birthright, only gradually establishing a conception of natural law which operates without 'teleological' interference. Just here is, in fact, in my opinion, the great service rendered by Darwinism to philosophical thought: it once for all established a natural law of adaptation.

[2] In my discussion of 'genetic series' as such (the theory of 'Genetic Modes,' *Development and Evolution*, chap. XIX., described by Professor Creighton as a sort of invalid compromise), I have pointed out that such series present both aspects, the quantitative or mechanical and the qualitative or in the large sense 'worthful': they show a form of sequence or conditioning which is not exhausted by either interpretation taken alone. Professor Creighton is, I think, in error in saying (p. 182) about this theory that 'the something new' that it recognizes as arising in a genetic series 'simply comes into the series as a miracle.' I reply: it is not a miracle except to one who has already adopted a quantitative or mechanical conception of all natural change. Such a cast-iron quantitative conception apart — why should not nature produce novelties? James and Bergson, as well as the present writer, have recently protested against the arid 'energistic' conception of 'cause and effect.' For my part, I am not willing to prejudice the case by using the terms of mechanics for such sequences; I have employed the term 'progression.' . . . Further, I do not admit Professor Creighton's claim that a genetic series, as I conceive it, in my theory of 'genetic

If this actual genetic movement, so apprehended in experience — the progressive integration of contents, as on occasion both 'factual' and 'end-fulfilling' for the agent, is what Professor Creighton means by 'teleology' — then I am with him. I prefer that term to 'mechanism,' if one is to use but one term for the entire movement. But my aim is to go further constructively, and to discover what the issue is when the movement does not stop with the *mediation by ideas* in either of these two ways — with mediation as true for knowledge, and as good for purpose — but when it goes on to apprehend the contents in a further mode of direct contemplation. The movement then goes beyond the objectification of the contents in judgments of fact and value; and reaches a higher hyper-logical immediacy.[1] My present purpose is accomplished, however, in showing how it is possible to turn the edge of Professor Creighton's criticism. I accept both the terms of the supposed contradiction. I hold that when legitimately employed both mechanism and teleology are naturalistic or empirical categories, both valid, but both restricted, in their proper use, and both superseded in a hyper-logical mode of experience.

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modes,' 'exhibits no identity throughout the different stages of the process.' On the contrary, the varying degrees of identity which it actually has for consciousness serve as motive to the transformations of the 'sameness' meaning, as traced in my book in great detail, up to the logical judgment of identity (Vol. I., chap. VIII., § 3, and chap. IX., § 5; Vol. II., chap. X.).

[1] To the development of this point much of the third volume of the *Genetic Logic* is to be devoted. In an article entitled 'Knowledge and Imagination,' Psychological Review, May, 1908, I have stated in outline the characters in virtue of which æsthetic experience appears to discharge this office.

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